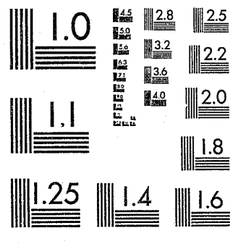

A METHODOLOGICAL EXPLORATION OF THE CRIME OPINION DATA ARCHIVE

by
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In collaboration with
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I. INTRODUCTION

This report, as one of the products of the University Center for Social and Urban Research under grant 78-NI-AX-0126, concerns the major methodological lessons which can be learned from the development, and existence, of the crime opinion data archive of the Center.

The archive contains 164 files of surveys which cover the period 1960-1978. Not all surveys are included: some have contained only one or two items pertaining to the central issues of the study, and these data were not acquired and not processed into the archive; a few surveys lack the necessary documentation, especially as regards sampling or coding information, and they have also not been included; a few surveys exist only in output form while the original computer files or I.B.M. cards had been lost, misplaced or destroyed; such surveys, by definition, are omitted.

Nor do the archival data cover the whole spectrum of possible concerns: such studies as may pertain to capital punishment, gun control, drugs, juvenile delinquency per se, organized crime or even white collar crime have not been sought in light of the initial LEAA mandate under grant 76-TA-99-0026 (of August, 1976).

The report considers, first of all, some of the main pragmatic lessons. Chapters II and III provide a brief evaluation of the methods-related issues having to do with the development, updating and maintenance of data archives of this type.

In Chapter IV, we focus on some of the main linkages between theory (broadly conceived) and methodology (broadly conceived) to establish the basic limits to, and ranges of, utilizability of the archival information.

The Chapter helps to establish the context in which the more specific questions of generalizability and comparability are placed.

Indeed, concern over generalizability as a principle and generalizability to particular populations or segments of the study population is a crucial consideration. For this reason, Chapter V discusses, in some detail, the basic sampling designs of the various surveys in the archives and seeks to evaluate the extent to which statistically reliable generalizations are appropriate, and to what extent.

A major issue, discussed in Chapter IV, is, however, not resolved: it has to do with the relevance of concern over substantive significance rather than statistical significance (only) and the relations between the two issues. This is particularly salient if we assume, as we do, that interpretability of data does not rest with an assessment of (distributions of) single variables but that the researcher's main payoff probably lies in the discovery of patterns and of changing in patterns over time.

Chapter VI, as it must, confronts both questions of generalizability and comparability. It deals with the extent to which the surveys in the archives yield data which can be generalized to particular demographic and socio-cultural segments of the populations studied. And it addresses the extent to which comparable demographic information may have been acquired by the researchers, and the degree to which it appears, in the files, in comparable formats.

Chapters VII and VIII are devoted to an assessment of some of the main substantive issues which the surveys have raised. In Chapter VII, we deal with generalized perspectives on crime: how much of a problem has it been, as perceived by the respondents, at the national, community and neighborhood level; what cause-effect reasoning seems to underlie the lay interpretations of crime as well as changes in crime rates; what steps to combat crime are seen as effective; whether or not law enforcement activities call for further funds; and how the public way of life may have been affected by crime and threat of crime.

Here, of course, we do not present any findings. Rather, the analysis concerns the patterns of questions included in the surveys with a view to determining the extent to which comparable issues have been raised so that it becomes profitable to conduct secondary data analyses across surveys and over time (given their generalizability).

Chapter VIII, in turn, provides a succinct summary of the types of questions that have been used in the surveys with regard to the police, courts and the prison system. Again: this is not a discussion of findings of any kind. Rather, it is an assessment of similarity among questions across surveys and over time, and an evaluation of the range of probes for which the coverage facilitates such comparative analyses as the research community may wish to undertake.

The last section of the report (Chapter IX), apart from highlighting the central conclusions and stating several principles which the present inquiry seems to suggest, identifies several important areas of concern in which the present data base shows particular weaknesses and which, therefore, may provide pointers to the inclusion of items, and clusters of items, in future surveys in which crime-related issues might be included.

II. ORIGINS OF THE SURVEY DATA ARCHIVE

Under LEAA grant 76-TA-99-0026 covering the period between August 18, 1976, and December 17, 1976, (subsequently extended at no cost to March 31, 1977), the University Center for Social and Urban Research was involved in an effort (a) to establish a data archive of studies, since 1960, bearing on attitudes toward and opinions about crime and some key related issues (see below), (b) to summarize, in descriptive form, the main results, and trends to which these studies pointed, (c) to produce, and transmit to LEAA, a computer tape of the data as well as a (noncomputerized) document identifying each study in the data bank and each relevant question in each such study.

The 4-month study, eventually stretching over a 7½ month period due to the no cost extension of the grant duration, was carried out at a total expense, to LEAA, of $24,990 of which, discounting University overhead, $20,104 were attributable to direct research costs. To meet the grant stipulation to locate, acquire and process public opinion studies concerning crime issues, we established some simple criteria.

For one, there were substantive criteria for exclusion of some classes of studies and these were agreed upon between us and LEAA. Thus, we did not conduct a systematic search for studies which may have focussed, either entirely so or partially, on many important crime problems: juvenile delinquency, gun control, capital punishment, drug abuse and drug trafficking, gambling, prostitution (and other "victimless" crimes), rape, civil disorders (riots), acts of terrorism, white collar crime, organized crime.

In so far as some of the studies in the data archive which eventually resulted included items bearing on these issues we sought to acquire such studies as well. But the main point is this: There is no systematic effort to identify, acquire and process research data which may have placed as their primary or sole purpose, an emphasis on the substantive problems which were not within the purview of our inquiry.

Instead, our search emphasized perspectives of crime as a national, community and neighborhood problem, attributions of causes, defensive measures contemplated or taken, recommendations for ways of dealing with crime and the like.

It also stressed data having to do with the nation's opinions about the police, the courts and the court system, prisons and jails.

Second, our search was limited to research carried out in the larger body politic of the nation--whether at the level of cities, Standard Metropolitan Statistical Areas, states or the nation as a whole.

This means, by definition, that we did not attempt to acquire data which may reflect the views of various significant actors in the nation's criminal justice system: policemen, lawyers, or judges.

Third, given the substantive search criteria, not all studies were actually acquired or even tried to be acquired. Many public opinion surveys may include but a few relevant questions and not really focus on crime itself. As a rule, which was guided mainly by acquisition costs relative to acquisition benefits, we did not incorporate such surveys as may have included fewer than three salient questions--and when the cost factor exceeded what we thought to be prudent, five relevant questions established the minimum.

It can be immediately noted that none of these explicit criteria of archival inclusion concern several important points:

1. We did not consider, as an aspect of the decision to include or not to include a study, the sampling design itself--the nature of which affects the relative precision with which conclusions might be drawn.

2. We did not consider, in a similar vein, the specification of the relevant population from which, by whatever means (as under 1 above), the samples may have been drawn--and this, of course, affects the character of generalizability of such research in that it indicates what populations the results can be "extrapolicated to."

3. We did not pay systematic attention to problems of comparability of the studies as regards the questions asked, the context of the questions (their nesting within a larger instrument) or the possible implications of item wording itself.

4. We did not pay systematic attention to comparabilities of some of the most salient socio-cultural and demographic information about the respondents--an issue particularly important when it comes to items in which the research styles of organizations tend to differ and for which no clear-cut standardization exists: age, education, income, occupation being prototypical of such problems, while no difficulties arise (apart from interviewer, coding or data processing errors--all likely to be negligible) with regard to such items as sex, race (at least as far as black/white differentiation is concerned), marital status and the like.

Now there are both pragmatic and broader theoretical reasons for not having shown major concern over the precision of results, their generalizability (in part a function of precision and in part, of course, a strategic component of the "population" definition), or item comparability or, for that matter, demographic comparability.

The pragmatic reasons are simple enough: since the project duration was but 4 months (and the extension by another 3½ months was necessitated mainly by the need to produce the descriptive summary reports rather than by delays in the search-decision (to include or not to include)--acquisition-processing dimension of the project, it did not seem at all possible to scrutinize each study of which we became aware along all the relevant methodological dimensions lest significant time delays in acquisition would result, with all the consequent--and cumulative --delays in processing, analysis, and reporting.

The second pragmatic reason, of course, revolves around "costs." It simply would have been also cost-ineffective (in addition, and also in relation to, the time costs mentioned above) to make important methodological decisions on an ex ante basis and without knowing what the pattern of all acquired studies is likely to be along the dimensions of precision, generalizability and comparability. In some sense, such judgements can be made either with important time delays (and at costs associated with such delays) on an ex ante basis or, at lower cost and much greater time effectiveness, on a post hoc basis. We chose the latter course for reasons which have been specified: The short duration of the grant period and the low project budget. However, there are also more theoretical ramifications of the decision which did play a role in our final judgement.

Many studies might have problematic generalizability and variable reliabilities (in terms of precision), but they provide clues and insights against which the more methodologically sound inquiries can be pitted. Many studies might not be comparable, along substantive or (some) demographic dimensions, but they also point to the directions of change or to the dynamics which underlie the nation's feelings about crime.

Entirely apart from such issues was our concern to establish something coming close to "closure." That is to say, to put together as complete a record as we could manage. This becomes especially salient when it comes to identifying the kinds of issues that have been raised repeatedly over time, the kinds of issues which seem to have been evolving over time, and the kinds of issues which the researchers may have paid less attention to than might be warranted. Thus "gaps" and "loopholes" in the body of information are easier to identify and specify by the approach we took, promiscuous as it was with regard to sampling designs and frames or comparability specifications, than had we simply looked for research with "rigor-cum-comparability."

The final theoretical reason for a more encompassing, rather than methodologically rigorous, inclusion of studies is simple enough as well: It establishes a record or a research reality of sorts, a kind of collection of (most) studies which were actually carried out, as imperfect as each might have been or as problematic as some, if not many, might have been. It is, to our best knowledge, the best and most complete record of its kind and its problems are problems of the evolving research process itself.

In meeting the grant stipulation as to the production of substantive reports, the Center summarized the main results in three papers:

1. The Nation Looks at Crime: Crime as a National, Community and Neighborhood Problem, by Jiri Nehnevajsa and Ann P. Karelitz, University of Pittsburgh, Center for Urban Research, September 1977 (pp. 184 plus appendices).

2. The Nation Looks at Crime: Police, Court and Prison Systems, by Jiri Nehnevajsa and Lawrence Stockman, University of Pittsburgh, Center for Urban Research, December 1977 (pp. 168 plus appendices).

3. Some Perspectives on Crime in the United States: Executive Summary, by Jiri Nehnevajsa, University of Pittsburgh, Center for Urban Research, December 1977 (pp. 26 plus appendix).

The reports on The Nation Looks at Crime: Crime as a National, Community and Neighborhood Problem (1 above) and The Nation Looks at Crime: Police, Court and Prison Systems (2 above) are descriptive rather than analytic in character--as, in fact, they were stipulated to be in the initial study period. They highlight the main themes which we discovered in the multifacetted and varying surveys and they, at best, border on a modest analytic concern with their emphasis on disaggregation, whenever possible, of the results in terms of major demographic and sociocultural segments of the populations studied.

In relation to methodological concerns, the reports did not seek to unravel issues of generalizability or precision of the respective studies, nor did they consider significant aspects of the comparability across studies, or provide a systematic appraisal of such trends, save for the major ones, which the results seem to have pointed to.

As a consequence of the summary, and implicitly somewhat non-discriminating highlighting, focus of the initial phases of the inquiry the Executive Summary (3 above) may be easily questionned on both, somewhat contradictory, grounds: For one, that it remains too much confined to the explicit and simple meanings of the data base and second, that it may, on occasions, leap to conclusions or recommendations which go so far beyond the data from which they seem derived as to be, or appear to be, almost unrelated to the data patterns themselves.

Without seeking to explain (or explain away?) such legitimate statements of limitations pertaining to these reports (or, perhaps, objections to them)--limitations rather well recognized by the researchers themselves from the outset--one major point is of somewhat fundamental importance and it needs to be expressed. Too often, the research user community (including research sponsors, other researchers, journalists, practitioners and the like) tends to view "final reports" as "final" rather than particular iterations (and in our instance, the first major step) in an ongoing search process, and, thereby, "final" only with respect to the specific purposes of a given phase of inquiry.

In keeping with the third main requirement of the LEAA grant, a computer tape of all surveys involved was produced and, in standardized form, transmitted to LEAA.

The documentation bearing on the project, and on the usability of the tape by others, was incorporated in:

> The Nation Looks at Crime: Documentation for Data Archive Questions: User's Manual for Data Files, by Ann P. Karelitz, Robert Kominski and Steven D. Manners, University of Pittsburgh, Center for Urban Research, November 1977.

Given this initial LEAA grant, it is of some value to LEAA as well as to other researchers to provide even a crude assessment of fiscal accountability. It may help in recognizing the kinds of costs which are involved in the process of archival development.

1. Some 140 studies were considered for inclusion in the archive.

   Direct research cost (discounting University overhead) amounted to $143.60 per study.

   Total costs to Government (including all indirect or overhead charges) amounted to $178.50 per study.

2. About 70 of these studies were actually both eligible for inclusion and acquired.

   Direct research costs were $223.38 per study.

   All costs to Government were $277.67 per study.

3. The studies actually acquired and processed involved data, during the grant period, from some 300,000 Americans (interviewed as individuals or as respondents on behalf of their households).

   The direct research costs amounted to $0.07 per record.

   The total costs to the Government resulted in $0.08 per record.

More specifically, the direct research costs included:

a. Search for potentially relevant surveys.

b. Review of such responses from various agencies and organizations as were involved along with follow-up search procedures.

c. Decisions to include or not to include a particular study.

d. Acquisition process, along with necessary follow-ups.

e. Cost of acquisition (when relevant).

f. Data cleaning and standardization for Center computerization along with the development of the needed documentation.

g. Computerization of the studies.

h. Documentation of the resulting computerization, the writing and production of the documentation report.

i. Writing and production of substantive and summary reports, including revisions upon initial reviews of the draft reports.

j. Project clerical and secretarial assistance.

k. Project management and administation.

l. Travel, communications, supplies.

Following the completion of the activities under the initial grant, LEAA 76-TA-99-0026, we thought it would prove both prudent and advantageous to LEAA to provide support to maintain, further refine, expand and up-date the data archive, and, at the same time, to initiate concerns over the more subtle, and crucial, methodological and substantive meanings of the information. Following a period of discussions as to how this might best be accomplished, a matter now within the purview of NICDLE in this second step of the program, the University Center for Social and Urban Research was awarded grant 78-NI-AX-0126, effective October 1, 1978, and covering the one year period up to September 30, 1979, so that some of the additional steps, agreed upon between the Institute and the Center, could be taken.

III. STATUS OF THE OPINION DATA ARCHIVE

Grant 78-NI-AX-0126 (October 1, 1978, through September 30, 1979) of $74,278 (of which $55,431 is budgeted as direct costs, thus some 75 percent) provides for the attainment of four tasks:

1. The Center for Social and Urban Research is to expand and up-date the archive established during the initial grant period.

2. The Center is to assess the studies in the archive along some key methodological dimensions, specifically in terms of their generalizability and comparability so that the variable usefulness, or differentiation with regard to purposes of subsequent secondary data analyses, can be taken into account.

3. The Center, in consultation with the National Institute, is to produce a limited number of substantive reports on specific selected issues (both in light of the archive up-date and the methodological limitations which prove appropriate).

4. The Executive Summary of the prior grant period is to be updated and, above all, refined.

By mid-1979, the up-dating and expanding process has led to the following:

1. 164 studies have been included in the data archive, involving

2. Approximately 750,000 records (each representing a respondent either as individual or as a representative of a particular household).

Considering both grants, totalling $99,268 including indirect costs and $75,535 in direct research costs, we may note that:

1. In direct research costs, $463.40 has been spent per archive study, and $609.01 has been spent in terms of all costs to the Government (including all University overhead charges).

2. Per record, the cost comes to $0.10 in direct research costs, and to $0.13 in total costs.

These cost factors, however, include not only all the search-decision-acquisition-processing-analysis-reporting items previously mentioned, but also all effort under grant 78-NI-AX-0126 of which the updating and expanding of the archives is much less of a component than it had been under the prior (76-TA-99-0026) award. We do not have data on the actual costs of the surveys themselves. But assuming that the average cost per survey may run somewhere between $20,000 and $100,000 for the most part, our experience shows that the production of a usable archive, by centrally locating and standardizing such studies as may be relevant, ranges somewhere between 0.6 and 3.0 percent of cost of the surveys themselves. In fact, if we discount other tasks for which the Social and Urban Research Center has been responsible (including limited reporting of the results and not merely of the status of the archive), it is quite fair to say that the cost of archiving for future usability and in standardized form tends to average about 1.5 percent of the initial costs of a survey (and varies only marginally with the "size" of the survey).

While we also wish to make the fiscal accountability of our effort quite explicit, we point to this pattern of experience mainly to provide the National Institute with a somewhat crude, but altogether realistic, assessment of costs involved in these types of archival efforts.

Two other lessons, though quite related to each other, are quite pertinent.

For one, the cost-per-acquisition (whether the units counted are studies or individual records within the studies) increases over time.

Second, the time-to-acquire grows longer over time, and this, of course, partially accounts for the unit increase in archival costs.

But, indeed, neither costs as such nor time investments keep simply increasing without any sign of "levelling off." Our experience indicates that approximately a factor of 3-4 is involved between the initiation of an archive and the cost-and-effort stabilization upon such initiation.

On the whole then, the cost of starting a data archive is likely to amount to 1/3, per annum, of the cost of keeping it up-to-date and maintaining it and it could, perhaps, represent only 1/4 of the initial archive establishment investment, per year.

Apart from "probable use" criteria (both regarding the anticipated nature of use and its frequencies over time), decisions to generate data archives to begin with clearly depend on the number of inquiries in a particular domain of issues and on the types of organizations and research groups which may have undertaken such studies.

Thus archives make more sense, rather obviously, when there are many studies on particular topics. They also are more appropriate when such studies as have been done were conducted by a variety of organizations and research groups rather than by one, or only a few of them.

Given a relatively large pool of studies and many organizations responsible for them, it follows that it is initially much easier to tap the pool: The search process is both facilitated and speeded up. Since studies vary in their acquirability (as they do in the level of documentation which pertains to them), it also follows that the initial phases of data-archiving are facilitated by the fact that the more "acquirable" studies are first to be included and ease of acquisition also means that they are acquirable at a faster rate.

Both factors (search and acquirability) then play a significant role in the fact that data banking phases beyond the initial one are both more difficult and more time consuming.

Our experience shows, for instance, that an average of 3 weeks lapsed between search and acquisition in the initial phase of our effort. Under the present, follow-up, grant, however, the search process itself lengthened to 2 to 6 weeks and, upon a decision to acquire a study, the acquisition apsect was lengthened to about 4 to 8 weeks: Overall then, the average search-to-acquisition dimension of the current project comes to about 10 weeks--3 to 4 times the amount of time (at no personnel saving to speak of) expended per average study in the initial development of the archive.

The actual inclusion of information into the archive system, once the data are acquired, involves the same basic process, and similar time requirements, both in the initial and subsequent data banking stages.

The documentation--to wit, the raw data (generally obtained on a computer tape), questionnaires, code-books and research design (especially sampling) documentation--has to be checked for completeness, as well as accuracy (especially applicable to data files which often have to be further "cleaned"). Variable labels (names of items in the respective study) and variable values (names of coded responses to each item) have to be generated.

Frequently, computer tapes which are not directly compatible with the University of Pittsburgh (PDP-10) system have to be translated from their original tape and format into formats acceptable by the system, and a new clean data file developed as a subfile of the already existing archive.

Our experience shows that the average time involved in this "inputting" process does not exceed 2 weeks per study whereafter clean, standardized and well documented output becomes available--and remains available for whatever future uses.

Under the 1976-1977 grant, we produced a standardized and variable--as well as value-labelled computer file of some 90 surveys for the 1960-1975 time frame. The remaining documentation, however, was in no way computerized so that, for instance, a search for similar items (questions or types of questions) or a search for the population frame definition, had to be carried out manually.

To enhance usability of the data archive as well as to facilitate the methodological assessment for which the 1978-1979 grant has called for, we computerized, and rendered increasingly retrievable, other salient information about the studies.

1. The question file was computerized--in effect, the Karelitz-Kominski-Manners paper, The Nation Looks at Crime: Documentation for Data Archive Questions (November, 1977) along with those items which were contained in newly acquired surveys.

   The first iteration of the computerized question file permitted retrieval of each item only in terms of exact words or strings of words: Thus, any question which may have contained the word "policeman" would be retrievable, by this iteration would not have retrieved questions in which the term "policemen" or "police force" or "police" would appear--unless all such detailed options would be specified on an ex ante basis.

   The second iteration, completed by mid-1979, augments such "lexical" retrievability by broad category codes: "nation," "community," "neighborhood" (all with reference to crime issues in these contexts), "police," "courts," "prisons," "laws." These types of key-words then have become tags associated with each question, thereby greatly enhancing the flexibility of the system.

   The third iteration, which will be completed before the termination of the current grant period, goes into further detail and provides for subcategory codes: For instance, "politeness," "speed of response," "brutality," and the like become further qualifiers of the broader "police" category--since many questions about various specific forms of police behavior and performance involve such issues.

   The study name, responsible organization, study data, the sample size, general population definition (whether a study is national, statewide, SMSA-wide, city-wide or other; whether it focussed on "households" or "individuals" as delineated respondents) are among the major items associated with each computerized question (and with each retrievability-coded iteration of each question).

2. Basic sampling information was also computerized. In addition to the study descriptors (as in the above), each survey was coded to identify the type of sampling design, the population frame information, criteria of eligibility (for inclusion into the eventual sample--sex, age, and race specifically), whether the study

involved the "individual" or a "household" as the unit, time points of the study (one time cross-sectional, repeated survey, panel) as well as data about sampling stratifiers, if any.

A second iteration of this sample design documentation, as yet not undertaken, might prove useful to provide detail beyond the most general sampling characteristics.

3. Basic demographic information was also computerized--that is, a file has been produced which contains study descriptors along with a summary of the background variables which are included in the archive.

The respective files (of questions, sampling characteristics, demographics used) can be used either separately or, as is more useful, in a merged form since all contain the same basic study descriptors.

As it is, the opinion data archive includes:

* 55 studies which involve nationwide samples

* 1 study of a subregion (southwestern Pennsylvania)

* 6 surveys on a statewide level

* 9 surveys with the sampling frame involving a Standard Metropolitan Statistical Area, and

* 93 surveys in which a central city, or in a few instances, a portion of a city only, was used as the focus of inquiry.

In the SMSA-based studies, it is often possible to disaggregate the data so as to identify the subset of respondents who reside in the central city of the area. Such subsamples can then be treated, within the sampling design itself, as other instances of data on the particular cities themselves.

By contrast, neither the statewide surveys or the national samples generally make disaggregation at the level of particular cities feasible simply because the resulting subsamples are too small, and the sampling procedures as such are not grounded in representativeness within particular cities. At best then, such inquiries allow us to consider similarities and differences between urban and non-urban areas, as is often the case, between respondents from communities of variable sizes.

All in all then, it is perhaps most advisable to consider the existing data archive as providing insight into crime-related issues at the national level on the one hand, and at the level of (some 32) central cities on the other hand.

Before we consider generalizability and comparability problems in more detail, it may be worthwhile to discuss, if very briefly, some of the main issues of the archive with a focus on the underlying methods questions.

IV. SOME GENERAL ISSUES

It would not, we believe, serve much purpose to attempt to reiterate what so many excellent, or at least adequate, books and articles on research methods in general and on surveys in a more specialized manner have to say. Nor would it prove of value to resummarize such material as that contained in Hyman's treatise on Secondary Data Analysis--a data archive of the kind we have developed being, indeed, a prime source of opportunities for secondary data manipulations. Thus only a few key principles, such as we consider most salient for our purposes, will be outlined and discussed here. They apply, to be sure, to surveys in general and to data archives in general. As such, they are also altogether applicable to the crime opinion data archive on which we focus here both in its own right and as exemplary of more underlying methodological issues.

For our immediate purpose, we will simply hold some major methodological issues "constant" and take them (or rather, their resolution within the state-of-the-arts) as givens. We will have a subsequent occasion in this report to consider some of these issues as well. But here, we will simply postulate that all is well with the sampling designs, with the fieldwork to implement the sampling designs, with question wording, with question formating, with overall survey instrument design (in its item sequencing), with data reduction, processing and analysis strategies.

With these caveats, and with other warnings to follow, we may then outline some rather central principles.

> Many, perhaps most, survey questions are not keyed to specific issues of policy formulation, adoption or implementation in a direct sense although insightfull interpretations of findings may well help in an understanding of the extent to which current policies seem to work or not work, why they appear to be effective or not effective, as well as an insight into the domain of plausible policy options which may merit consideration for the future. But in this realm of survey questions, if they are to have any policy relevance, the key issue rests with interpretation of data which provides the linkage between "facts," their "meanings" and their variably distal or proximal linkage to existing or plausible (collective) action courses.

We do not propose to construct an adequate, if not truly comprehensive, taxonomy of the dimensions of questioning which constitute the substance of surveys. Rather, we shall limit ourselves to a few clusters of issues which are both most pertinent given our objectives and also most prevalent as the strategic modalities.

Many questions concern some aspects of lay descriptions of reality.

The issue has its own highly salient time perspective: Thus the "reality description" may address the past (in the way of recall), the present (in the way of current assessment), or the future (in the way of expectations).

The past dimension describes the "state of the system" (the individual, the family, the community, the nation, the world) subject to problems of memory as well as to problems having to do with the confounding effects of the "present" system state assessment as well as its future (expected or anticipated) trajectories.

The present dimension is subject to both the historical and the expectational "bias" but also, above all, to knowledge and information in terms of which such a lay evaluation of the system state can be anchored.

The future dimension is, of course, affected by the trajectories which link the past to the present (that is, not only actual but also objectifiable trends and their perception), but also centrally by such more specific factors as optimism-pessimism and, indeed, the time horizon perceptions and relevancies.

Hence, the meaning of lay descriptions of reality along the time axis is extremely difficult to interpret, and this is a problem altogether apart from the impact of intervening (major as well as molecular) events, or the impact of such aggregate secular trends which may seem to be, or are, at work in the broader dynamics of time.

Now everything else we say about the "description of reality" data ought to be phrased within the context of time dimensions already briefly explained. In other words, the past, present and future dimensionalization of each issue remains applicable throughout.

In this vein then, the next concern with "reality description" as a thrust of survey questioning falls roughly into the area of "sociology of knowledge." On the one hand, "descriptions of reality" become a crucial, and perhaps cumulatively important, lesson in the way in which our people go about constructing reality. This, if anything, is a phenomenological aspect of the issue. Clearly, we discover more and more (upon interpretation of results) about the ways in which the (lay) American public goes about building images of the world (past, present and future--once the time dimension is incorporated into the analytic concern).

On the other hand, we stand to learn in the "social fact" sense of scientific epistemologies. This, too, seems rather clear: Many of the lay descriptions of reality can be pitted against otherwise "objective" or "objectifiable" evidence (for instance, perceptions of crime rates and actual crime/victimization rates) so that we may interpret the survey data displaying variable degrees of societal validity.

The third main axis of the "lay reality description" has to do with the actor himself, or herself. On the one hand, such descriptions entail ideational constructs: That is, questions about "problems" of the nation, the community, or the neighborhood, or, for that matter, the police departments, the courts or the prison system.

The second dimension, along these lines, has to do with behavioral or action issues: Who intends to do what about what and who does what about what.

These, too, are ways of "reality description" and, again, the time dimension applies as does the more philosophical confrontation of a more phenomenological (reality construction modes) or positivist (social fact confrontation between reality description and some other objective or objectifiable indices) bend.

The issue here, of course, is essentially one of sublimation vs. action propensities or actual actions, sublimation vs. behavior.

Now the next dimensionalization pertains to the referent of questions (as they pertain to this "description of reality" issue). Thus questions may be asked to describe the reality as it bears on the individual as a person, on the family or household, on the neighborhood, on organizations to which the individual belongs, aspires to belong, or, in fact, does not belong, on communities, on other geographic areas (the state, the region), on the nation as a whole. Clearly, in some types of existing or possible data archives, questions about the global-regional (e.g., North America vs. South America vs. Asia vs. Europe vs. Africa vs. Australia and New Zealand and the like), or about worldwide descriptions might be appropriate. The crime opinion data bank does not, of course, include such inter-nation and world-regional or even world-wide concerns at this time.

The lessons we can learn, of course, not only have to do with who believes what about what regarding the alternative references, but above all, the extent to which the reality descriptions are variable as a function of the referent's proximity to the more immediate concerns of the individual and his/her family or household.

The final issues along this dimension of analysis has to do with the fact that, along this "description of reality" axis, we often deal with questions which are "value-neutral" and, at other times, with questions which are "value-ladden."

Thus questions having to do with "problems" (of the nation, of the community, of the neighborhood--to exemplify) are value-ladden simply because "problems" are never thought of as "good" things so that the response pattern focuses on the negative aspects of whatever existential conditions.

By contrast, questions of many kinds are, in this sense, relatively value-free: Whether people have or have not provided themselves with bars on their windows, double locks or other ways of coping with

anticipated or possible burglaries (or, for that matter, robberies and assaults). The action itself may be value-imbued--it is preventive against some anticipatable negative occurrence. But the survey questions themselves do not imply either the worthwhileness or devaluation of such actions: They simply ask whether people think this or that, or whether they may have intended to do this or that, or actually done this or that.

There are then, in the most general sense (illustrated here with respect to "description of reality" questions included in so many surveys), considering the following major axes:

1. The time referent (past, present, future).

2. The philosophical underpinning of both the question and its interpretation (roughly definable along the phenomenological vs. positivist dimension).

3. The ideational/action dilemma--items which pertains to ideas of concepts "about" or "of" something versus statements about intended or actual actions.

4. The referent of the "reality description"--that is, whether the issues raised refer to attitudes/actions of the individual, of family/household members or to perceptions of attitudes/actions by other entities more distal than self, family or household (and thus seen also mainly from afar and from a lay reality construction perspective).

5. The extent to which the "reality description" questions are implictly evaluative in character or relatively free of a built-in valuation.

Now beyond the "reality description" thrust of many survey questions, normative questions also play a crucial role. They pertain, of course, to items which amount to respondent statements of prescriptions, permissions, preferences and prohibitions along the spectrum of what ought to be and what ought not to be, what should be done and what should be avoided. Here, the time dimension is generally somewhat truncated in that questions about what "should have been done" in the past are both rare and of unsure interpretive value anyway. But what "should be"/"shouldn't be" and what "should be done and "shouldn't" right now (in the immediate future) or at some other future time are highly salient.

In an epistemological sense, responses to normative questions and clusters of such questions help us to understand lay modes of cause-effect reasoning: That is, "given" some perceived "reality" what kinds of policies and actions are believed to alter an unwanted reality to a better one, and what kinds of policies and actions might reinforce or even enhance the likelihood that a more desired aspect of "reality" will be maintained or even further improved. The cause-effect interpretations (lay thinking about causalities) are further

strengthened by the inclusion of "reason" (why) items along with the Lazarsfeldian "reason analysis" conceptualization of the interpretive processes.

If such "lay" assessments of causal chains form, in a way, a phenomenological axis of analysis, the more positivist dimension has to do with pragmatics: What policies and actions are (simply?) seen to work toward what more desired ends?

The referent issue is also relevant: Not only might we speak of probes along the normative spectrum as to what should be done and what should be avoided, but also about who is to do the doing or avoiding. Obviously, one possible actor is self (along with, perhaps, one's family), while other actors to do the doing are alters--other people in general (along with self or not), organizations and agencies of various kinds, Governmental agencies at various level (local, county, state, Federal).

Not only do such data disclose, if indirectly, who is seen responsible for policy or action but also whose actions might prove effective (in that advocacy of what ought to be done or avoided is unlikely unless it is coupled with a belief that such actions might have some chance of succeeding in accomplishing what is "intended" or "hoped for"). Many questions in surveys, in turn, seek some form of performance assessment. They are evaluative both in purpose and by implication. Thus an individual (in relationship to a particular social position--for instance, that of the President), an organization or agency, Governmental or otherwise gets somehow "rated" on a continuum of performance quality.

The perceived and imputed reasons for variable performance also constitute important follow-up items in terms of which the researcher (and the research result users) can learn not only how our people assess the functioning of various institutions and power holders in our midst but also something about the nature of the underlying reasoning in which such evaluations are grounded.

In these regards, the truncation of the time dimension occurs in the direction of the future: Thus questions about "future" performance (expectations) are not as salient as are questions about the current performance levels and antecedant functioning (some past performance assessment, evaluated by recall). The more phenomenological thrust is built into interpretive thinking in that we learn, as has been pointed out above, about the lay perceptions of "sources" of ("reasons for") good or poorer functioning of the nation's (or local) organizations and agencies, whereas the pragmatic implications simply suggest that performance gets evaluated thus or so for the particular "reasons" given so that improved performance, in reality or at least in the way of imagery, might result if situations which such imputed "reasons" delineate were altered by appropriate policy intervention and implementation measures.

Still other major clusters of survey questions tend to tap information and knowledge and, frequently, the sources of such information and knowledge. This, of course, has to do with information and knowledge of, and about, a particular issue, reality description, policy options or agencies and organizations the functioning of which is being evaluated.

Whether such items probe simply whether the respondent is or is not aware, informed or knowledgeable about "something," or whether they seek a more general self-assessment of the individual's information and knowledge state or even entail batteries of questions which provide a proxy for a more extensive information or knowledge "test," one of the key premises is, of course, that variable information and knowledge states may account for differences in reality perceptions, in preferences for action courses, in performance evaluations as well as in the chain of reasoning which itself underlies the sentiments.

But, of course, there is a purpose behind the purpose: If such information and knowledge related patterns of attitudes, actions and their interrelations exist, then it may be possible to impact the state of the system by improved dissemination of information and knowledge (especially, indeed, if misperceptions of reality, policy options and their potential, or performance assessments seem to be grounded in inadequate or distorted information or knowledge).

Questions concerning information/knowledge sources, of course, then allow an analytic assessment of the channels through which information/knowledge dissemination might be "best" enhanced and in what ways.

Finally, all surveys include observations and questions the data from which become descriptors of the socio-cultural and demographic characteristics of the respondent, often of the household or family, and, occasionally, of the residential (neighborhood) context.

Needless to say, it is not some form of idle curiosity which prompts the research to inquiry into such matters as approximate income levels or occupation or marital status or age. Rather, the socio-cultural and demographic attributes provide an essential interpretive anchorage for all other data and this is so both on theoretical and pragmatic grounds.

In terms of more theoretical considerations, who the individual is and where and how he/she is located in the social structure is an important factor in defining appropriate social roles and their institutionalized linkages, and since such definitions are normative in character to begin with, their tie to values and attitudinal dispositions along with behavioral propensities is easily postulatable. Thus, key socio-cultural and demographic subaggregates of our population may be often thought of as subcultures, either in reality or in the way of potential, and a central theme in survey analysis revolves around an understanding of attitudinal and behavioral similarities, and differences, among such "subcultures" (or, as a minimum, population segments with similar background traits).

The pragmatic consideration, of course, has to do with the simple fact that many socio-cultural and demographic characteristics (such as sex, race, marital status, age, education, occupation, income, urban/rural context and the like) actually do tend to show consistent differences in attitudes and (reports of) behavior on many, if not most, issues under study.

Thus aggregate results--which would essentially disregard such demographic and subcultural variabilities--often mask important and persistent differences so that their interpretive value is rather limited.

Now apart from the interpretive meaning of the major clusters of questions (reality descriptions, normative, performance assessment, knowledge and information, background) and their more subtle dimensionalization (in terms of the time dimension, along epistemological lines, in referent terms), another more general statement needs to be made. It amounts, to be sure, to a form of truism but its importance cannot be sufficiently stressed:

Statistics do not speak for themselves.

Indeed, with a poor sampling design or with poor field implementation of even a very good sampling design, with problematic questioning formats or sequences or both, with careless data reduction routines which amplify, rather than avoid, errors and all other analytic problems which may beset data processing, statistics may well be unusable at all because neither the researcher nor the user can place any confidence in them even within broader ranges.

But the problem remains even under the best possible survey design and implementation circumstances. This is so because statistics acquire their meaning only by, and through, interpretation and this is a matter quite different, though not obviously altogether independent, from statistical significance or sampling tolerance types of questions.

One aspect of the issue relates to linguistic habits themselves, and to the thought patterns in which language usages are anchored. Thus (a) "25 percent of Americans say, feel, do. . ." is, in its implications, a different statement from (b) "only 25 percent. . ." which, in turn, is sharply at odds with an assertion such as (c) "as many as one in four Americans. . ." Only statement (a) avoids an implicit evaluation though there are, of course, many shades of grey on the range between statements (b) and (c).

But statement (a), when made by the researcher, only invites--and of necessity so--an evaluation somewhere on the (b) to (c) dimension on the part of other researchers, research report readers, data users. Thus, in principle, the researcher could avoid the dilemma by non-interpretation but this simply amounts to turning over the need for coming to grips with the information to other users of the data.

The second major aspect of the issue is of the following kind: Whatever words the researcher or user deplays to make statements about statistics, thereby imbuing them, so to say, with value, it is quite reasonable to suggest that one and the same statistic almost forces different interpretability dependent on its context--that is, the issue to which the statistic is addressed.

Thus if, for instance, in face of an impending disaster 25 percent of people from a particular area were to evacuate spontaneously (without any official directive or action), the dislocation in the production of goods and services both in the abandoned area and in the destination areas of such evacuees might be quite severe so that the "25 percent" represent a major, if not actually dramatic, event.

At the same time, if 25 percent of people were to evacuate a threatened area upon urging or directives by appropriate officials to do so, the "compliance" at the level of 25 percent would clearly be considered very low, and the "dramatic event" would consist of not-leaving-even-when-asked.

Now 25 percent of people who might put double locks or other safety devices on their doors or windows, perhaps, do not represent a major national happening. Most analysts, however, would probably agree that 25 percent of people who profoundly change their life style (such as by staying indoors much more or avoiding some parts of a community altogether or walking around armed or with other self-defense devices) to increase their safety vis-a-vis the risk of crime do represent an event (or better, evolving state of affairs) of quite a different order of magnitude from the double lock type response. But there really exist no adequate rules in terms of which to categorize classes of issues and problems for which one would always argue that 25 percent is "only 25 percent" and others for which one might assert that it is "as much or as many as 25 percent."

The third key facet of the issue concerns the difference in meaning which must be attributed to one and the same statistical value dependent on the underlying pattern from which it is derived (and, to repeat: independent, in this respect from all other survey design questions).

On one end of a theoretical-methodological continuum lies a situation in which "the" 25 percent of Americans who say, feel or do something different from the remaining 75 percent are a representative subsample of the total sample (which, in turn, is a sample of some design-defined universe). This means, of course, that the background, attitudinal, behavioral and information/knowledge characteristics have essentially the same distribution in the 25 percent as in the remaining 75 percent (and the difference lies in the criterion issue being investigated "only").

On the other end of the theoretical-methodological spectrum is a situation in which the 25 percent are in all, many or some important ways different from the remaining 75 percent--that is, in terms of background traits, or other attitudinal dispositions or other reports of behavior or information and knowledge levels. In this case, of course, the "25 percent" acquires a new meaning because it actually, in view of the intercorrelation implicit in the schematization, represents a much larger percentage of some otherwise definable segment of the society.

The search, indeed, is not only one to discover how, to what extent and why (the latter mostly by interpretation only) background characteristics, information/knowledge, reports of actions and attitudes are intercorrelated and which ones are and how systematically and pervasively but the "meaning behind meaning" provides crucial clues to the lines of division that run through society and how deep they are.

It is our conviction that a great deal of secondary analysis, and especially of data over time, needs to be devoted to such inquiries into broader and fundamental patterns through which the nation's life pulsates between the more unifying and the more fragmenting impulses.

The fourth major aspect of the interpretive problem has been implicit throughout. But it merits being stated quite explicitly as well.

There is, indeed, a difference between highlighting, as a major finding, the "fact" that "75 percent" are rather "satisfied with the performance of police officers" and the reporting focus on the "fact" that "25 percent" are not satisfied.

Whatever the intent of the researcher--and whether or not the choice of an alternative report thrust is itself a sort of index of the researcher's own predilections--there seem to be two different messages conveyed: One, with an emphasis placed on the approving 75 percent, suggests that "things are rather good" and, perhaps, there is "little or nothing" to worry about and "little or nothing" needs to be done. The second message, with its 25 percent dissatisfaction as it were underlined, suggests that "enough people are dissatisfied" so that "something" ought to be, possibly, done to alleviate the situation.

Probably only a great deal of self-discipline on the part of a reader or actual potential user would avoid the pitfall of reading into the data (and into the researcher's intentions, rightly or mistakenly) "messages" of this kind.

The fifth, and in this vein last, major consideration has to do with a form of interpretive disjunction between the researcher and the user. We need not dwell on the all too well established fact that interests and concerns of the community of researchers and of the community of potential and actual research users are generally

not identical. This means, of course, that the user tends to substitute her/his interpretations of the meaning of data for those which the researcher unavoidably provides explicitly or by implication, and it also means, more often than not, that the criteria which govern interpretive modes are not the same ones (nor are we suggesting that these criteria must converge in every respect lest results remain unusable). The proclivity of the researcher is to emphasize statistical significance and precision not infrequently with lesser attention paid to substantive significance of findings. A correlation of .1, for instance, can be "statistically highly significant" (that is, at some appropriate level of significance significantly different from a situation in which the researcher would accept the hypothesis of zero correlation between/among the variables) but it is invariably trivial in substantive terms.

A confidence interval around a mean (or the special mean of a dichotomy that we call "a proportion") can, indeed, be made as small as desired by, for the same basic sampling design, increasing the sample size. And, of course, by "sampling" everyone in a defined population (that is, by "canvassing"), the sampling tolerance issue can be, at intolerable cost, avoided altogether.

All this is quite important--and, in fact, rather unproblematic. But the researchers and users hardly ever interact either prior to the conduct of a study or even in its analysis-and-reporting phases to determine what kinds of differences would make a difference.

Would, for example, a police chief consider, formulate, adopt and implement a different policy if 10 percent of residents in the community complain about "instances of police impoliteness" than if 15 percent were to do so? The difference between 10 and 15 percent may well be statistically very significant indeed, but its interpretation by the researcher would be quite different along substantive lines if the scientist knew whether the difference will, or will not, be likely to lead to different decisions by the user(s).

Even if such a difference referred to overtime data and thus suggested an incipient trend (or, say, two time points on already existing trendlines), it is clearly important to know whether the "5 percent change" over time would command attention of decision makers sufficient to warrant consideration, if not adoption and implementation, of some strategies of intervention or whether such a "5 percent change" fails to cross the cutting edge beyond which one decision preference yields to some alternative preference. Now, furthermore: Even if the difference between 10 and 15 percent reflected complaints about "police impoliteness" in two cities and if the difference were statistically significant (which, by the way, would require a sample of about 2,000 in each such city), the researcher's conclusion that police officers in City A are "significantly" more polite (as perceived by the public) than are officers in City B would be warranted.

But the total contextual embeddedness of the respective users could easily lead to efforts at remedial action in City A (with 10 percent complaints) but no policy response, for the time being, in City B (with its "significantly" higher 15 percent complaints). Apart from many plausible reasons for this (including the respective personalities of the decision makers as well as the political climate and political functionning of the communities and their fiscal policies and so on and so on), it is certainly possible that in the "higher complaints" (regarding "politeness") city there are compensatory performances of police officers which outbalance the concern--for example, they "might" get to the scene of crime faster, they might be generally more effective, they might be working in a community environment in which the crime rate is higher to begin with, and so on.

This all, of course, emphasizes that a central component of the analytic and interpretive thrust has to be concerned with patterns of findings and overtime changes in such patterns rather than with the implications of any particular statistic, no matter how otherwise precise or even compelling.

On balance, in terms of knowledge utilization, the issue of substantive significance amounts to a definition, often only vaguely recognized even by the users, of actionable differences. A finding at one level of magnitude becomes an input, often a major one, to provide support for one (policy) option, whereas a finding different from it by this "actionable difference" margin would tend to lend support for a different option. In terms of more underlying sociological or social psychological theory, the issue of substantive significance rests largely with the patterning of findings both at one time and over time in that the results, no matter how otherwise important, with respect to any single item in any survey instrument would hardly have a profound effect on the more intricate web of propositions and speculations of which social theory is made.

Overtime tracelines of comparable items in comparable samples, not to speak of tracelines of stabilities and changes in patterns (configurations of findings across comparable items and, more or less, comparable samples), of course, acquire substantive significance of their own. But, indeed, while this may amount essentially to the identification and description of trends (or, when the more powerful panel design were used, to the identification and description of both trends, as gross changes, and turnover, as net changes), the interpretation must almost invariably extend beyond the narrower perspective of the studies themselves. Thus to explain the dynamics underlying such trends as may be found, and to assess the "reasons" for the trends, the researcher must almost always address speculations concerning the likely implications of intervening events and event sequencies (as helping to explain the probable "whys" of the nature of the trends) and thereby, in effect, consider the relations among various trends, and the crucial--but generally neglected (except in economic research) time lags.

Since real-life experiments are never quite possible even were one intent to engage in them (though quasi-experimental designs are not only possible but have been recently quite often used), the interpretation of trends in the way of substantive explanation remains uncertain and, at best, approximate in a speculative manner, and a more formal statistical explanation (resulting from successive and cummulative data elaboration) rapidly reaches a point of diminishing returns due to the inherent limitations of sample sizes, a problem bothersome even when the overall sample is quite large to begin with.

But certainly, and despite the built-in interpretive difficulties (compounded as they are when we deal with overtime information as compared with one time survey data), trends yield insights of greater potential significance than do the findings from single time-specific surveys, and one of the central purposes of establishing, maintaining and updating issue-focussed data banks and archives is precisely to permit, and facilitate, a more systematic analysis of trends, and of their underlying dynamics.

It may well go without saying, but it is, perhaps, better to make another explicit, if obvious, statement:

Findings of surveys do not represent votes or results of referenda.

This is not because of sampling limitations per se, though in the legal framework of our political institutions this, alone, is an important factor. Rather, it stems from the fact that surveys have not been institutionalized as decision-making settings. The "interview" aspect of a survey design (whether face-to-face encounters, phone questionning or "by mail" inquiries are involved) has been institutionalized, rather, as an information gathering interaction and not as a setting in which the individual (properly, in this context, referred to as a "respondent" rather than as a voter or decision-maker or what not) make a decision which, in its majority (or even plurality) thrust would have binding consequences for the larger body politic.

Nor is it a matter that results of surveys would be inaccurate in terms of the aggregate relationship between action/decision dispositions and the outcome of actual decisions as they may manifest themselves in the voting booth or, in the way of consumption patterns, in the market place. In fact, "intentions to act" (statements about candidates one is likely to vote for, statements about the basic character of probable near-future purchases and the like) are generally, and quite accurately, translatable into the kinds of statements out of which outcome predictions can be made.

Nonetheless, even when such "dispositional" questions (relating to approval/disapproval of alternative policies) or "intention-to-act" questions are asked, it is not, both on methodological grounds and theoretical ones, appropriate to interpret them as actual actions or actual decisions even if the results were predictive of an outcome were

the question presented, in the context of the workings of our political institutions, as a decision issue. To repeat: This derives, if anything, from the fact that there exists no societal legitimation to make "intentions" or "dispositions" somehow directly equivalent to "actions" and "decisions". In other words, to say that we might be able, and quite accurately so, to forecast the outcome of a decision-making situation on the basis of "intentions" and "dispositions" is not the same as equating "intentions" and "dispositions" to the actual subsequent decision, thus equating the evidence giving rise to a prediction with the actualization itself.

Furthermore, there exists no generalized, not to speak of adequately tested, theory which identifies the classes and types of "intentions" or "dispositions" which are more directly translatable into decision actualizations and those which are less so, and those which, possible, are not at all so easily linked.

Thus "intentions to move" tend to be less actualized in the way of factual residential change than are "intentions to vote for candidate A".

Thus "discriminatory dispositions" (say, toward the blacks) often do not translate themselves into overt behavioral discrimination because of "fencing-in" mechanisms (a particular black or a particular group of blacks then comes to be viewed as "atypical" and, therefore, "deserving" of a treatment, in action, different from "blacks in general"), or because of existing legal and social constraints (laws inhibiting discrimination; fear of legal or social/personal consequences of being viewed, and dealt with, as a "racist"; and the like). In the domain of crime issues, that is with respect to crime prevention and crime mitigation policies, we are simply unsure about the extent to which "intentions" and "dispositions" would, or would not, predict the outcome of such things as specific policy related referenda.

> But survey results provide crucial information bearing on policy mixes which "ought to be" considered, perhaps adopted and implemented.

Thus when many, if not most, Americans worry about "inflation" as a national problem above all, they are not implying that the Government "ought to" attempt to combat inflation to the exclusion of "other" deep concerns--such as unemployment, energy, and crime.

When a majority of residents in a particular city advocate (in the way of "normative" policy preference questions) "better street-lighting" while others emphasize the need for "better neighborhood policing" (and these, in turn, are split between preference for foot patrols and motorized ones), the prudent decision consideration concerns the mix of such measures rather than simply going all the way to improve street lighting (because the postulated "majority" so indicated in a survey) and neglecting those alternatives which "minorities" feel are the best prevention and mitigation measures.

In sum, the "majority rule" principle is neither intrinsic nor relevant to survey findings, and the kinds of inquiries with which surveys are concerned are not institutionalized in a way in which specific policies would be decided upon in the "yeah" or "nay" manner of votes and referenda.

This all leads us to the following conclusion:

> Policy options, and policy mixes can, and should, be considered and assessed in the light of survey findings--but not necessarily adopted and implemented.

The "should be" dimension of the statement is not in some manner idiosyncratically normative. Rather, it postulates that policy-related findings of survey research are usefully viewed as a form of public advice as to the broader riverbeds within which consideration of options might flow if it is to be, in part, in keeping with the sentiments and preferences of the respective (community, state, national) body politic.

Such findings yield advice as to the acceptability/nonacceptability of various options but it does not mean, of course, that today's "less preferred" or even "relatively unaccepted" alternatives cannot become tolerable, if not altogether acceptable, upon further public enlightenment or upon evidence that their implementation actually works.

A more systematic use of surveys to help to identify viable policy alternatives along with assessments of the lay rationale for the endorsement, or lack of it, would probably prove quite beneficial as long as there is policy-maker follow through in the way of evaluating such implied public recommendations and insights. This "follow-through" notion, of course, concerns the final major conclusion we would like to specify in this section of the paper:

> Results of surveys alone should really never be used as the basis on which policy is formulated, adopted and implemented.

Thus while we emphasize the sensitizing value of survey findings, and their high saliency in decisions to consider various policy alternatives, it is equally important to stress that surveys are not designed, or structured, to be the sole, or even key, determinant of what action courses might be taken or not taken.

This caveat applies to all specific single studies, regardless of their methodologies, survey or otherwise. It holds precisely because real-life issues cannot be, in their existential fullness and complexity, uniquely mapped onto any single research design so that the actual dynamics of policy making involve a mosaic of inputs of varying relative centrality to the issue, but do not (and should not) rest on any particular methodological modality.

In this context, the "should be" formulation is also not somehow subjective: Rather, it merely asserts that issues and problems in real-life functionning of a society or a community involve dimensions and intricacies which cannot be encompassed in a single research design, no matter how good the design.

To be sure, there are significant (a) fiscal, (b) contextual-economic, (c) manpower, (d) political, (e) legal, (f) moral/religious/ethical as well as more narrowly (g) technological considerations which must be taken into account along with problems of timing, both with regard to the timing of policy adoption and implementation and in terms of the kinds of phasing sequences always implicit in processes of deliberate (policy-induced) social change.

In this sense then, surveys form only one input out of many and while, as they reflect the public perceptions of problems and public identification of needs and preferences, an altogether crucial one their design is not, and cannot quite made to be, isomorphic to the general dimensionalization of an actual social issue.

Against this kind of a broadly cast, methodology-theory oriented, backdrop of concerns, we may now consider some of the more specific issues related to the opinion data archive on crime problems. But the lessons, too, have applicability beyond the archive which the University Center for Social and Urban Research developed for LEAA on the selected cluster of problems. They are, in principle terms, relevant for data banks and archives most generally--that is, to those types of data banks and archives which are problem-focussed rather than being overall repositories of data from widest variety of studies on a widest variety of issues.

V. SOME SAMPLING CONSIDERATIONS

If our preceding discussion did not seek to reproduce the detailed thrust of books and papers on survey design problems, or on secondary data analysis issues, it is also accurate to say that we do not propose to reiterate the expositions on sampling theory contained in such excellent sources as those by Deming, Kish, Stephan and McCarthy or Hansen, Hurwitz and Madow.

However, two more generic statements might as well be made explicit:

> The extent to which the results of surveys are generalizable to the population of elements from which a sample has been selected is, of course, a function of the sampling design above all, and of the fieldwork procedures which implement the sampling design.

The second statement is, of course, of the following kind:

> In the purer statistical sense, the implications of sampling theory (and the resulting data generalizabilities) apply only to those sampling designs in which the probabilities of element inclusion are known or knowable, and in which the fieldwork implementation reproduces the sampling design.

"Generalizability" is then driven by the sampling design and its actualization in the course of fieldwork. "Accuracy" of findings (that is, the confidence which might be placed that a canvass of population elements would produce findings within some measurable range) is, as is altogether well known, a function of sample size.

"Generalizability" always raises the question: generalizable to what set(s) of elements?

"Accuracy" considerations, in turn, always raise the question: given generalizability to a defined set of study elements (whether individuals or households or whatever "units"), what are the probable errors in generalization provided we might be willing to take some finite risk of being wrong. "Accuracy" requirements then, of necessity, affect the sample size. "Generalizability" requirements, in turn, drive whatever "given" or "preferred" sample size toward the specific subset of survey designs which involve known probabilities of selection.

In our general statements about "generalizability" and "accuracy" we included, as a constraint, a substatement about "fieldwork implementation". For the most part, the problem gets glossed over and is, more often than not, relegated to a chapter here or there about "special problems".

Apart from other "fieldwork implementation problems" (having to do with the selection and training of interviewers and the variety of interviewer-respondent encounter patterns along with possible, and modestly probable, errors in response reporting), the central issues have to do with "refusals" in general, with "refusals" to answer particular questions (as contrasted with "don't know" responses to the same questions), and with "not-at-homes" (regardless of numbers of designed-for callbacks). Of course, pragmatic and empirically validated methods to minimize "refusals" have been suggested, and all of the more prestigious research organizations have sought to follow such advice.

Of course, pragmatic and empirically validated methods have been recommended, and used, to minimize the "not-at-home" problem by increasing the designed numbers of callbacks.

However, to the best of our knowledge,

> there exists no mathematically sound (e.g., axioms-derived proven or provable) method for handling such "data loss" problems, so that even in the purest probabilistic sampling designs, important elements of unknown probabilities tend to enter as confounding factors.

There are, therefore, no a priori reasons to assume that "refusals" and "not-at-homes", even of given sociocultural and demographic traits in terms of which they may be quite comparable with "effective" (actualized) respondents, are homogeneous with like-respondents relative to the dependent variables under study. Above all, no aspect of sampling theory in its mathematical derivation or formulations is helpful unless "weak" assumptions about such subsets of respondents are made, including the assumption, unwarranted as it is, that "sociocultural" and "demographic" similarities to others, who turn out to be respondents, permit us to assume that they would respond in an "essentially" like manner.

The very fact of "refusing" suggests crucial dissimilarities even when "all other things" (background traits) are "otherwise equal". The very fact of "not-at-homeness" upon repeated callbacks, three to six as they range in the practice in their maximum, establishes a fundamental difference in life style which can give us no mathematically viable assurance that such "not-at-homes" display attitudinal or behavioral propensities essentially similar to those who become reachable upon callbacks, or are reachable to begin with.

We are certainly not suggesting that such respondents in the sampling design are altogether "different" from all those who are, or

become, "reachable" and don't refuse to be interviewed. But we are saying in no uncertain terms that

> the effect of refusals and not-at-homes is, and remains, unknown so that generalizability is appropriate, given the probabilistic sampling design, for those who responded and accuracy of statistics is also estimatable for this subset of sample-selectees but not for the universe from which the sample was originally drawn.

Even this statement, in purer terms of mathematical analysis, is quite questionable because the "refusal" patterns and "not-at-home" rates alter, if only in a subtle way, the web of "known" or "knowable" probabilities of inclusion into a sample on which the whole body of sampling theory ultimately rests.

Not infrequently then, detailed calculations of variance estimates may well be a form of exercise as a consequence of which the precision of the calculations gets interpreted as precision of the substantive results of the calculations. Furthermore, there exist no mathematical procedures (and no theory to substantiate whatever procedures would be improvised) to assess the impact of

(a) systematic errors which may result from recall problems, interviewer effect, questionnaire wording and/or sequencing and the like, or

(b) processing errors occasioned by duplication or omission of units in the sampling frame, coding, classification, editing errors and the like.

There exist, of course, pragmatic and even good procedures to minimize such additional sources of problems: by better training of interviewers, by careful questionnaire pretesting, by checking and doublechecking on all aspects of data reduction. But no such procedures can assure us, and certainly not in a more puristic sense, that possible bias has been altogether removed from the data, or how little or how much of it may remain to have some confounding effect. The issue then comes to revolve around a sort of article of faith, and more specifically, faith in the research organization's "quality control" procedures and, by proxy, faith grounded in the relative "prestige" of such organizations, itself a function of "track records" as well as images we have about the qualifications of the relevant personnel.

We state some of these limitations, though our expressions of concern are certainly in no way particularly original, not because we want to suggest that the problems of the relationship between survey design and survey implementation (fieldwork as well as data reduction) is highly problematic, or that survey findings, ipso facto, are not particularly useful. For this, indeed, is not the case.

Rather, we seek to place the issue of generalizability into a proper perspective, and to move away from the simplistic notions of statistical purists who may tend to hide the fuzziness of actual data sets behind a screen of precise and detailed (if arithmetically rather simple) calculations the numbers of decimal places of which are sometimes but another pretense at precision that simply is not there. But this is really why we emphasized previously that the search for patterns and changes in patterns overtime is at the crux of interpretation and of softer-than-might-seem generalizability. This is why we emphasized that survey results are not votes or results of referenda in which the nation's decision makers (voters) render a definitive and institutionally binding judgement. This is why we emphasized that the formulation, adoption and implementation of policy, as contrasted with willingness to consider policy options derived from such public advice, cannot rest on survey results alone anyway.

Within the framework of these cautionary remarks then, probability samples of varying sizes (weighted samples when necessary or self-weighted ones) have the approximate tolerances, at .95 level of confidence, shown in Table 1.

Table 1

APPROXIMATE TOLERANCES AT .95 LEVEL
FOR SAMPLES OF DIFFERENT SIZE

	When observed percentage is about				
SAMPLE SIZE	10 or 90	20 or 80	30 or 70	40 or 60	50
100	7	10	11	12	12
200	5	7	8	8	9
500	3	4	5	5	5
800	3	3	4	4	4
1000	2	3	4	4	4
1500	2	3	3	3	3
2000	2	2	3	3	3
5000	1	2	2	2	2

Surveys included in the opinion archive on crime issues have sample sizes within such ranges. Thus for aggregate results of those surveys to which the probabilistic sampling design is applicable to begin with, the findings are generalizable, with confidence of about .95 and including conservative estimates of design effects, within the approximate margins tabulated above.

These generalizations, of course, disregard all sources of non-sampling error, and while we need not assume that such confounding errors are of major magnitudes (including even the effect of non-response and not-at-homeness), it is prudent to view the tolerances as crude guidelines rather than precise ones even for samples to which (due to their probabilistic design) they are most directly applicable.

In turn, comparisons over time at the aggregate level involve some statistical assessment of the significance of differences such as may be found. Similarly, comparisons of sample segments upon disaggregation (thus, for instance, of females and males, or blacks and whites and the like) also involve two groups. The same provision applies to the comparison of results for a sample segment (for instance, female respondents) on two different time occasions. Table 2 gives the aproximate percentage differences--with confidence .95--at which, and beyond which, the differences between sample or sample segments are statistically significant.

Again: Nonsampling sources and magnitudes of errors are disregarded and the applicability of such difference-percentages is, strictly speaking, confined to probability samples only (in which some relevant subsamples, such as "younger" and "older" respondents are also, by definition, probabilistically selected).

Table 2

SIGNIFICANT DIFFERENCES, AT .95 LEVEL,
BETWEEN SAMPLES OF VARYING SIZES

		When observed percentage is about				
SAMPLE A	SAMPLE B	10 or 90	20 or 80	30 or 70	40 or 60	50
100	100	10	14	16	17	17
250	100	9	12	13	14	14
	250	7	8	10	11	11
500	100	8	11	13	14	14
	500	5	6	7	8	8
1000	100	8	10	12	13	13
	500	4	5	6	7	7
	1000	4	4	5	5	6
1500	100	8	10	12	12	13
	500	4	5	6	6	6
	1500	3	4	4	4	4
2000	100	7	10	11	12	13
	500	4	5	6	6	6
	1000	3	4	4	5	5
	2000	2	3	4	4	4

For the 164 archive files, summary sampling information is provided in Appendix A. The bulk of the surveys have been carried out either at the national or at the (central) city level. Only a few of the studies were "statewide" and one, a Pittsburgh 12-county study, which is subregional in character (File #069) is not generalizable at all because of the wide discrepancy between the rather carefully designed probability sample and the actual response rate (to the mailed out questionnaire which was used in this instance).

Thus our subsequent discussion focuses on the national and city surveys, with occasional references, as appropriate, to the relevant statewide studies: To be sure, there also exist two well designed surveys in Maryland (1974 and 1976), but only computer output of the results was made available to us and the raw data which could be standardized for secondary data analysis, and thus made an input into the archive, was simply not available at all anymore than seems to have been the detailed documentation which would be necessary to evaluate the potential of the survey (in the statewide survey context, or, upon disaggregation, for Baltimore as it might be comparable with city-related Baltimore samples).

Most of the national surveys in the archive originated from but a few research organizations. Table 3 shows the distribution, by time.

From the sampling standpoint, the matter becomes, to an extent, even more simplified: the sampling design and the fieldwork for Potomac Associates studies (010-012, 013-015 and 163-164) is that of A.I.P.O., and thus identical with the series of surveys listed under A.I.P.O. in the Table (archive file numbers 001-009). In turn, the University of Pittsburgh survey pertaining to issues of civil defense, but with items of relevance to the crime opinion archive, used N.O.R.C.'s sampling design and, in fact, N.O.R.C. also carried out the fieldwork for the study (archive number 050 of 1966).

A.I.P.O.'s documentation labels the organization's sampling design as one of "modified probability" type.

1. Seven size-of-community strata are used, to wit:

   a. Cities with 1 million or more inhabitants

   b. Cities in the 250,000 to 999,999 size category

   c. Cities with 50,000 to 249,999 residents

   d. (Census-defined) urbanized areas of the cities of 50,000 and more population

   e. Towns and communities with 2,500 to 49,999 residents

   f. Rural villages

   g. Farm and open country areas.

Table 3

NATIONWIDE SURVEYS IN THE DATA ARCHIVES[1]

	AIPO	CPS[2]	HARRIS	NORC	POTOMAC	OTHER
1960	001	051				
1961						
1962						
1963	002					
1964		052	022			
1965	003					
1966		053		066		
1967	004		023[3],024[4]			050[a]
1968	005	054	025[5],026[6],027[7]			
1969	006		028[8],029[9]			
1970		055				049[b]
1971						
1972	007[10],008[11]	056		018	010-012	048[c]
1973			031[12],032[13]	019		064-065[d]
1974		057		020	013-015	
1975	009		152[4],153[14],154[15]	021		
1976			155	148	163-164	158-160[e]
1977				149		
1978				150		

Notes

1. Numbers in the cells of the table refer to the archive file number (001-164).
2. The CPS (University of Michigan) surveys in the file carried out in the field between November and January. The surveys are tabulated for the year of the study's start.
3. 21 years of age and over.
4. 16-20 year olds only.
5. March, 18 years and older.
6. October, 19 years and older.
7. October, 14-18 years of age.
8. May survey, adults.
9. October survey, adults.
10. March survey.
11. October survey.
12. August survey.
13. September survey.
14. Harris H2055A, adults.
15. Harris H7490, 18 years of age and older.

a. University of Pittsburgh civil defense survey. NORC's sample and fieldwork.
b. Institute for Social Research, Michigan.
c. Quality of Life study.
d. National crime study. Bureau of the Census/LEAA.
e. Yankelovich, families with child(ren) 13 years of age and younger. 158: primary parent interviews, 159: self-administered instrument for subsamples of other parent, 160: subsample of children.

2. Within these size-of-community strata, the population is divided into seven regions: these parallel the Bureau of the Census Divisions as far as New England, Middle Atlantic, East Central, West Central, Mountain and Pacific divisions are concerned, but the three Census divisions of the Southern tier of states form just one ("South") category in the A.I.P.O. design.

3. For each of the 49 "cells" of the stratification (seven size-of-community strata by seven "regions"), Census-based population data are arrayed by geographic location and population size and clustered into "zones" of approximately equal population size.

4. P.P.S. (probability proportionate to size) procedures are utilized to select pairs of localities in each of the zones, the consequence being a choice of essentially two replicated samples.

5. Where data are available by census tract or enumeration district, sample subdivisions are drawn by PPS procedures; where data exist on other small geographic basis (but not on the basis of tracts or ED's), small, definable geographic areas are selected (PPS); otherwise, such small geographic subareas are selected randomly with equal probability of inclusion.

6. Where block statistics permit it, blocks or block clusters get selected with probabilities proportionate to numbers of dwelling units; otherwise, random (equal probability) procedures are used.

   In rural and open country areas, segments "approximately equal in population" are defined and randomly chosen.

7. In the selected blocks and segments, a random starting point is chosen for interviewers who are asked to complete a preassigned number of interviews by systematic visits (each n^{th}) to occupied dwelling units (households) following a predetermined path from their random starting point.

To this stage of the process, this is clearly a probability sample though it may be subject to some small errors: some inaccuracies in Census reporting itself (typographical or fieldwork based); changes which may have occurred between the latest Census data and the timing of the survey; arithmetic and/or clerical errors in the stratification, subsampling and zoning procedures. But, in principle, probabilities of inclusion into the sample are knowable within only very minor margins of error.

The "modified probability" aspect of A.I.P.O.'s design enters at the interviewing stage itself: the interviewer is instructed to interview (a) the youngest man, 18 years of age or older, who is at home at the time, or, (b) if there is no man at home, the oldest woman, 18 years of age and over, who happens to be at home.

This, of course, introduces a non-probabilistic component into the sampling design in that one of two "types" of respondents are certain to be included, with a female respondent chosen contingent on the absence, at the time of the interview, of a male (and, of course, if there is no male in the household to begin with), while other residents of a household have zero probabilities of inclusion whether they are at home or not.

A.I.P.O.'s rationale is empirical (and thus experiential) in character rather than being grounded in mathematical sampling theory: the "element" (individual respondent) selection procedure yields an empirically better age distribution, for men and women separately, because it increases the probability of including younger men who are at home less frequently, and also the likelihood of including older women who otherwise might be under-represented in the actualized sample.

Interviewer choice (as in many "quota" sample approaches) is eliminated, the procedure is systematic and, within its definition, objective. Be it as it may, it is not probabilistic and thus the eventual sample straddles the thin boundary between purer "probability samples" and "probability-to-the-household-but-not-within-household" approaches, that is, "quota samples", sophisticated or otherwise.

But some non-probabilistic "adjustments" are generally made even prior to the element selection. The organization compares the outcome of its sampling procedures with the (latest) Bureau of the Census estimates of the regional distribution of the population. Also, annual Census estimates (for men and women separately) of educational attainment may lead to minor adjustments of the sample (using the most up-to-date "current population survey" estimates of the Census).

The documentation on A.I.P.O. surveys does not amplify the criteria by which a decision is made whether or not an "adjustment" is necessary, or the decision by which an "adjustment" ought to be considered minor, or the effect of such adjustments on the selection probabilities. To maximize the chances that the "adult" to be interviewed will be, in fact, at home, the interviewers are asked to carry out their assignments on weekends and, if on weekdays, after 4 P.M. (women) or after 6 P.M. (men). Callbacks are not used: rather, a "times-at-home" weighting routine is built into the data, an empirically based approach to minimize the sample bias which "not-at-homes" might otherwise introduce into the result.

In no way do we raise these issues to somehow "criticize" the sampling designs of A.I.P.O. Rather, the brief presentation of the whole spectrum of the approach simply shows that in puristic terms, the conventional statistics of generalizability (e.g. sampling tolerances, based on variance estimates) would not be precisely applicable even in surveys as well designed, and as well implemented, as most of the A.I.P.O.'s studies turn out to be.

But, at the same time, those surveys in which some validation criterion is available (such as actual votes following surveys on voting preferences and intentions) show, on an empirical though not mathematical basis, that the use of generalizations rooted in these "modified probability" designs does not lead to grossly inaccurate conclusions (in this instance, "predictions") and certainly does not affect issues of substantive significance which are, themselves, less sensitive to single statistic precision and more driven by data patterns and configurations.

Now, with reference to Table 3, these remarks then apply to the survey files listed under A.I.P.O. as well as to the Potomac Associates studies for the sampling design and fieldwork of which A.I.P.O. was responsible.

Within the limitations mentioned, the survey series permits then soft generalizability to the population of

(a) those who are 18 years of age and older

(b) living in the 48 contiguous states of the Union, and

(c) are civilians who were non-institutionalized (hospitals, prisons).

C.P.S. (University of Michigan) documentation shows that the samples are probability samples (studies 051-057 in the archive files).

1. Largest metropolitan areas in the country are included with a certainty (for instance, in the 1970-1971 survey, 12 such areas are included).

2. The remainder of the country is stratified by region and size-of-community (to follow up on the example: 62 such strata containing one or more primary sampling units were specified for the 1970-1971 survey).

3. Primary sampling units (a county or a group of counties) were then drawn from these strata by PPS procedures (where size of the population was defined in terms of Bureau of the Census data base).

4. In the 74 resulting PSU's (12 from the largest metropolitan areas and 62 from each of the strata), private households were selected (PPS) thus excluding group quarters, institutionalized population, noncivilians, and those with no place of residence).

5. No substitutions were permitted at the household level so that the individual randomly selected within the household was to be interviewed.

In this instance, of course, generalizations to the underlying population are applicable even in the puristic sense.

a. In the earlier surveys (1960-61, 1964-65, 1966-67, 1968-69), the generalizations apply to Americans (in the contiguous states) 21 years of age and over (eligible, at least potentially, voters).

b. In the latter surveys (1970-71, 1972-73 and 1974-75), the generalizability extends to Americans (in the 48 states) who are potentially eligible voters; 18 years or older.

c. Since age information is provided on a year by year basis rather than in grouped form, all of the CPS studies permit comparisons of those who were 21 years of age or older, whereas only the last three surveys (1970 plus) yield also data about those in the 18-20 year old bracket.

d. There are some "strays", however, The 1960-61 survey also includes one 20 year old, and thus in sampling terms ineligible respondent. The 1964-65 survey include one 19 year old and 2 (otherwise ineligible) 20 year olds. The 1968-69 survey includes one record which identifies the respondent as being 20 years of age. The 1972-73 survey (with eligibility extended to 18-20 year olds) contains 5 records of 17 year olds.

e. The 1968-69 and 1970-71 surveys oversampled black Americans. For instance, 158 blacks "fell" into the cross-section sample, but another 114 were interviewed (in small segments with 3-6 dwellings in which prior surveys located at least one black household). These surveys then provide for better reliabilities in estimating statistics from black Americans, but for aggregate purposes (and thus for aggregate comparisons with the other surveys in the CPS series), a weight of approximately .58 is applicable to the responses by black Americans.

f. The surveys are not generalizable to noncivilians, to those living in group quarters (dormitories, fraternities, rooming houses and the like), to those who were institutionalized (hospitals, homes for the aged, convents and the like), and those without a place of residence.

Furthermore, C.P.S. estimates about 1.5 coding errors per interview (on the basis of their quality control which involves check-coding of every one out of ten interviews). There is no reason to suspect, either on the basis of the marginal distributions of the data or on the basis of the documentation, that such errors would be systematic and would tend to characterize one variable more than another one. Thus an essentially random distribution of such data processing errors would not introduce an unmeasured bias into the findings and would have no measurable effect on the conclusions which might be drawn upon generalizing. The main thrust of the sampling designs of HARRIS surveys might be summarized as follows:

1. Stratification by four regions (East: including the Bureau of the Census divisions of New England and Middle Atlantic, along with Delaware, Maryland, D.C. and West Virginia; South: South Atlantic, East South Central and West South Central Census divisions except for the South Atlantic division states included in the HARRIS Eastern region as above; Midwest: East North Central and West North Central divisions; and West: Mountain states and the Pacific division, except for Hawaii and Alaska).

2. Stratification by size-of-community:

   a. cities (as every place defined as a central city by the Bureau of the Census)

   b. suburb (as every place not part of a central city but within a Census-defined "urbanized" area)

   c. town (city, town or village with 2,500 or more inhabitants except for those included under the prior city/suburb categories)

   d. rural (all other places and areas not included in any of the above).

3. Proportionate to size (having arrayed the 16 resulting strata by population and location), 100 sample points are selected.

4. Within such 100 PSU's, random selection yields blocks, census tracts or enumeration districts.

5. Interviewers are assigned, on the average, 16 cases with a random starting point and a random routing pattern, with systematic selection (every n^{th}) of households within the fieldwork areas.

6. Enumeration of all adults, by age and sex, within the household is required of the interviewer, and a randomized table pin-points the specific person to be interviewed.

7. Callbacks are asked for if the randomly selected interviewee is not at home at the time in that the interviewers are to make an appointment to be able to talk to the specifically selected individual.

8. When no one is at home initially, three callbacks are provided for - at different times of day and on different days.

9. No substitutions are made at all when there is no one at home upon such three callbacks.

10. Refusals to be interviewed lead to the substitution of the given household by the next one in the random pattern.

The HARRIS practice, however, differs somewhat from the design specifications: Normally, no callbacks are made for the not-at-home households and substitutions are made following the initial failure to "reach" someone in the household.

While interviewers keep detailed records on not-at-homes and refusals, the documentation is somewhat spotty and not, to our knowledge, in the public domain in detail that would permit the assessment of both the overall design as well as its relationship to practice and to the outcome of the practice.

Yet, the basic designed-for procedure, in its multi-stage cluster approach, is probabilistic in character so that it seems quite legitimate to generalize the results to the underlying population.

a. File studies 022, 023, 028 and 153 are generalizable to the noninstitutionalized civilian population 21 years of age and older of the 48 contiguous states (plus, of course, the District of Columbia).

b. Studies 031, 032, 154 and 155 use 18 years of age as the eligibility cut-off at the lower end of the age distribution.

c. Subsamples of 16-20 year olds (acquired by the same procedures as have been previously discussed but by altering the age eligibility criterion) are available in studies 024, 029 and 152.

d. Survey file #026 uses 19 years of age and over as the adult cut-off point, but also involves a subsample (file #027) of respondents 14-18 years of age (though the actual distribution of the age cohorts shows a few stragglers who are 13 years of age, and a few who are 19 and 20 years of age and thus "should" be really transferred to the #026 file for analytic purposes).

Except for files #024, 027 and 026 in which age is coded on a year-by-year basis, the HARRIS surveys provide precoded, and not altogether consistent, age groupings. But: all of the surveys in the series make it possible to select out respondents 21 years of age and older and generalize to the resulting population of the 48 states - save only for those surveys which focusses also on teenage respondents (024, 027, 152).

In turn, generalizability to those 18 years of age and over is appropriate for all surveys under (b) above as well as by selecting such respondents from the teenage surveys 024, 027 and 152 in all of which it is possible to specify "18" as the cut-off age (024 and 027) or the category (18-20 as in 152).

The N.O.R.C. surveys included in the archive reflect three distinct (chronological) phases. The studies with file numbers 050, 066, 018, 019, 020 involve multi-stage probability samples to the block level, but quota samples at the household level. The surveys in the 1975-1976 time frame (021 and 148) entail split sampling: At the household level, half of the sample is a quota sample and the other half, a probability sample of respondents within the household. Finally, the 1977 and 1978 surveys involve strict probability sampling at all levels of the multi-stage process.

The key documentation, General Social Surveys, 1972-1978, July, 1978, the "quota provisions" of which also apply to the Ennis 1966 survey (066) and the University of Pittsburgh survey of 1966 (050), has the following to say in its "Sampling Design" Appendix A, pp. 171 ff. of the cited document:

> "In the original National Science Foundation grant, support was given for a modified probability sample. Samples for the 1972 through 1974 surveys followed this design. This modified probability design. . .introduces the quota element at the block level. The NSF renewal grant, awarded for the 1975-1977 surveys, provide funds for a full probability sample design, a design which is acknowledged to be superior.
>
> Thus, having the wherewithal to shift to a full probability sample with predesignated respondents, the 1975 and 1976 studies were conducted with a transitional sample design, viz., one-half full probability and one-half block quota. The sample was divided into two parts for several reasons: 1) to provide data for possibly interesting methodological comparisons; and 2) on the chance that there are some differences over time, that it would be possible to assign these differences to either shifts in sample designs, or changes in response patterns. . .
>
> There is considerable controversy and ambiguity about the merits of these samples (emphasis added). Textbook tests of significance assume full rather than modified probability samples. In general, the question of what to do with a mixture of samples is no more easily solved than the question of what to do with the "pure" types. Investigators who have applied statistical tests to previous General Social Survey data should continue to apply these tests (emphasis added). Investigators who have refrained from applying such tests may now want to perform analyses on the probability subsample (emphasis added). This would, of course, reduce the number of cases by one half. Whatever choice investigators make, it should be remembered that the two subsamples represent the same universe (emphasis added).
>
> Having allowed for the appearance of all items in the transitional sample design, the General Social Survey then switched to a full probability sample for the 1977 and 1978 surveys." (op. cit. p. 171).

In fact, of course, the "merit of the samples" is neither controversial nor ambiguous.

1. In strict terms, one cannot generalize from quota samples and this is an altogether unambiguous assertion with its noncontroversial rooting in mathematical sampling theory.

2. In practice, many researchers do use tests of significance (and sampling tolerance statistics) with quota samples and this, too, is a fact which is both noncontroversial and unambiguous.

3. The only controversy, and quasi-controversy it is, has to do whether such research "ought to" or "ought not to" use statistical manipulations which are not strictly warranted by the data base. But this, it would seem, is more a matter of taste or analytic style (an artistic and esthetic matter so to say) than it is a real controversy or real ambiguity: Really, "they" (such researchers) cannot use tests of significance "meaningfully".

4. But it is also clear, and we adhere to that position, that the search for patterns and configurations is one of inquiry into substantive significance more than statistical significance, and that there really exists no mathematical theory on generalizability of, or limits to generalizability of, configurations of data anyway.

5. Thus the assumption of researchers dedicated to tests of significance that quota samples will not dramatically alter the results which would be obtained in strict probability samples - whereby the use of statistics based in probability sampling theory becomes possible, if not appropriate - is probably justified - though this, too, could not be proven - and the use of statistical tools for generalization becomes a kind of proxy for data dredging out of which issues of substantive significance can be derived better and faster than it might be otherwise doable.

Whether or not investigators who have been applying "tests of significance" to inappropriate sets of data "should continue" to do so, as the N.O.R.C. document suggests, is something of a moot point because potential cummulation of errors certainly does not increase one's confidence that this or that interpretive error will have been avoided.

But since even quota samples use (a) similar interviewers, if not the same ones, (b) similar interviewer training, (c) similar written instructions to interviewers, (d) similar quota assignments - and ipso facto involve similar random or patterned errors (which is which cannot be told), comparisons of data sets from different quota surveys or overtime quota studies might be easily assumed to produce valid statistical comparisons even if the statistics used in each single survey are less than (mathematically) appropriate ones.

Thus, pragmatically, N.O.R.C.'s advice is probably quite sound but it has no more justification than just the opposite advice - that is, to discontinue the use of tests of significance rather than to continue using them.

The main caveat which is made explicit in the documentation is as follows:

> "Although the mean squared error cannot be estimated directly from a quota sample, one can make estimates of sampling variability using procedures such as those outlined by Stephans and McCarthy (Chapter 10 of Sampling Opinions: remark added by this author). Past experience would suggest that, for most purposes, (this) sample of 1,500 could be considered as having the same efficiency as a simple random sample of 1,000." (op. cit. p. 173)

Again: the 1.5 design effect factor is experientially based and, in part, on the Stephans and McCarthy approach which Kish (Survey Sampling, esp. pp. 562-566) considers to be more sanguine about the problem than might prove desirable.

The use of a design effect factor, such as 1.5, has the overall impact of increasing the belief of the researcher that a procedures which is really inadmissible in sampling theory terms becomes somehow appropriate and admissible when the factor is analytically applied to the data. But this is, of course, not quite the case and empirically reasonable and intuitively arrived at conclusions cannot, in the purer sense, substitute for theorems derived from mathematical sampling theory.

The characteristic N.O.R.C. sample involves:

1. Selection of Primary Sampling Units in Standard Metropolitan Statistical Areas and non-metropolitan counties all of which, in turn, have been stratified by national region, age and racial composition.

2. Block groups and Enumeration Districts, stratified prior to selection by race and income, form the second major stage in the process.

3. PPS method of selection is then used to identify specific blocks to be included in the sample.

4. Where block statistics are not available, N.O.R.C. obtained the appropriate size measure by actual field counting (of dwelling units).

5. On the average, 5 respondents are selected in each cluster (block).

6. Travelling in a predetermined direction, the interviewer begins in the Northwest corner of each block and proceeds until the respective quota is filled.

7. Approximately, equal numbers of men and women are to be interviewed, but the precise proportion in each sampled segment is predetermined in light of Census tract data.

8. Among men, the quota is to replicate, as much as possible, the proper proportion of those who are under 35 (the most difficult group of potential respondents to find at home) and 35 or more years of age.

9. Among women, the quota is to represent the appropriate proportion of employed and unemployed women in the selected locations.

N.O.R.C.'s strict probability sample, stratified, multi-stage area (probability) sample of household clusters as it is, entails:

1. Within each of the nine Bureau of the Census Divisions (excluding Hawaii and Alaska from the Pacific states), SMSA's and non-metropolitan counties were grouped by population size (1970).

2. Within each size stratum, groupings were further based on geographic location and/or racial composition of the population.

3. "Zoning" established subgroups of the stratified areas into clusters of approximately equal population.

4. The selection of Primary Sampling Units was then accomplished to produce four independent samples of equal size.

5. The subsamples were then randomly combined into two larger subsamples (of 101 PSU's each).

6. One of the two resulting subsamples was selected by N.O.R.C. as the principal frame of households to be used until the end of 1970's.

7. Census block groups of Enumeration Districts were then selected directly (without the need for clustering the selections initially within urbanized areas or counties). Prior to the selection, the tracts, minor civil divisions and county divisions in which the block groups or E.D.'s were located were stratified by geographic location, income and race.

8. PPS procedures were used in the selection of block groups and E.D.'s "in numbers sufficient to satisfy survey demands for households expected throughout the decade." (op. cit. p. 175).

9. As in their third stage in the quota sampling methods, where detailed data on blocks or E.D.'s were not available, N.O.R.C.'s field personnel carried out a listing of all separate households either in the field or from such directories as may have been available.

The resulting sample amounts to an inventory of identifiable private households, and the selection procedures in the various stages of the process insure that the selection probabilities of each such household are known, and due to the PPS approach, the probabilities are equal for each individual household so that the sample is a self-weighting one.

The data are then generalizable to

a. English-speaking residents

b. 18 years of age and older

c. civilian

d. non-institutionalized

e. in the contiguous (continental) States of the Union.

To be sure:

The 1972, 1973 and 1974 surveys (as well as the 1966 Ennis and University of Pittsburgh studies) are characterized by the block quota design; and are, therefore, not strictly generalizable.

The 1975 and 1976 surveys are half-quota and half-probability samples, so that strict generalizability is applicable to one half of the sample (and an identifier tag is available in the data set to permit the distinction between respondents in the two halves of the total sample).

The 1977 and 1978 surveys in the archive are probability samples (of households) and they, self-weighting as they are, allow for generalizability even in the strict sense of the term.

The UCSUR archive also include the Attitude Supplement File from the 1972 National Crime Survey (File #064 and 065). The documentation is quite detailed so that there seems to be little need to do much more than provide a succinct summary.

Memorandum No. 15 of November 17, 1975, prepared by Leon Martin, Linda Murphy and Patricia Rogers and approved by Barry M. Cohen, Assistant Division Chief of the Demographic Surveys Division of the Bureau of the Census, specifies that the file, both with the household and individual base, was "prepared by DUALabs under the sponsorship of LEAA." It contains, as UCSUR received it, data from Form NCS-5 along with neighborhood characteristics data based on the 1970 Census information. The compilation has been edited and weighted.

The basic sampling information is detailed in National Crime Survey, National Sample, Survey Documentation, Bureau of the Census, 1976.

1. The Primary Sampling Units (1931 of them) were formed from counties or groups of contiguous counties in both the coterminous United States as well as in Alaska and Hawaii.

2. The 1931 basic PSU's were the same as those used in the Census Bureau's Current Population Survey.

3. The PSU's were regrouped into 376 strata such that 156 strata consisted of a single PSU, and one PSU was selected from each of the remaining 220 strata.

4. The 220 strata were formed by combining PSU's with similar characteristics: geographic region, population density, 1960-1970 population change, racial composition, principal industry, number of farms, retail sales per capita.

5. The selection of the specific PSU from each of the strata was done proportionate to size (PPS procedure).

6. With equal probability of initial selection, thus yielding a self-weighting sample at the household level, 72,000 households were included in the sample, and divided into 6 panels (of 12,000 each).

7. In selecting households, the first stage of the process involved a choice of Enumeration Districts by PPS procedures within each PSU, and systematically with respect to a geographic array (within the PSU) to spread the sample ED's over the entire Primary Sampling Unit.

8. The second stage of the household selection process in each ED (of about 300 households each) involved a choice of clusters or segments of about 4 households each. For about 66 percent of the ED's, 1970 Bureau of the Census address lists were used in the clustering process, while small area clusters were selected for the remaining ED's. Units constructed after the 1970 Census were sampled almost entirely on the basis of new construction permits issued by the responsible authorities in each particular area. Group quarters (dormitories or boarding houses and the like) were field-listed and sampled.

Experience has indicated that about 10,000 of the 12,000 sampled households in a given month get actually interviewed.

(a) A-type noninterviews include those cases in which the interviewers were unable to obtain an interview (not-at-homes on a repeated basis, refusals).

(b) B-type noninterviews involve vacant, or otherwise ineligible, dwellings (and these, according to the documentation, might be revisited at a future date to determine such status changes as may occur).

(c) C-type noninterviews involve units which may have been demolished, converted to nonresidential uses or are otherwise outside of the scope of the Survey's design specifications.

The nature of the sampling design, along with the quality control measures (see documentation referred to above) on both fieldwork implementation and all aspects of the data reduction process, then permit generalizability to

(a) households (household file) or individuals (individual file) who are 12 years of age and older,

(b) live in the coterminous United States or in Alaska or Hawaii,

(c) in private dwellings or in group quarters

(d) are civilian or, if members of the armed forces, do not live in military barracks,

(e) are not institutionalized (hospitals, prisons).

Some of the most pertinent commonalities and differences among the national surveys in the archive may be summarized.

1. All of the surveys involve stratified, multi-stage probability samples up to, and including, the block level.

2. The modes of stratification, however, are somewhat variable.

3. Bureau of the Census data are consistently used as the basis on which to stratify, with varying attempts - as a decade goes on - to update the information in the field itself or by perusal of the most up-to-date Census Bureau estimates.

4. Except for the LEAA sponsored and Bureau of the Census conducted national survey (File #064 and 065) which includes Hawaii and Alaska and also respondents in specified group quarters, all of the surveys refer to the coterminous United States (48 continental states plus the District of Columbia) as the referent population, all pertain to nonmilitary personnel, and all exclude institutionalized residents.

5. Due to the prevalent year-by-year of age coding (except for some of the HARRIS surveys) and the age-delineated eligibility for inclusion, all surveys (including HARRIS) make it possible to consider, for analytic purposes, those who are 21 years of age and over at the time of the survey.

6. With a few exceptions (HARRIS surveys for the most part, and only some of them), the underlying population frame includes also those who are 18 years of age and older, and by a select routine which would combine the "adult" (21 years of age and over) with data from 18-20 year olds, it is also quite possible to use those HARRIS data which otherwise pertain to only those who are 21 years of age and older (thus part of file #024 can be pooled with #023, as can #152 with #153).

7. A.I.P.O.'s final (respondent) selection procedures are neither quite of a "quota variety" (since the interviewer has specific instructions whom to interview) nor are they probabilistic (since some potential members of a household have zero probabilities of inclusion) so that soft, rather than pure, generalizability seems appropriate - in other words, a sampling purist might be tempted not to generalize, in statistical terms, at all.

8. N.O.R.C.'s data series, until 1974, involves quota sampling at the element level, and half of the samples in the 1975 and 1976 files are also quota based. Thus a similar caution applies as it does to the A.I.P.O. files.

9. N.O.R.C.'s gradual transition to full probability samples (1977 and 1978), however, permits methodological comparisons

(a) of the quota and probabilistic halves of the 1975 and 1976 surveys so that an empirical assessment of the sampling design effect is possible, and,

(b) comparisons with antecedent quota based series as well as with the subsequent strict probability sample results (1977 and 1978) makes it possible to provide an empirical evaluation of such statistical trends as seem to exist.

(c) Since the General Social Surveys of N.O.R.C. use many "core questions" repeatedly, and in identical form, such comparisons are not merely facilitated but the potential for them has been built into the transitional survey designs quite deliberately - and this, clearly, is a strong advantage.

10. At the same time, even if such analytic comparisons show that the quota samples (when considered, perhaps, "equivalent" to probability samples smaller by a factor of about .67) yield essentially the same results as to probability samples in their stricter definition, we cannot assume automatically that such equivalences hold over all relevant variables or clusters of variables, or that they will, invariably, hold in future surveys. The decision does, and will, remain with the style of the researcher in each instance on the one hand, and on the other hand with the probable consequences (in policy terms) of arriving at inaccurate, if not outright "wrong", conclusions.

That much of the lore of survey research analysis supports, or minimally tolerates, generalizations derived from non-probabilistic samples does not, in itself, resolve the pestering technical problem, although search restricted to robust findings, without necessarily bracketing them in confidence interval statistics, may well be actually more, rather than less, conducive to the development of better theories and better classes of policy relevant inputs as long as such findings are not the sole, or dominant, determinants of actual policy.

11. Overtime comparabilities of results are enhanced by the fact that many of the surveys in the archive have been carried out by only a few major research organizations. Regardless of the eventual sampling design, within each time series comparisons are facilitated because the organizations have generally used, in this time period, essentially the same sampling design over time or else, as N.O.R.C., have modified the sampling design in a well defined, and therefore interpretable, manner.

12. Such overtime comparabilities (and the softer generalizations possible from them) within each data series make good analytic sense unless we were willing to assume that in quota-based or "modified probability" samples there exist systematic and time-correlated biases. If we relax such an assumption and assume some bias in each (quota) survey but not systematic

variabilities over the time periods of the surveys, there is not much of a problem in trend assessments whether or not tests of statistical significance (of difference) are, pragmatically rather than appropriately, used or not.

A further study of Table 3 reveals also that, for many of the years covered by the archive, we have two or even three studies available for a number of the specific years.

1. Apart from sampling differences, the studies for a given year cannot be usefully "pooled" together simply because the styles of questionning of the various organizations differ and the foci of the studies vary.

2. However, such pooling is, in principle, possible for the A.I.P.O. surveys #007 (March, 1972) and #008 (October, 1972) and HARRIS Surveys #031 and #032 (March and October, 1973, respectively) if only to determine, on comparable items, whether intervening "events" might account for differences or whether the findings yield homogeneous results and thus become jointly "representative" of the pattern of national thinking in that year (rather than only at the time of the study).

3. The fact that most of the archive data come from only a few research organizations and that there are data sets available for some of the years from several of the organizations and that the foci of the inquiries varied permits a wider ranging search for data patterns across the year's surveys even if each one needs to be treated as a separate perspective on the nation's sentiments. As a consequence, in the years in which two or three studies are available, we have a broader variety of information, of somewhat differing reliabilities (as a function of the sampling designs discussed here), which allows the piecing together of a more configurational mosaic of national thinking and which, contrasted with the generic intersurvey issues raised in other years, makes it possible to trace underlying patterns of thought better (even if the components of the pattern, each and every one taken alone, are not generalizable with desirable precision).

Table 4 provides a summary of the archive surveys in which the data pertain to specified central cities around the country. It will be noted that some data are available from each of 32 major cities. In many instances, several data sets are applicable. It will be also noted that the central city data sets cover only the period between 1968 and 1975, although some of the Detroit Area studies, prior to 1968, are simply not tabulated here. Finally, it may be noted that the bulk of the surveys in the file derives from three sources: the LEAA/Census central city samples, both in their household and individual modalities and, when available, for two time points; the Michigan 1968 study in which samples were drawn from 15 major cities; and the Urban Observatory, 1970, study in which 10 major cities were included.

Table 4

1968-1975 SURVEYS IN CENTRAL CITIES

	1968	1969	1970	1971	1972	1973	1974	1975
1. Albuquerque			X[1]					
2. Atlanta			X[1]		X[2]		X[2]	
3. Baltimore	X[4]	X[3]	X[1]		X[2]			X[2]
4. Boston	X[4]	X[3]	X[3];X[1]				X[2]	
5. Brooklyn	X[4]							
6. Buffalo							X[2]	
7. Chicago	X[4]				X[2]			X[2]
8. Cincinnati	X[4]					X[2]	X[2]	
9. Cleveland	X[4]				X[2]			X[2]
10. Dallas					X[2]			X[2]
11. Denver	X[4];X[6]		X[1]		X[2]			X[2]
12. Detroit[5]		X[7]			X[2]			X[2]
13. Gary	X[4]							
14. Houston							X[2]	
15. Kansas City, KS			X[1]		X[2]			
16. Kansas City, MO			X[1]		X[2]			
17. Los Angeles					X[2]			X[2]

Table 4 (continued)

	1968	1969	1970	1971	1972	1973	1974	1975
18. Miami							X^2	
19. Milwaukee	X^4		X^1				X^2	
20. Minneapolis							X^2	
21. Nashville			X^1				X^2	
22. New Orleans					X^2			X^2
23. Newark	X^4					X^2		X^2
24. New York City							X^2	
25. Oakland					X^2			X^2
26. Philadelphia	X^4						X^2	
27. Pittsburgh[8]	X^2				X^2			X^2
28. Portland							X^2	
29. San Diego			X^1		X^2			X^2
30. San Francisco	X^4					X^3	$X^2;X^3$	
31. St. Louis	X^4						X^2	
32. St. Petersburg					X^2			X^2
33. Washington	X^4						X^{10}	X^{10}
							X^2	

Notes

1. LEAA Computer File #041; ten cities survey (Urban Observatory).

2. National victimization studies, attitude supplement data; data available for both individual respondents and households.

3. Disaggregatable from SMSA-wide surveys.

4. LEAA Computer File #042; 15 cities study (Campbell and Schuman) white samples; #045 - black sample.

5. Other Detroit area survey data from 1963, 1967, 1967-1968 and 1969 also available. Not tabulated herein.

6. LEAA Computer File #060, black respondents only.

7. LEAA Computer File #061, white respondents only.

8. A 1976 Pittsburgh Neighborhood Atlas data also available.

9. Police Foundation studies.

10. Citizens Survey.

There exists, of course, ample documentation concerning the central cities samples in the LEAA/Bureau of the Census National Crime Surveys. Multi-state probability samples were used with selections of some 12,000 respondent households of which about 10,000 were expected to yield actual data (accounting for refusals, not-at-homes, vacant dwelling units).

1. The major sampling frame consisted of the 20 percent Bureau of the Census 1970 sample of the Census of Housing and Population.

2. The (computer) file of this 20 percent sample was differentiated into (1970) occupied dwelling units, vacant ones, and group quarters.

3. Occupied housing units were further stratified by income, tenure (owner/renter) and family size (1, 2, 3, 4, 5 or more) resulting in 50 basic strata.

4. The strata were further subdivided by race of the household's head (in light of 1970 Census sample data).

5. For vacant units and group quarters, the records were computer-sorted into five strata, to wit: low value vacants, medium value vacants, high value vacants, residential vacants and group quarters.

6. Group quarter records included: rooming and boarding houses, communes, missions, flophouses and the like, group quarters of general hospitals, military installations, religious group quarters, college dormitories, fraternity and sorority dwellings, dormitories for agricultural workers, other (possible) worker dormitories.

7. The second sampling frame (beyond the 20 percent Census sample of the 1970 data base) including dwellings for which construction permits were issued after 1970, and these were arrayed chronologically by the date of permit issue prior to sampling.

8. Units which fell into other samples of the Bureau of the Census were excluded from the final sample.

9. All residents, 16 years of age and over, at the selected address were eligible to be interviewed, and a "household" data interview was carried out with an eligible adult for the household dimension of the survey.

10. Tourists, other seasonal residents, institutionalized residents of the cities were not eligible as potential interviewees.

As was done in the nationwide survey (file #064-065), the initial household contact was on a face-to-face basis, but if subsequent contacts were necessary (to interview all eligible respondents), discretionary decisions by interviewers (or, better yet, by field supervisors) permitted phone contacts.

While "household"-relevant data generally come from one of the adults in the dwelling (and any adult was eligible to be interviewed), the interviewers were asked to talk to the "most knowledgeable" member--almost invariably, as might be expected, the head of the household or the spouse.

> "If it become apparent that the particular household member being interviewed for the household information ("Control Card" items, "Household Screen Questions" and "Household Attitude Questions"), a more knowledgeable respondent was found, or arrangements were made to call back when a knowledgeable respondent was available."

The problem, in this regard, has to do with the fact that the final eligibility system (and the resulting data collection modality) was driven by knowledge of "facts" about the household and its members so that the "attitude" component of the survey involves a non-probabilistic element based on interviewer judgement as to who was knowledgeable, how much and about what.

In the "individual" file, this is not a problem, since the attitude supplement questions were to be asked of all household members 16 years of age and older responding on their own behalf (and thus in terms of their own opinions and sentiments).

In Introduction to Data, Central Cities Sample, DUALab (undated!), we find that

> "There are no geographic identifiers for areas smaller than the city as a whole due to the requirements for preserving confidentiality of individual respondents." (op. cit. p. 2)

In UCSUR files, there are also no identifiers which would permit the use of the panel feature of these repeated surveys: Thus it is not possible to "trace" the responses of specific respondents on the two interview occasions (in cities, as in Table 4, in which two waves of interviews were carried out.

Thus even though the data files include "household information" (attitude supplement variety) for 6,000 or more households in each of the city, no disaggregation is possible in terms of neighborhood types and subcity contexts.

Even though the files contain information about some 10,000 individual respondents (from the respective households), no net evaluation of changes, which one designs panel research for to begin with, seems possible at least from the material which was made available to UCSUR.

But then, we are also told (in the document referred to above) that,

> ". . .the data provide only an estimate of the true population characteristics. An analysis breaking down the population into small groups may result in unreliable estimates." (op. cit. p. 2)

But if, in fact, we are dealing with multi-stage stratified probability samples, as the documentation makes clear that we do, then segments of the population (e.g., income groups, age groups, and the like) are themselves probability subsamples and the only issue of "unreliability" has to do with the varying ranges of estimates which can be made with similar confidence dependent on the subsample size only. In this instance, the DUALab caution that "unreliable" estimates "may" result is not interpretable either in statistical or substantive terms.

LEAA/Bureau of the Census must ask themselves, if they have not already done so:

1. If the only statistical generalizability that is valid to the "eligible" universe on a city-wide level, why such large samples? The sampling errors decrease only as a square root of increases in sample size. In effect, multi-stage probability samples of 1,500 would produce "reliability" of estimates not in any dramatic way different from the samples of some 10,000.

   The advantage, apart from whatever political reasons, may well rest in the search for more "unique" or "unlikely" events (victimizations) some of which a sample of 1,500 might easily mask (e.g, "rape" attempts).

2. If the researcher cannot treat the data as a panel (so that respondents from a prior wave can be identified in subsequent waves as well), what is the possible use of a panel design?

   Perhaps the "panel features" are preserved properly in the Government's data files; perhaps more specific residential identifiers (neighborhood and the like) are also preserved in some such files; but the research community outside of Government simply stands to loose the opportunity to assess the information in its original richness (by making some theoretically crucial, such as "neighborhood", disaggregations impossible), and it also looses the capability to study the real dynamics of evolving attitudes (by masking the panel feature of the reduced data).

That all this somehow "protects confidentiality" so that "identities of particular households" will not be disclosed is a totally specious argument, and it is certainly not in keeping with the privacy law's intent, and quite probably not even with the letter of the law.

To be sure: our crime opinion archive files then contain extremely rich, and carefully collected, data but in a poor analytic mode, resulting from narrow bureaucratic interventions NOT IN THE DESIGN OF THE SURVEY, THE CAREFUL SAMPLING DESIGN, THE CAREFUL DATA COLLECTION EFFORT, but from interventions in the data reduction and in accessibilities of the data to the research community.

With central cities of about 400 in each of the ten cities, the Urban Observatory Program's survey of 1970 is based on

1. A sample from City Directories, corrected by block supplements (in the way of field visits).

2. Each household in each of the 10 cities had an equal probability of selection (a random sample of households).

3. One adult, 18 years of age or older was the respondent in each household, and the specific individual to be interviewed was chosen randomly (using tables of random numbers which interviewers applied given the composition and size of the household).

4. No substitutions were allowed either at the household or the individual level, and up to six callbacks were required (with at least two weekend or evening hour calls) before a "not-at-home" record was established.

The samples, according to the documentation by the Urban Observatory Program, do not pretend to represent the population of the nation's central cities as a whole, nor are they representative of the nation's SMSA's, or even of the SMSA's within which the study cities (Atlanta, Albuquerque, Baltimore, Boston, Denver, Kansas City, Mo., Kansas City, Ks., Milwaukee, Nashville, and San Diego) were located.

(a) The Observatory samples are generalizable to the respective central cities,

(b) to the population of households in private dwellings,

(c) to residents 18 years of age and older,

(d) civilian,

(e) noninstitutionalized,

(f) not living in group quarters.

In the 15-city survey (1968, file #042), we have separate samples of black and white residents. In each of the cities, the respective subsamples (of blacks and of whites) involve some 120 to 242 residents. Because of the method of selection, the white and black samples cannot be pooled together without applying appropriate (1960 Census) weights to "discount" the black subsample (by proportioning it to the 1960 racial composition data on the cities).

1. Randomly, city blocks were selected within each city.

2. This was done, upon racial stratification of areas, by selections with probabilities proportionate to size (wherein size refers to the number of private dwellings).

3. Five dwellings were randomly selected within each block.

4. All persons, within the selected dwellings, 16 years of age and up to 69 years of age were prelisted by the interviewer.

5. In one half of one (eligible) person housing units, the eligible respondent was interviewed. In two person dwellings, one of the two eligibles was randomly chosen for the interview. In households with three or more (eligible) respondents, at least one (randomly) but not more than two were selected for the interview.

6. In the larger households (three or more eligibles), in so far as two individuals were to be interviewed, systematic procedures were followed to be sure that one of these eligibles is a member of the "older generation" and the other one of the "younger generation" upon field-stratification of the household "census" by age.

7. No substitutions were permitted at the individual level once the selections were made by the above procedures.

Now some indeterminacies enter into the data because the fieldwork was carried out by several different research organizations, and the documentation states that they "designed their own interviewer instructions".

Thus the surveys in Baltimore, Cincinnati and Detroit were conducted in the field by the University of Michigan's Survey Research Center; the surveys in Boston, Brooklyn, Chicago, Gary, Newark, Pittsburgh, St. Louis, San Francisco, and Washington, DC, were conducted

by N.O.R.C.; the Survey Research Laboratory of the University of Wisconsin was responsible for the fieldwork in Milwaukee, and the Philadelphia component was launched by Temple University's Institute for Survey Research.

Basically then, the results are generalizable to

1. Black residents of the 15 cities, 16 to 69 years of age,

2. White residents of the 15 cities, 16 to 69 years of age,

3. In private housing units,

4. Civilian,

5. Noninstitutionalized,

6. Not living in group quarters.

Generalizability to subgroups of black or white respondents is limited by the overall sample sizes. Thus comparisons between men and women, or in an age dichotomization (younger and older) or trichotomization, or by formal education (such as high school or less, some college or more and the like) are statistically possible and even meaningful but the sampling tolerances are rather large so that only truly robust differences matter with respect to any given statistic.

But, of course, for comparisons of black and white attitudes in a good number of major cities of the nation the data base is exceptionally rich and generalizations, given the probabilistic sampling design, are quite appropriate and thoroughly meaningful.

In terms of potential generalizability to the nation's cities, the archives also include a number of other surveys. In some instances, the researcher needs to select the central city residents from a larger sample of the area (Metropolitan Area as in the Detroit studies referred to, as SMSA as in other cases):

1. Baltimore Harris Crime Survey of 1969 (File #030).

2. Boston area 1969 and 1970 surveys (File #043 and #044).

3. Detroit Area Survey File #059 (1967), 061 (with 1969 white respondents only), and the 1971 survey #062, further details and findings of which can be obtained from Duncan, O. D., Schuman H. and Duncan B., Social Change in a Metropolitan Community, Russell Sage Foundation, 1973.

4. Portland 1974 survey (File #161).

The researcher who might want to use the results to generalize to the underlying wider area population needs to note that, for instance, the Boston 1969 survey (File #069) sampled out-of-city residents at a rate of but 1:9 to the city dwellers. In the Detroit Area survey of 1967 (File #059), the city residents were selected at twice the rate of residents outside of the city limits. Other surveys in the files were, in terms of their geographic area definition, confined to the central cities to begin with:

1. Cincinnati survey of 1973 (File #035 for household data and File #036 for individual responses).

2. San Diego surveys of both 1973 and 1974 (File #037 and #038).

3. St. Petersburg surveys of 1974 and 1975, with File #039 and #040.

4. Pittsburgh Neighborhood Atlas survey of 1976 (File #047).

5. Black household heads or their spouses in the City of Detroit in the 1968 survey (File #060).

6. Detroit residents in the first wave of a longitudinal inquiry of 1967-1968 (File #063).

7. The 1977 (File #162) survey in Portland (comparable with the 1974 survey #161 from which, however, city residents have to be selected).

The Kansas City study of 1972 (File #034) includes a sample from 15 policing areas only, and based on clusters of housing units within these specified areas with programs of crime intervention, the results are not generalizable to the city as a whole (and not, therefore, comparable with Kansas City data from the Urban Observatory (1970) survey or the LEAA/Census 1972 attitude supplement in the victimization research program.

But there are other important cautions which apply to the various surveys of the cities files which render some of them less than reliable.

(a) The Pittsburgh Neighborhood Atlas aimed at a 5 percent random sample of registered voters (from computerized registration lists) in each Ward of the City. But the survey, due to its financial constraints, had to rely on mail-out procedures and the response rate of

about 22 percent does not allow for appropriate generalizability (though the data set provides rich information for more substantive interpretation of patterns both for the city as a whole, for major sections of the city, and for major socio-cultural and demographic segments of the population).

(b) The survey of black residents of Detroit (1968) was stratified by socioeconomic level of the residential subareas from which the households were selected. To allow for better interpretation of data obtained from more well to do black (or mixed) areas, the higher socio-economic area stratum was oversampled by a rate of 2:1, so that, in aggregate analysis, it is necessary to use a weight of 2 for the lower SES-area respondents or else, indeed, treat the two groups of respondents (one from each SES stratum) "analytically" separate without an effort to pool the results.

(c) Similarly, in the Detroit black household study, the reality of many female headed households necessitated overselection of males. Thus the recommended weight, when the sex groups are analytically pooled, is 1.5 for the women (and, of course, 1 for men).

(d) The 1967-1968 Detroit study (File #063) includes a specific supplemental sample in "riot areas" (which were defined for both East and West of the City in terms of the occurrence of allegedly riot-related fires). Thus the "community" sample without the supplemental "inflation" needs to be selected out if the analyst desired to disregard the "riot/non-riot" factor. Even so, the basic sample was stratified (though proportionate to size) into riot-East, riot-West, non-riot East and non-riot West so that considerable caution would have to be exercised in generalizing to the underlying population in any event: in this instance, in fact, the basic sampling frame consisted of the 2-volume (East and West) Polk City Directory.

(e) The St. Petersburg surveys of 1974 and again of 1975 also used, as their basic sampling frame, the City's Polk Directory.

Except for the surveys the sampling frames of which have been noted (registered voters in the Pittsburgh Atlas study, Polk Directories in the St. Petersburg and the Detroit, #063, survey) as being somewhat special, or surveys with a limited community focus (as in Kansis City, #034 and, in part, Detroit #063), rather standard

procedures were used throughout which, in basic terms, increase both generalizability and comparability (at the city level):

1. Multistage probability samples with household clusters (of 3-5 households in the finally sampled segments) are characteristic of the studies.

2. Specific respondents were also selected randomly from among the eligibles, and callback procedures were used to insure that the sampled individual will be actually interviewed.

3. Specific eligibility criteria, however, varied so that the researcher needs to pay special attention to determining what underlying population the data are, in their aggregate form, generalizable to.

4. As was the case with the nationwide surveys, selection routines make it generally possible to redefine the underlying population as those who were 21 years of age and older, even if some of the surveys used other age-related criteria. For instance, the San Diego studies include as respondents those 16 years of age and older; the St. Petersburg surveys, those 18 years of age and older; the Boston Area surveys, both 1969 and 1970, include everyone 21 years of age and older, or younger respondents if married, or 18 years and older respondents if not living with their parents (and thus, in effect, forming a household in their own right).

There are a few major conclusions which we need to make explicit:

> Even if, from the sampling design and implementation standpoint, it would be basically possible to pool data across cities for the same given time tag (say, 1968, 1970, 1972, 1974 and the like), and even if the questionnaire items are identical or very similar it makes no sense to "pool" the data simply because the resulting aggregate has no underlying population definition to which the results might be legitimately generalized or, for that matter, usefully generalized.

Thus the cities files need to be treated each in their own distinct right and, within the limitations we have discussed, they permit comparisons among the cities both at particular times and over time but not any kind of perspective on "cities" in general or on "cities of a certain size" in general.

> Thus even if the national survey results are disaggregatable by resident city size, and even if the sampling designs permit generalizations to a specified population, the city-level survey results, which cannot be pooled across cities, are not a representation of the same type of universe.

This is also empirically, and thus pragmatically, the case: the results from the various cities are quite heterogeneous (as are the results of such aggregate data as those which the FBI collects and provides in the Annual Crime Reports) so that statements about cities, say, of 250,000 and over are really not applicable to any specific city in the size category.

> Hence, generalizations to such categories as "large cities" (say, cities of 250,000 and over) from national surveys, despite disaggregatability and statistical generalizability, are likely to distort the heterogeneous pattern which actually seems to characterize the specific cities themselves.

In other words, national data disaggregations by residential area size provide a more general, ambient, type of profile of differences and similarities between metropolitan, urban and rural America, but they have low utilizability for specific cities (and city areas) and their particular problems in terms of possible policy guidelines.

The best way then is to utilize the national data (upon size-of-community form of disaggregation) as contextual, while the city files permit a more community-focussed concern.

> In terms of the cities data themselves, the major LEAA/Census surveys establish the most appropriate anchorage. This means that we suggest that comparisons of other city data, antecedant to the LEAA/Census surveys, or following them, can best be made relative to the generalizable standards which can be developed from the National Crime Survey materials--but for each city treated as an entity in its own right rather than, in any manner, across the cities.

The archive also includes six statewide files:

Minnesota, 1969 (#016) and 1970 (#017)
California, 1972 (#046)
Texas, 1973 (#067)
Michigan, 1972 (#156) and 1974 (#157).

The generalizability of these surveys to the states themselves is somewhat limited. The Minnesota samples, multistage

probability samples though they are up to the household level (with PPS procedures in tracted areas, and with equal probability area sampling in rural Minnesota), are actually quota samples in which there are relatively complex interviewer judgements involved. The interviewers, following quota assignments, are to insure the proper representation of respondents by race, age, socioeconomic status and town/rural distribution.

The California and Texas studies are, in this regard, perhaps most comparable: in both instances, probability samples of households were selected with systematic choices of interviewees within households, and both studies involved 18 years of age as the lower age eligibility limit.

Furthermore, conducted under the auspices of the Texas Commission on Law Enforcement by Field Research Corporation, the Texas study instrument replicated, almost entirely, the California study of the previous year. But while the interviewee selection procedures are labelled as "systematic", other aspects of the documentation suggest a quota procedure to insure representation by socioeconomic status, race and age. Thus statistical generalizability of these surveys to their respective states (population 18 years of age and older, civilian, noninstitutionalized) is open to question and an analyst insistent on using statistical significance approaches only when the samples meet the appropriate assumptions would be probably hesitant in drawing conclusions about either California or Texas. The Michigan surveys of 1972 and again of 1974 (with 16 year olds and older respondents eligible) included a probabilistic selection of respondents with three callbacks. The household "next door" was substituted (and a specific respondent again selected randomly on the basis of age/sex combinations of eligible residents).

The Michigan data are then generalizable to the state's civilian, noninstitutionalized residents of 16 years of age and older within about 3.5 percent (at the .95 confidence level), and the two successive cross-sectional samples permit an overtime comparison (1972 with 1974) in statistical terms.

In turn, the Minnesota quota samples render comparisons overtime (1969 and 1970) possible for those researchers who are willing to generalize on the basic (empirically <u>not</u> too problematic) premise that quote samples do not really yield results different from purer probability samples.

And finally, the California and Texas data of relatively low generalizability (much like the Minnesota polls) permit strong comparisons across identical questionnaire items even if one were reluctant to draw conclusions about the states as a whole.

Thus in the statewide dimension of the archive, there is essentially a holding operation involved. There are not enough data sets to permit significant comparisons among various states or to allow comparisons of statewide data overtime, and the data sets which are included in the file thus far, except for the Michigan surveys, are possibly of lower quality than might be desired.

VI. BACKGROUND CHARACTERISTICS

It goes without saying that selected sociocultural and demographic traits of respondents, their families and households, their neighborhoods or their geographic areas form crucial independent variables in all surveys. From a massive, and cumulative, body of empirical experience we know that some background traits tend to differentiate among the respondents in a robust manner, or at least in a way which is so persistent across studies and over time that it would be difficult to persuade a survey researcher to disregard them.

In so far as the sampling design and its implementation permits generalizations at the aggregate level, that is, for the sample as a whole to the population from which the data were acquired, it is, for the most part, also altogether appropriate to consider disaggregated groups of respondents as subsamples of the total sample. Thus generalizations become possible regarding the significance of differences between such subsamples within a study, in comparisons of identically defined demographic groups between and among studies, and in comparisons of such subgroups across time.

That statistical comparisons involving subsamples of less than 100 respondents are unlikely to produce very reliable results or that, in effect, only dramatic differences would prove to be statistically significant, is also rather well known so that the researcher would, on the whole, be disinclined to draw generalizations from such small demographic groups at all.

It is also accurate to say that the overall sample sizes in most surveys--and certainly in most of the surveys in the crime opinion data archive we have developed--rarely allow statistically meaningful elaboration of the data by more than two independent variables simultaneously, and even this is more often possible only when both such joint variables do not produce more than four or six subsamples (hence, they are either both dichotomies or one is a dichotomy and the other one a trichotomy). The use, and testing, of path analytic schematizations or other multiple regression models obviates the elaboration difficulty to some extent, but generalizability to an underlying population of such statistics as beta coefficients or path coefficients is certainly open to doubt if only because the use of many of the demographics (especially of the "nominal" level of measurement) involves the use of dummy variables and assumptions which go with it, and the structure of the data in terms of level of measurement does not usually meet the basic assumptions on which multiple regression or path analyses are based.

But viewed as searches for more complex patterns more in a substantive way than in a purely statistical way, the analytic problems which beset elaboration because of the rapid decline in numbers of respondents in each resulting category are partially addressed.

In Tables 5, 6 and 7, we provide a simple tabulation of the basic demographics which are contained in the respective surveys in the archive. Table 5 contains information about the national surveys, Table 6 concerns the central city surveys (including those few instances where SMSA's were used but the results for the city itself can be selected out and remain meaningful) and Table 7 sums up the limited statewide inquiries along the same lines.

Table 5

DATA ARCHIVE DEMOGRAPHICS

NATIONAL STUDIES

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
001	American Institute of Public Opinion, 1960	AIP 634	X	X	X			X		X		X	X		X	
002	American Institute of Public Opinion, 1963	AI6 69N	X	X	X			X	X			X	X	X	X	
003	American Institute of Public Opinion, 1965	AI7 09N	X	X	X			X	X			X	X	X	X	
004	American Institute of Public Opinion, 1967	AI7 49N	X	X	X			X	X			X	X		X	
005	American Institute of Public Opinion, 1968	AI7 57N	X	X	X			X	X			X	X	X	X	
006	American Institute of Public Opinion, 1969	AI7 73N	X	X	X			X	X			X	X	X	X	
007	American Institute of Public Opinion, 1972	AI8 47N	X	X	X		X	X	X			X	X		X	
008	American Institute of Public Opinion, 1972	AI8 61N	X	X	X		X	X	X			X	X	X	X	
009	American Institute of Public Opinion, 1975	AIP 931	X	X	X			X	X	X		X	X		X	

Table 5 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
010	Potomac Associates - State of the Nation, 1972	GA7 235	X	X	X			X	X	X		X	X	X	X	
011	Potomac Associates - State of the Nation, 1972	GB7 235	X	X	X			X	X	X		X	X	X	X	
012	Potomac Associates - State of the Nation, 1972	GC7 235	X	X	X			X	X	X		X	X	X	X	
013	Potomac Associates - State of the Nation, 1974	GA7 445	X	X	X			X	X	X		X	X		X	
014	Potomac Associates - State of the Nation, 1974	GB7 445	X	X	X			X	X	X		X	X		X	
015	Potomac Associates - State of the Nation, 1974	GC7 445	X	X	X			X	X	X		X	X		X	
163	Potomac State of the Nation 1976A, 1976	GAI 976	X	X	X			X	X	X	X	X	X		X	
164	Potomac State of the Nation 1967B, 1976	GBI 976	X	X	X			X	X	X	X	X	X		X	

Table 5 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
051	University of Michigan Center for Political Studies, 1960-1	CPS 60N	X	X	X		X	X	X			X	X			
052	University of Michigan Center for Political Studies, 1964-5	CPS 64	X	X	X		X	X	X			X	X	X		
053	University of Michigan Center for Political Studies, 1966-7	CPS 66N	X	X	X		X	X	X		X		X		X	
054	University of Michigan Center for Political Studies, 1968-9	CPS 68N	X	X	X		X	X	X		X	X	X	X		
055	University of Michigan Center for Political Studies, 1970-1	CPS 70N	X	X	X		X	X	X		X	X	X	X		X
056	University of Michigan Center for Political Studies, 1972-3	CPS 72N	X	X	X		X	X	X		X	X	X	X	X	X
057	University of Michigan Center for Political Studies, 1974-5	CPS 74N	X	X	X		X	X	X		X		X	X		

Table 5 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
022	Harris, 1964	H13 84	X	X	X		X	X		X	X	X	X			
023	Harris Crime and Corrections Study, 1967	H17 58A	X	X	X		X	X	X	X	X	X	X			
024	Harris Crime and Corrections Study, 1967	H17 58T	X	X	X		X		X	X	X		X			
025	Harris National Malaise Survey, 1968	H18 13	X	X	X		X	X	X		X		X			
026	Harris Violence Survey, 1968	H18 87A		X	X		X	X	X	X		X	X	X	X	
027	Harris Violence Survey, 1968	H18 87T	X	X	X			X	X						X	
028	Harris Morals and Values Survey, 1969	H19 33	X	X	X	X	X	X	X	X	X	X				
029	Harris, 1969	H19 70			X	X	X	X	X	X	X		X			
031	Harris, 1973	H23 44	X	X	X	X	X	X	X	X	X	X	X			
032	Harris, 1973	H23 43P	X	X	X	X	X	X	X	X	X		X	X		

Table 5 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
152	Harris, 1975	H20 55A	X	X	X	X	X	X	X	X		X	X			
153	Harris, 1975	H20 55T	X	X	X	X	X	X	X	X		X	X			
154	Harris, 1975	H74 90	X	X	X	X	X	X	X	X		X	X			
155	Harris, 1976	H76 89	X	X	X	X	X	X	X	X		X	X			

Table 5 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
066	National Opinion Research Center, 1966	ENN IS	X	X	X		X	X	X							X
018	National Opinion Research Center, 1972	NOR 72N	X	X	X		X	X	X		X	X	X		X	
019	National Opinion Research Center, 1973	NOR 73N	X	X	X		X	X	X		X	X	X		X	
020	National Opinion Research Center, 1974	NOR 74N	X	X	X		X	X	X		X	X	X		X	
021	National Opinion Research Center, 1975	NOR 75N	X	X	X		X	X	X		X	X	X		X	
148	National Opinion Research Center, 1976	NOR C76	X	X	X	X	X	X	X	X	X	X	X		X	
149	National Opinion Research Center, 1977	NOR C77	X	X	X	X	X	X	X	X	X	X	X		X	
150	National Opinion Research Center, 1978	NOR C78	X	X	X	X	X	X	X	X	X	X	X		X	

Table 5 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
048	The Quality of American Life, 1971	QAL 71N					X	X	X		X		X		X	
049	Justifying Violence: Attitudes of American Men, 1968	JV6 9N		X	X		X	X	X		X		X		X	
050	Defense Civil Preparedness Agency (UCSUR), 1966	CD4	X	X	X		X	X	X	X		X	X	X		X
064	National Crime Survey Attitude Supplement, 1972	DUA LHH		X					X					X		X
065	National Crime Survey Attitude Supplement, 1972	DUA LIN	X		X		X	X		X	X		X			
158	Family Study - 1976 Adult by Yankelovick, Skelly & White	FAM 76A	X	X	X	X	X	X	X	X	X	X	X		X	
159	Family Study - 1976 Children by Yankelovick, Skelly & White	FAM 76C	X	X	X											
160	Family Study - 1976 Household by Yankelovick, Skelly & White	FAM 76H	X	X	X	X	X	X	X	X	X	X	X		X	

Table 6

DATA ARCHIVE DEMOGRAPHICS

CITIES

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
070	LEAA - National Crime Survey, Atlanta	HAT L72	X	X	X	X	X	X	X		X			X	X	X
071	LEAA - National Crime Survey, Atlanta	PAT L72	X	X	X	X	X	X		X	X					X
072	LEAA - National Crime Survey, Baltimore	HBA L72	X	X	X	X	X	X	X		X			X	X	X
073	LEAA - National Crime Survey, Baltimore	PBA L72	X	X	X	X	X	X		X	X					X
074	LEAA - National Crime Survey, Cleveland	HCL E72	X	X	X	X	X	X	X		X			X	X	X
075	LEAA - National Crime Survey, Cleveland	PCL E72	X	X	X	X	X	X		X	X					X
076	LEAA - National Crime Survey, Dallas	HDA L72	X	X	X	X	X	X	X		X			X	X	X
077	LEAA - National Crime Survey, Dallas	PDA L72	X	X	X	X	X	X		X	X					X
078	LEAA - National Crime Survey, Denver	HDE N72	X	X	X	X	X	X	X		X			X	X	X
079	LEAA - National Crime Survey, Denver	PDE N72	X	X	X	X	X	X		X	X					X
080	LEAA - National Crime Survey, Newark	HNW K72	X	X	X	X	X	X	X		X			X	X	X

Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
081	LEAA - National Crime Survey, Newark	PNW K72	X	X	X	X	X	X		X	X					X
082	LEAA - National Crime Survey, Portland	HPL D72	X	X	X	X	X	X	X		X			X	X	X
083	LEAA - National Crime Survey, Portland	PPL D72	X	X	X	X	X	X		X	X					X
084	LEAA - National Crime Survey, St. Louis	HST L72	X	X	X	X	X	X	X		X			X	X	X
085	LEAA - National Crime Survey, St. Louis	PST L72	X	X	X	X	X	X		X	X					X
086	LEAA - National Crime Survey, Chicago	HCH I73	X	X	X	X	X	X	X		X			X	X	X
087	LEAA - National Crime Survey, Chicago	PCH I73	X	X	X	X	X	X		X	X					X
088	LEAA - National Crime Survey, Detroit	HDE T73	X	X	X	X	X	X	X		X			X	X	X
089	LEAA - National Crime Survey, Detroit	PDE T73	X	X	X	X	X	X		X	X					X
090	LEAA - National Crime Survey, Los Angeles	HLA 73	X	X	X	X	X	X	X		X			X	X	X

CONTINUED

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Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
091	LEAA - National Crime Survey, Los Angeles	HLA 73	X	X	X	X	X	X		X	X					X
092	LEAA - National Crime Survey, New York	HNY 73	X	X	X	X	X	X	X		X			X	X	X
093	LEAA - National Crime Survey, New York	PNY 73	X	X	X	X	X	X		X	X					X
094	LEAA - National Crime Survey, Philadelphia	HPH L73	X	X	X	X	X	X	X		X			X	X	X
095	LEAA - National Crime Survey, Philadelphia	PPH L73	X	X	X	X	X	X		X	X					X
096	LEAA - National Crime Survey, Boston	HBO S74	X	X	X	X	X	X	X		X			X	X	X
097	LEAA - National Crime Survey, Boston	PBO S74	X	X	X	X	X	X		X	X					X
098	LEAA - National Crime Survey, Buffalo	HBU F74	X	X	X	X	X	X	X		X			X	X	X
099	LEAA - National Crime Survey, Buffalo	PBU F74	X	X	X	X	X	X		X	X					X
100	LEAA - National Crime Survey, Cincinnati	HCI N74	X	X	X	X	X	X	X		X			X	X	X

Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
101	LEAA - National Crime Survey, Cincinnati	PCI N74	X	X	X	X	X	X		X	X					X
102	LEAA - National Crime Survey, Houston	HHO U74	X	X	X	X	X	X	X		X			X	X	X
103	LEAA - National Crime Survey, Houston	PHO U74	X	X	X	X	X	X		X	X					X
104	LEAA - National Crime Survey, Miami	HMI A74	X	X	X	X	X	X	X		X			X	X	X
105	LEAA - National Crime Survey, Miami	PMI A74	X	X	X	X	X	X		X	X					X
106	LEAA - National Crime Survey, Milwaukee	HMI L74	X	X	X	X	X	X	X		X			X	X	X
107	LEAA - National Crime Survey, Milwaukee	PMI L74	X	X	X	X	X	X		X	X					X
108	LEAA - National Crime Survey, Minneapolis	HMI N74	X	X	X	X	X	X	X		X			X	X	X
109	LEAA - National Crime Survey, Minneapolis	PMI N74	X	X	X	X	X	X		X	X					X
110	LEAA - National Crime Survey, New Orleans	HNO R74	X	X	X	X	X	X	X		X			X	X	X

Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
111	LEAA - National Crime Survey, New Orleans	PNO R74	X	X	X	X	X	X		X	X					X
112	LEAA - National Crime Survey, Oakland	HOA K74	X	X	X	X	X	X	X		X			X	X	X
113	LEAA - National Crime Survey, Oakland	POA K74	X	X	X	X	X	X		X	X					X
114	LEAA - National Crime Survey, Pittsburgh	HPI T74	X	X	X	X	X	X	X		X			X	X	X
115	LEAA - National Crime Survey, Pittsburgh	PPI T74	X	X	X	X	X	X		X	X					X
116	LEAA - National Crime Survey, San Diego	HSD G74	X	X	X	X	X	X	X		X			X	X	X
117	LEAA - National Crime Survey, San Diego	PSD G74	X	X	X	X	X	X		X	X					X
118	LEAA - National Crime Survey, San Francisco	HSF R74	X	X	X	X	X	X	X		X			X	X	X
119	LEAA - National Crime Survey, San Francisco	PSF R74	X	X	X	X	X	X		X	X					X
120	LEAA - National Crime Survey, Washington, DC	HWD C74	X	X	X	X	X	X	X		X			X	X	X

Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
121	LEAA - National Crime Survey, Washington, DC	PWD C74	X	X	X	X	X	X		X	X					X
122	LEAA - National Crime Survey, Atlanta	HAT L75	X	X	X	X	X	X	X		X			X	X	X
123	LEAA - National Crime Survey, Atlanta	PAT L75	X	X	X	X	X	X		X	X					X
124	LEAA - National Crime Survey, Baltimore	HBA L75	X	X	X	X	X	X	X		X			X	X	X
125	LEAA - National Crime Survey, Baltimore	PBA L75	X	X	X	X	X	X		X	X					X
126	LEAA - National Crime Survey, Chicago	HCH I75	X	X	X	X	X	X	X		X			X	X	X
127	LEAA - National Crime Survey, Chicago	PCH I75	X	X	X	X	X	X		X	X					X
128	LEAA - National Crime Survey, Cleveland	HCL E75	X	X	X	X	X	X	X		X			X	X	X
129	LEAA - National Crime Survey, Cleveland	PCL E75	X	X	X	X	X	X		X	X					X
130	LEAA - National Crime Survey, Dallas	HDA L75	X	X	X	X	X	X	X		X			X	X	X

Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
131	LEAA - National Crime Survey, Dallas	PDA L75	X	X	X	X	X	X		X	X					X
132	LEAA - National Crime Survey, Denver	HDE N75	X	X	X	X	X	X	X		X			X	X	X
133	LEAA - National Crime Survey, Denver	PDE N75	X	X	X	X	X	X		X	X					X
134	LEAA - National Crime Survey, Detroit	HDE T75	X	X	X	X	X	X	X		X			X	X	X
135	LEAA - National Crime Survey, Detroit	PDE T75	X	X	X	X	X	X		X	X					X
136	LEAA - National Crime Survey, Los Angeles	HLA 75	X	X	X	X	X	X	X		X			X	X	X
137	LEAA - National Crime Survey, Los Angeles	PLA 75	X	X	X	X	X	X		X	X					X
138	LEAA - National Crime Survey, New York	HNY 75	X	X	X	X	X	X	X		X			X	X	X
139	LEAA - National Crime Survey, New York	PNY 75	X	X	X	X	X	X		X	X					X
140	LEAA - National Crime Survey, Newark	HNW K75	X	X	X	X	X	X	X		X			X	X	X

Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
141	LEAA - National Crime Survey, Newark	PNW K75	X	X	X	X	X	X		X	X					X
142	LEAA - National Crime Survey, Philadelphia	HPH L75	X	X	X	X	X	X	X		X			X	X	X
143	LEAA - National Crime Survey, Philadelphia	PPH L75	X	X	X	X	X	X		X	X					X
144	LEAA - National Crime Survey, Portland	HPL D75	X	X	X	X	X	X	X		X			X	X	X
145	LEAA - National Crime Survey, Portland	PPL D75	X	X	X	X	X	X		X	X					X
146	LEAA - National Crime Survey, St. Louis	HST L75	X	X	X	X	X	X	X		X			X	X	X
147	LEAA - National Crime Survey, St. Louis	PST L75	X	X	X	X	X	X		X	X					X

Table 6 (Continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
030	Harris Crime Survey, Baltimore, 1969	H1935	X	X	X		X	X	X	X	X		X			
043	Boston Area	BAS69	X	X	X		X	X	X	X	X	X	X	X		
044	Boston Area	BAS70	X	X			X	X	X		X	X	X	X		X
035	Police Foundation Study in Cincinnati	CHH73	X	X	X		X	X	X	X			X			
036	Police Foundation Study in Cincinnati	CAT73	X	X	X		X	X	X	X						
058	A Study of Family-School Relationships in Detroit	DAS63N	X	X	X		X	X	X				X			
059	Citizens in Search of Justice -- Detroit	DAS67N		X	X		X	X	X		X	X	X	X		
060	Black Attitudes in Detroit	DAS68N	X	X	X		X	X	X		X					
061	White Attitudes and Actions in Urban Problems -- Detroit	DAS69N	X		X		X	X	X			X	X	X		

Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
062	Social Problems and Social Changes -- Detroit	DAS 71N	X		X		X	X	X			X	X			
063	Detroit Longitudinal Study WAVE 1	DLS N	X	X	X		X	X	X		X	X			X	X
034	Police Foundation Survey Kansas City	KC1 972	X	X	X		X	X	X					X		X
037	Police Foundation Study in San Diego	SD1 973	X		X	X	X	X	X	X				X		X
038	Police Foundation Survey in San Diego	SD1 974	X		X	X	X	X	X	X				X		X
039	St. Petersburg Citizens Survey	STP 74	X	X	X			X	X					X		
040	St. Petersburg Citizens Survey	STP 75	X	X	X				X					X		
047	Pittsburgh Neighborhood Atlas Neighborhood Study	CRM NPA	X	X	X			X	X					X		
161	Portland 1974	POR T74	X	X	X		X	X	X	X				X	X	X
162	Portland 1977	POR T77	X	X	X		X	X	X	X				X		X

Table 6 (continued)

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
041	Urban Observatory Program Ten Cities Survey of Citizen Attitudes Toward Local Government	UOP 7ON		X	X	X	X	X	X				X		X	
042	Racial Attitudes in Fifteen American Cities by Campbell and Schuman	WHI TEN	X	X	X		X	X	X		X		X	X	X	
045	Racial Attitudes in Fifteen American Cities by Campbell and Schuman	BLA CKN	X	X	X		X	X	X		X		X	X	X	

Table 7

DATA ARCHIVE DEMOGRAPHICS

STATEWIDE SURVEYS

File number	STUDY NAME	File Name	SEX	RACE	AGE	ETHNICITY	MARITAL STATUS	EDUCATION	INCOME	OCCUPATION	EMPLOYMENT STATUS	POLITICAL PREFERENCE	RELIGIOUS AFFILIATION	OWN/RENT	AREA	LENGTH OF RESIDENCE
016	Minnesota Poll, 1969	MIN 284	X		X			X		X		X	X	X	X	
017	Minnesota Poll, 1970	MIN 297	X		X			X	X	X		X	X	X	X	
046	Public Opinion of Criminal Justice in California, 1972	CAL 72	X	X	X			X	X	X	X				X	
067	Public Opinion of Criminal Justice in Texas, 1973	TEX 74	X	X	X			X	X	X	X				X	
156	Perceptions of Crime by Residents of Michigan, 1972	MICH	X	X	X		X	X	X	X					X	
157	Perceptions of Crime by Residents of Michigan, 1974	MICH	X	X	X		X	X	X	X					X	

All the A.I.P.O. surveys ("modified probability samples"), as shown in Table 5, contain sex, race, age, education, "area" of residence (size of community), religious affiliation and political preference as background variables. Except for the 1960 survey (File #001), income information is also available, while occupational background was included in the 1960 (File #001) survey and the 1975 one (File #009). Marital status data come only from the two 1972 polls (Files #007 and 008), while five of the surveys (#002, #003, #005, #006 and #008) also provide data regarding ownership status of the respondent's residence.

The Potomac Associates studies of 1972, 1974 and 1976 which were carried out by A.I.P.O. do include occupational information in all the subfiles, and ownership of residence status was incorporated into the 1972 forms of the State of the Nation inquiry (#010-012).

A closer look at the demographic pattern is well illustrative of some of the underlying issues.

A.I.P.O. has been using, quite consistently a race/sex coupling as a single variable. Apart from a few stray records (with missing values--presumably either due to interviewer error or to some other clerical mistake in data processing), six groups result: white females, white males, black males and black females, "other males" and "other" females.

In all the surveys, the subsamples of males and females, disregarding race, are large so that comparisons within each survey as well as across surveys (and thus over time) are nonproblematic for those willing to accept the "modified probability" sampling approach as adequate for generalization purposes.

Respondents "other" than black or white (a category generally reserved for "orientals" or for "American Indians") is, for obvious reasons of their small numeric representation in the population at large, so miniscule in all surveys (A.I.P.O. serving but as an example for an altogether general problem) that it cannot be used for any valid comparisons at all. But even adequate comparisons of white and black respondents, regardless of sex, are somewhat limited: In the 1968 (#005) and 1969 (#006) surveys the black subsample as a whole comes to fewer than 100 respondents, and in the subsequent surveys (#007, #008 and #009) it exceeds N=100 only by a small margin.

As a consequence, while the race-sex groups permit good generalizability for the earlier surveys (#001, #002 and #003 as well as #004) provided other than black or white respondents are discounted anyway, the remaining surveys yield black-male and black-female groups so small (generally around N=50) that it would not be prudent to draw nationwide conclusions about such subsamples, nor would it prove of great value to compare the responses of these subsamples with the white cohorts or with black or whites, males and females, from other prior or subsequent surveys. The data provide <u>insights</u> into the ways in which such groups of respondents react but they do not really permit any kind of statistical generalization in which confidence could be placed.

Age, by contrast, has been recorded and coded identically in all the surveys and turns out to be a non-problematic variable altogether: The A.I.P.O. surveys (as do others, save for the Harris series) provide age-data on a year by year basis so that the researcher can group them dependent on the purposes of analysis. For the most part, however, more than four groupings tend to yield subsamples which are too small, but the files allow for considerable flexibility as to how such three or four age groups might be structured for analytic purposes.

Standardized are also responses to religious affiliation and party preference. The former allows consistent generalizations for Protestants (without any further denominational specification) and Roman Catholics, and for comparisons of these groups across surveys and over time. But the numbers of Jewish respondents are too small in all the surveys, as are the numbers of all "others"--a category which would prove uninterpretable even were there enough cases in it (as there occasionally are) since it might include adherants of Islam or of Eastern religions, agnostics, atheists or what not.

Political party preferences, on the whole, permit comparisons of Republicans, Democrats and Independents while "other" party preferences, such as respondents may report, are so rare that no possible use can be made of the information.

Occasionally (as for instance in the 1960 survey), the Independents are further subdivided into those who are "truly" Independent and those who "lean toward the Democrats" and those who "lean toward the Republicans".

Republicans and Democrats, in turn, are sometimes identified as "strong" or "moderate" (or "weak") in their party preference so that further subcategories result.

But in all surveys in which the party preference question is raised, the basic generalizability and comparability includes the three main groups, Republicans, Independents and Democrats, whereas further refinements depend on the subsample size as much as it does on the nature of the item which yielded such more refined data.

Size of community of residence ("area" demographic) has also been used in a standard manner by A.I.P.O. as has been the "educational" variable.

In terms of education, comparisons of three groups turn out to be generally quite justifiable: Those with high school education or less, those with technical or business schooling, and those with at least some college education.

Often, there are enough data to distinguish between those who had just "some college" and those who "completed college" as well as those who didn't complete their high school education and those who did.

As far as community size is concerned, the A.I.P.O. survey approach is not uncharacteristic of other codes. It tends to tag each respondent as belonging into one of twelve categories:

* open country, rural areas
* places of up to 2,500
* communities with 2,500 to 4,999 residents
* communities with 5,000 to 9,999 population
* towns with 10,000 to 24,999 inhabitants
* towns with 25,000 to 49,999 residents
* cities of 50,000 to 99,999 and their suburban areas
* cities of 100,000 to 249,999 and their respective suburbias
* cities of 250,000 to 499,999 inhabitants plus the suburbs of such cities
* cities of 500,000 to 1,000,000 residents and the suburbs, and finally,
* cities with 1,000,000 or more inhabitants plus their respective suburban areas.

Generally, grouping into four size categories, or better yet into three, produces subsamples which are sufficiently large to permit generalizations (about such-size types of places) in the ambient sense and to facilitate comparability with other surveys in which the size-of-place codes may follow a simpler scheme than that which A.I.P.O. has been consistent in using.

No ways are built into the overtime surveys to provide for inflation effects when it comes to studies including "family income". The surveys, exemplified by the A.I.P.O. series reflect some changes but generally lagging in time behind the apparent fiscal course of events of the nation.

Thus survey #002 (1963) includes 10 income codes:

* Up to $1,000
* $1,000 to $1,499
* $1,500 to $1,999
* $2,000 to $2,499
* $2,500 to $2,999
* $3,000 to $3,999

* $4,000 to $4,999
* $5,000 to $6,999
* $7,000 to $9,999
* $10,000 to $14,999
* $15,000 and more

In subsequent surveys, the $500 incremental categories tend to be collapsed into $1,000 groupings in the $1,000 to $3,000 subgroups, and the $5,000 to $6,999 gets subdivided into $5,000 to $5,999 and $6,000 to $6,999 subgroups, while the $10,000 to $14,999 category is further broken down into those earning between $10,000 and $11,999 and $12,000 and $14,999.

Only the 1975 survey (#009) changes the lower limit of the income grouping: it now includes, as the lowest category, all those with earnings of up to $2,000 (rather than $1,000). And the same survey now also comes to include a further subdivision of those with incomes of $15,000 or more: one group with earnings of up to $19,999, and another one with incomes of $20,000 or higher.

The fact that such relatively refined categorizations have been used as well as the fact that they are comparable over time subject only to the kind of category collapsing or subdividing makes it easy to regroup in a more usable manner and flexibly so. Three or four income groups are generally suggested by the distributions and such regroupings permit generalization (due to the size of the resulting groups) as well as comparisons with other surveys from which similar income groups can be also selected.

Occupational data in the surveys, by and large, tend to be categorized into a good number of classes as well. A.I.P.O.'s system is quite typical of the major surveys and, again, can serve as a good example. The occupational groupings include

* farmers
* businessmen, executives, managers
* professionals
* clerical workers
* sales workers
* skilled workers
* semiskilled workers and operatives
* unskilled workers
* farm laborers
* service workers
* not in labor force respondents.

In many other studies, the "not in labor force" respondents are subdivided, as is advisable, into "retirees" on the one hand and "housewives" on the other hand with whatever other "not in labor force" segments in still another category. The minimum comparisons which can

be made, and minimum groupings to which generalizations are applicable include such broader categories as those of managers, businessmen, executives and professionals as one group, white collar workers as another group, and blue collar workers as a third subsample (along with "housewives", if identifiable, and "retirees", if identifiable: otherwise, the "not in labor force" category is too undefined to be of interpretive value).

In terms of marital status comparisons of those who are married with those who have never been married are invariably possible, as are comparisons with a residual, but interpretively difficult, group of "others" (a category resulting from collapsing into one group the "divorced", "the separated" and the "widowed").

To the extent to which data on "ethnic", rather than only "racial", background are available (and this is quite rare in the surveys in the archives), the distributions for various ethnic strains that might be of considerable theoretical interest (such as Americans of Italian descent; or those of Polish ancestry; or those of German extraction; or those with Irish roots and so on) yield such small groups that the variable is really not usable for any generalization or comparative purposes.

By contrast, whether the respondents own their place of residence (as most do) or whether they are renters is a relatively straightforward variable and a consistently reported one (in surveys which use it to begin with), while the residual category of "other living arrangements" is generally of little value for further analysis especially since by far most of the sampling frames exclude group quarters and most, in fact, involve private housing units.

A summary evaluation of the background variables in the surveys contained in the archive, and certainly applicable to other issue-oriented archival activities, might include the following major points:

1. In so far as the sampling design permits generalizability, elaboration by demographic variables produces groups about which, as subsamples, generalizations (within greater ranges of uncertainty) are also appropriate.

2. The limits of generalizability, at the lower bound, depend on the size of the resulting subsample and it is probably not advisable to disaggregate for subgroups smaller than about 100 respondents--though in terms of a search for substantive significance and patterning of results even smaller groups may prove of considerable value.

3. Standardization of the kinds of background variables within series of surveys and across various surveys and over time facilitates comparability. A great deal of

such standardization has, in fact, been achieved in the survey research community and, subject to purpose-related variation in which "additional" information gets included, many items are incorporated in practically all surveys as it is.

4. Standardization of the way in which the data on each demographic trait are collected and processed also facilitates comparability. A great deal of standardization has been also achieved so that, in fact, despite some differences in data collection on such items as "education", the resulting categories are, or can be made, comparable from survey to survey and from period to period.

5. Maximum flexibility for the researcher, and especially the secondary data analyst who may address archives of prior data, is attained when observations and the information recording are quite detailed, and when data reduction produces more categories ("codes") than would be actually used in the course of analysis. This permits a variety of ways to regroup the information, and otherwise seemingly noncomparable surveys can be rendered comparable.

6. Increases in analytic comparability across surveys and over time generally entail some loss of information: the resulting groupings which become comparable are relatively crude ones, an approach dictated further by the fact that only such cruder groupings yield subsamples of generalizable size.

7. Multivariate analyses by the more conventional data elaboration methods are generally not very productive because the subgroups become far too small very rapidly, but fallback, while violating some statistical assumptions, on multiple regression or path analytic modelling and techniques provides a theoretically viable but methodologically problematic alternative.

VII. SOME SUBSTANTIVE ISSUES: PERSPECTIVES ON CRIME

The specific questions contained in the Center's crime opinion data archive have been computerized and indexed. Thus the researcher can retrieve identical, or similar, items across the various surveys in terms of the index itself and obtain, at the same time, information about the surveys from which the items derive.

Be it as it may, it is quite desirable to consider the comparability problem, if briefly, in this discussion. Given sampling designs and field implementation procedures as well as the nature of the background data available about the respondents, we may now provide a generalized assessment of the extent to which the studies yield substantive data which are, across surveys and over time, comparable.

Needless to say:

> Repetition of the identical question in samples of the same design maximizes comparability across surveys and over time.

On the whole, however, this condition tends to be satisfied only in some of the surveys and, for the most part, in surveys carried out by one and the same research organization. Thus the A.I.P.O., HARRIS, N.O.R.C. or POTOMAC ASSOCIATES studies tend to involve similar (or predictably different, as in the N.O.R.C.'s evolution of the surveys) samples and, often, identical questions. In the central cities files, the questions of the 15 city sample (files #042 and #045), or of the Urban Observatory study (file #041) as well as those of the LEAA/BUREAU OF THE CENSUS surveys (files #70 through #147) involve standardized questions within the domain of each study.

Yet, strict adherence to the desirability of identical sampling designs (in terms of their generalizability implications or of identical question wording which still would not handle the problem of the question sequencing) would produce but little in the way of comparative data (save for the within-organization series of studies and the above cited city surveys).

In any event, as we have previously emphasized (especially in Chapter IV above), the issue is one of underlying patterns and response configurations rather than one of identicity across studies and over time.

> Questions may be somewhat different and the responses derived from somewhat different sampling designs, but the underlying dimensionalities of the items often point in the same, or basically same, substantive direction.

A major caveat, however, obtains:

> Data from the central city files are not comparable with nationwide data, even when such data are disaggregated by city size, and even if essentially the same kinds of questions are asked because of the variabilities among the nation's cities coupled with the fact that nation-wide samples are not city-representative in their design even if there are "fair" numbers of respondents drawn from particular cities in the process.

From an analytic standpoint, nonetheless, much can be done across the different sampling frames (given their generalizability to different respective populations).

> To the extent to which the questions are "similar" and the sampling designs permit generalizability, national studies provide a convenient benchmark against which city-based data can be pitted and their variabilities interpreted, though not (statistically) explained. This is even more so when the national data are disaggregated by city size (generally a statistically sound decision because city size tends to be a stratifier in the sampling designs) and compared with "similar" response patterns in specific cities of the nation.

Many of the surveys in the file seek to establish the respondent's perception of major problems in the nation, the community or the neighborhood. For the most part, the probes are open-ended and they also are introduced at the outset of the interview. Thus there exists some standardization of the questions, at least in intent, and of their sequencing.

Table 8

QUESTIONS ABOUT NATIONAL PROBLEMS

File #	Year	One-Problem Focus	Multi-Problem Focus
001	1960	X	
003	1965		X
050	1966		X
066	1966		X
004	1967	X	
025	1968		X
006	1969	X	
055	1970-1971		X
007	1972	X	
056	1972-1973		X
032	1973		X
033	1973		X
057*	1974	X	X

*Having asked about "problems", the researchers here also sought to inquire as to the "most important" problem of those mentioned.

Table 8 provides a simple summary of the data bearing on perceptions of "national problems." In principle, the questions are comparable, whether they elicit information about the one key problem or about several problems.

> The multi-problem focussed questions can be further rendered comparable with the one-problem focussed surveys by using only the "first" answer in the multi-problem studies along with the answer in the "one-problem" inquiries.

This, of course, assumes that the first spontaneous, off-the-cuff response is the most "valid" one as far as problem identification is concerned, and there is then an underlying Freudian interpretation ("associations") involved which, in some sense, neglects the effect of moods, or of immediately antecedent experiences, or of immediately antecedent exposure to media-transmitted information about the nation's issues and problems.

Despite such limitations (apart from sampling issues previously discussed), we feel that the results yield comparable information about the generalized feelings of our people at the time and, indeed, regardless of the precise wording of each of the questions.

> The researcher interested in tracelines of other major national problems, that is, other than "crime," might find the data base of considerable value as well in that it is possible to utilize the information for the trajectories of concerns having to do with such problems as peace and war issues, inflation, unemployment and energy.

In a significant manner then, comparability does not rest merely with the tracing of crime-related questions or responses across surveys and over time, but also with the changing saliency of particular issues.

> Indeed, such comparisons across issues across surveys and over time make it better possible to understand, and interpret, the formation and relevance of problems on the nation's (often "silent" but nonetheless crucial) agenda.

Other surveys, too, are comparable in an underlying sense. They attack the issue of "major problems" in somewhat different ways and with different statistical implications but with essentially identical substantive meanings. For example, surveys #032 and #033 (1973)--themselves also listed in Table 8 above--also probe into the ways in which life quality in the nation may have changed over the past ten years, and thus elicit "crime-related" (and other important "issue-related") responses in the context of perceptions, by (distorted) recall, of changes.

Surveys #028 and #038, for instance, probe into the 10-year (past-to-present) perspective on perceived changes in national "morality" and why such changes may have taken place. Survey #048 (1971) probes into ways in which "life" may have been getting better or worse in the recent years and why this may be the case.

Survey #025 is concerned with some of the "main things" that might be "missing from American life" and surveys #029, #031, #032 and #033 also inquire as to the feelings of the respondents whether "crises" simply come and go (and are thus just the "order of business" on the whole) or whether there might be something "deeply wrong" with the nation, and what it is.

Potomac Associates surveys (especially file #010 and #011 as well as #013 and #014), in turn, probe into the most important wishes (desirable states of affairs) and fears (unwanted states of affairs) of the respondents with a 10-year futuristic perspective.

The past-to-present inquiries, of course, tell us something about perceptions of the evolution of various issues. The present-to-future questions allow us to interpret the results in terms of anticipations or guesstimates of the respondents regarding the future evolution of various issues.

But, all these inquiries permit an assessment of the saliency of a problem at the time of the study and, in this regard, they provide more valid information about the system state than about how the system state came about (because of recall, and "revision of history" problems) or about actual forecasts about the future (because of sensitivity of anticipations to past-to-present recall patterns as well as to problems of underlying optimism-pessimism and the like).

It is often important, as it is relevant, to determine the extent to which "problems," of whatever kind, manifest themselves differently across the nation. Thus "crime" may be an important, or relatively unimportant, national issue over time, but it may have an altogether different significance at the level of the nation's communities. This, too, of course have to do with the variability of crime epidemiology and its etiology both at one time and over time.

Research evaluations of the most pressing "community" problems then seek to address the issue in its more general manifestation.

In our data files, such studies as #005 (1968), 008 (1972) or 009 (1975)--all, by the way, of the A.I.P.O. series and thus internally consistent and comparable--seek to assess the most serious "community problems."

Survey #032 (1973) gets at the issue by asking what kinds of problems people might discuss if they had an opportunity to talk to the Governor or to local officials about the most pressing needs of their communities.

The Potomac Associates surveys (especially file numbers 010 and 011, combinable as they are, and numbers 013 and 014, collapsible as they are also) concern the imagery of the "best" and "worst" possible futures for the community, and through this questioning dynamic, help to identify the key problems as well.

Not comparable with national data directly, the city-based inquiries also probe systematically into the community problems: thus #041 (Urban Observatory in 10 cities) seeks to determine the ways in which "local government could do a better job" than it has been doing, the "things which would be needed to make the community a better place to live in," "the problems which the local government and police ought to deal with" above all and the like.

Comparisons of data pertaining to perceptions of community problems in general and to national problems make it possible to make limited, though systematic, comparisons of the ways in which more local and more national issues get defined in the lay assessment of difficulties which face the nation.

Indirectly, such results point to evaluations of the appropriate scopes of authority (problems over which the national versus community government has authority) and the resultant feasibility of appropriate level of response (federal, state or local). But these are, of course, at best clues rather than clear cut indicators of who, in terms of governance patterns, is to do what and why.

With respect to neighborhood problems in this most general vein, we have to rely on the central city data almost exclusively.

Which problems are most serious in the neighborhood is addressed for instance, in the LEAA/CENSUS city studies (household files, #070, 072....and all even numbers through #146).

The Urban Observatory (1970) study deals with the issue by asking the respondents to rate various neighborhood services and establish which one(s) require improvements the most. The 15-cities study of blacks and whites (files #042 and #045 respectively) seek an evaluation of police services in the neighborhood.

In the Observatory and the Black and White (1968) 15-city study the samples are too small to permit any disaggregation in terms of potential neighborhoods so that actual comparisons among neighborhoods within cities or similar types of neighborhoods across cities are not possible, thereby making overtime comparisons across the same cities also impossible at the neighborhood level.

Whatever the reasons may be, the LEAA/CENSUS files available to us, with their substantially larger samples, present still other difficulties:

The central city surveys of LEAA/CENSUS make neighborhood comparisons impossible both within cities and across cities because the privacy-related restrictions (the nature of which the researcher may find hard to comprehend!) make disaggregations impossible.

It is, as a consequence, either a problem of small overall sample size or of other (legal) provisions which do not make it feasible to really consider objective crime indicators, victimization data and attitudes toward problems for specific neighborhoods or even types of neighborhoods.

Furthermore, none of the studies in the archives help to define a "neighborhood" so that the elusive concept itself remains amorphous anyway. We know, in fact, that people are responding to some kind of an imagery of a "neighborhood" but what the neighborhood consists of and what area of the community it may contain remains

largely quite mysterious. Generalized statements about "neighborhoods" are thus quite possible, but location specific statements or neighborhood-type specific statements are all but impossible.

> The underlying data base is far richer in its conception than it is in its formatting for analysis resulting in major and unsurmountable losses of information especially at the most actionable level ("neighborhood," or a particular small "geographic area") of the nation.

Thus, in all, where utilizability payoff (for policy formulation, adoption and implementation) might be the highest, the data base is at its weakest--for a variety of constraints, some having to do with sample sizes (itself a fiscally determined constraint) and others having to do with "privacy protection" (itself a constraint which, despite its necessary and good intentions, also impinges upon interpretive value of research--entirely different from possible abuse of data for other purposes). Some of the surveys, beyond their more general emphasis on national, community or neighborhood problems, attempt to tap the respondent concerns over family and personal problems. Here, too, of course, crime-related issues are most relevant for our purposes when, and if, they come up in the response patterns.

Surveys which include appropriate items of this most proximal (to the respondent) problem type involve, in effect the following ones:

* File #052 of 1964-1965
* File #066 of 1966
* File #025 of 1968
* File #054 of 1968-1969
* File #056 of 1972-1973
* File #057 of 1974-1975
* File #158 of 1976

If the data on national, community or neighborhood problems amount to a mixture of information about one's own sense of experience and projections to a particular body politic, the findings pertaining to "self" and "family," as in the surveys above, are most direct in that they attempt to fathom the extent to which some of the wider problem projections also amount to more specific individual experiences, if only by recall. In several of the surveys we find some relevant, though highly limited, data on lay interpretations of causes of selected problems:

* The 1967 Harris surveys (#023 and #024) probe into two to three "causes of crime."

* Postulating, in the formulation of the question, that there may exist more crime in the United States than in other major nations, the 1972 A.I.P.O. survey (#007) inquires as to why this should be the case.

* The 1975 surveys #152 and #153 seek to establish the respondent perceptions of the causes of the relative "breakdown of law and order" in the nation.

* In a narrower context, causes of juvenile delinquency are explored in an A.I.P.O. 1963 survey (#002).

How much information may be lost, or possibly somewhat distorted, by the development of limited coding categories is not clear. But comparability and interpretability of the results rests with groupings even broader than those which are provided, for instance, in an effort to evaluate the extent to which the roots of crime are seen in underlying socio-economic problems of society (unemployment, poverty, costs of living and the like--these being among the typical response patterns) or in problems of the moral order (such as failures of the American family or of the educational system), unavailability of options for the outlet of, especially juvenile, energies (lacking recreational facilities) or even in other offenses which, in turn, "induce" further criminal activities (drug abuse).

The basic dynamics of these lay interpretations of causes of crime can be further enhanced with data on reasons which the respondents give for changes in crime rates, nationwide, community-wide or in their neighborhood. This is so because the factors reported as reasons for increased crime rates parallel the factors cited as causes of crime to begin with. At the national level, such "reason" information is contained in, at least, the 1964 Harris survey (#022), in #065 (1972) and #031 (1973).

Reasons for cited increases in crime rates in the resident's area are probed for in Harris surveys #023 and #024 as well as in the 1972 A.I.P.O. study #008.

Whether or not crime has increased (or, perhaps, decreased or remained the same) over some time period is, of course, the key question of which the "reasons" for such changes become a frequent branch-off.

A one year time frame seems to have been most generally used. Surveys #022, #065 and #031 ask about changes over the preceding 12 month period in national crime rates. In the central city files, a general question about changes in national crime rates is also asked

(files 71, 73...and all odd numbered files up to and including #147) but with a looser time referent: changes in "about the last year or two."

Similarly, increases or decreases in crime rates over a 12 month period in the respondent's residential area are probed in such surveys as #007, #023, #024, #029, #025, #008, #031 and #065. In the LEAA/Census city studies, the issue is again raised with respect to the "past year or two."

Questions as to whether people worry more (or less or about "the same") about "crime in the streets" than they recall to have been worried about a year prior to the interview also appear in several of the studies: in the 1964 Harris survey #022, and in Harris surveys #031 (1973) and #154 (1975). A 1965 A.I.P.O. study (#003) includes a similar probe but asks about changes in concern over the 5 years of the decade.

Insofar as questions about neighborhood and community problems provide a context in which to interpret responses to probes about changes in community and neighborhood crime rates, the items reflecting personal and family concern over crime in a generalized way provide an appropriate context relative to which the answers to questions about changes in one's fear of "crime in the streets" can be, perhaps, best evaluated.

> Since the national survey results are not directly comparable with surveys in the central cities anyway, the time referent in the questions concerning crime rate dynamics is not methodologically problematic.
>
> The national surveys are, at the same time, comparable both in their focus on changes in national crime rates and community crime rates since they involve all a standard 12-month time horizon.
>
> The LEAA/Census surveys, too, are internally comparable in inquiries to determine degrees of homogeneity or heterogeneity, and their patterning, across the cities and over time.

Even so, we would be inclined to view the differences in time horizon as analytically negligible. After all, people are not responding in some clearly objective, or even objectifiable, manner about changes in precisely one year or, alternatively, within two years or so. Rather, the responses must be construed as further clues to the seriousness of the problem at the time of the interview, and of some proxy evidence to the perceptions of an underlying

generalized drift toward "better" or "worse" conditions from some recent past to the approximate present.

> Since crime rates, at least in terms of such sources as the FBI Crime Reports, have been increasing and the differences over time are unlikely to be simply a function of changing patterns of crime reporting, the analytically most interesting use of such data may well be grounded in an understanding of the lay logic of those who, contrary to apparent evidence and contrary to the prevailing view, hold that crime rates have not increased or that they have even somewhat declined.

Perceptions of increases in specific crimes have also been investigated though the evidence is rather limited. For the most part, it comes from the LEAA/Census individual files (#71, #73.... #147) and from the 1975 survey #154 (Harris). The LEAA attitude supplement also poses a question about changing likelihood that the respondent might be a victim of an attack and of a robbery, though the latter item is substantively more comparable with questions about crime as a personal concern than it is with answers to questions about changing rates of particular crimes. Little is known, however, about the ways in which people arrive at the conclusion that crime has, in fact, increased (or decreased) or that specific types of crime have been on the rise. That the media play a key role as do possibly friends and neighbors would seem rather self-evident but it is not clear what pattern of reports, media or otherwise, prompts a decision regarding changes in rates or reasons for such shifts.

Occasional comparisons of the respondent's (otherwise undefined) "neighborhood" with other areas of the same community (as in surveys #041 with its 1970 samples in ten cities or in survey #065 of 1972) are difficult to interpret because the responses cannot be anchored in particular neighborhoods or types of neighborhoods. Hence, such comparisons tend to be ambient at best even when the sample sizes are large, as they are in the LEAA/Census series #71, #73....#147, and when the sampling design clearly makes statistical generalizability quite appropriate.

> The actual geographic localization of crimes along with nondisaggregatability of data about specific small geographic areas present a serious problem both in interpretability and in the usability of such interpretations as may be speculatively offered.
>
> The data bearing on changing fears of "crime in the streets" suffer from similar limitations since it remains unclear whether the respondents are reacting to the (partially antiblack) euphemism of

"crime in the streets" in general or to "crime in the streets" which they themselves use in the patterning of their daily activity rhythms, and even less is known, in this specific context, about the nature and spatial distribution of their activities.

As questions are posed which lead to "problem identification," an evaluation of the "reasons" for which such a problem may exist, inquiries into recalled changes in the magnitude or intensity of the problem and, again, factors which seem to influence such changes as are reported, the logic of survey instrument construction often suggests that the respondents also be asked to express their view of what ought to be done about the problem. Table 9 lists the main national studies in which questions were asked, in an open-ended manner in general, about ways to combat crime.

Table 9

SURVEYS WITH ITEMS ON MEASURES TO REDUCE CRIME

Source	File #	Year
A.I.P.O.	005	1968
Harris	025	1968
CPS	054	1968-1969
A.I.P.O.	012	1972
Harris	031	1973
Harris	033	1973

As is the case with imputed "reasons" for crime or for changes in crime rates, the specific answers are probably less interesting and less comparable than are their appropriate taxonomizations: thus responses which indicate the need for changes in the societal arrangements tend to be of one general class (doing away with unemployment; poverty; discrimination, especially in racial grounds). Changes which imply the need to impact the potential pool of offenders are of another kind (improved parental discipline, changes in educational provisions and the like).

And finally, the need for reforms in the nation's law enforcement system form yet another major category of responses (better or more policing, better general law enforcement, court and judicial reforms and the like). In these broader terms, the data from the various studies are quite comparable and it is unfortunate that only a relatively short time span of five years, 1968-1973, is covered by surveys in which the more general questions about ways to combat crime have been raised.

It is interpretively not improper to argue that such taxonomizations yield distributions of data which can be used as a proxy for further understanding of some of the main reasons for crime--that is, the extent to which it is "society-caused," "individual-caused" or even a by-product of inadequate functioning of the law enforcement system which is, after all, designed to deal with crime problems both preventively and post hoc.

Thus at an underlying philosophical level the items bearing on major steps to combat crime can also be viewed as comparable to items regarding "reasons" for, or factors influencing, crime to begin with, or factors affecting changing crime rates.

Some suggested measures to deal with crime problems do not seem to entail additional expenditures or else, some reallocations of existing fiscal resources. But many of the steps which come to the mind of Americans when asked about ways to deal with crime would clearly call for further funding. Whether, in fact, more money ought to be spent, or less money is needed or the then-current amounts are just about right is an issue raised in a good number of the national surveys: Harris surveys #023 and #024 (1967), surveys #019, #020 and #021 (1973, 1974 and 1975 respectively) include such comparable questions. In the surveys in central cities, the Urban Observatory study (#041 of 1970) raises the issue of increasing or decreasing local expenditures in efforts to deal with crime. The item is phrased both in terms of programs for which more money ought to be allocated and in terms of those which might be cutback either because the communities appear, to the respondents, to have been spending more than is needed or else to release such funds for use in programs the expansion of which is advocated.

In survey #029 (Harris, 1968), similar questions about the need to expand some programs and, perhaps, cutback others is phrased on the national level, and surveys #148, #149 and #150 (N.O.R.C., 1976, 1977 and 1978) probe into the need for funding changes for several different programs, thereby contextualizing the need for more funds to combat crime relative to other compelling social needs. The 1975 A.I.P.O. survey (#009) asks, finally, specifically about the top three priorities for which more money ought to be spent if more were made, or became, available.

Thus the style of questionning includes items which tap, so to say, an "absolute" need for more funds, those which pertain to redistribution of funds (by expanding some programs but cutting back, possibly, on others), and those which have to do with increased funding priorities on condition that there would be more money to spend.

Whether or not the responses mean that there exists an actual need for more funds or for redistribution of existing funds or whether there is an underlying tone that enough money investment

might go a long way toward solving the crime problem, or any problem for that matter, is something of a moot point which cannot be resolved on the basis of the data alone.

Yet, whatever else the data indicate it is fairly clear that responses to questions concerning program funding or program cutbacks can be comparatively viewed as revealing something about the perceived severity of the underlying problem. Thus the "money" variable often becomes a proxy for assessments of problem importance either in its own right or relative to other compelling needs of the nation or of the communities.

At the individual or family level, various national and local problems find their possible reflection in changing the ways in which people go about their business. Thus potential activity changes, with their profound effect on established life-styles, might accompany crime and increased crime rates, or gasoline shortages, or general inflationary pressures, or unemployment and the like. In the LEAA/Census series of studies, both at the national level (surveys #064 and #065) and at the central cities' level (individual respondent files, #71, #73....#147), data are provided on the extent to which the interviewees felt that

* there has been a change in activities of people in general due to crime,

* there has been a change in activities of the respondent as a consequence of crime or fear of crime.

Comparable data from other studies, unfortunately, do not exist but information such as that provided by the LEAA/Census material is an exceptionally valuable clue to the gross impact of crime on the nation's way of life.

Residential change is, of course, a particularly significant behavioral response. The LEAA/Census studies probe into reasons for which people selected their present neighborhood, reasons for which they moved from their previous neighborhood, what they particularly dislike about their present residential area, and whether or not they have been considering to move away because of prevalence of crime.

Tapping the possible effect of crime on satisfaction with one's residential area, the Urban Observatory study (1970) also probed into ways in which people might say their house or neighborhood is not a particularly good place to live--providing thus a basically comparable item for these 10 cities with the LEAA/Census item on factors inducing some dissatisfaction with the present residential neighborhood. Ample data exist on behavioral changes which manifest themselves in fear of walking alone in one's neighborhood at nighttime or even during daytime hours.

Table 10

DATA ON FEAR OF WALKING IN NEIGHBORHOOD AT NIGHT

Source	File #	Year
A.I.P.O.	003	1965
N.O.R.C.	066	1966
A.I.P.O.	004	1967
Michigan	049	1969
Life Quality	048	1971
A.I.P.O.	007	1972
A.I.P.O.	008	1972
LEAA/Census	065	1972
C.P.S.	056	1972-1973
N.O.R.C.	020	1974
A.I.P.O.	009	1975
N.O.R.C.	148	1976
N.O.R.C.	149	1977
N.O.R.C.	150	1978

Information about daytime concerns is, however, much more limited: the item appears in the N.O.R.C./Ennis survey of 1966 (#066) and in the 1972 LEAA/Census (#065) instrument.

Whether people are fearful of being in their neighborhood alone at night is also included as an item in the LEAA/Census central cities studies as it is in the 1970 Urban Observatory study (#041), and parallel daytime worries are reported in the LEAA/Census survey.

Whether or not there are "unsafe places" in other parts of the city comes up as an important item in the 1966 Ennis survey (#066) as it does in the central cities studies of LEAA/Census. The latter inquiries, in their attitude supplement, also probe into changes in evening entertainment and shopping patterns and the reasons for such changes (including why people do not shop in their neighborhood, if they do not, and why they shop downtown, if they do).

Information about other important public response to the threat of crime is, however, very limited.

* Whether people always lock their doors when they leave their home for even a few minutes or a couple of hours (only N.O.R.C. #066, and Life Quality study, #048).

* Whether they keep their doors locked at night at all times (only #066).

* Whether they keep their doors locked at all times when they are at home (only #066).

* Whether they keep their outside lights on at night (only #066).

* Whether they have a gun or pistol (only #066 and #020).

* Whether they have a dog to protect them (other than a pet dog)--an item also included only in the 1966 N.O.R.C. survey #066.

Despite the obvious significance of such defensive public behavior, the data do not allow for comparisons either across surveys or over time and items of this type have simply not found their way into the survey routines of the major organizations.

Even items on which the data base is relatively rich (general changes in activities, changes in shopping or entertainment behavior, walking around by night) suffer from the same weakness which limits the analytic opportunities of many of the data sets: the impossibility to disaggregate at the neighborhood (or neighborhood type) level either due to overall sample size limitations or due to privacy concerns.

In all, our exploration into the more generalized issue of crime might be summed up in the following manner:

1. There exists a robust body of comparable evidence on public perception of problems which face the nation, the communities and, to some extent, the particular neighborhoods (though it is generally not possible to analyze the data at the neighborhood level).

2. Insofar as "crime" and "crime-related" issues appear among such problems, comparisons across surveys and over time are possible especially when the results are viewed as indicators of issue saliency in the context of other problems of the nation, community or neighborhood.

3. It is possible to compare the differential saliency of crime as an issue at the several levels at which the questions have been raised: thus whether or not "crime" is more or less of a problem at the national or the community level; whether it is more of a problem in some (though undefined) neighborhood than in other community neighborhoods.

4. There exists some evidence on the factors which influence, or cause, crime. Without being able to generalize to national or even community distributions in any manner, the data lend themselves to the development of the main lay patterns of cause-effect reasoning (the distributions of which could, of course, be established in subsequent surveys).

5. The data base is relatively rich in information about perceived changes in crime rates although it is not necessary, and probably unwise, to interpret the results as reflecting actual changes in perception (apart from the possibility of contrasting the perceptual information with more objective crime rate indicators), but the information certainly further buttresses our understanding of the changing saliency of crime problems.

6. There exist some data on imputed reasons for changes in crime rates, and coupled with the information on reasons for crime itself, it seems both possible and appropriate to develop various taxonomies of cause-effect pathways for subsequent empirical testing.

7. The archive provides reasonable clues to the manner in which people define potentially effective ways of dealing with crime if we are willing to assume, as we should, that they mention "ways to combat crime" to the extent to which they believe that such ways as they cite would also work.

8. The desire, or willingness, to alter funding priorities (and to invest more in crime prevention or intervention) is also well documented in the surveys and the data are comparable over many surveys and over the time period covered by the archive, but especially the past 10 years or so.

9. There exists a relatively rich data base in terms of which to assess the general effect of crime or fear of crime on activities in a generic sense, on fear of walking around one's neighborhood at night or even during daytime, on changes in entertainment and shopping behavior as such changes might be attributable to fear of crime (and this factor compared with other reasons for such changes).

10. Data on defensive behavior of individuals and of families is, however, quite weak so that the spectrum of impacts of crime on the public's way of life--along with effects of other major problems (energy shortages, inflation, unemployment and the like)--remains largely unclear.

VIII. SOME SUBSTANTIVE ISSUES: POLICE, COURTS AND PRISONS

In the previous section of the report, we considered some of the more general issues having to do with opinions and sentiments regarding crime. Here, we focus on the substantive items of the surveys as they have been raised about the police, the courts and the court system, and the nation's prisons.

If the data in terms of index questions are computerized and retrievable on issues having to do with crime in a most generic perspective, the items on police, the courts and the prisons are, of course, also included in the computerized files of survey questions and are, indeed, retrievable by the key dimensions of each such question.

Four basic types of probes typify the data base regarding the nation's views regarding the police.

One has to do with evaluations of performance. The second one concerns "problems" in policing or police officer behavior. The third one pertains to items in which the respondents identify the kinds of improvements or changes they claim they would like to see. And the fourth cluster of items refers to particular experiences of the respondents themselves as they relate to the police force.

The performance assessment items come, basically, in five different forms. They involve a simple "equidistant" verbal scale from excellent to poor (with good and poor as the intervening scale values). They involve a thermometer index with numerical values as a substitute for the more qualitative rating expression. They involve measures of satisfaction with the police. And, finally, they involve measures of confidence in police officers (or detectives). They involve questions about respect for policemen and police forces.

The results are, of course, not exactly comparable but they are, for all practical purposes, conceptually altogether compatible with each other. Thus substantive comparability is quite high if the key issue is one of determining what our people think about police performance rather than one of deciding what answers to a specific, and specifically posed question, are supposed to imply.

Even in numerical indices, it is quite easy to transform the rating scales (of police performance, or satisfaction with the police, of confidence in the police, or of respect for the police) onto a thermometer-like numeric (0-100 scale equivalent). Of course, while

such numbers themselves become numerically more comparable than are the data from which they derive, the subtle differences in the usage of specific scale remain. None, however, detract from interpretability except were we to find (as we do not) that such transformations yield inconsistent results which, therefore, could mean either an actual difference in performance evaluation or a difference attributable to the underlying methodological difference.

> Thus, conceptually, questions about police performance (on whatever evaluation scale) can be considered comparable to questions concerning satisfaction with the police, confidence in the police, or respect for the police.

This is so as long as we are willing to postulate, as we should, that "dissatisfaction" amounts to an unfavorable performance rating, that "low confidence," too, amounts to a basically unfavorable rating, and that "low respect" also is an indicator of less than satisfactory performance. Such an assumption is certainly not far-fetched and it is quite defensible in the context in which most people, as respondents to survey questions, react to such items.

In the nationwide surveys, equidistant verbal performance ratings of the police force appear in several of the studies: the N.O.R.C./"ENNIS" 1966 survey (#066), the 1971 Life Quality survey (#048), and in the LEAA/Census national attitude supplement for respondents as individuals (1972, #065).

In the former two studies (#066 and #048), the issue is also phrased in terms of police performance in the neighborhood.

In central city surveys, the LEAA/Census study at the national level (#065) is paralleled in the "individual respondent" files (#071, #073....#147), and rating of neighborhood police services is included in the 10-city Observatory research (1970, file #041).

A thermometer rating of police performance is used in a consistent and standardized manner in the C.P.S. (Michigan) series--that is, in surveys #053 (1966-1967), #054 (1968-1969), #055 (1970-1971), #056 (1972-1973) and #057 (1974-1975).

An index of "confidence in the police" appears in two Harris surveys (#023 with respondents 21 years of age and older and survey #024 with 16-20 year olds as respondents, 1967).

It is also incorporated into surveys #049 (Michigan: Justifying Violence, 1969) and the 1973 survey #032 (Harris) in which the respondents were also asked to compare the pattern of police performance (in terms of "confidence in the police") with the situation which they "recalled" to have existed some five years before.

A.I.P.O.'s surveys #003 (1965) and #004 (1967) measured the "respect for the police."

"Satisfaction with police services" comes up only in one of the city studies: the 15-city inquiry among white and black respondents of 1968 (files #042 for white respondents and #045 for black respondents).

With regard to police performance problems, or actual perceptions of police misconduct, the data bank presents a spotty record. Only a very few questions have been asked repeatedly and over time. Only a few issues have been tapped in a manner which permits significant comparisons.

At the same time, a generalized profile of problems in the archive time period, especially for the time frame of 1965 to about 1975, can be "put together" without being able to evaluate the processes of change, and without much analytic capability to assess the implications of the difference between the turbulent period of the mid and late 1960's and the 1970's.

* At the national level, information about the speed with which police officers react to calls and complaints can be at best glimpsed only from the N.O.R.C. 1966 study (#066); surveys #042 (whites) and #045 (blacks) also include an item on perceptions of police "response time."

* The Observatory study of 10 cities (#041) provides a rating of response speed perceptions, and the LEAA/Census surveys (#71, #73...#147) give an indirect clue to dissatisfaction with reaction times by probing whether improvements along these lines, in each of the cities of the research, would be desirable.

* A clue to the public's perception of police honesty comes only from N.O.R.C.'s Ennis study of 1966 (#066).

* The extent to which policemen are seen as treating citizens with respect is also measured in survey #066, and a basically comparable item is contained in the black-and-white national inquiry (#042 and #045) of 1968.

* But respect for citizens, as manifested in police behavior, can be viewed as another dimension of more generalized fairness, non-discrimination and, in fact, of "equal treatment for all."

* In several surveys, the "fairness" question appears in the context of respondent expectations as to whether he/she might anticipate "fair," "non-discriminatory" (e.g. "equal") treatment "if in trouble." Surveys #024, #042, #045, #032, #033, #037, #053 contain relevant items and the questions are conceptually quite comparable.

* In the central cities of the LEAA/Census surveys (#71, #73...#147), an item about police discrimination, too, was asked.

* Though fairness is not the only major dimension of police treatment of citizens, the more general question about police treatment contained in the Observatory research of 1970 (#041) provides further insight for the 10 cities of the study and thus allows some, if limited, comparisons with the subsequent LEAA/Census data.

* Assessment of police helpfulness, too, are an issue. As such, it appears only in the 1969 #049 survey--but the dimension is substantively, although only in a proxy manner, comparable with both "speed of response" and "treatment fairness."

* Items pertaining to the possible excessive use of force, too, are quite relevant. They take the form of questions about police brutality (as in A.I.P.O.'s surveys #003 and #004), unnecessary search and frisking as seen by the respondents (surveys #042 and #045) or "roughing up" of suspects (the same two surveys as above).

* Whether or not officers may use more force than necessary shows up in the 1968 survey #026 (Harris), and in connection with Kent State (though here, of course, the National Guard was the instrumentality rather than police officers) and Chicago incidents surrounding the Democratic convention in survey #031 (Harris, 1973, in the context of this larger "Watergate-related" survey).

* The A.I.P.O. surveys are national in scope, as are the referenced studies #026 and #031. The 15-city surveys (#042 and #045) address the "use of force" issue at the city level, though the question is worded in a more generic manner.

When it comes to public suggestions for improvements, a general conclusion is applicable:

Surveys which we identified (Chapter VII) as containing information about "ways to combat crime" or to deal with "crime problems" generally include, as coded responses, suggestions regarding improvements of police services (more policemen, more foot or motorized patrols, better street lighting and the like).

However, the more specific surveys which we wish to address here have to do with particular recommendations rather than suggestions derived from more open-ended probes.

* At the national level, surveys #066 (N.O.R.C., 1966) and #049 (1969) concern the need for more power for police officers, and the 1969 #049 surveys also probes as to the extent to which then-recent Supreme Court decisions may have made the law enforcement roles of police officers more difficult.

* The worthwhileness of civilian review boards is probed in the N.O.R.C. survey of 1966 (#066).

* The possible need for a Federal police force (distinct from the Federal Bureau of Investigation) comes up in surveys #022 (1964) and #029 (1969) not to reappear again in subsequent studies.

* The cities files provide suggestions for specific improvements especially in the LEAA/Census studies.

    (a) The need for more police officers

    (b) The need for more street patrols

    (c) The need for greater promptness, alertness and responsiveness

    (d) The need for better training, qualifications, pay, or recruitment of police officers

    (e) The need for more traffic control

    (f) The need for assignments of particular types of officers to certain parts of cities at certain times (an item which obviously refers, if indirectly, to the deployment of black and white officers in neighborhoods of varying racial composition).

If we have some limited data on excessive use of police force on the one hand, and difficulties which police officers encounter by

constraints on their authority on the other hand, a few indicators of the appropriateness of "toughness" are also available.

* Whether or not a police officer might be right in "striking an adult" (and thus use physical force) shows up as an item in a number of national studies: #021 (1964), #026 (1968), #027 (1968), #019 (1973), and the late 1970's N.O.R.C. surveys #148, #149 and #150.

* In most of these surveys, the respondents were also asked to identify the kinds of situations and circumstances under which the use of such physical force would be appropriate.

* In the 1968 surveys (#026 and #027), we also find items about the possible legitimacy of "shooting" a citizen by the police officer, and the conditions under which this would not be out of place altogether.

As we have already shown, quite a few of the surveys--though in a somewhat haphazard manner--probe into specific difficulties with police officers and into specific types of complaints.

Unfortunately, information about personal experiences of this type is even more scarce. Only in the 15-cities surveys (#042 among whites and #045 among blacks) is there any kind of follow-up. In these two studies, the respondents were asked about speed of police response, indications of respect or disrespect for citizens, unnecessary frisking and searching, and unnecessary "roughing-up" of suspects. The survey follows up these questions by asking whether the respondents themselves underwent such an experience, and the question is asked about each of the basic items identified above.

Occasional "victimization" types of items also appear in the studies (such as in surveys #008, #009, #019, #020 and #021) but we disregard them, for our immediate purposes, altogether: the major LEAA/Census victimization studies are much richer in this regard, and the surveys we mention do not link such victimization experience to items concerning problems with the police force or, for that matter, with systematic performance evaluation of police officer actions.

As we consider the data archive in terms of perceptions of the nation's courts, we find that the following major dimensionalization of the items is most applicable:

* There are questions concerning the fairness of our courts.

* There are questions along the "toughness"--"leniency" dimension of court decisions.

* There are questions about appropriateness of various penalties for specific types of offenses, upon conviction.

At the same time, there are a few other items of general relevance but these do not permit any kind of real systematization.

(a) In the Harris surveys (#023 and #024, with adults of over 20 years of age and with younger Americans in the 16-20 year old bracket), the ratings of "confidence" include ratings of judges and district attorneys.

(b) In the 1969 survey (#028), we find responses to a question as to whether the actions of the courts tend to encourage or discourage crime and why they seem to do so.

(c) We did not include, in the archive, studies which may focus on the performance of the United States Supreme Court. Insofar as a few items appear, as they do, they are incidental to the scope of the surveys--and to the purposes of the data bank. But survey #057 (C.P.S., 1974-1975) contains a "performance rating" of the Supreme Court, surveys #020 and #021 (N.O.R.C. 1974 and 1975 respectively) as well as survey #028 (Harris, 1969) contain a question about "confidence in the Supreme Court," and the 1969 study (#049, Justifying Violence, Michigan) asks about difficulties in dealing with crime as a function of Supreme Court decisions regarding proper conduct of the police and the courts.

Thus the archive does not permit any meaningful assessment, at one time or over time, of perceptions of court performance except in the context of "fairness," "leniency" or "punishment appropriateness" imagery.

Furthermore, we find no data which would permit the differentiation in the evaluation of the performance, and reasons for potential problems, among courts of different kinds and courts at different levels of government.

There exists no evidence in the archive, or anywhere to our knowledge, to consider family courts, traffic courts, district or Federal courts in a distinguishable manner across any of the dimensions which might be salient.

As far as decisions of the Supreme Court are concerned, the archive--as we have stated previously--does not attempt any kind of closure.

Nothing in this particular data archive makes any conclusions about public views about the United States Supreme Court warranted.

Furthermore, we have found no systematic data anywhere which would deal with the State Supreme Courts.

There exists no evidence, to our knowledge, to permit any interpretation of the public's perception of Supreme Courts of the nation's States.

In a similar vein:

Data on the roles and performances of public prosecutors and other officers of the court (including defense attorneys) are all but nonexistent, at least in the context of the scope of the archive.

Finally, the researcher would seek in vain, in this archive, any information about juries and the jury system:

In the hundreds of surveys we looked into before the inclusion of some 164 of them into the archive, we found no studies in which there is a systematic exploration of the roles of, and problems with, juries and the jury system.

In any case, data on the court system are limited to begin with. Thus we learn about perceptions of "fairness" from only a few surveys:

* The 1969 survey (#028) and the 1969 study (#049) contain an item regarding expectations of court treatment: the former survey differentiates only between the chances of the richer versus the poorer defendants; the latter survey incorporates, along with the richer versus poorer dichotomization, a fairness assessment regarding the treatment of blacks versus whites and the treatment of self versus "others" (whereby the "self" can be further delineated by the demographic characteristics acquired in the survey).

* A court fairness rating, in the most general terms (but, at the same time, comparable to the rating of police performance) is contained in the Observatory study of 10 cities (#041).

* In the national 1969 survey (#049, Justifying Violence, Michigan), questions are also asked about the ways in which the expected treatment of blacks as opposed to whites,

the richer versus the poorer, "self" versus "others" would not be the same, therefore identifying some of the presumed factors out of which some elements of judicial unfairness might be woven.

Questions about relative leniency-harshness of the courts appear in a number of the surveys. All of the studies are of the national variety. Table 11 provides a quick summary.

Table 11

PERCEPTIONS OF HARSHNESS-LENIENCY OF THE NATION'S COURTS

Source	File #	Year
A.I.P.O.	003	1965
Harris	023, 024	1967
A.I.P.O.	005	1968
Michigan	049	1969
A.I.P.O.	006	1972
N.O.R.C.	018	1972
Harris	031	1973
N.O.R.C.	020	1974
N.O.R.C.	021	1975

The data from these surveys, as they bear on perceptions of the leniency-harshness of the court system, are the richest component of the archive when it comes to the nation's views about the courts. The questions, in this instance, are quite comparable and the results, therefore, are comparable both across the surveys and thus, implicitly, over time.

There are two main modalities in which further information about harshness and leniency comes into place by raising issues of appropriate punishment. One has to do with the kinds of offenses for which particular punishment might be in order. The other modality has to do with appropriate punishment for specific types of offenses.

In the former dimension (offenses for which particular punishment might be appropriate), we find surveys #026 and #027 (Harris, 1968) probing into deviance for which one to two years of "hard labor" would appear as the proper sentence, and acts for which the "death penalty" might be appropriately used.

The data archive, however, has not sought to include surveys dealing with capital punishment as such so that such findings, too, are somewhat incidental and

must be, for the purposes of interpreting the state of the national thinking and the prevailing trends, placed into the context of other studies not contained in the archive.

Whether "probation," a "short sentence" or a "long prison term" is appropriate for various offenses is also included in Harris surveys #023 and #024, though the issue of possible appropriateness of death penalty is not raised.

Along the second main dimension of the problem, that is, what are appropriate penalties for particular offenses, the data provide quite a scatter of items and, as a consequence, of answers:

* What to do about an alcohol abusers "loitering" in the streets (whether to put him in jail or treat him as a health problem)--in surveys #023 and #024

* What to do about drug addicts (surveys as above)

* What to do about drug peddlers (survey #006, A.I.P.O., 1969)

* What to do about a car thief (survey #006)

* What to do about a bad check artist (#006)

* What to do about an arsonist (#006)

* What to do about a "sex offender" (#026 of 1968)

* What to do, if anything, about someone "stirring up trouble" in the community (survey #049 of 1969)

* What to do about a rapist (survey #006)

* What to do about a robber (survey #006).

Whether or not capital punishment ought to be used for convicted murderers has come up in several of the national surveys: #028 (1969), #018 (1972), #019 (1973), #020 (1974), #057 (1974-1975), #021 (1975).

The denial of parole to second time offenders was considered in the 1969 A.I.P.O. survey #006, as was the possibility of routine doubling of sentence to convicted offenders who use a gun in the commission of their crime.

The studies which provide data on the kinds of penalties which might be appropriate for various offenses, or which deal with questions on offenses

to induce specified types of penalities are comparable only with respect to the publicly desirable harshness of leniency, and interpretable in the context of harshness-leniency of the courts' perceptions rather than having any concrete, policy-oriented, meaning in any item-by-item analytic exercise.

In these broadest terms, the results are comparable and meaningful and they provide, on the one hand, some overtime evidence on the views regarding the behavior of the courts (on this "toughness dimension") as well as on the desirable behavior of the courts (on the same underlying dimension of "toughness").

Further, though substantively limited, insights about public views of the nation's courts can be acquired from those surveys in which we identified (Chapter VII) general probes about "ways to combat crime."

The data are, in this regard, "substantively limited" in that the response patterns of any significance refer to either (a) generalized needs for court reform (with, perhaps, an implicit referent to harshness-leniency and to trial delays), or (b) needs for "tougher" sentencing standards (which, once again, directly taps the harshness-leniency dimension).

When it now, finally, comes to archival data on prisons and the prison system, we find the data bank woefully inadequate. Somewhere, perhaps we have missed a vast body of information on the matter but this does not seem too likely. Rather, it would appear that the researcher's view of limited experience with prisons and limited knowledge about them on the part of the body politic inhibits the appropriate mode of questioning since every survey becomes, in the final choice, a compromise between what it may be worth knowing and what simply cannot be done (due to time or fiscal limitations or both) at the time.

Many items relevant to an understanding of attitudes toward prisons appear in both the (identical) California and Texas surveys. But the statewide inquiries, while directly comparable with each other, are not generalizable to any other national or subnational population, and cannot be compared with either national or central city data of any kind in a justifiable and methodologically sound procedure.

The general consequence is relatively simple: the only basic insight we have into attitudes toward the prison system comes from Harris surveys #023 and #024 (adults and teenagers respectively, 1967) and from the same organization's studies #153 and #154 (of 1975).

The very limited information we have reduces to the following types of probes:

* How successful have prisons been in their rehabilitative efforts.

* Whether rehabilitation chances are best in Federal, state or local prisons.

* Whether certain types of convicted offenders are basically rehabilitatable (someone convicted of having passed bad checks; someone convicted of assault and battery; someone convicted a second time of armed robbery).

* Whether staying in prison tends to increase the chances of future criminality upon release.

* Whether prisons ought to emphasize rehabilitation or punishment (the former having to do with reintegrating the offender into society's mainstream, the latter having to do with removing the offender from society so that he/she has less of a chance to commit further crimes).

* Whether more tax money ought to be spent on efforts to improve rehabilitation programs.

* What problems, upon release, the ex-offender faces.

* Whether the releasee (on parole or having served a sentence) is more likely to be influenced by "the authorities" or by "crime syndicates."

* Ratings of "confidence in" parole officers, juvenile delinquency workers, prison guards, prison wardens, probation officers, psychiatrists working with criminals, social workers working with criminals, and criminologists.

If we seek to sum up the major lessons learned from an assessment of the substantive survey questions in the UCSUR archive as they bear on the police, the courts and the prisons, we would be lead to the following main conclusions:

1. The researchers themselves (and the respective respective research organizations) seem to focus quite differentially on the various subsystems of the law enforcement system, and the characteristic which drives the choices appears to have to do with proximity (to respondents) and likelihood of more direct exposure (by respondents).

2. This results, of course, in a richer data base regarding the police than the courts, and a richer data base about some aspects of court performance than about the prison system.

3. Evaluations of general police performance are both frequent and quite comparable so that an analysis both across surveys and over time, in an assessment of trends, is altogether feasible as it is appropriate.

4. An assessment of more specific difficulties in police conduct is much more problematic, and we can at best recommend the development of more generalized, almost time-insensitive (and thus partially invalid), profiles of perceptions thereby creating a baseline for more careful future inquiries.

5. When evaluations of how police services might be improved are coupled with data from more generic studies on "how to combat crime," a procedure which we find altogether justifiable on substantive grounds, there exists reasonably solid data base in terms of which the sentiments about recommended measures can be gauged both across the various surveys and over time.

6. Specific experiences of respondents with police officers or the nature of such encounters are rather unclear, so that measures of performance, problem identifications or steps toward improvement cannot really be well anchored in the more concrete exposures of the respondents.

7. As we have already indicated, the data on perceptions of the court system are generally more limited than are the data on police forces.

8. The harshness-leniency dimension is, perhaps, most powerful in its interpretability: it comes about both in the way of judgements, in many surveys, as to whether the courts are, perhaps, too harsh or too lenient, and also, upon reinterpretation, in the way of judgements of what kinds of penalties ought to be imposed for different kinds of offenses.

9. Data on such issues as "court fairness" or "speed" with which the courts manage to bring cases to trial are simply too limited to permit much in the way of generalization or comparison.

10. The least information is available about the prison system, although good clues might be obtained from statewide studies without any attempt to trying to draw conclusions from them about various segments of the nation's public or about other states of the nation.

IX. CONCLUSIONS

In this concluding section of the report, we must first highlight the central findings of the methodological inquiry. Second, we want to provide a brief summary of some of the major limitations, as they apply to the crime opinion data archive. Third, we will point to some areas in which substantive or methodological improvements might be most desirable. And finally, we will indicate a few strategic principles which seem applicable to the archive, and perhaps to similar problem-focussed data banks, and which seem to have been indicated by our development and assessment of the present data system.

To begin with, some of the major pragmatic conclusions which bear on problems of cost-effectiveness of issue-oriented archival activities.

1. The initial establishment of an issue-oriented archive seems to be more cost-effective in the absence of detailed criteria governing the inclusion or exclusion of particular studies, while using, as broad standards, only guides as to the time frames of the studies, the appropriateness of question items included in such studies, and availability of reasonably good documentation about sampling, coding, data reduction and the like.

This seems to be the case because

(a) the initial pool of studies on any relatively important issue is quite large so that time-staggered acquisition (governed by ease of access) does not slow down the process, and

(b) standardized procedures for inputting the studies into an archive has to be developed whether there are but a few or many of them, so that it essentially does not matter how many studies are incorporated into the files to begin with, and

(c) the process of inputting requires a detailed assessment of each study and of its documentation so that it facilitates a subsequent decision as to the worthwhileness of some studies in the system.

2. Thus, it appears to be relatively easy to apply standards of study generalizability or comparability in a post hoc manner and, if needed, delete inappropriate studies from the resulting archive because their initial inclusion does involve an explicit, or minimally implied, evaluation of each survey.

3. The updating and maintenance of an archive beyond its initial development and beyond its initial decisions regarding possible deletions of studies mistakenly incorporated entails higher relative costs than does the establishment of the archive to begin with, at least by the kinds of procedures which we mention above.

This, of course, is largely a function of the fact that

(a) the originally substantial pool of studies is reduced drastically by the first phase development,

(b) not that many new studies relevant to a given issue are undertaken in any specific time period thereafter,

(c) studies easier to acquire are already in the system and the more difficult ones, by definition, involve more time delays (and greater manpower costs), and

(d) the relative use of research time over any time period is less efficient due to the more sporadic and irregular flows of new data into the archive--and yet, the need for researchers to keep abreast of what is going on is undiminished.

If questions of generalizability and comparability are considered, in some manner, as central ones, then the following might be said about the late 1979 status of the crime opinion data archive:

1. The archive is rich in nationwide data over the time frame covered by the archive, 1960 to 1978. But the bulk of the surveys span the period of mid-1960's to mid-1970's since many recent surveys are not yet available for inclusion into any more general data bank, and the very early 1960's were not marked by many surveys of which we became aware.

2. The archive is quite rich on surveys bearing on central cities, with information of considerable value for some 32 major cities of the nation. The bulk of the central city surveys, however, derives from the repeated attitude supplement material of the LEAA/Census series on National Crime studies--a factor which limits diversity of inquiries but maximizes comparability across the surveys and over the (limited) time period.

3. Only a few surveys on a statewide level are available and they are probably not enough to conduct any systematic inquiry into comparative perspectives of residents of the various states of the nation.

4. The remaining surveys in the data bank, relatively few in number, are restricted by the nature of the sampled population or by their geographic locale (to a few precincts in some instances, or to a subregion of the nation and the

like) and their usefulness rests mostly with the kind of data dredging out of which insights for future studies might come.

Now in a more technical sense, generalizability (to some underlying population) depends on the sampling design and on the implementation of the design in the course of the fieldwork.

In terms of the sampling designs of the surveys in the archive, we may speak about hard generalizability and soft generalizability, somewhat relaxing the more precise meaning of the "generalizability" concept in its latter usage. Hard generalizability, of course, characterize surveys in which we deal with probability samples. There is no question, in this sense, that valid statistical inference indices and tests can be used.

Soft generalizability, in turn, depends somewhat on the willingness of the researcher to take (limited) risks: it has to do with quota or "modified probability" samples of varying kinds. Soft generalizability is justified by empirical experiences of the researchers. Hard generalizability has its own built-in mathematical justification and is not dependent on empirical experiences.

1. At the national level, "hard generalizability" standards are applicable to the C.P.S. (University of Michigan surveys), to most of the Harris studies, to the latest N.O.R.C. General Social Surveys and to the LEAA/Census national study of 1972.

2. At the central cities level, "hard generalizability" applies to the Urban Observatory data on 10 cities, to the 15-city Michigan study and, of course, to the LEAA/Census surveys of the mid-1970's.

3. At the state level, the Michigan surveys (of mid-1970's) meet the criteria for such "hard generalizability."

4. "Soft generalizability" is more characteristic of the "modified probability" approach of A.I.P.O., of the quota samples of N.O.R.C. (through 1975), and to the A.I.P.O. conducted surveys for Potomac Associates.

5. "Soft generalizability" is, perhaps, also most relevant for some of the city surveys which we have not cited above: to Boston, Detroit, Baltimore and St. Petersburg data in some of which the issue is not so much one of the sampling design as it is one of fieldwork implementation.

6. Minnesota statewide polls, by their quota approach, as well as the California and Texas studies which appear to come close, but not probably so, to a probability sampling design, are also probably at best of the "soft generalizability" variety.

7. Obviously, in any and all surveys for which generalizations are possible, comparisons of data over time are also statistically legitimate so that it is also appropriate to make generalizations regarding the significance of trends and evolving patterns.

8. Such overtime comparisons, of course, are most facilitated by surveys carried out by one and the same research organization or under the same auspices since the basic approaches to sampling tend to remain identical, or are predictably altered (as in the N.O.R.C. shifts from quota to probability samples).

Now background characteristics (socio-cultural and demographic indices) along with sex/age/race based eligibility criteria for inclusion into a sample provide a further refinement and open up the additional and customary, analytic opportunities.

1. Data on sex, race, age, education, income, city size and occupation tend to appear in most of the surveys and tend to be usable, in comparative terms, across the various studies and over time.

2. Generalizations to such demographic population segments are also possible (within the previously identified patterns of generalizability on the whole) although some of the characteristics require grouping (such as age) or regrouping (education, occupation or income) to produce subgroups of conceptually useful sizes to which appropriate statistical indices can be applied.

3. Thus along these lines, and with simplified demographics on those items for which the data codes are too cumbersome or only partially comparable, the relative significance of trends for particular segments of our population can also be evaluated.

4. In some of the city-level inquiries which are otherwise generalizable (Urban Observatory and the 15-city study), disaggregation by demographic characteristics is, of course, quite possible and appropriate, but the resulting subgroups are too small to permit statistical generalizations to be made. In this instance, the meaning of the population segment data must be grounded in their comparability with similar population segments from studies with larger sample bases.

Unless, of course, the substantive questions raised (the dependent variables of an inquiry) are also comparable, it is of relatively little value to assert that generalizations are possible or are not, or that demographic comparisons and attendant generalizations are also feasible.

We may say that we deal with strong comparability along substantive lines when the same questions, or essentially the same questions, are asked and their formatting, too, is either the same or essentially the same. Weak comparability, in turn, might be said to characterize items which seek the same, or essentially the same, information but the format or metric of the questions varies so that some assumptions must be made to transform the data into comparable numerics (or comparable verbal statements). Finally, we might speak of conceptual comparability when questions differ and even their basic metric may vary, but the key underlying dimension which they seem to tap is, or can be considered as being, the same.

1. Police performance ratings, for instance, display strong comparability (such as in LEAA/Census and Urban Observatory studies), and the C.P.S. use of performance thermometers, too, yields strong comparabilities within the series.

2. Police performance indices involving comparisons of both equidistant verbal scales and such metrics as the thermometers (and thus across different series of studies) exemplify weak comparability in our terms.

3. Police performance evaluations, when based on ratings, thermometers but also on questions regarding respect for police, satisfaction with police services, or items about particular types of police behavior (courtesy toward citizens, fairness, use of force and the like) establish a good example of conceptual comparability.

Thus, as a rule, many items from many surveys can be make conceptually comparable, while weak comparability characterizes a subset of items, and strong comparability is often limited to data series acquired by one and the same research organization only.

Strong comparability, therefore, exists for such questions as those which repeatedly probe into the nation's major problem, the community's or the neighborhood's problem. Similarly, questions about problems (not merely the single most important one) of the nation, community and the neighborhood are strongly comparable and the two sets of items become comparable with each other if we are willing to assume that the first response to problems can be adequately mapped onto answers to questions about the single most important problem.

Repeated questions about the relative safety of walking around one's neighborhood at night are strongly comparable, and remain so whether or not a distance limit (such as "within 1 mile") is specified or not.

Conceptually, when analyzed in the context of items such as "walking around the neighborhood during daytime," entertainment and shopping patterns, the comparability occurs at the level of crime's effect on activities.

But in this regard, it would seem most prudent for the archive user/researcher to retrieve particular clusters of questions and make decisions about the degree of their comparability on a concrete case-by-case basis, especially when the effort involves the development of conceptual taxonomies the components of which may well be derived from various types of questions and from various surveys.

1. Data on crime in general, alleged causes of crime, changes in crime rates, factors contributing to such changes, ways to combat crime, funding needs and the like are the richest, and internally most comparable, aspect of the data base.

2. Data on the effects of crime on activities of the public are less adequate mainly because they lack time dimensionality over the years of the surveys and also because they simply rarely address those activities which might be specific home-based defenses against crime.

3. Data on police performance are quite abundant, but items from which patterns of possible police misconduct might be surmised require conceptualization of a higher order since individual specific items (brutality, fairness, politeness, honesty, use of force and the like) are not often asked, and are rarely repeated in a strongly comparable fashion.

4. Data on ways to improve police services are also available but the coverage in the surveys does not appear to have been either detailed or spanning many surveys over time so that changes in public preferences for improvements would be quite difficult to establish with any degree of confidence.

5. With regard to the court system, data on court leniency-harshness are quite rich and, generally, strongly comparable.

6. Much as is the case with possible patterns of police misconduct from which only a more generalized profile can be obtained when many items are used jointly, data on appropriate penalties for various offenses also come in a rather scattered manner though overall conceptualization in terms of "toughness-softness" of desirable penalties renders the results comparable (at the conceptual level of harshness-leniency).

7. Data on perceptions of the prison system are quite scarce, and the most comparable (and rich) information comes from the two statewide studies, California and Texas, which are, however, not quite comparable with the remaining studies in the archive and not even with the two Michigan surveys (due to both different scope and different sampling procedures) or with the Minnesota studies (due to their use of quota, rather than probability samples).

Now there are six major points we would like to raise regarding some of the limitations of the archive. These are, of course, a few issues apart from such problems of generalizability and comparability which we have already addressed.

1. For the time frame of the archive, 1970-1978, the studies included do not represent any sense of closure.

   (a) There may have been surveys done about which we have not learned, though due to the careful effort to contact all relevant research organizations, both within Universities and outside, this may not amount to much of a limitation at all.

   (b) Some studies about which we know (Maryland Crime Commission surveys, a Washington, D. C. study in several police precincts) have adequate documentation, including basic data output (marginals and some cross-tabulations) but the original records, in card form or on computer tape, are simply not available.

   (c) Some studies lack adequate documentation even though raw data do exist--and we did not seek to acquire such surveys so that they are simply not included in the archive at all (two or three such surveys are known to us).

   (d) Occasionally, proprietary reasons precluded our acquisition of a study (otherwise available and well documented).

   (e) Some of the most recent studies (late 1978 or 1979) have not yet become, or been make, available for inclusion into any archive since the researchers or research organizations have not completed their own evaluation and reporting.

2. Many of the surveys in the archive were not designed to probe into crime-related issues as the study's key focus so that they contain only few relevant items and lack the kind of interviewing follow-throughs which would characterize research with a particular issue focus. This, of course, results from the fact that many of the surveys are of the multi-purpose variety (as, for instance, the Harris series, or, at the city level, the Observatory research) and others deal with selected aspects of the crime problem mainly because other more specific purposes of the research call for it, the issue being "salient" for other reasons (as in the political surveys of C.P.S.).

3. Many sample sizes are such as to inhibit multi-variate elaboration unless the researcher is willing to make the necessary assumptions which permit such methods as those of multiple

regression or path analysis. Thus beyond considering two, and occasionally three, independent variables (such as demographics) jointly, more complex profiles of the relevant population's sentiments are quite difficult to develop.

4. There is some "noise" in the system of studies which could not be avoided on a post hoc basis by our processing and standardizing because it has been built into the initial data base by the researchers and the research organizations. Thus there are some "unclean" data especially with regard to conditional questions, that is, questions which are to be asked on condition that the respondent answered a prior question in a particular manner. Whether something less than adequate care in the interviewing process or less that adequate quality control over data coding or even reduction are at the roots of this difficulty we cannot tell. The substantive thrust of the data is, however, not affected (the "fuzziness" involving generally but a few cases) but the issue does point to quality control difficulties at the organizational data source.

5. Even in surveys, limited though they are in numbers essentially to the LEAA/Census sequence, in which there is a panel design (the same respondents asked the same questions over time), the panel feature is obliterated from the records so that the dynamics of net changes cannot be analyzed, and the researcher is left with a data base which is quite impoverished compared with what it might be were the panel design dimension fully preserved.

6. Though many problems concerning crime have a particular neighborhood as a referent (including general questions about crime and changes in rates, comparisons of one neighborhood with others, changes in public behavior in the neighborhood, police performance and problems in the neighborhood and the like), the surveys are not disaggregatable either by "neighborhoods" or by "types of neighborhoods" (say, by SES or by racial composition and the like) so that the interpretive value is lessened significantly.

There are a few things which are simply too much to ask for, or to expect, in the way of improvements. For instance, it might be quite ideal if the research community, on problems such as crime (or the police, courts, prisons and so on), could reach an agreement on a common core of questions that would be asked identically and repeatedly and regardless of the organization conducting a survey. This would, of course, in no way preclude the incorporation of additional, more organization or researcher typical items, but would allow for rapid accumulation of fully comparable data. We do not think, unfortunately, that such a standardization is achievable in the foreseeable future.

It would be also equally worthwhile if the research community could reach an agreement, not merely in theory but in practice, that only probability samples will be used. But such a diseratum, on the theoretical justification of which all would readily agree, is not achievable if financial constraints vary from survey to survey and, perhaps, from organization to organization.

It would be similarly desirable if there were an agreement on the background variables to be always included and the exact manner in which data on such characteristics are to be acquired in the course of a survey. In this regard, of course, a tacit agreement has emerged out of the survey practice at least on some items--sex, race, age, marital status, income, occupation, education.

Now there are, however, some possible improvements, to which our assessment of the crime opinion data archive points, which are quite feasible. We will outline a few of them along three lines. One dimension is technical-methodological. The second one has to do with the types of survey elements and the populations from which they are sampled. The third one concerns some substantive issues the availability of data on which would greatly enrich our understanding of a problem as complex as that of crime or of the criminal justice system.

In terms of research technologies:

1. It would be quite desirable to develop ways to <u>standardize research documentation</u>, that is, the detailed manner in which information about sampling, field work implementation, and in-house data reduction (editing, coding, processing) ought to be maintained. This is not something which the American Association for Public Opinion Research could not encourage and undertake, and even if some, and not all, researchers and research organizations were a party to such an agreement, significant improvements regarding interpretabilities of data could be made rather rapidly.

2. It would be similarly possible to establish agreements, perhaps again with A.A.P.O.R.'s interest and help, on <u>standardized coding</u> of those items for which such standards are not shared or do not exist: For instance, age-coding to the nearest year (something being done quite generally already but not consistently so) which maximizes flexibility in grouping; standardized coding of respondent's formal education, standardized (though time-related) coding of income categories as well as occupational categories; standardized coding of city-size, including an agreement to code suburban residential contexts separately from the respective metropolitan areas.

3. It would seem relevant to include, in the data records themselves and not only in aggregate documentation, "tags" for respondents who became available for interviews only after

1, 2,...n, callbacks, and similar "tags" for those instances in which one household (or specific respondent) was substituted for another, previously sampled, household or individual. Over time, we would stand quite a bit to learn as to whether those who have to be found upon repeated callbacks (not being at home initially, the first callback period, the second, and so on) vary systematically with respect to either the independent (demographic) traits or on the dependent (substantive) variables of the research, and whether "substitutions" (following refusal or failure of repeated callbacks) affect the pattern of results either with respect to the independent or the dependent variables.

4. In samples in which some population segments are disproportionately included by the sample designer's choice, the inclusion of weight factors in the data set itself (a practice occasionally encountered but usually not seen) would prove of great value on longer run.

5. Neighborhood level disaggregatability of data would seem quite essential when surveys raise "neighborhood-related" questions so that the analyst would be in a position to develop appropriate typologies of neighborhoods and anchor the results in such prototypes as may result.

6. Maintenance of information which does not obliterate such major features as those of a "panel design" would be similarly crucial, though in Government conducted or sponsored research (or in making such Government produced data available to other researchers) it may be necessary to explore the laws, rules and regulations which cover such problems and, perhaps, to identify the classes of exceptions (to such things as the "privacy" provisions) for possible Congressional or Administrative change of rules which limit the actual utilizability of the data (and thus lead to findings of lower value than might be obtained otherwise).

There are also some special populations about which it would be worth knowing much more than we do. This is, of course, apart from the desirability of more generalized national or city-wise surveys.

1. It would be of value if more statewide generalizable and comparable surveys were carried out.

2. It would be of considerable importance to use suburbanites (at the national level) as a specific sampling frame.

3. Surveys with a focus on America's youth (regarding crime-related problems) would be valuable. Such few studies (Harris especially) as exist involve younger respondents as subsamples only and on rare occasions at that, so that our understanding of attitudes of young people on the issues with which the archive is concerned remains quite limited.

4. It would similarly be quite important to carry out (nationwide, statewide, citywide) surveys among samples of the elderly.

5. Though millions of Americans (or the country's residents) are of Hispanic origin, we know next to nothing about crime-related perspectives of the people of Hispanic roots.

In substantive terms some suggestions regarding improvements of crime opinion data base in general, and of its archival variety more specifically, can also be made. They reflect, of course, our assessment of the major lacuna in the data system at this time and we make no pretense to provide an exhaustive and detailed listing of all the things it might be worth knowing.

1. It would be very desirable to acquire, and keep acquiring, more data on the chains of causal reasoning, that is, the chains of lay interpretations of causes of crimes, the links to perceived steps to deal with crime, and the relation of such measures as advocated to their perceived effectiveness and the "whys" of such effectiveness of the various measure.

   A more systematic application of reason analysis--in the Lazarsfeldian vein of the "art of asking why"--would prove most appropriate.

2. We need to know more about specific experiences of the public with the police, the courts and the prison system--both direct personal experiences and experiences of other family members as well as those which are shared with the members of the public by friends and neighbors. In this regard, the critical incident approaches might prove to be the best ones. This, to be sure, is not an issue in victimization for which LEAA/Census have established a valuable, and massive, data base in the National Crime Surveys (both nationwide and in the selected central cities). Rather it has to do with experiences with the criminal justice system.

3. We would like to see much more data about the way in which crime (and in the context of other issues, most specifically energy shortfalls and costs, inflation, unemployment) affect the nation's life styles. Thus we would like to see more data on behavior and changes in behavior rather than more and more data on attitudes only, or mainly (worries, concerns, fear of crime and the like). Especially important would be data, thus far quite scarce, on the kinds of crime prevention techniques and anti-crime measures which people and their families have taking, plan to take or think would be worth taking--along with the reason-analytic "whys"

in an effort to understand the more concrete manner in which particular individual/family/household measures and behavior changes serve as a deterrent to criminal insult.

4. We would be strongly in favor of more data about public attitudes toward, and perceptions of, different courts, prosecuting attorneys and defense attorneys as well as judges, juries and the jury system as well as of data on such factors as the way our people interpret "mitigating circumstances" and on conditions under which they might administer a different punishment for the same type of offense(s).

5. On the nation's prison system, we would simply like to see more data in general--whether on exposures (self, family members, friends or neighbors) to it (as inmates or, for that matter, as visitors), experiences with it, evaluations of the system, assessments of ways to improve its effectiveness in the context of its possible roles (and the "whys" of such perceived effectiveness) and the like.

Finally, we may suggest a few overall conclusions which are, we think, quite applicable to all issue-oriented survey data archives.

The first major conclusion is, to an extent, value-ladden to that we identify it somewhat separately: We think that the development, maintenance, updating and use of issue-oriented archives is as extremely worthwhile undertaking--whether the issue by "crime," or "energy", or "peace and war perspective" or views on "labor-management issues" or others. Apart from the rich potential for easily accessible secondary data analysis, and apart from the possibilities to address police-maker (and other user) questions across a variety of surveys, and often, over time, there exists also an important future value (for historians who will attempt to analyze the dynamics of our times).

Our other key conclusions are not as value-imbued as the one with which we began. So we state them in their own right:

1. Basic approaches to sampling designs have become well standardized. Multi-state probability samples with household clusters are almost invariably used up to, and including, the household selection.

   (a) National regions and city sizes are common stratifiers, while there are subtle differences in the use of other important variables for the initial stratification (SES, racial composition, age composition).

   (b) The practice indicates that 5-10 households tend to be included in each sample within the final block selections.

   (c) There are differences, of course, in the way in which the actual respondents are chosen--from probabilistic models to quote assignments to interviewers--with attendant differences in mathematical generalizabilities.

   (d) Three to six callbacks seem common (in probabilistic samples) but while this is so in the design phase, the practice occasionally varies (in that no callbacks are made even though called for by design).

   (e) There are differences regarding substitution rules (for chronic not-at-homes or refusals): Some organizations prefer to substitute (usually the "household next door") and others do not.

2. The acquisition of data about background characteristics of the respondents and, as appropriate, about the family of household seems to have also become rather well standardized so that comparability, in these terms, tends to be maximized even if occasional regrouping or category collapsing is necessary.

3. The styles of individual researchers and of particular research organizations, however, seem to dictate the nature of questions, their sequencing and their formats so that there exists relatively low standardization at the substantive level.

4. Subtle effects of the "times" are also often distinguishable not only in the numbers of questions asked about particular issues, but also in the use of language (questions about "crime in the streets" or using the "law and order" terminology, so frequent in the late 1960's have all but disappeared in that particular form).

5. Studies carried out by one and the same research organization over time are both most generalizable and most comparable since there is a strong tendency to use the same sampling design and (some of) the same questions in the same contexts and with the same (or only slightly adapted) wording.

6. Even surveys which, in a technical sense, do not allow generalizability are, however, of great value: Generally, they provide the best information there is about an issue and are, almost by definition, superior to guesswork or "armchair" speculations about what the nation, or a particular segment of the population, seems to think. Thus some analytic value and even use value is partially independent from the generalizability criterion.

7. The search for underlying patterns and for changes in patterns (including explanations of the patterns and of such changes as may be found) is probably much more profitable, in an analytic as well as user sense, than even the most rigorous interpretations of data acquired from single, or but a few, specific items.

And, finally, another somewhat value-affected conclusion. A data archive, such as this one, once developed and reasonably well up-to-date can become obsolete in but a few years. This means that we consider it desirable to maintain such an archive, or any suchlike archive, up-to-date on an ongoing basis.

At the same time, not many new studies (on any given issue) tend to be carried out annually or are made available for yearly archiving. It seems to us that an up-date cycle of about every two, or even three, years would do the job very well.

Of course, it is particularly desirable for the archived data to be put to use both by researchers and, either directly or with the aid of researchers, by practitioners. But the value of an archive need not be judged solely by the intensity and frequency of its immediate use. Rather, nonuse over a period of time (such as five years) might signify lower worth of an archive than that for which the developers of the data bank designed it. Even the simple time delay in information dissemination (about the availability of a data base) and in information as to how to best access an archive may well account for a time lag of two to three years.

It would, therefore, be prudent to delay judgments as to the real use-value of an archive, including the crime opinion archive developed and maintained at the University Center for Social and Urban Research of the University of Pittsburgh, for a period of three to five years. Prior to that kind of a time frame, we do not expect the archive to be put to much use except by ourselves, other faculty members of the University, and graduate students in the relevant social science departments of the University.

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NILECJ SURVEY

DATA ARCHIVE SAMPLING INFORMATION

APPENDIX A

NILECJ SURVEY

DATA ARCHIVE SAMPLING INFORMATION

To further standardize the archive and to generate summary materials for the methodological assessment, the University Center for Social and Urban Research (University of Pittsburgh) produced the attached tabulation of major sampling information for each of the surveys in the data bank.

Apart from its tabular form, as presented here, the documentation is computerized and it is retrievable in terms of any study characteristic or combination of characteristics.

1. Each "study name" (generally a name of the organization responsible for the survey) is preceded by a number (1-164). This number is the computer file number of the survey.
2. "File name" reproduces the designation of the survey by the originating organization itself.
3. "Study date" refers to the period of fieldwork. The computerized sampling tables include, however, detailed information about when the fieldwork began as well as when it ended.
4. "Sample size" refers to the actual number of records in the archive and thus to "completed interviews" rather than to the designed sample size.
5. "Time points" information simply identifies whether a particular survey was carried out only on one occasion or on more occasions.
6. Any major criteria defining the study population (from which the sample was drawn) are specified under "Other population criteria."
7. "Sample area" delineates each survey in terms of geographic area coverage. The term "national" refers to the 48 contiguous states. We found two surveys (conduced by U.S. Department of Commerce—Social and Economic Statistics Administration--Bureau of the Census) that have included Alaska and Hawaii as well. No studies include other United States territories and holdings.

8. "Inclusion criteria," further detailed under the columns for "sex," "age" and "race" have to do with definitions of eligibility for the inclusion into the study sample. When "race," for instance, remains unspecified, the survey simply included eligible (age/sex/other criteria) respondents regardless of race.

9. "Unit" designation pertains to still another aspect of the sampling design: whether the sampling frames involved an "individual" as the respondent (with generalizability to individuals) or a respondent representing a "household."

10. The type of sample is identified but more detail is available in the documentation since many of the samples are not precisely definable by the summary "catch phrase" tabulated here. In each instance, however, the key designation reflects the specificat on of the sample as stated by the responsible organization itself and it does not represent our own decision as to the sampling mode.

11. The last column of the table identifies such stratifiers as the sampling design documentation specifies.

NILECJ SURVEY

DATA ARCHIVE SAMPLING INFORMATION

NILECJ SURVEY
DATA ARCHIVE SAMPLING INFORMATION

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
1	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AIP634	August 60	3340	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geo-graphic area, Pairs of location
2	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AI669N	March 63	4338	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geo-graphic area, Pairs of location
3	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AI709N	April 65	3534	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geo-graphic area, Pairs of location
4	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AI749N	August 67	3528	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geo-graphic area, Pairs of location
5	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AI757N	February 68	1500	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geo-graphic area, Pairs of location
6	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AI773N	January 69	1464	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geo-graphic area, Pairs of location

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
7	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AI847N	March 72	1518	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
8	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AI861N	December 72	1506	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
9	AMERICAN INSTITUTE OF PUBLIC OPINION --GALLUP	AI?931	June 75	1560	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
10	POTOMAC ASSOCIATES - STATE OF THE NATION --GALLUP	GA7235	May 72	670	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
11	POTOMAC ASSOCIATES - STATE OF THE NATION --GALLUP	GB7235	May 72	614	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
12	POTOMAC ASSOCIATES - STATE OF THE NATION --GALLUP	GC7235	May 72	528	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
13 POTOMAC ASSOCIATES - STATE OF THE NATION --GALLUP	GA7445	April 74	630	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
14 POTOMAC ASSOCIATES - STATE OF THE NATION --GALLUP	GB7445	April 74	630	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
15 POTOMAC ASSOCIATES - STATE OF THE NATION --GALLUP	GC7445	April 74	615	1	None	National	P Youngest S Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
16 MINNESOTA POLL	MIN284	January 69	645	1	None	Minnesota	None	M F	21+	-	I	Block Quota[3]	Urban; sex, age rural, town-farm; sex, age, socio economics
17 MINNESOTA POLL	MIN297	April 70	600	1	None	Minnesota	None	M F	21+	-	I	Block Quota[3]	Urban; sex, age rural, town-farm; sex, age, socio economics
18 NATIONAL OPINION RESEARCH CENTER - ROPER INSTITUTE, GENERAL SOCIAL SURVEY	NOR72N	February to April 72	1614	1	None	National	None	M F	18+	- -	I	Block Quota[3]	Quotas based on age, sex and employment status within blocks selected probabilistic

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
19	NATIONAL OPINION RESEARCH CENTER - ROPER INSTITUTE GENERAL SOCIAL SURVEY	NOR73N	February to April 73	1506	1	None	National	None	M F	18+	-	I	Block Quota[3]	Quotas based on age, sex, and employment status within blocks selected probabilistic
20	NATIONAL OPINION RESEARCH CENTER - ROPER INSTITUTE GENERAL SOCIAL SURVEY	NOR74N	February to April 74	1485	1	None	National	None	M F	18+	-	I	Block Quota[3]	Quotas based on age, sex, and employment status within blocks selected probabilistic
21	NATIONAL OPINION RESEARCH CENTER - ROPER INSTITUTE GENERAL SOCIAL SURVEY	NOR75N	February to April 75	1491	1	None	National	None	M F	18+	-	I	One half full probability[4] One half block quota[3]	Probability by size geographic region, SMSA, geographic location within area, race, income by block; sex, age, employment status
22	LOUIS HARRIS POLL	H1384	Fall 64	1121	1	None	National	None	M F	18+	-	I	Multi Stage Cluster[5]	Geographic region & metropolitan residence

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
23	LOUIS HARRIS CRIME AND CORRECTIONS STUDY ADULTS	H1758A	November 67	943	1	None	National	None	M F	21+	-	I	Multi stage cluster randomly designated "R"	Geographic region & metropolitan residence
24	LOUIS HARRIS CRIME AND CORRECTIONS STUDY TEENAGERS ONLY	H1758T	November 67	198	1	None	National	None	M F	16-20	-	I	Multi stage cluster randomly designated "R"	Geographic region & metropolitan residence
25	LOUIS HARRIS NATIONAL MALAISE SURVEY	H1813	March 68	1586	1	None	National	None	M F	18+	-	I	Multi stage cluster[5]	Geographic region & metropolitan residence
26	LOUIS HARRIS VIOLENCE SURVEY ADULTS	H1887A	October 68	1175	1	None	National	None	M F	19+	-	I	Multi stage cluster[5]	Geographic region & metropolitan residence
27	LOUIS HARRIS VOILENCE SURVEY TEENAGERS	H1887T	October 68	496	1	None	National	None	M F	14-18	-	I	Multi stage cluster[5]	Geographic region & metropolitan residence
28	LOUIS HARRIS MORALS AND VALUES SURVEY	H1933	May 69	1575	1	None	National	None	M F	Adults	-	I	Multi stage cluster[5]	Geographic region & metropolitan residence

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
29 LOUIS HARRIS POLL	H1970	October 69	1982	1	None	National	None	M F	Adults	-	I	Multi stage cluster[5]	Geographic region & metropolitan residence
30 HARRIS CRIME SURVEY	H1935	69	1545	1	None	Baltimore 2,070,670[32]	None	M F	Adults	-	I	Multi stage cluster randomly designated "R"	Geographic region & metropolitan residence
31 HARRIS SURVEY ON WATERGATE	H2344	August 73	1547	1	None	National	None	M F	Adults	-	I	Multi stage cluster randomly designated "R"	Geographic region & metropolitan residence
32 LOUIS HARRIS POLLS	H2343P	September 73	1596	1	Public	National	None	M F	Adults	-	I	-	
33 LOUIS HARRIS POLLS	H2343L	September 73	274	1	Government Leaders	National	None	M F	Adults	-	I	-	
34 POLICE FOUNDATION STUDY IN KANSAS CITY	KC1972	July 72	1200	1	15-beat[6] experimental area	Kansas City, Missouri	None	M F	18+	-	I	Strict Probability 4	Occupied dwelling units
35 POLICE FOUNDATION STUDY IN CINCINNATI	CHI73	February April 73	1264	1	None	Cincinnati 452,550[32]	None	M F	14+	-	H	Multi probability	Sector of city racial, economic differences[7]

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
36	POLICE FOUNDATION STUDY IN CINCINNATI	CAT73	February April 73	1264	1	None	Cincinnati 452,550[32]	None	M F	14+	-	I	Multi Probability	Sector of city racial, economic differences[7]
37	POLICE FOUNDATION SURVEY IN SAN DIEGO	SD1973	73	608	1	None	San Diego 696,566[32]	None	M F	16+	-	I	Equal Probability[8]	Cluster of 6 adjacent housing unit and selection of respondents within
38	POLICE FOUNDATION SURVEY IN SAN DIEGO	SD1974	74	541	1	None	San Diego 696,566[32]	None	M F	16+	-	I	Equal Probability[8]	Clusters of 6 adjacent housing unit and selection of respondents within
39	ST. PETERSBURG CITIZENS SURVEY	STP74	October 74	509	1	None	City of St. Petersburg 216,067[32]	None	M F	18+	-	I	Random Probability[8]	None
40	ST. PETERSBURG CITIZENS SURVEY	STP75	October November 75	511	1	None	City of St. Petersburg 216,057[32]	None	M F	18+	-	I	Random Probability[8]	None
41	URBAN OBSERVATORY PROGRAM, TEN CITIES SURVEY OF CITIZEN ATTITUDES TOWARD LOCAL GOVERNMENT	UOP70N	70	4266	1	Politically defined limits of each central city[9]	10 cities	None	M F	18+	-	I	Equal Probability[8]	Cities

CONTINUED

2 OF 3

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
42 RACIAL ATTITUDES IN 15 AMERICAN CITIES BY CAMPBELL AND SCHUMAN	WHITEN	January April 68	2584	1	None	15 cities[10]	None	M F	16-69	White	I	Representative Probability[4]	City blocks dwelling units
43 BOSTON AREA STUDY	BAS69	January March 69	723	1	None	Boston 2,753,800[32]	None	M F	Adult[11]	-	H	Area Probability[4]	Geographic area occupied housing
44 BOSTON AREA STUDY	BAS70	January March 70	571	1	None	Boston 2,753,800[32]	None	M F	Adult[11]	-	H	Area Probability[4]	Geographic area, occupied housing
45 RACIAL ATTITUDE IN 15 AMERICAN CITIES BY CAMPBELL AND SCHUMAN	BLACKN	January April 68	2809	1	None	15 U.S. cities[10]	None	M F	16-69	Black	I	Representative Probability	City blocks dwelling units
46 PUBLIC OPINION OF CRIMINAL JUSTICE IN CALIFORNIA	CAL72	January February 72	937	1	None	California	Adults Teenagers	M F	18+ 14-17	-	I	Representative Probability	Social economic levels ages, race

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
47	PITTSBURGH NEIGHBORHOOD ATLAS, NEIGHBORHOOD SURVEY	CRMPNA[12]	Spring 76	9767	1	Registered Pittsburgh Voter as of November 75	Pittsburgh 520,167[32]	None	M F	18+	-	I	Random	Voting districts
48	THE QUALITY OF AMERICAN LIFE BY CAMPBELL, et.al.	QAL71N	July August 71	2164	1	None	National	None	M F	18+	-	I	Multi stage area probability	Geogr[illegible]ic region, SMSA, counties
49	JUSTIFYING VIOLENCE: ATTITUDES OF AMERICAN MAN BY BLUMENTHAL, KAHN AND ANDREWS	JV69N	Summer 69	1374	1	None	National	None	M	16-64	-	I	Equal Probability	Small compact geographic areas[13]
50	DEFENSE CIVIL PREPAREDNESS AGENCY (UCSUR)	CD4[14]	February 66	1496	1	None	National	None	M F	18+	-	I	Probability	Region of country, Size of city
51	UNIVERSITY OF MICHIGAN CENTER FOR POLITICAL STUDIES AMERICAN NATIONAL ELECTION STUDY	CPS6ON	November 60 January 61	1181	1	None	National	None	M F	Adult Voting Age	-	I	Probability	Population density geographic location

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
52 UNIVERSITY OF MICHIGAN CENTER FOR POLITICAL STUDIES AMERICAN NATIONAL ELECTION STUDY	CPS64	November 64 January 65	1571	1	None	National	None	M F	18+	-	I	Probability	12 largest cities[15] Proportion to size
53 UNIVERSITY OF MICHIGAN CENTER FOR POLITICAL STUDIES AMERICAN NATIONAL ELECTION STUDY	CPS66N	November 66 January 67	1291	1	None	National	None	M F	18+	-	I	Probability	12 largest cities[15] Proportion to size
54 UNIVERSITY OF MICHIGAN CENTER FOR POLITICAL STUDIES AMERICAN NATIONAL ELECTION STUDY	CPS68N	November 68 January 69	1673	1	None	National	None	M F	18+	-	I	Probability	12 largest cities[15] Proportion to size
55 UNIVERSITY OF MICHIGAN CENTER FOR POLITICAL STUDIES AMERICAN NATIONAL ELECTION STUDY	CPS70N	November 70 January 71	1694	1	None	National	None	M F	18+	-	I	Probability	12 largest cities[15] Proportion to size

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
56 UNIVERSITY OF MICHIGAN CENTER FOR POLITICAL STUDIES, AMERICAN NATIONAL ELECTION STUDY	CPS72N	November 72 January 73	2705	1	None	National	None	M F	18+	-	I	Probability	12 largest cities[15] Proportion to size
57 UNIVERSITY OF MICHIGAN CENTER FOR POLITICAL STUDIES, AMERICAN NATIONAL ELECTION STUDY	CPS74N	November 74 January 75	1575	1	None	National	None	M F	18+	-	I	Probability	12 largest cities[15] Proportion to size
58 A STUDY OF FAMILY - SCHOOL RELATIONSHIPS IN DETROIT --DETROIT AREA STUDY	DAS63N	63	1536	1	Mothers of children in grades 5a, 6a, 6b	Detroit area School Districts	None	F	Adults[17]	Black White	I	Random	Classroom listing, race[18]
59 CITIZENS IN SEARCH OF JUSTICE --DETROIT AREA STUDY	DAS67N	67	780	1	Head or wife of head of primary family[19]	Detroit area[16] 4,199,931[32]	None	M F	Adults	-	I/H[19]	Area Probability Random	Inner city suburbs[20]
60 BLACK ATTITUDES IN DETROIT --DETROIT AREA STUDY	DAS68N	April to July 68	619	8 of 10 33	None	City of Detroit 1,511,336[32]	None	M F	69 or less	Black	I	Multi stage Probability	High and Low income Upper and Lower socio-economic, Strata twice as many from high strata as low strata

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
61 WHITE ATTITUDES AND ACTIONS IN URBAN PROBLEMS --DETROIT AREA STUDY	DAS69N[33]	69	640	9 of 10 33	Head or wife of head of primary family[19]	Detroit[16] area 4,199,931[32]	None	M F	69 or less	White	I/H[19]	Multi stage Probability	Socio-economic patterns
62 SOCIAL PROBLEMS AND SOCIAL CHANGE --DETROIT AREA STUDY	DAS71[33]	71	1881	10 of 10 33	None	Detroit[16] area 4,199,931[32]	None	M F	21+	-	I	Multi stage Probability	Race, size[21]
63 DETROIT LONGITUDINAL STUDY WAVE 1	DLSN	August 67 March 68	847	1	Community sample and riot area sample	Detroit[16] area 4,199,931[32]	None	M F	16+	-	I	Cluster[34]	Proportionally riot non riot[22]
64 NATIONAL CRIME SURVEY, ATTITUDE SUPPLEMENT	DUALIH	September 72	5900	1	None	National[23]	None	M F	Adult	-	II	Representative Probability	Geographic region population density rate of growth 1960-1970 [24]
65 NATIONAL CRIME SURVEY, ATTITUDE SUPPLEMENT	DUALIN	September 72	9933	1	None	National[23]	None	M F	12+	-	I	Representative Probability	Geographic region population density rate of growth 1960-1970 [24]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
66 NATIONAL OPINION RESEARCH CENTER VICTIMS OF CRIMES SCREENER BY PHILLIP ENNIS	ENNIS	Summer 66	3781	1	None	National	None	M F	21+	-	I	Block quota[3]	Geographic area & metropolitan & non metropolitan, median family income, economic characteristic 25
67 PUBLIC OPINION OF CRIMINAL JUSTICE IN TEXAS	TEX74	January 16 to February 28 73[26]	749	1	None	Texas	None	M F	Adults	-	I	Probability	None
68 COLUMBIA UNIVERSITY - BEDFORD STUYVESANT STUDY ON ADDICTION RESEARCH AND TREATMENT CORPORATION	CUBS	71	612	1 of 2	Community leaders businessmen community residents	Brooklyn 2,602,012 32		M F	Adults	-	I		
69 UNIVERSITY OF PITTSBURGH CENTER FOR SOCIAL AND URBAN RESEARCH	SWPA76	October 76	373	1	Positive response to request permission to interview	Southwestern PA[27]	None	M F	Adults	-	H	Proportional Probability sample	Stage used - enumeration district, block groups

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
70 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HATL72	July to November 72	5803	1 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frame	Atlanta 497,024[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
71 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PATL72	July to November 72	9267	1 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frame	Atlanta 497,024[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
72 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HBAL72	July to November 72	5960	1 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frame	Baltimore 905,759[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
73	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PBAL72	July to November 72	10,376	1 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frame	Baltimore 905,759[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
74	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HCLE72	July to November 72	6028	1 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frame	Cleveland 751,046[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
75	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PCLE72	July to November 72	9248	1 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frame	Cleveland 751,046[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
76 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HDAL72	July to November 72	5933	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Dallas 844,189[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
77 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PDAL72	July to November 72	9472	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Dallas 844,189[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
78 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HDEN72	July to November 72	5895	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Denver 514,678[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
79 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PDEN72	July to November 72	9430	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Denver 514,678[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
80 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HNWK72	July to November 72	6037	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Newark 382,377[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
81 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PNWK72	July to November 72	9017	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Newark 382,377[32]	None	M F	16+	-	1	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
82 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HPLD72	July to November 72	5953	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Portland 381,877[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
83 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PPLD72	July to November 72	9571	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Portland 381,877[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
84 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HSTL72	July to November 72	6044	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	St. Louis 622,236[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
85 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PSTL72	July to November 72	8754	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	St. Louis 622,236[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
86 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HCHI73	January to March 73	6098	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Chicago 3,362,825[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
87 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PCHI73	January to March 73	9451	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Chicago 3,362,825[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
88 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HDET73	January to March 73	6083	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Detroit 1,511,336[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
89 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PDET73	January to March 73	9863	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Detroit 1,511,336[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
90 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HLA73	January to March 73	5984	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frame	Los Angeles 2,816,111[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
91 LEAA - NATIONAL CRIME SURVEY (CITITES ATTITUDE SUBSAMPLE)	PLA73	January to March 73	9864	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Los Angeles 2,816,111[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
92 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HNY73	January to March 73	6002	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	New York 7,894,851[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
93 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PNY73	January to March 73	9839	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	New York 7,894,851[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
94 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HPHL73	January to March 73	6094	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Philadelphia 1,948,609[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
95 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PPHL73	January to March 73	10,160	1 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Philadelphia 1,948,609[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
96 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HBOS74	January to March 74	6217	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Boston 641,053[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
97 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PBOS74	January to March 74	8998	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Boston 641,053[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
98 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HBUF74	January to March 74	5954	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Buffalo 462,783[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
99 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PBUF74	January to March 74	9646	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Buffalo 462,783[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
100	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HCIN74	January to March 74	6007	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Cincinnati 452,550[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
101	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PCIN74	January to March 74	9110	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Cincinnati 452,550[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
102	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE).	HHOU74	January to March 74	6199	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Houston 1,232,407[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
103 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PHOU74	January to March 74	9748	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Houston 1,232,407[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
104 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HMIA74	January to March 74	6070	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Miami 335,075[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
105 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PMIA74	January to March 74	9909	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Miami 335,075[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
106 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HMIL74	January to March 74	6077	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Milwaukee 717,124[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
107 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PMIL74	January to March 74	10,627	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Milwaukee 717,124[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
108 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HMIN74	January to March 74	5940	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Minneapolis 434,381[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
109 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PMIN74	January to March 74	9151	2 of 2[28]	20 percent 1979 census augmented by building permits used as sampling frames	Minneapolis 434,381[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
110 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HNOR74	January to March 74	6075	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	New Orleans 593,471[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
111 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PNOR74	January to March 74	9778	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	New Orleans 593,471[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
112	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HOAK74	January to March 74	5824	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Oakland 361,613[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
113	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	POAK74	January to March 74	8601	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Oakland 361,613[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
114	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HPIT74	January to March 74	6058	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Pittsburgh 520,167[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
115 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PPIT74	January to March 74	9992	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Pittsburgh 520,167[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
116 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HSDG74	January to March 74	5851	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	San Diego 696,566[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
117 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PSDG74	January to March 74	9521	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	San Diego 696,566[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
118 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HSFR74	January to March 74	5881	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	San Francisco 715,674[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
119 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PSFR74	January to March 74	8713	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	San Francisco 715,674[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
120 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HWDC74	January to March 74	5862	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Washington DC 756,510[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
121 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PWDC74	January to March 74	8484	2of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Washington DC 756,510[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
122 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HATL75	March to May 75	5858	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Atlanta 497,024[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
123 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PATL75	March to May 75	8731	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Atlanta 497,024[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
124 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HBAL75	March to May 75	5953	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Baltimore 905,759[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
125 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PBAL75	March to May 75	10,451	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Baltimore 905,759[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
126 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HCHI75	January to March 75	6255	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Chicago 3,362,825[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
127 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PCHI75	January to March 75	10,602	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Chicago 3,362,825[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
128 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HCLE75	March to May 75	6315	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Cleveland 751,046[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
129 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PCLE75	March to May 75	9678	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Cleveland 751,046[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
130 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HDAL75	March to May 75	6233	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Dallas 844,189[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
131 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PDAL75	March to May 75	9816	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Dallas 844,189[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
132 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HDEN75	March to May 75	6159	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Denver 514,678[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
133 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PDEN75	March to May 75	9342	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Denver 514,678[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
134 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HDET75	January to March 75	5893	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Detroit 1,511,336[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
135 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PDET75	January to March 75	9369	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Detroit 1,511,336[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
136 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HLA75	January to March 75	5973	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Los Angeles 2,816,111[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
137 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PLA75	January to March 75	9873	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Los Angeles 2,816,111[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
138 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HNY75	January to March 75	5862	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	New York 7,894,851[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
139	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PNY75	January to March 75	9638	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	New York 7,894,851[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
140	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HNWK75	March to May 75	6187	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Newark 382,377[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
141	LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PNWK75	March to May 75	9292	2 of 2^{28}	20 percent 1970 census augmented by building permits used as sampling frames	Newark 382,377[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
142 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HPHL75	January to March 75	6048	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Philadelphia 1,948,609[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
143 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PPHL75	January to March 75	10,151	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Philadelphia 1,948,609[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
144 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE).	HPLD75	March to May 75	6029	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Portland 381,877[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
145 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PPLD75	March to May 75	9455	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	Portland 381,877[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]
146 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	HSTL75	March to May 75	6410	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	St. Louis 622,236[32]	None	M F	16+	-	H	Systematically[30]	Occupied housing units and others[29]
147 LEAA - NATIONAL CRIME SURVEY (CITIES ATTITUDE SUBSAMPLE)	PSTL75	March to May 75	9281	2 of 2[28]	20 percent 1970 census augmented by building permits used as sampling frames	St. Louis 622,236[32]	None	M F	16+	-	I	Systematically[31]	Occupied housing units and others[29]

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
148	NATIONAL OPINION RESEARCH CENTER -ROPER INSTITUTE GENERAL SOCIAL SURVEY	NORC76	February to April 76	1499	1	None	National	None	M F	18+	-	I	1/2 Full Probability and 1/2 block quota[4]	Probability by size geographic region, SMSA, geographic location within area, race, income by block sex; age employment status
149	NATIONAL OPINION RESEARCH CENTER -ROPER INSTITUTE GENERAL SOCIAL SURVEY	NORC77	February to April 77	1530	1	None	National	None	M F	18+	-	I	Full Probability[4]	None
150	NATIONAL OPINION RESEARCH CENTER -ROPER INSTITUTE GENERAL SOCIAL SURVEY	NORC78	February to April 78	1532	1	None	National	None	M F	18+	-	I	Full Probability[4]	None

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criterial	Sex	Age	Race	Unit	Type	Stratified by:
151	A PROFILE OF CRIME IN GREATER EGYPT CRIMINAL JUSTICE PLANNING REGION	EGYPT	June to August 76	10,679	1	None	Southern Illinois - 15 Counties[35]	None	M F	Adults	-	.I	Random Digit Dialing Technique	Geographical area, 3 digit telephone exchanges
152	LOUIS HARRIS -- ABC TELEVISION	H2055T	January 75	400	1	None	National	None	M F	16-20	-	I	Block Quota[3]	Geographic region Size of place
153	LOUIS HARRIS -- ABC TELEVISION	H2055A	January 75	2,688	1	None	National	None	M F	21+	-	I	Block Quota[3]	Geographic region Size of place
154	LOUIS HARRIS STUDY # 7490	H7490	January 75	1,543	1	None	National	None	M F	18+	-	I	Block Quota[3]	Geographic region Size of place
155	LOUIS HARRIS STUDY # 7689	H7689	December 76	1,459	1	None	National	None	M F	18+	-	I	Block Quota[3]	Geographic region Size of place
156	PERCEPTIONS OF CRIME BY RESIDENTS OF MICHIGAN WAVE I	MICH1	September to October 72	800	1 to 2	None	Michigan	None	M F	16+	-	I	Proba-bility Propor-tionate to size	Sample based on 1970 US Census count of occupied dwelling units in Michigan

	Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
157	PERCEPTIONS OF CRIME BY RESIDENTS OF MICHIGAN WAVE II	MIC H2	January 74	900	2 of 2	None	Michigan	None	M F	16+	-	I	Probability Proportionate to size	Sample based on 1970 US Census count of occupied dwelling units in Michigan
158	FAMILY STUDY - 1976 ADULT BY YANKELOVICK, SKELLY & WHITE ROPER # 8084	FAM 76A	1976	1200	1	Interviews conducted only if a child under 13 resided in household	National	None	M F	Adults	-	I	Probability	None
159	FAMILY STUDY - 1976 - CHILDREN BY YANKELOVICK, SKELLY & WHITE ROPER #8084	FAM 76C	1976	476	1	Conducted with children of parents, who participate in primary interview	National	None	M F	6-12	-	I	Probability 1/3 Random[36]	None
160	FAMILY STUDY - 1976 - HOUSEHOLD BY YANKELOVICK, SKELLY & WHITE ROPER # 8084	FAM 76H[37]	1976	794	1	None	National	P - Adults S - Children	M F	Adults 6-12[37]	-	H	Probability 2/3 Random[38]	None

Study Name	File Name	Date Year-Month	Sample Size	Time Points	Other Population Criteria	Sample Area	Primary and Secondary Inclusion Criteria[1]	Sex	Age	Race	Unit	Type	Stratified by:
161 PORTLAND 1974[39]	POR T74	1974	4192	1	(39)	Portland SMSA 1,007,130 32	(39)	(39)	(39)	(39)	(39)	(39)	(39)
162 PORTLAND 1977[39]	POR T77	1977	1216[40]	1	(39)	Portland 381,877 32	(39)	(39)	(39)	(39)	(39)	(39)	(39)
163 POTOMAC ASSOCIATE STATE OF THE NATION 1976	GAI 976	1976	524	1	None	National	P - Youngest S - Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location
164 POTOMAC ASSOCIATE STATE OF THE NATION 1976	GBI 976	1976	547	1	None	National	P - Youngest S - Oldest	M F	18+ 18+	- -	I I	Modified Probability[2]	Size of community Regional, Geographic area, Pairs of location

FOOTNOTES

[1]Primary and secondary inclusion criteria indicate that respondents are selected in two stages. For example, AIPO studies instruct their interviewers to interview youngest male over 18 years of age, but if that person is not available they are to interview oldest female over 18 years of age.

[2]Modified Probability indicates that some minor non-probabilistic augmentation is used in the selection of respondents. This is usually done to insure representation of particularly important subgroups. Primary and Secondary Selection as in above characterizes the sampling procedure as the "modified" type.

[3]Block quota means randomly selected areas at the block level, quotas are used within blocks.

[4]Full, strict, and representative area probability are taken as equivalent.

[5]Multi-Stage Cluster -- selecting geographic areas at the minor civil division level, i.e., cities, towns, townships, with probabilities proportionate to their respective household population size.

[6]15-beat experimental area -- a police foundation defined area which does not encompass all of Kansas City. The actual population involved is being pursued.

[7]Sector of city -- essentially equivalent to a borough.

[8]Equal and Random Probability Samples will be considered equivalent.

[9]Ten cities include: (1) Atlanta, Georgia; (2) Albuquerque, New Mexico; (3) Baltimore, Maryland; (4) Boston, Massachusetts; (5) Denver, Colorado; (6) Kansas City, Kansas; (7) Kansas City, Missouri; (8) Milwaukee, Wisconsin; (9) Nashville, Tennessee; (10) San Diego, California.

[10]Fifteen cities include: (1) Baltimore; (2) Cincinnati; (3) Detroit; (4) Boston; (5) Brooklyn; (6) Chicago; (7) Cleveland; (8) Gary; (9) Newark; (10) Pittsburgh; (11) St. Louis; (12) San Francisco; (13) Washington, D.C.; (14) Milwaukee; (15) Philadelphia.

[11]The following criteria are used to define an adult: (1) adult age 21 years or older; or (2) married regardless of age; or (3) anyone who is not living with parents, or guardians.

[12]Mail Survey.

[13]Small compact geographic areas -- 620 segments defined by the University of Michigan, Survey Research Center.

[14]Fieldwork conducted by National Opinion Research Center (NORC).

[15]The twelve largest cities drawn with certainty; the rest of the country drawn with probability proportionate to size of population. Sample is representative of the entire contiguous U.S. as well as the four (4) major regions of the U.S. -- Northeast, Northcentral, South, West.

[16]Detroit area SMSA includes Wayne, Oakland and Macomb counties.

[17]Mothers of school age children (in grades 5a, 6a, 6b).

[18]One-half white, one-half black.

[19]Some items may be considered household items, while others can be considered individual items.

[20]Inner city sampled at twice the number of suburban residents to increase the number of black interviews.

[21]Census Tracts were stratified by racial composition in 1960, proportion to population size in 1960.

[22]Riot areas defined as those areas that apparently had riot related fires. Four (4) such strata were defined non-riot east, non-riot west, riot east, and riot west.

[23]Includes Alaska and Hawaii.

[24]PSU's are grouped into self-representative PSU's which are not stratified and PSU's which are grouped according to similarity in characteristics mentioned in the table.

[25]Block quota means blocks are selected using a standard multi-stage probability. Sampling within the block uses quotas based on age, sex, employment status, size.

[26]Further documentation from the University of California's State Data Program, Texas 74, was actually conducted in the year and month specified on the table. We are not making a change in the file name since that file name exists in copies of the archives of a variety of locations.

[27]Twelve counties: (1) Allegheny; (2) Armstrong; (3) Beaver; (4) Butler; (5) Cambria; (6) Fayette; (7) Greene; (8) Indiana; (9) Lawrence; (10) Somerset; (11) Washington; (12) Westmoreland.

[28]While these studies were conducted in a panel design, no information is available to make matching of cases from Wave 1 to subsequent waves possible.

[29]Stratified by income, owner or renter, family size, further stratified by race of head of household, vacant units, low value, medium value, high value.

[30]For all of the household files systematic selection of the 20 percent sample of the 1970 census was used, for further detailed information see Survey Documentation Central Cities Sample, 1975.

[31]For all the individual files systematic selection of the 20 percent sample of the 1970 census was used, for further detailed information see Survey Documentation Central Cities Sample, 1975. Enumeration within households.

[32]Population Figures are according to 1970 Bureau of Census report County and City Data Book, 1972.

[33]The Detroit Area studies conducted in the years 1968, 1969, and 1970 are studies of social change focusing on replication of items included in prior Detroit Area studies (i.e., 1953-1959). This replication represents a time series.

[34]We assume "cluster" refers to a "multi-stage-cluster" sampling technique.

[35]Fifteen counties include: (1) Alexander; (2) Franklin; (3) Gallatin; (4) Hamilton; (5) Hardin; (6) Jackson; (7) Jefferson; (8) Johnson; (9) Massac; (10) Perry; (11) Pope; (12) Pualiski; (13) Saline; (14) Union; (15) Williamson;

[36]One third (1/3) random selection of children (6-12 years) in the national sample used in study #158.

[37]We define a household record as a record containing a parent and child interview set or a double parent interview set.

[38]Two thirds (2/3) random selection of second parent in the national sample used in study #158.

[39]Block probability is the random selection of blocks with interviews in every N^{th} structure, a maximum of 3 structures per block.

[40]Completion rate is 60.8 percent.

END