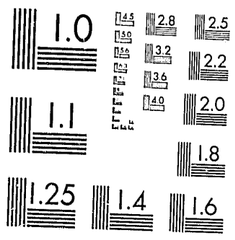

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SOURCES OF ERROR IN SURVEY DATA USED IN CRIMINAL JUSTICE EVALUATION:

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Brelby/Berk
LEAA Draft Report

I.

SURVEY DATA ON ATTITUDES ABOUT CRIME AND CRIMINAL JUSTICE

Survey Data and Criminal Justice: Uses and Shortcomings

Survey data have become and integral part of the empirical base on which many evaluation studies rest. Across a range of social interventions and public policies, researchers commonly query the people to whom services are delivered, the individuals who deliver the services, and the policy makers ultimately responsible for the content and form of services (Rossi, Berk and Eidson, 1974; Haveman and Watts, 1975; Bigelow and Ciaro, 1975). Evaluations of criminial justice programs are certainly no exception (Empey and Lubeck, 1971; Empey and Erickson, 1972; Lipton, et al., 1975; Lenihan, 1977; Chelmisky, 1977; Lewis, 1978; Regens and Hobson, 1978, Gillham et al., 1979), and if the ongoing investment in the victimization studies is a fair example (Ennis, 1967; Biderman, et al., 1967; U.S. Department of Justice, 1976; Penick and Owens, 1976; Sparks, Genn and Dodd, 1977), the use of survey data will likely continue.

The enormous commitment to survey data in criminal justice evaluations has not gone uncriticized. Building on more general concerns about survey data (Sudman and Bradburn, 1974) and the reactivity of experimental settings (Rosenthal and Rosnow, 1969; Cambell and Stanley, 1963), a number of authors have expressed a range of anxieties (Weaver and Swanson, 1975; Biderman, 1975; Schneider, 1975; Penick and Owens, 1976) about such things as normative response sets, telescoping, recall decay, interviewer-respondent biases, habituation to the interviewing instrument and outright lying.

A growing body of methodological research on victimization and incidence documents procedures for obtaining more accurate survey assessments of these behavioral events. At the same time, however, attitudinal measures are increasingly utilized in criminal justice program evaluations,

while their measurement properties are virtually unknown. For example, the lead article in a recent issues of the LEAA Newsletter (LEAA, 1979) summarizes the results of an LEAA funded survey conducted by researchers at Rensselaer Polytechnic Institute:

> Well lit streets may reduce the fear of crime,
> but there is not statistically significant evidence
> that street lighting reduces crime itself.

The investigators also found that in some areas, better lighting may actually increase crime. The article suggests that some criminals (e.g., car theives) can better see what they are doing, that more crime is reported because residents are better able to observe it, and seems to imply that unsuspecting victims are more vulnerable because they feel more secure. The attitudinal consequence of the street lighting program (reduced "fear of crime") may or may not be a desirable outcome. It depends on what the survey items are in fact measuring and on the consequences. attitudes toward crime have for both subsequent psychological well-being and exposure to the risk of victimization. Unfortunately, in the absence of sound methodological research on such subjective outcome measures, responses are simply taken at face value. The purpose of this study is to contribute to a more informed use of these and similar measures in future criminal justice evaluations.

Research Objective of Our Study

When attitudinal measures are used in evaluation studies, two assumptions are usually made. The first is that the survey report on each individual item accurately reflects something that might be called a "true" attitude or subjective state of mind of the respondent. The second is that variation in the "true" attitude is generated primarily

by factors to which it is substantively related. (For example, "fear of crime" is presumably a function of contact with and perceptions of crime but not directly related to health, weather, sex life, or baseball scores.) Of course, neither assumption follows from the other. On the one hand, we might obtain an accurate measurement of an underlying attitude that is not linked to what we think are its substantive determinants. On the other hand, an attitude may behave just as we expect, but we simply cannot measure it accurately. Unless both assumptions are valid the statistical manipulations we typically perform on attitudinal survey data are meaningless, as are the evaluative judgements of social programs that are based on the derivative statistical analyses.

Our objective here is to evaluate both assumptions, the accuracy of measurement and substantive sources of variation for thirty-three survey items of attitudes toward crime and criminal justice that were lifted verbatim from several well known general purpose surveys (e.g., NORC, Harris, Gallup) and specialized surveys on crime (e.g., the National Crime Survey, the Kansas City Preventive Patrol Experiment). In other words, we will explore the statistical properties of a number of survey items selected to represent the sorts of outcome measures commonly used in criminal justice evaluations; we will assess the quality of measures previously employed by others. The empirical characteristics of the thirty-three will be assessed with data from a seven wave panel and through estimation procedures that allow one to simultaneously model substantive relationships and the measurement processes (Joreskog and Sorbom, 1977). In particular, we will:

1. document substantive sources of variation in common survey measures which are often confounded with variation attributed to criminal justice program effects;
2. document the impact of systematic sources of measurement error;
3. estimate the amount of random measurement error; and
4. model these random components.

The thirty-three survey items are measures of nine underlying dimensions: five dimensions of fear of crime ("limit activities," "perceived increase," "fear city," "fear neighborhood," and "likelihood of victimization,") and four dimensions of respondent evaluations of the criminal justice system--("police services," "police abuses," "criminal justice effectiveness," and "criminal justice leniency"). The items and dimensions are described more fully in the chapters which follow.

Despite some superficial similarities, our efforts should be carefully distinguished from important work exploring measurement error in the National Crime Survey (Feinberg, 1977; Lehnen and Reiss, 1977). First, experience with crime is not a central concern; we focus instead on citizen evaluations of the quality of services provided by the criminal justice system. Second, even considering experience with crime, we are concerned about overall subjective assessments of such things as "safety" rather than precise figures for the amount and type of victimization. Third, the kinds of measurement error emphasized are somewhat different. Research based on the National Crime Survey has necessarily stressed the particular problems associated with its research design: biases introduced by the use of rotating panels, the accuracy of retrospective accounts covering a six month interval, and various difficulties in providing appropriate aggregate estimates of the "amount of crime." We are concerned more broadly with many different sources of measurement error by collecting and analyzing data less tied to the unique characteristics of the National Crime Survey. Finally, our interests are primarily methodological. While concerns with the National Crime Survey have centered on actual estimates of the amount of crime, we are condsidering the properties

of common criminal justice survey measures per se as used in program evaluations. Hopefully, our critical study of current measurement procedures will provide useful recommendations for better measurement technology in the future.

Conceptualizations: Sources of Error in Survey Data Used in Criminal Justice Evaluations

Broadly conceived, measurement may be approached through the mathematical properties of various quantifying devices (Krantz, et al., 1971), philosophical underpinnings (Stinchcombe and Wendt, 1975) and/or the nuts and bolts of scale construction (Guilford, 1954). For this report, however, it will suffice to describe in rather concrete terms the likely sources of variation in criminal justice survey data and some of their consequences for the assessment of criminal justice programs. And, for purposes of exposition, we will consider a respondent's "satisfaction with police services" as used in the Kansas City Preventive Patrol Experiment (Kelling, et. al., 1974).

At any point in time, satisfaction with police services may be a function of both substantive and error sources of variation. Specifically, we can identify five sources of variation:

Type I. Substantive sources of variation which are commonly measured.

- A. Amount and kind of contact with crime
- B. Amount and kind of contact with the criminal justice system
- C. Criminal justice programs
- D. Background of respondent (e.g. sex, age, race)

Type II. Substantive sources of variation that could be measured.

- A. Daily experiences having no "obvious" relations to satisfaction with police (e.g., health, employment, family relations)

- B. Attitudes perhaps related to satisfaction with police (e.g., satisfaction with the neighborhood)

Type III. Substantive sources of variation not directly measured.

- A. Substantive factors missed for one reason or another

Type IV. Systematic measurement error.

- A. Characteristics of the measurement procedure (e.g., "learning effects" in multi-wave panels, coding errors)
- B. Interaction between respondent and instrument (e.g., misunderstandings with less educated respondents, interactions of "true" satisfaction with police and reporting errors)

Type V. A. Measurement error that is orthogonal to all sources of variation listed above but over time is not necessarily independent.

In the usual consideration of such issues as satisfaction with police, researchers seem to assume that they are tapping Type I sources of variation, and that all other sources of variation are "random." Even if this is correct, the use of such "imperfect" measures as outcome (endogenous) variables will unnecessarily reduce statistical power by inflating standard errors. Should these measures be used as exogenous variables, their estimates of effect (e.g., regression coefficients) will be biased and inconsistent. And should these measures be used as "controls" for covariance adjustments, "underadjustment" (Campbell and Erlebacher, 1970; Cooley, et al., 1976) will likely result, leading to biased and inconsistent estimates of program effects (even if these are measured without error).[1]

To make matters worse, the assumption that other sources of variation are only "random" (i.e., independent of everyting else) is typically wishful thinking. If just the Type V errors are correlated with one another

over time, estimates from a panel study of both the program effect and the stability of measured satisfaction will be biased and inconsistent (Wheaton, et al., 1977). Any other sources of neglected variation that are correlated over time compound the biases.

In short, the practical reality is that most common survey measures used in criminal justice evaluations are "imperfect" to some unknown degree. Despite the pleas of some (e.g., Boruch and Gomez, 1977), the likely consequences are either ignored as unimportant or dismissed as a necessary evil. This report explores whether either assertion is justified.

Implications of Our Research Findings for Future Criminal Justice Evaluations

The research reported here should have several payoffs for criminal justice evaluations. First, we may find that by and large, common survey measures used in criminal justice evaluations have large and stable components which in fact respond to the criminal justice environment. For example, certain respondent assessments of police effectiveness may not fluctuate widely over short intervals of time except in response to exposure to crime or actions of local law enforcement officials. Validation of such measures will allow others to use them more confidently. Second, if some survey measures have better statistical properties than others, the stronger and weaker measures can be distinguished. Researchers can draw upon our evaluations of multiple indicators of the various dimensions in order to incorporate only the most reliable measures in their own subsequent efforts. Third, we may find that many popular survey items suffer from a number of serious measurement problems. In these instances we will attempt to document the likely sources of such distortions and suggest ways in which the

items may be improved. Fourth, to the degree that such difficulties exist, we can suggest reanalysis that might provide more accurate findings. Similar reassessments have been underway for some time in educational research (e.g., Magidson, 1977) and in studies fo manpower programs (e.g., Cooley, et. al., 1976). Finally, and more generally, to the extent that the research reported here is even partially successful, we will have demonstrated the power of some rather recent developments in statistical technology which may then have wide application to the criminal justice evaluation settings where measurement seems problematic. These techniques demonstrate that an accurate representation of substantive processes must simultaneously incorporate an explicit model of the research design and measurement procedures. In short, there should be an immediate payoff in terms of the actual questionnaire items considered and a long run payoff in the demonstration of the statistical procedures employed.

In summary, while we have not undertaken the definitive study of the quality of survey measures used in criminal justice evaluations, we are presenting a wide ranging, systematic examination of common questionnaire items on which many evaluations rest. By and large, these measures have gone unchallenged in the past and could clearly benefit from closer scrutiny.

NOTES TO CHAPTER I

1. Note that a randomized experiment only eliminates the problems produced by inadequate covariance adjustments. The other difficulties resulting from measurement error necessarily remain.

II.

STATISTICAL MODEL AND RESEARCH DESIGN

Our research analyzes attitudes about crime and criminal justice as reported on seven separate occasions over a 17 week period by 402 respondents in four metropolitan areas. This chapter presents: first, the specification of our formal multi-wave panel data; second, the research design for obtaining data with which to estimate the model; third, descriptive statistics on our sample respondents; and fourth, the strategy for analyses reported in subsequent chapters.

A Multi-wave Panel Model of Survey Reports of Attitudes toward Crime and Criminal Justice

The substantive and error sources of variation in measured reports obtained from criminal justice survey data can be represented by a structural equation model. In a series of equations, measured variation can be partitioned into substantive and error variation, while each component can in turn be partitioned into systematic and random components. Thus, each of the five sources of variation noted above can be rigorously specified and empirically estimated. In this chapter, we specify a model for multiple measured reports of a single dimension of "fear of crime," although the format of the model is identical for any other criminal justice measure (see Figure 2.1).

The empirical data for the model consist of:

1) X_{jk}, K measures of fear of crime obtained from respondents on each of J separate occasions;

2) $\underline{S}_j$, a vector of measured determinants of fear of crime (e.g., contact with crime, employment, other attitudes about criminal justice), also obatined on each of the J occasions;

3) $\underline{B}$, a vector of measured background variables (e.g., age, sex, race, education), obtained just once.

A structural equation model is presumed to generate the observed covariation among the/ fear of crime measures, so knowledge of the covariation allows to estimate the parameters of the model. Specifically, given the specification of the structural model, the variances and covariances among the measured variables can be expressed in terms of the model's parameters. If, in turn, each parameter can be uniquely determined from the observable variances and covariances, then the model is "identified," and the parameters can be estimated from the sample data. If some parameters are "overdetermined" (i.e., if they can be computed in more than one way from the observable covariation) then the model is "over-identified" and certain "overidentifying restrictions" must hold among the observable variances and covariances. This provides for a test of the proposed model from the sample data. If the sample covariation is/in consistent with the overidentifying restrictions, then the model must be rejected (or modified). The models we propose are typically overidentified, therefore we will be able to evaluate the internal consistency of the model as well as empirically assess the components of variation in attitude measures obtained from criminal justice surveys. Our model is presented in the following sections, and the identification issue is discussed in Appendix 1.

The Measurement Model

For any given respondent /At the jth assessment, measured variation in/the all of K reports of single, fear of crime, X_{jk}, has a/common source of "true" substantive variation, T_j, while each report has a source of error variation, e_{jk}, /Specifically, uncorrelated with T_j.

$$X_{jk} = \lambda_{jk} T_j + e_{jk} \quad (j = 1, \ldots, J; k = 1; \ldots, K) \qquad (2.1)$$

(Subscripts for individuals have been suppressed to simplify the exposition.) Parameter λ_{jk} is simply the slope of the conditional expectations of X_{jk} given T_j and therefore, is an increasing function of (positive) correlations between

the error and true components (Bielby, et al., 1977a, 1977b).[1] For each of the K measures, the error term has a systematic component corelated with error variation in the same item measured at different points in time, and a unique random error component. For example, with K = 2 measures of fear of crime, a plausible measurement error structure might be:

$$e_{j1} = \lambda_{j,j-1}\, e_{j-1,\,1} + v_{j1} \qquad (2.2)$$

$$e_{j2} = \theta_{j,j-1}\, e_{j-1,\,2} + v_{j2} \quad (j = 1, \ldots, J) \qquad (2.3)$$

where v_{jk} is the unique random error component, and the systematic component is simply a function of error in the same measure at the previous point in time.[2]

The Substantive Model

For any given respondent, /there are four types of determinants of "true" attitudes about fear of crime, T_j: a vector of measures background factors (that vary abross individuals) constant over time, $\underline{B}$; a vector of measures substantive determinants that vary over time, $\underline{S}_j$; "true" fear of crime at the previous point in time, T_{j-1}; and the cumulative impact of unmeasured substantive determinants, aggregated into a single factor, u_j. Including / term for an intercept the mean, μ_j, the "true" component can be expressed / suppressed (with subscripts for individuals as:

$$T_j = \mu_j + \underline{\alpha}_j' \underline{B} + \underline{\delta}_j' \underline{S}_j + \beta_j T_{j-1} + u_j \qquad (2.4)$$

The above equation is a substantive model of the determinants of fear of crime. Embedded within the measurement model of equations (2.1), (2.2) and (2.3), it is "purged" of the biasing influences of the pattern of response errors. Parameter β_j represents the true stability of the attitude over time parameter vector $\underline{\delta}_j$ the influence of measured attributes believed to affect

those attitudes, and parameter vector $\underline{\alpha}_j$ the effects of socioeconomic and demographic background characteristics.

Figure 2.1 presents a schematic "path diagram" of the model described for four points in time. To summarize, the measurement equations (lower portion of the diagram) allow for reporting errors that are systematic over time (e_{ij}) and random (v_{ij}),[3] and also capture potential covariation of the error and true components (λ_{ij}). Thus, we can model variation due to the measurement procedures and due to interactions of those procedures with the respondents. Estimates of parameters of the measurement equations provide a quantitative evaluation of the quality of the survey indicators and the reactivity of the measurement procedures. The substantive equation (upper portion of the model) allows us to estimate the true stability of the attitude, and the degree to which the attitude is affected by background characteristics theoretically related factors, and less obvious measurable substantive influences. Furthermore, the estimates of the substantive equation are not contaminated by the biasing effects of response error.

Maximum likelihood computer programs are available (Jöreskog and Sörbom, 1976) that allow the measurement and substantive equations to be estimated jointly or separately. The advantage of the former strategy is that it uses all the information implied by the model to obtain efficient statistical estimates. Its disadvantage is that misspecification of the substantive equation can bias estimates of the measurement equations, and vice versa. In addition to providing efficient statistical estimates, the programs allow one to statistically test whether the model successfully reproduces the measured covatiation among $\underline{B}$, $\underline{S}_j$, and the X_{jk}, and appropriate modifications of the model can be made accordingly.

Described above is a generic model; various modifications are made in both the specification and the estimation procedures when it is applied to our empirical data. These changes are introduced at several points for either substantive or pragmatic reasons (or both) and are discussed in the following chapters as each modification is imposed.

Research Design

Our initial problem was to select the survey items for assessment. There exists, of course, no sampling frame for survey questions about crime and criminal justice, and we were no more successful in finding summary discussions of the kinds of items commonly used. Consequently, we searched for references to published and unpublished studies in which crime and criminal justice were a major focus and in which survey questions provided salient outcome measures. We also consulted with colleagues in the field and professional staff at the National Institute of Justice to find out about relevant research in progress.

With approximately 20 studies in hand, we culled through their questionnaires to find survey items appropriate for study. This led to a set of about 150 survey questions from which the 33 discussed here were chosen. The 33 were chosen in a purposive fashion in which we first eliminated all but one of a set of nearly identical items. We then eliminated several questions that were either severely dated or simply incomprehensible. Finally, we selected among the remaining items in an effort to insure that a) a number of substantive domains were tapped, b) each domain was addressed by several items and c) the items from the same domain would in general terms "go together." We tried to avoid, for example, clustering and sequencing of survey questions

that would violate usual survey construction practices, that would mislead or confuse respondents, or flow in an akward fashion. The result was an amalgamated questionnaire that looked a great deal like the kind of instrument we were trying to assess.

Turning to the selection of respondents, we were far more concerned with collecting data permitting an examination of measurement quality than with collecting data from some substantive population of interes. That is, our interests were primarily methodological. Moreover, realistic budget constraints restricted the options available. It is apparent therefore, that our findings should be replicated on a larger scale and with a more diverse sample of respondents.

In order to unravel the sources of variation described earlier, we collected data on a seven-wave panel of about 400 adults. Analyzing the panel data with the statistical procedures described above, we are able to estimate the impact of a wide range of measurement artifacts. For example, we are able to gauge the effect of "learning" during multiple exposures to the survey instrument without "matched" samples of naive subjects. Similarly, we are able to determine the responsiveness of measures tapping criminal justice concerns to other events in people's lives. Indeed, with just the two waves of data it is possible to distinguish persistence over time due to correlated errors from true stability of the attitudes underlying the measured reports of fear of crime.

Our sample was designed to obtain data on approximately 100 respondents in four metropolitan area (SMSA's): Houston, Minneapolis-St. Paul, Los Angeles, and Washington, D.C. Within metropolitan areas, respondents were

stratified by ethnicity of neighborhood: predominately white, black, mixed, or hispanic. Hispanic neighborhoods were sampled only in Houston and Los Angeles. Blocks were randomly sampled within neighborhoods and households randomly sampled within blocks. Our central concern was to obtain respondents in a variety of socioeconomic circumstances. Consequently, no attempt was made to select a strictly representative sample of neighborhoods in each SMSA.

Data were collected by Audits and Surveys, Incorporated. Their field staff in the four metropolitan areas conducted the first wave of personal interviews. Responses were obtained from about 550 households in order to ensure a final sample size of 400. In the initial interviews, respondents agreeing to participate were offered a gift worth about ten dollars and a nominal gift was sent to each respondent during the final weeks of the study. Waves two through seven were administered as computer assisted telephone interviews (CATI) from Audit and Survey's New York offices. The CATI procedures minimize coding errors by directly entering responses into the computer and by allowing for checks of illegal codes and internal inconsistencies during the interview. While CATI is rapidly becoming state-of-the-art technology in opinion polling, our data should be of superior quality to that collected in previous research using more traditional methods.

The first five waves of interviews were conducted every two weeks, while waves five, six, and seven were administered over four week intervals. This arrangement allows us to examine the effect of the length of time between interviews on the measurement characteristics of responses. Interviews began in late April, 1979 and concluded in mid-September.

For the most part, our research design was successfully implemented. Five hundred thirty-two interviews were obtained in Wave 1, and 402 interviews were completed in Wave 7, for an overall attrition rate of 24 percent. The highest rate of attrition was in the Houston area, where 66 percent (N=87) of the Wave 1 respondents were interviewed in the final wave. Final sample sizes for the other three areas were 104 in Washington, 110 in Minneapolis-St. Paul, and 101 in Los Angeles.

Only 306 of our 402 respondents were interviewed in all seven waves, largely because some respondents were not interviewed while on vacation during one or more of the intervening waves. Wave 5 (administered the week of the Independence Day holiday) exhibited the highest attrition rate; only 356 of the original 532 respondents were successfully interviewed.

The median interval between the initial personal interview and the final telephone interview exceeded the intended 17 week interval by 11 days. The figures in Table 2.1 reveal that the intervals between each of the successive waves were a bit longer than planned.[4]

Various questions on crime and criminal justice were administered in each wave, and data were also obtained on personal events (health, employment, etc.) which might impact the outcome measures of interest. Demographic characteristics of respondents (age, sex, income, education, etc.) were obtained during the first wave, as well as detailed information on previous contact with crime and the criminal justice system and respondents' perceptions of the incidence of crime in their neighborhoods. A copy of Wave 1 questionnaire appears in Appendix 2. Items included in the telephone interviews were (by item number on Wave 1 questionnaire): 10-31, 33-34, and 43-64. Questions which refer to events of either the "past month" or

"past year" (item 25-31 and 33) in Wave 1 were changed to "past two weeks" and "past month" for Waves 2 through 5 and Waves 6 and 7 respectively. In an attempt to reduce attrition in the later panels, several items were omitted in the last three waves. Information on events experienced by "friend/relative outside the household" was not elicited on items 26 and 31 nor was information on "others in household" in item 25.[5] Analysis of the first two waves revealed that several of the attitudinal measures (items 12, 13, 24) were extremely unreliable (see next chapter), and these items were also omitted in the later panels.

Social Characteristics of the Respondents

Demographic characteristics of the 532 respondents interviewed in Wave 1 are summarized in the first 11 lines of column 1, Table 2.2.[6] As a consequence of our stratified sampling design, a fourth of the respondents are from each of the four metropolitan areas, and about three-quarters live in minority or integrated neighborhoods. Race of respondent was obtained in the last wave rather than the first (through an oversight in construction of the Wave 1 questionnaire), and about a third of the 402 respondents in Wave 7 were black, about 11% hispanic, and the remaining 57% predominantely white (see Table 2.3). Both the black and hispanic neighborhoods contain sizable white minorities, 18 and 13 percent of the respondents, respectively. Less than a fourth of the Wave 7 respondents in "mixed" neighborhoods and less than 5% in "white" neighborhoods were black or hispanic.

Our respondents seem, on average, to be more affluent and more highly educated than would be found in a strictly representative stratified sample. Median income (not shown in table) is just over $20,000 and only

10% of the respondents reported income of less than $6,000.

LInes 12 through 17 report means and standard deviations on Wave 1 reports of selected attitudinal items and measures of victimization. Line 12, "CRIME SERIOUS" refers to item 13 (see Appendix 2), the respondent's report of whether crime is "less serious" (coded 0), "as serious" (coded 1) or "more serious" (coded 2)"than the newspapers and TV say." Our respondents appear to express modest concern about the "seriousness of crime ($\bar{x}$ = 1.40); only eight percent report that crime is less serious than portrayed by the media. Line 13 refers to item 34a (see Appendix 2), the respondents perception of the likelihood of a break-in while they are away from home as reported on a scale from zero ("no possibility") to ten ("extremely likely"). Many respondents perceive a break-in to be more than a remote possibility (sixty-two percent report scores of at least four), although there is considerable variation within our sample ($\bar{x}$ = 4.68, s.d. = 3.18). Lines 14 and 15 are two measures of victimization and refer to indicies constructed from items 28 and 29 (see Appendix 2). They indicate respectively how many of five different property crimes and three different personal crimes the respondent had ever experienced. While most respondents (85 percent) had never experienced a personal crime (robbery, beating or sexual assault), a majority (62 percent) had experienced at least one of the five property crimes (break-in, auto theft, vandalism, major or minor burglary). Seventy-seven percent of our respondents perceive the courts to be too lenient; line 16 ($\bar{x}$ = .27) refers to item 55 (see Appendix), where "too easy" had been coded 0, "too severe" coded 2, and both "fair" and "it varies" coded 1. Finally, our respondents provide an overall favorable evaluation of police performance ($\bar{x}$ = 1.17 on item 15 in Appendix 2, for which "good" is coded 2, "fair" coded 1, and "poor" coded 0), although, again, there is considerable variability in our sample (s.d. = .71).

Thirty-five percent rated police services as "good," 47 percent "fair," and the remaining 18 percent "poor."

Column 2 of Table 2.2 provides the same statistics for the 76 percent of the original respondents who were interviewed during the final wave. Comparing columns (1) and (2) allows us to examine systematic sources of attrition. The higher attrition among respondents in the Houston area has already been noted. The Table also shows an unusually high attrition rate among hispanic respondents (43%), and those dropouts were disproportionately from the Houston area.[7] Those who remained with the study were more homogenous with respect to income, education, and age, slightly less affluent and a bit more educated, but the differences between the original 532 respondents and the "survivors" are quite small. Finally, differences on both the attitudinal and victimization measures are trivial. Except for the disproportionate attrition in the Houston area, the Wave 7 sample of 402 cases appears reasonably representative of the 532 respondents contacted during the initial interviews.

Analysis Strategy and Presentation of Statistical Results

Only 306 of the 407 respondents interviewed in the last panel were successfully interviewed in each preceeding wave. Of these, many failed to provide complete information on the 33 attitudinal measures analyzed for this report. Models will be estimated for four subsamples, each having complete data on a subset of the attitudinal measures over a subset of the seven waves. Respondents with complete data on the 17 "fear of crime" items in each of the first four waves-the FEAR-1234 subsample-will be analyzed separately. Similarly, the CRIM-1234 subsample consists of respondents with complete data on the 16 "criminal justice evaluation" items. Each subsample comprises a four wave

panel with an interval between waves of about two weeks. The other two subsamples consist of respondents with complete data on the two sets of measures for waves one, four, six and seven. Thus, the FEAR-1467 and CRIM-1467 subsamples each comprise a four wave panel with intervals between waves of approximately six weeks (between waves one and four and between waves four and six) and four weeks (between waves six and seven).

Columns three through six of Table 2.2 present social characteristics of the four analysis subsamples. Differences between the subsamples and the original sample of 532 respondents are similar to but a bit larger than those between the Wave 7 and original samples.

Chapter 3 reports the measurement characteristics of the thirty-three survey items. These characteristics are obtained from estimates of four wave measurement models for each subsample. Chapter 4 focuses on the determinants of attitudes which underlie the survey reports, taking into account both random and nonrandom sources of response error in reports of those attitudes. It includes our findings on both "fear of crime" and attitudes toward the criminal justice system. The final chapter summarizes our major findings, discusses implications for evaluating criminal justice programs, and suggests some strategies for further research on unresolved issues.

NOTES TO CHAPTER 2

1. Specifically, equation (2.1) is equivalent to a simple additive model, $X_j = T_j + e^*_{jk}$, where e^*_{jk} and T_j may be correlated. For each time period, one of the λ_{jk} must be fixed a priori in order to establish a metric for the unobservable T_j. Consequently, correlation between error and true components can only be determined relative to that in the measure that establishes the metric (Bielby, et al., 1977b: 724-727).

2. Alternatively, we can allow for less restrictive representations of the correlations among error in reports of the same measure at different points in time. The least restrictive model would allow all e_{j1} to be freely intercorrelated with all e_{j2} to be similarly intercorrelated (Wheaton, et al., 1977).

3. With more than two measures of the attitude at each point in time, it is possible under certain conditions to assess whether errors in reports of different measures obtained on the same occasion covary (i.e., correlations between v_{jk} and v_{j2}).

4. Interview dates were not subject to computer editing, so the figures on intervals between interviews are only approximate. At each wave, date of the interview was not reported for several cases, and a few cases had dates recorded which were inconsistent across waves (e.g., interview for Wave 3 completed before Wave 2).

5. Analysis of data from the early panels revealed an extremely low incidence of events referred to in these items. Furthermore, these events appeared to be unrelated to the attitudinal measures.

6. Fifteen cases with missing data on education were assigned the modal value, 12 years. Missing data on household income (58 cases) was assigned from a regression prediction based on education, employment status, sex, martial

status, city, neighborhood ethnicity, and (when available) respondent income. The open-ended income category--"over $75,000"--was coded $75,000 to minimize the effect of extreme "outliers."

7. Attrition was particularly severe among Hispanics in Houston. Only 14 of 31 (45.2 percent) were interviewed in Wave 7. In contrast, of the 34 Hispanics originally interviewed in Los Angeles, 23 (67.6 percent) were interviewed in Wave 7.

III

MEASUREMENT PROPERTIES OF SURVEY ITEMS ON CRIME AND CRIMINAL JUSTICE

This chapter introduces the 17 measures of "fear of crime" and 16 measures of attitudes toward the criminal justice system, presents estimates of a measurement model for assessing the reliablility of the items and stability of the attitudes, and evaluates the "discriminant validity" of the measures (the degree to which the various items can successfully distinguish among different attitudinal domains). A summary of the items' measurement properties is presented, emphasizing the relationship of the relatively unstable survey indicators to the remarkably stable underlying attitudes which they reflect.

Measure of "Fear of Crime"

The seventeen measures of five dimensions of "fear of crime" appear in Table 3.1, and descriptive statistics for the FEAR-1234 and FEAR-1467 subsamples appear in columns 2 through 9 of Tables 3.3 and 3.4. The first dimension, limit activities because of crime (LIM), has three indicators with the respondent, neighbors, and "people in general" as the respective referents. The respondents see their own activities as least affected by crime, while "people in general" are most affected. The mean responses exhibit virtually no change across the seven panels.

Respondents typically see crime as increasing (PI) in society in general and in their neighborhoods, they see an increasing likelihood of being victimized themselves, and they tend to view the media as understating the seriousness of crime. As in the first dimension, respondents typically find crime to be more of a problem the society in general than in their own metropolitan neighborhoods. Again, there are no detectable trends in average responses across the seventeen weeks of the study.

In each wave, about two-thirds of the respondents indicate that they avoid certain parts of the metropolitan area (FC) at night because of fear of crime, While about forty percent report that they avoid some areas during the day. Respondents typically report that their own neighborhoods (FN) are safer than others in the metropolitan area, safe for being out alone during the day, but not completely safe at night. Neither the "fear city" nor "fear neighborhood" items show any trend in typical responses across panels.

Of the five dimensions of fear of crime, only "perceived likelihood of victimization" (PV) exhibits a clear trend across panels. The likelihood of each type of victimization declines monotonically across Waves 2, 3, and 4, while the variation among respondents declines across the first four waves. Since the decline in the means occurs from late May until late June, it is unlikely that seasonal trends in crime account for the pattern of typical responses; a response bias due to repeated telephone interviewing seems more plausible. However, seasonal trends might account for the increase in perceived likelihood of victimization at the end of the summer.[1] Results from the measurement model, reported below, may explain why the variation among respondents declines across waves. If respondents become increasingly familiar with the instrument over time, the susceptibility of their responses to random measurement errors may decrease across panels. If so, this should be reflected in the measurement model by declining error variances.

Comparing the five victimizations assessed by the respondents, a break-in while no one is home isviewed as most likely, followed by the street crimes. A purse/wallet snatching and a street robbery by force are seen as about equally likely, while a physical attack is perceived as somewhat less likely. At each wave, a break-in while someone is home is seen as most unlikely.

In assessing the measurement properties of each of the seventeen items, we are less concerned with its specific content than with its ability to tap one of

five "global" dimensions: "limit activities because of crime," "perceived increase in crime," "fear of crime in the metropolitan area " "fear crime in the neighborhood," and "perceived likelihood of victimization." Nevertheless, its specific content can provide clues to its measurement properties. For example, respondents may be able to report on attitudes that refer to their personal safety more accurately than on those referring to "people in general." Further, if a specific indicator has a sizeable unique substantive component, we should detect a modest response error correlation across waves, since only part of the item's stability will be reflected in the stability of the underlying global dimension.

Measures of Attitudes toward the Criminal Justice System

The fifteen measures of attitudes toward the criminal justice system appear in Table 3.2, and descriptive statistics for CRIM-1234 and CRIM-1467 subsamples appear in columns (2) through (5) of Tables 3.6 and 3.7. There are four measures of each of four dimensions--"police services," "police abuse," "criminal justice effectiveness," and criminal justice leniency--although one item, perceptions of the proportion of crimes solved, is an indicator of both the first and third dimensions.[2] That item appears twice in the tables, since its measurement qualities are a function of the dimension it is presumed to index.

Less favorable evaluations of police services (PS) are reported on each of the four measures in the initial personal interview, and there appears to be no trend in typical responses over the subsequent telephone interviews. The less favorable evaluation may have been elicited by either the initial contact or the personal interview situation; unfortunately the two factors are completely confounded. Wave 1 interviewers resided in each of the metropolitan areas, while all telephone interviews were conducted out of Audits and Survey's New York offices. Perhaps respondents were less critical of local services when speaking to "outsiders;" the trend in the means for the "effectiveness" items are consistent with this speculation, but the pattern for the "police abuse" items

is not.

The four measures of "police abuse" (PA) show no clear trend across the seven waves. While respondents tend to disagree with the statement that the police treat all people the same regardless of race, they typically do not report that the police frisk without cause, show lack of respect, or unnecessarily rough-up people.

Criminal justice effectiveness (CE) is indicated by respondent perceptions of the proportion of crimes solved, and/proportions leading to arrest, trial, and prison. With just a few exceptions, estimated proportions / decline between Waves 2 and 7, although the mean value on each of the measures is lowest for Wave 1. It is unlikely that this pattern reflects seasonality in either the true proportions or true attitudes about effectiveness. Instead, it is probably attributable to differential response effects in personal and telephone interviewing and to conditioning of responses across repeated telephone interviews.

The preceeding discussion focused on trends in typical responses over the seventeen week period across Waves 1, 2, 3, 4, 6, and 7. The few trends that were apparent involved differences in means that spanned only a fraction of one standardard deviation of the respective measures. Overall, mean responses appear remarkably immune to the effects of repeated measurement.

The measurement characteristics reported below do not address patterns in typical responses. Instead, they index various components of inter-individual differences and their stability across waves. We shall examine the degree to which overall variation in a measure is attributable to individual differences on one of the underlying substantive dimensions (the "true" component) as opposed to random or unique individual differences (the "error" component). We shall also examine how the observed correlation of inter-individual differences across waves reflects stability in the substantive ("true") component and unique ("error")

component.

Measurement Characteristics -- Parameters of the Measurement Model

For each of the nine dimensions, the measurement equation for the k'th measure in the j'th wave is/ $X_{jk} = \lambda_{jk} T_j + e_{jk}$, as described in Chapter 2. (with subscripts for individuals suppressed) The error components of each measure are allowed to covary across waves (i.e., $Cov\ (e_{jk} e_{j'k'})$ is zero only for $k \neq k'$). The most important parameters of the measurement model are: (1) σ_{T_j} and $\sigma_{e_{jk}}$, the "true" and "error" components of variation; (2) the reliability coefficient for each measure, $\rho_{kk'}$, the proportion of total variance in the k'th item attributable to the underlying substance dimension;[3] (3) the intertemporal correlations for underlying dimension T_j,[4] particularly the correlations for adjacent waves, $\rho_{T_j, T_{j-1}}$; and (4) the intertemporal correlations for the unique or "error" component of each item, particularly error correlations for adjacent waves, $\rho_{e_{j,k} e_{j-1,k}}$. The slope coefficients, λ_{jk}, are less informative. Each is determined relative to the slope of the reference indicator selected to normalize the metric of the unobservable "true" component, so only the ratios of slopes ($\lambda_{jk}/\lambda_{jk'}$ where $k \neq k'$) are uniquely determined.

The terms "true" and "error," borrowed from classical measurement theory, must be differentiated from their conventional usage. In the absence of an objective validation criterion, the "true" component is definded by the common variation among the multiple indicators of an unobservable variable: The "error" component is the unique variation in a given indicator uncorrelated with the variation common to the multiple indicators. Note that /individual differences in the unique component might persist for two reasons. First, respondents might tend to be subject to similar random errors of measurement on different occasions/. (e.g., making the same "mistake" over successive waves) Second, individuals might differ substantively on an attribute measured uniquely by a specific questionnaire item (e.g., expressing a truly "deviant" response on one indicator over successive waves).

For each of our nine dimensions, correlations among reports of a given item across adjacent waves typically range from about .40 to .70 (see columns 22-24 in Tables 3.3, 3.4, 3.6, and 3.7). Unquestionably, many respondents change their reports on these items over a two- or four-week interval. However, if survey reports are unreliable, responses may change even though attitudes are quite stable. Alternatively, if attitudes are unstable, responses will change even when measurements are accurate. In short, survey reports may be unstable because of volatile attitudes, inaccurate measurements, or both. Unfortunately, much previous attitude research has simply accepted modest observed correlations as prima facie evidence of attitude instability (Converse, 1980).

The multiple-indicator measurement model contains parameters which capture both the covariation among unobservable attitudes and the relationship between attitudes and survey reports. For example, an intertemporal correlation among unobservable "true scores" ($\rho_{T_j,T_{j-1}}$) in excess of 190 would indicate a stable attitude regardless of the instability in survey reponses. Similarly, a reliability coefficient ($\rho_{kk'}$) lower than .50 indicates that less than half the variance in the survey report is attributable to the attitude being measured, since the coefficient is simply the ratio of "true" to total variance. The few published multiple-indicator studies of attitude surveys (e.g., Wheaton et al., 1977; Judd and Mulburn, 1980) have detected low to modest reliabilities (.60 or lower) and modest to large year-to-year stabilities (.50 to .90).

The parameters of the measurement model contain other important information about the effects of repeated measurements on the quality of survey responses. While the relative propositions of true and error variance in an item are summarized by the reliability coefficient ($\rho_{kk'}$), the trend across waves in the magnitude of the error standard deviation itself ($\sigma_{e_{j,k}}$) indexes any increase

or decrease in the quality of responses. Declining error variances across waves indicate increasingly accurate reports across waves, and, conversely, larger error variances in the later waves reflects an erosion of the quality of responses over the course of the survey. Finally, any "response set" elicited in an item should be reflected in correlations among "error" components across adjacent waves ($\rho_{e_{j-1,k},e_{j,k}}$). Specifically, the "response set" is introduced when recall of the previous response to the item causes the same unique error to be repeated from wave to wave. Statistically insignificant error correlations support the hypothesis of serially independent response errors. Large error correlations (greater than .50) would indicate difficulty in obtaining truly independent reports of an item across panels. If the "response set" is introduced by repeated measurements, we would expect error correlations to largest in the later waves.

Measurement Characteristics--Attitudes about "Fear of Crime"

Parameter estimates for measurement models of four dimensions of "fear of crime" appear in Tables 3.3 (for the FEAR-1234 subsample) and 3.4 (for the FEAR-1467 subsample). The observed correlations (columns 22 through 24) range from .33 to .82 for reports obtained two weeks apart (Table 3.3) and from .31 to .82 for measures obtained four to six weeks apart (Table 3.4). However, very little should be made of these correlations since the stability or volatility of individual items not be attributed automatically to the underlying attitudes. Random errors of measurement attenuate observed correlations, while (positive) covariation of errors across waves biases them upward. Both sources of contamination must be incorporated into the measurement model specification in order to obtain an accurate assessment of the underlying attitudes.

The dimension, "limit activities because of crime" (LIM) has an intertemporal stability of about .90 after the second wave (figures in brackets in columns 22 through 24; note that lag-1 correlations under "W6" in Table 3.4 refer to stability across <u>two</u> waves, Wave 4 to Wave 6). There is some evidence that error variances decrease (columns 10 through 13) and reliabilities increase (columns 18-21) slightly across waves. The pattern is far from overwhelming, but it suggests that the periodic surveys may solidify respondent attitudes about "limit activities," leading to increased stability in the underlying attitude and greater accuracy of the survey responses.

The remote referent, "people in general" (not assessed in the final waves), is a far less accurate indicator of the <u>underlying dimension</u> than the other two more proximate referents. Observed reports correlate only .4 to .5 with the underlying dimension.[5] Of course, the item may provide an accurate measure of an attitude <u>not</u> captured by the underlying dimension, since the "error" component of variation in respondent reports (relatively large for the "people in general"

question) is technically just an <u>item-specific</u> source of variations. But if respondent "errors" are actually reflections of a nontransient, specific attitude, they should be moderately consistent across waves. However, this is not the case, since error correlations (columns 25-27) of the more reliable "respondent" indicator are substantially larger than those for the "people in general" indicator. In short, the more generalized indicator appears not to measure anything very well.

underlying
In sum, a remarkably stable/attitude about "limiting activities be-
(as gauged by the number in brackets)
cause of crime"/is most accurately tapped by asking about the behavior of
(not "people in general")
the respondent or his or her neighbors/. Repeated measurement may contribute to solidifying respondent opinions, but the increasing error correlations across waves suggest that a modest "response set" reaction may also emerge.

The four items indicating "perceived increase in crime" (PI) have measurement characteristics similar to the indicators of "limit activities." The underlying attitude, like "limit activities" is almost perfectly stable by the end of the study (brackets, columns 22-24), and the proximate referents (respondent and--to a lesser extent--neighbor) provide the most reliable assessments. "Chances of being victimized," the most reliable indicator, is an increasingly accurate report across waves (correlating .73 with the underlying dimension by Wave 7), and exhibits no serial correlation in its error component after Wave 2. The two items with remote referents, "media" and "the U.S.," differ from the corresponding measures of "limit activities" in exhibiting substantial correlation of errors across waves (columns 25-27). In these instances, their "unreliability" may in fact reflect individual differences on a unique substantive component that persists across waves.

Not surprisingly, individuals have quite stable opinions about areas of the metropolitian area to be avoided because of crime (FC, see brackets, colums 22 through 24). Nor is it surprising that it is the existence of areas to be
(when the risk of victimization is greatest)
avoided at night/which most reliably reflects their fear (columns 10-13, and 18-21). Again, the stability of the underlying attitude increases over the course of the study, from .8 for a two-week interval to over .9 for the final four-week interval. The modest serial correlation in errors for the "day" item (columns 25-27) may reflect unique substantive content, but the larger error correlations for both measures in the last two waves probably reflects the emergence of stable item-specific "response sets" or recall biases.

In evaluating the respondent's "fear of crime" in his or her own neighborhood, (FN) it is agian fear at night which provides the most reliable assessment (columns 18 through 21). The measure comparing respondent's neighborhood with others in the area, clearly a less direct assessment, is least reliable. However, it is characterized by uniformally large serial correlation of errors throughout the study (columns 25-27), presumably reflecting a unique, stable substantive component.

Unlike the three dimensions discussed above, "fear neighborhood" exhibits remarkably high stability from the beginning of the study (brackets, columns 22-24). It may be that fear of crime in one's own neighborhood is both a stable and salient attitude, not requiring the stimuli of repeated measurements to elicit a well-formulated opinion.

Perceptions of the likelihood of the three street crimes provide the most reliable subjective assessments of the probabiliby of vicitimization (PV, see columns 10-13 and 18-21), even though a break-in with no one home is typically viewed most likely and a break-in with someone home is probably as threatening as street crime. Only the two break-in items have consistently large error correlations across waves (columns 25-27) while these correlations suggest unique "break-in" components that persist across waves for these items, they may instead (or in part) reflect recall contamination across waves. The five items require a relatively complex subjective judgement by the respondents,[6] and they are administered successively in the interview. It is possible that the initial items alone elicit recall of the "response set" from the previous wave, and consequently the latter items show little or no serial correlation of response errors.

If substance rather than recall contamination accounts for error correlations in the two "break-in" items, they should share a common substantive component within waves. To test this hypothesis, we added a single error correlation for

the break-in items and three error correlations among the "street crime" items within-wave. The hypothesis that the within wave error correlations are jointly zero is easily rejected (χ^2 = 97.55 with 12 degrees of freedom and $p < .001$ for the FEAR-1467 subsample). Nevertheless, the indicators with significant error correlations differed across waves, and the error correlations for the break-in items never exceed .18, so our test is inconclusive. For example, ... That is, the data suggest that at least some of the error correlations are non-zero, but point estimates of them fail to show a consistent pattern of "break-in" and "street crime" components in each wave.

The underlying "perceived likelihood of victimization" dimension, like "fear neighborhood," show little increase in stability after the second wave (brackets, columns 22-24).[7] Again, attitudes concerning personal well-being may be sufficiently salient so that a well-articulated attitude exists before the repeated survey measurements. Finally, "perceived likelihood of victimization" is a bit less stable across waves than the other four dimensions of "fear of crime" (although a stability of .8 is still rather large).[8] Perhaps attitudes about the risk of victimization are more strongly infludenced by changes in perceptions of the crime rate and other determinants that vary between waves; the analysis in the next chapter is designed to address this issue.

In sum, while individual items have correlations as low as .33 and typically in the range of .4 to .7, the five underlying dimensions of "fear of crime" are remarkably stable over the seventeen week period. Measures that refer to either the respondent or his or her neighborhood provide more reliable assessments than do items with less proximate referents (e.g. "people in general," or "the U.S.").

Three of the five dimensions exhibited increasing stability over the course of the study, suggesting that for some content domains the measurement process itself contributed to the construction of well-articulated attitudes. However, this process was not evident in the assessment of attitudes presumed to be more salient to the respondent: the safety of the neighborhood and the likelihood of victimization.

The stability of each dimension over the six weeks between Waves 1 and 4

was nearly as large as over the two weeks between Waves 1 and 2. (In two instances the six-week stability was actually slightly greater; compare brackets, column 22 in both tables.) Since the stabilities do not drop appreciably over longer intervals, these attitudes apparently are not transmitted over time by a Markov-type process, but instead are characterized by a short-term "memory" or "state dependence." Further evidence of this is presented in the next chapter.

For several items intertemporal correlations of unique "error" components increased over the course of the study, suggesting the crystallization of "response sets" as respondents became increasingly familiar with the survey instrument.[9] However, other "learning effects" that we anticipated did not materialize. There was no uniform tendency for the magnitude of response errors to decrease and reliabilities increase over the course of the study, although this did occur for several items. Telephone interviews did appear to elicit more accurate responses than the initial personal interviews, suggesting that the opportunities for quality control within the CATI format offset the advantages of face-to-face interviewing.

The goodness-of-fit statistics (columns 28 through 30) provide an evaluation of the adequacy of the measurement models for each dimension. The number of restrictions implied by the model equals the degrees of freedom (DF), and the chi-square statistic provides a test of the hypothesis that all restrictions are satisfied by the population moments. Acceptable goodness-of-fit statistics were obtained in at least one of the two subsamples for four of the five dimensions. However, the hypothesis that all restrictions hold must be rejected at conventional levels of statistical significance in both subsamples for the "perceived victimization" (PV) measurement model. Unfortunately, no plausible alternative model provided a satisfactory fit, so we can conclude only that there is some distortion in our representation of the measurement process for the PV dimension.[10]

Each measurement model discussed above is a representation of the covariation

among indicators of a single dimension across panels of the survey. The "discriminant validity" of these indicators (their ability to discriminate among empirically distinct underlying dimensions) cannot be evaluated from such models. To examine whether the seventeen items do indeed measure five distinct dimensions, we estimated measurement models for all items <u>within</u> each wave, across dimensions.[11] Table 3.5 presents zero-order correlations among the five <u>underlying</u> dimensions for the FEAR-1467 subsamples.

The correlations clearly demonstrate that the dimensions are empirically distinct. More than half the variance is shared between dimensions only for "fear neighborhood" (FN) and "perceived victimization" (PV) in Wave 7 ($.77^2 = .59$). However, there is a tendency for the dimensions to become more highly correlated across the course of the study, and this could reflect either a crystallized orientation toward "fear of crime" as a result of participation in the survey or simply a tendency to overstate consistency across all-too-familiar items by the seventh assessment in seventeen weeks. Finally, the disappointing chi-square statistics for Waves 1, 4, and 7 are probably due to poor fit among the five "perceived victimization" items, since the fit for Wave 6 (where they are absent) is quite satisfactory.

<u>Measurement Characteristics--Attitudes about the Criminal Justice System</u>

Measurement characteristics of the criminal justice items appear in Tables 3.6 and 3.7. Like the "fear" dimensions, the attitudes <u>underlying</u> the criminal justice items are quite stable; indeed, three of the four dimensions are almost perfectly stable by the end of the study (brackets, columns 22-24).

Attitudes toward "police service" (PS) are most reliably assessed by the two general evaluative items on police performance and protection (columns 18-21). One measure, "police response time," is nearly as reliable, but the other, "percent of crimes solved" is barely responsive to "true" variation in the underlying dimension. The latter item is a direct measure of the "effectiveness"

(CE) dimension as well, which should account for the unusually large stability of its unique "error" component (columns 23-25) across waves. All four indicators have modest to large serially correlated errors by the last two waves, suggesting again that recall contamination becomes increasingly important as the measurement process is repeated over the seven panels.

Three of the four measures of "police abuse" (PA)--"frisk without cause," "show no respect," and "unnecessarily rough-up" are each quite reliable assessments of the underlying dimension (ρ_{ii}, of .70 or greater after Wave 3.). The item referring to "equal treatment regardless of race" apparently taps a different aspect of perceived police abuse, since its reliablity never exceeds .10, while the intertemporal stability of its unique "error" component, as high as .7 in the last two waves, greatly exceed that of the other three items.

"Criminal justice effectiveness" (CE) is the only underlying dimension that does not approach perfect stability by the end of the study (brackets, Columns 22 through 24), but an intertemporal stability of .92 in the final wave hardly suggests a "volatile" attitude. "Percent of crimes solved," the indicator of "effectiveness" which also measures "police service," is the least reliable measure of both the CE and PS dimensions (columns 18-21).[12] Variation due to the "police service" dimension presumably accounts for the item's stability in its unique "error" component as a measure of effectiveness (columns 25-27). About forty percent of the variance in the "percent solved" item was attributable to the CE and PS dimensions together in each of the within-wave multidimensional models. But as study progressed, the item became increasingly sensitive to the "effectiveness" dimension and a less reliable indicator of "police service."

It is not immediately apparent why "percent arrests" and "percent trial" should be the most "accurate" indicators of the "effectiveness" dimension (columns 10-13 and 18-21). Indeed, it could be argued a priori that perceived conviction and incarceration rates should be most salient to respondents' concerns

about crime and criminal justice. The four items which appear sequentially on the questionnaire refer to events which occur in the same sequence: solve--arrest--trial--prison. But the occurrence of each event is conditional on the outcome of the preceeding event (e.g. a trial cannot take place if there is no arrest), and a similar conditional dependence might structure individual responses. Unfortunately, the measurement model does not incorporate such dependence; it simply represents what is common to the four items. Thus, it is not surprising that perceptions about the two intermediate events most accurately capture whatever common response is elicited by all four, even though the measurement model itself might be a less than accurate portrayal of the measurement process. Indeed, the relatively large goodness-of-fit statistics in both subsamples for the "effectiveness" model are evidence of some misspecification (columsn 28-30).

Attitudes about leniency in the criminal justice system (CL) are measured by items referring to the courts, parole boards, the Supreme Court, and the law. The first two (with possibly less remote referents) provide more accurate assessments, although the reliabilities never exceed .60 (columns 18-21). Responses about the Supreme Court and the law have reliabilities which typically range from about .20 to .30, while all items but the first have moderately stable unique "error" components (columns 25-27). While no one item is an overwhelmingly accurate measure of the "leniency" attitude, the underlying dimension iteself is remarkably stable across the course of the study (brackets, columns 21-24).

Table 3.8 shows that the four dimensions of attitudes toward the criminal justice system are even more distinct than the five "fear" dimensions. Apart from "police service" and "police abuse" which consistently correlate about -.66, the dimensions are virtually mutually orthogonal (including the two which share an indicator), and there is no tendency for the dimensions to become less distinct during the course of the survey.

To summarize, the "criminal justice" dimensions, like the "fear" dimensions,

are remarkably stable, particularly during the final waves. With few exceptions, more accurate assessments were elicited in the computer-assisted telephone interviews than in the initial personal interviews, although this may be attributable in part to the order in which the instruments were presented (personal before CATI). Further, there is no uniform tendency to elicit more reliable responses in the later panels. The "criminal justice" dimensions are less highly intercorrelated than the "fear" dimensions, and unlike the latter they show no evidence of becoming less distinct in the last panels. Similarly, there is less evidence that the unique "error" components of the individual items become more stable at the end of the study. Consequently, there is little evidence of "response sets" or other forms of recall contamination emerging after repeated measurements. In short, the "criminal justice" items appear "cleaner" than the "fear" measures (even if not always as reliable). However, this interpretation is subject to one important qualification: the "criminal Justice" measurement models typically have less adequate "goodness-of-fit" statistics, so the individual items may be subject to sources of systematic bias not captured by the models.

Conclusion

At the onset of the study, our speculation was that attitudes toward crime are: 1) like all subjective phenomena, very difficult to measure; and 2) quite volatile, even if they can be measured. Thus, alanyses that take responses to public opinion surveys at face value would be quite misleading, since variation in a poor measure of a transient phenomenon is largely noise.

Although our findings emphatically indicate that survey responses should not be taken at face value, they have not specifically confirmed our speculations. First, we find that one can measure subjective "attitudes" about a topic if these "attitudes" are defined operationally as the common component of survey items

eliciting respondent opinion on that topic. Second, we find that underlying dimensions or "attitudes" toward five dimensions of "fear of crime" and four dimensions of "criminal justice" are remarkably stable, often correlating .90 or higher over a two- to six-week interval. Third, we find some items are quite sensitive to underlying "attitudes," with reliabilities of .80 or higher, while variation in other items is largely orthogonal to the underlying dimension which they are presumed to index. Fourth, we find instances in which both measurement characteristics of survey indicators and the underlying attitude itself are affected by the measurement process. For example, repeated measurements may not only improve reliability of the responses while increasing their susceptibility to recall contamination; they may also increase the stability and coherence of the underlying attitude.

Most importantly, our findings confirm the concerns which motivated the study: that disentangling measurement artifacts from substantive variation requires an explicit model of the research design and measurement process. This concern is just as important in the following chapter, where we investigate the substantive determinants of attitudes toward crime and criminal justice. There, we focus on what determines individual variation on the nine underlying dimensions, and each analysis is imbedded within one of the measurement models presented in this chapter.

Finally, even before discussing the analyses presented in the next chapter, there are several specific recommendations that can be extracted from our findings. First, it is extremely unlikely that outcome measures in evaluation studies based on single survey items will be able to distinguish the signal from the noise. In other words, single items of an attitudinal nature will almost certainly be too unreliable to yield accurate measures of program effects.

Second, survey outcome measures should be tailored as closely as possible to the particular treatments being evaluated. Global assessments will typically be next to useless. Thus, it is far better to ask respondents, for example, if police "usually read people their rights" than to ask whether the criminal justice system is properly responsive to due process concerns.

Third, it is apparent that at least for the kinds of outcome measures we have considered, several different and nearly orthogonal, substantive domains are being tapped. Thus, the more usual concept of validity surfaces: one needs to be concerned with whether one's survey items are really measuring what they are intended to measure. In particular, survey items need to be designed with specific program effects in mind. For example, programs aimed at reducing street crime in particular neighborhoods should not be evaluated with survey questions about the city as a whole.

Fourth, our data suggest that the quality of survey-based outcome measures might be improved if some time were taken to "educate" respondents. Recall that there is some evidence that with each wave, underlying attitudes often become more stable (beyond the role of correlated errors across waves). This implies that efforts to help respondents think about the issues before a questionnaire is administered might improve measurement quality. This could perhaps be done with a brief discussion of the overall issues, some "warm up" questions introduced as "examples" of the kinds of items that were going to be employed, or "throw-away" questions early in the questionnaire.

Fifth, there is no evidence that interviews undertaken through CATI approaches produce data of lower quality; if anything, data of higher quality result. This means that researchers are apparently free to capitalize in the lower per-unit

costs of telephone interviews enhanced by CATI technology. Note, however, that the comparison is between face-to-face interviews and phone interviews coupled with CATI procedures. A comparison between face-to-face interviews and phone interviews without CATI procedures has not been undertaken here.

Sixth and last, when evaluation studies are planned in which survey outcome measures will play a salient role, researchers might well anticipate the need to apply the kinds of statistical procedures we have employed here. The use of multiple indicators, for example, will be of little use unless the information they provide can be formally integrated into one's results. This implies the need not only to become familiar with the relevant statistical technology, but also the need to think through in advance the required substantive and measurement models. In the case of the latter, for instance, it may turn out that one may only need to rely on two (good) indicators of some underlying attitude. Consequently, questionnaire space that might have been allocated to obtaining additional indicators can be put to other uses. In short, there should be at least two productive consequences; the statistical procedures will enhance the survey analysis once the data are on hand, and anticipation of the statistical procedures should help one design a more effective and efficient questionnaire.

NOTES TO CHAPTER III

1. Unfortunately, the Wave 5 and Wave 6 responses to the "perceived victimization" items were lost through a processing errors in the computer-assisted telephone interviewing. If the responses can be recovered, we will be able to provide a more definitive assessment of the time trends in mean responses.

2. Perceptions of the proportions of crimes which lead to arrest were also included as measures of police services in preliminary measurement models, but the item had virtually no relationship to the underlying dimension.

3. The reliability coefficient $\rho_{kk'}$, is equal to $\lambda^2_{jk}\sigma^2_{T_j}/\sigma^2_{X_{jk}}$ which is identical to $1 - (\sigma^2_{e_{jk}}/\sigma^2_{X_{jk}})$, since $\sigma^2_{X_{jk}} = \lambda^2_{jk}\sigma^2_{T_y} + \sigma^2_{e_{jk}}$. It is also equal to the square of the bivariate correlation between the observed measure and underlying true component.

4. Structural equations models which account for the covariation among the T_j are presented in Chapter 4. A single determinant of T_j (gender) was included in the measurement model in order to identify correlations among e_{jk} (see Appendix 1).

5. The correlation between the underlying trait and the observed report equals the square root of the reliability coefficient.

6. Consequently, it is not surprising that error variation (columns 10 - 13) appears to decline across waves, particularly between Waves 1 and 2, as respondents become familiar with the items.

7. Note, however, that the "W7" entry (columns 24) in Table 3.4 refers to stability between Waves 4 and 7, a <u>ten</u> week interval.

8. "Perceived likelihood of victimization" also appears to be the only dimension for which a trend emerges in the amount of "true" variation (brackets, columns 6-9). "True" variation seems to increase over the course of the study; that is, individual perceptions seem more differentiated at the end of the study. However, while error variances, reliabiliby (proportion of observed variance attrubutable to the "true" component), and the ratio of

"true score" slopes are all invariant with respect to the indicator chosen to normalize the metric of the unobservable dimensions, the <u>absolute</u> amount of "true" variation (and its trend across waves) does depend on the specific normalization imposed. Consequently, extimates of σ^2_T are less informative than one might expect.

9. There is no necessary or tautological connection between increasing stability and increasing serial correlation of errors. Indeed, we would expect only the latter in a situation where an emerging "response set" completely accounted for the greated consistency of responses across the final waves.

10. The poor fit may be due to small departures from many of the restrictions or large departures from just a few restrictions. We suspect the former, since no single plausible alternative provided an adequate fit.

11. Two error correlations were specified: for the "day" and "night" items respectively across the "fear city" and "fear neighborhood" dimensions.

12. We specified within-wave multidimensional models when both the "percent solved" and "percent arrested" items were measures of the two dimensions, PS and CE, but only "percent solved" had a significant relationship to the "police service dimension."

IV

DETERMINANTS OF ATTITUDES TOWARD CRIME AND CRIMINAL JUSTICE

The previous chapter assessed the measurement properties of 32 survey items which reflect nine underlying dimensions or attitudes about crime and criminal justice. It also examined the stability of individual differences on the underlying attitudes over a seventeen week period. This chapter examines the substantive sources of variation in those unobservable attitudes that are measured indirectly through the survey items. While only estimates for substantive equations (see eq. 2.4) are reported here, each is embedded within a measurement model identical to one reported in Chapter III.[1]

The remarkably high stabilities of the underlying attitudes that were detected in the measurement model modified our expectations about those attitudes' substantive sources of variation. Attitudes which approach perfect stability over a two to four week period are probably not very sensitive to the occurrence of daily life events that are unrelated to crime. Consequently, we devoted less attention to refining that part of our substantive model than we had originally planned. Further, since individual differences in underlying attitudes changed little during the course of the survey, both socioeconomic background traits and prior contact with crime should effect attitudes in the second and subsequent waves primarily through their influence on initial (Wave 1) attitudes. As a result, the substantively interesting findings should come from the reduced form equations, since they document the total effects of background and prior contact on later attitudes as mediated by earlier attitudes. Finally, since there is so little change in attitudes between adjacent waves, we present results of a substantive model for attitudes in Waves 1, 4, and 7 -- separated by median intervals of 49 and 74 days respectively (see Table 2.1). Models

that we have estimated for adjacent waves contain virtually no information in their structural forms (apart from the fact that Wave "T" attitude is almost exclusively a function of attitude at Wave "T-1") while the reduced forms of those models simply confirm the findings we present below.

Measures of Substantive Sources of Variation

The exogenous background variables (age, gender, city, ethnicity of neighborhood, income, and education) are described fully in Chapter II. Six measures of prior contact with crime and criminal justice are described in Table 4.1:

1) victimization by five different property crimes;
2) victimization by three different personal crimes;
3) extent of violent crime in the neighborhood during the past year;
4) victimization of others in the household over the past year;
5) receiving police assistance during the past year; and
6) prosecution of respondent by the criminal justice system over the past year.

The four crime variables refer to subjective reports of the number of different kinds of offenses that had been committed, not the incidence or absolute number of each. Respondent reports of incidence were included in preliminary models but had no significant effects net of the measures described in Table 4.1.

Daily life events were divided into four categories: "minor bad" (e.g., illness, arguement), "minor good"(e.g., good trip, received gift), "major bad"(fired, separated), and "major good"(e.g., married, received raise). Within each category, the number of such events that had occured since the previous interview were summed (see Table 4.1). Our

hypothesis was that individuals experiencing favorable events and fewer unpleasant events would report less fear of crime and less critical attitudes toward the criminal justice system. In short, we speculated that attitudes toward crime and criminal justice may simply reflect (at least in part) satisfaction or dissatisfaction with life in general.

Finally, our model included two subjective reports of respondent victimization and neighborhood crime during the interval between interviews. Again, each measure refers to the number of different kinds of crimes committed, not actual incidence.

Our recursive three equation substantive model is as follows:

1) Wave 1 (unobservable) attitude is directly affected by ten exogenous socioeconomic attributes and six measures of prior contact with crime and criminal justice;

2) Wave 4 attitude is directly affected by Wave 1 attitude, socioeconomic attributes, prior contact, life events during the previous two weeks, and perceived victimization and neighborhood crime during the previous two weeks;

3) Wave 7/is directly affected by Wave 4 attitude, all of the latter's determinants, life events during the previous four weeks, and perceived victimization and neighborhood crime during the previous four weeks.[2]

attitude

Reduced Form: Socioeconomic Differences in Attitudes Toward Crime and Criminal Justice

Reduced form coefficients are reported in Tables 4.2 and 4.3. To facilitate comparisons across outcomes, the unobservable attitudes have been converted to a standardized metric, so each coefficient represents an expected attitude difference in standard deviation units for individuals one (metric) unit apart on a predetermined variable. The LISREL IV maximum likelihood estimation procedure does not provide standard errors for reduced form coefficients, so there is no way to directly gauge the sampling variability of the estimates in Tables 4.2 and 4.3. However, estimates from

the structural form suggest that coefficeints for binary variables (gender, city, and neighborhood ethnicity) greater than .35, age and income effets greater than .010, and education coefficients greater than .090 in magnitude typically have t-ratios in excess of 2.0. Furthermore, it is unlikely that substantively meaningful effects which are replicable across waves and outcome variables are artifacts of sampling variability.

Table 4.2 reveals consistently large effects of gender, city, and neighborhood ethnicity on the five dimensions of "fear of crime." Male attitudes on "fear city," "fear neighborhood," and "perceived likelihood of victimization" are typically about one-half standard deviation below those of females, while gender differences are nearly as large for "limit activities" and "perceived increase." In other words, men are apparently less concerned about being victimized. / Consistent city differences appear on each dimension except "fear city": residents of the Houston and Los Angeles metropolitan areas tend to hold attitudes substantially higher/on (more fearful) the four dimensions than do residents of the Minneapolis and Washington D.C. areas (note that Los Angeles, the omitted category, has an implicit coefficient of zero). The city differences in attitudes are particularly large on the "limit activities" and "perceived increase" dimensions, spanning a range of nearly one standard deviation between the two seemingly crime conscious "sun belt" cities and the two other areas. However, the correlation between attitudes and latitude does not hold for the dimension "fear.. areas of the city." Washington area residents apparently recognize particularly dangerous parts of their city (making them similar to Houston's respondents) while our Minnesota and Southern California respondents are less likely to make such distinctions. Thus, Los Angeles area residents tend to feel vulnerable in their own neighborhoods but seem not to strongly differentiate across neighborhoods, while in contrast, Washington area residents

tend to feel relatively secure in their own neighborhoods but avoid particular parts of the city.

Attitudes about fear of crime are quite sharply differentiated between minority and white neighborhoods on two dimensions: "fear neighborhood" and "perceived likelihood of victimization." Residents of Hispanic neighborhoods feel particularly vulnerable; typical attitudes about fear of crime in those neighborhoods are more than a standard deviation above those expressed in white areas. Interestingly, at the initial interview, residents of white neighborhoods typically perceived more increase in crime (PI), but the ethnicity differences on this dimension are attenuated in the subsequent panels.

Both age and income effects were consistent across dimensions of "fear of crime," but in every case the effects were substantively negligible. Older respondents seem to be more fearful on each dimension, but the effects are typically no more than a fourth of a standard deviation for fifty years of age (i.e., a coefficient of .005 in Table 4.2). Similarly, high income respondents are typically less fearful, but in only one instance does the effect exceed a third of a standard deviation for $50,000 of income (i.e., a coefficient of -.007 in Table 4.2). The effects of education are neither consistently large nor consistent in direction.

Gender, city, and ethnicity differences may simply reflect differences among various groups in their contact with crime. If so, these differences should be sharply reduced once the six measures of prior contact with crime are controlled. Table 4.4 contrasts the total and direct effects of socioeconomic characteristics on initial (Wave 1) attitudes, showing that this is definitely not the case. While introducing the six measures of prior contact increases the coefficient of determination (R^2) by anywhere from .05 (for "fear areas of city") to .10 (for "limit activities"), prior contact does not mediate the effects of gender, city, and neighborhood ethnicity.

Comparing total and direct effects, most are reduced very little, a few are increased, and none are reduced by even 50 percent when prior contact is controlled.

These results suggest: 1) that our respondents hold quite stable attitudes about "fear of crime" which tend to be gender-, city-,and neighborhood-specific; and 2) that there are differentiated "climates of fear" among these subgroups which cannot be attributable to differential contact with crime. Nevertheless, consensus is far from complete within subgroups. Reduced form coefficients of determination (R^2) range from just .14 ("fear areas of city," Waves 1 and 7) to .35 ("fear neighborhood," Wave 1), indicating considerable "residual" diversity within subgroups with identical background characteristics.

The relatively low R^2's are all the more perplexing given the wide variety of individual variables included, the use of dummy variables to capture effects associated with the ethnicity of neighborhoods and city differences, and the corrections for unreliability and other measurement problems. With 20-20 hindsight there appear to be at least three kinds of exogenous variables that we failed to consider: particular physical characteristics of the environment in which people live and work (e.g., whether mass transit stops are above ground or below), the social characteristics of the immediate environment (e.g., whether teenage gangs are active) and the ability respondents believe they have to cope with crime (.e.g., whether they feel they are "street wise"). There is, of course, no guarantee that variables from these domains would "work," but in retrospect they are good candidates.

With a few exceptions, Tables 4.3 and 4.5 reveal a similar pattern with respect to the four dimensions of attitudes toward criminal justice. While women are more fearful than men about crime, gender differences in attitudes toward criminal justice are rather small. Men see slightly less effectiveness in the criminal justice system and are a bit less critical of the system's leniency (low scores indicate a more critical attitude). The "sun belt effect" persists in evaluation of police performance, with Houston and Los Angeles area respondents providing less favorable evaluation of police service[3] and responding more critically about police abuses. The police service evaluations span roughly one third standard deviation across cities, but the police abuse evaluations differ by more than one-half standard deviation in each wave (again, the Los Angeles area has an implicit coefficient of zero). By Wave 7, however, Houston area evaluations of police abuse are roughly comparable to those in the Washington and Minneapolis regions (holding constant other background variables), while Los Angeles area police still receive relatively critical evaluations by Wave 7. This may reflect in part a high incidence of shootings of police in several Los Angeles jurisdictions which received considerable media attention during the summer of 1979.

City differences in perceptions of effectiveness are not large; there is a slight tendency for residents of the Washington and Los Angeles areas to see greater efficacy. While Los Angeles area respondents are

significantly more critical of criminal justice leniency at the beginning of the survey (roughly a half standard deviation apart from the other three areas), city differences on this dimension virtually disappear by the end of the study.

Neighborhood ethnicity differentiates attitudes toward criminal justice even more than it does "fear of crime." Respondents in black and Hispanic neighborhoods provide evaluations of police roughly one standard deviation less favorable than residents of white neighborhoods on both the "service" and "abuse" dimensions./ Ethnicity differences are almost as large on the "leniency" dimension, with respondents in white neighborhoods holding more critical attitudes. Finally, respondents of black, hispanic, and mixed neighborhoods each evaluate criminal justice effectiveness about one-half standard deviation more favorable than those in white neighborhoods. In short, there is a sharp ethnic cleavage in which aspects of the criminal justice system respondents evaluate negatively. Residents of minority neighborhoods are dissatisfied with police service and are critical of police abuses, while those in white neighborhoods are critical of the leniency of the system and its efficacy in apprehending and convicting criminals.

This replicates, of course, much earlier work (e.g., Rossi, Berk and Eidson, 1974).

Age, education, and income have larger and more consistent effects on criminal justice attitudes than on the five dimensions of "fear of crime." Older respondents provide more favorable evaluations of police (on both the "service" and "abuse" dimensions) and hold more critical views on leniency in the criminal justice system. These effects are typically greater than a third of a standard deviation for a difference of 30 years of age. Highly educated respondents provide more favorable evaluations of "police service" and are somewhat less critical of leniency in the

criminal justice system. Finally, income has small but consistent effects on three of the four dimensions, with affluent respondents slightly more critical of "police service" and both leniency and efficacy of the criminal justice systems.

Again, comparing total (reduced form) and direct (structural form) effects of the ten exogenous variables on initial attitudes provides an assessment of the degree to which differential prior contact with crime accounts for the socioeconomic differences on the four attitudes toward the criminal justice system. Table 4.5 clearly shows that prior contact does not mediate the total effects of socioeconomic background. Indeed, the effects of neighborhood ethnicity, age, and education typically increase when the six measures of prior contact are controlled.

Thus, like "fear of crime," attitudes toward the criminal justice system are rather sharply differentiated by socioeconomic groupings (particularly neighborhood ethnicity) in a manner which cannot be attributed to perceived contact with crime. But again, consensus on the (unobservable) attitudes is far from complete. The ten background variables account for no more than a third of the variance (see R^2 entries in Table 4.3), so there remains considerable diversity of opinion among those with identical characteristics.

Again, the R^2's seem quite low given the corrections for measurement error and the wide variety of causal variables included. However, in this case it is a bit more difficult to think of important exogenous variables we have missed. Perhaps the major omission is an absence of variables tapping what respondents expect from the criminal justice system. Other work (e.g., Berk and Rossi, 1977) clearly indicates that there is considerable variation in people's hopes and expectations for the criminal justice system, and it

seems reasonable that the kinds of assessments we have measured imply an underlying yardstick that we have neglected. Whether the courts are too lenient, for instance, depends on one's a priori standards.

Structural Form: Contact With Crime, Life Events, and Attitudes Toward Crime and Criminal Justice

Table 4.6 presents estimated structural coefficients which have t-ratios of at least 2.0. The net effects of prior contact with crime, reported in the first six lines of the table, are far from overwhelming. Only 16 of the 162 coefficients meet the significance criterion, and 8 are expected to meet the criterion simply by chance. Nevertheless, the

seven "significant" effects on initial (Wave 1) attitudes are consistent in direction, and several are relatively large in magnitude. In every case, prior contact leads to more "fearful" stances on the five "fear" dimensions and more critical evaluations on the four "criminal justice" dimensions. Two variables affect initial levels of three separate dimensions. Respondents reporting more neighborhood crime over the past year are less secure on the "limit activities," "fear neighborhood," and "perceived likelihood of victimization" dimensions, and those who reported having received police assistance during the year are also typically less secure on "limit activities" and "fear neighborhood" and are more critical of "police service" in the initial interview. Respondents in households where others have been victimized over the past year perceive greater increase in crime (over a fourth standard deviation for each victimization) and are more critical of leniency in the criminal justice system (over a third of a standard deviation for each victimization). Nevertheless not one "prior contact" variable <u>directly</u> affects any given dimension on more than one occasion. In short, while the significant effects are reasonable, they pale in comparison to the effects reported immediately above.

Of the 72 coefficents representing the effects of Wave 4 life events on Wave 4 and Wave 7 attitudes, 10 satisfy our significance criterion (4 should by chance alone) and 7 of them are consistent with our hypothesis that when things are going well, respondents are both less fearful of crime and less critical of the criminal justice system. "Major good" events at Wave 4 have the largest and most consistent effects, leading to more secure attitudes with respect to "limit activities" (Wave 7), "fear city" (Wave 4) and "fear neighborhood" (Wave 7), and contributing to a less critical stance on criminal justice leniency (Wave 4). Again, however, the persuasiveness of the findings is tempered

by the fact that five of the ten significant coefficients represent effects of events reported in Wave 4 on attitudes held in Wave 7, <u>not</u> Wave 4.

Furthermore, the pattern is not replicated for Wave 7 life events. Six of those 36 coefficients meet the significance criterion (two should by chance), and four of the six are consistent with our hypothesis. However, "major good" events at Wave 7 have only one significant effect (attenuating "leniency" critiques), while "minor bad" events have the most consistent effect, increasing fear on the "limit activities," "perceived increase," and "perceived likelihood of victimization," dimensions. In short, the hypothesis that life events exhibit none of the predicted effects must be rejected. There seems to be <u>some</u> "spillover" of satisfaction with daily events on attitudes toward crime and criminal justice. However, the influences that are detected are modest and not nearly consistent enough to strongly support our original hypothesis.

Victimization in the two weeks prior to Wave 4 substantially increases fear in both the city and neighborhood at Wave 4, yet comparable effects are not detected in Wave 7. It may be that by Wave 7, attitudes have stablized so much that not even direct personal confrontation with crime can alter them. Similarly, while perceptions of increased crime at Wave 4 contributes to both attitudes about "limiting activities" and criticism of "police service," crime perceptions at Wave 7 only contribute to criticism of criminal justice leniency.

The meager findings on prior contact, life events, and contemporaneous victimization and crime perceptions are consistent with the remarkable stability in attitudes toward crime and criminal justice detected in the previous chapter. If anything, the estimates in Table 4.6 are biased in

favor of finding effects of the eighteen endogenous variables. Models we have estimated which control Wave 3 and Wave 6 attitudes show virtually no effects of the endogenous variables on Wave 4 and Wave 7 attitudes.

Table 4.6 also presents the direct effect of Wave 1 attitude on Wave 4 attitude and the effect of both on Wave 7 attitude. On seven of the nine dimensions, the Wave 4-Wave 7 link is stronger than the Wave 1-Wave 4 stability, even though the interval between measurements is almost twice as long. For each dimension except "perceived increase," there is almost one-to-one correspondence between individual differences in Wave 4 attitudes and differences in Wave 7 attitudes (see metric coefficients in parentheses). Again, this supports the speculations of the previous chapter that attitudes became more crystallized and stabilized as the study progressed. In effect, the process of repeated measurement appears to have both sensitized and desensitized respondents. It sensitized them to the extent that it contributed to the formulation of coherent stable attitudes about nine dimensions of crime and criminal justice. It desensitized them to the extent that attitudes became so stable that not even direct contact with crime altered their opinions and evaluations.

Conclusion

The analyses reported in this chapter revealed that socioeconomic attributes, particularly neighborhood ethnicity, gender, and city, sharply differentiated respondents' attitudes toward crime and criminal justice. Further, they demonstrated that socioeconomic cleavages were not attributable to subjective reports of prior contact with crime. Analyses here and in the previous chapter demonstrated that initial differences in unobservable attitudes were quite stable and became increasingly stable over the course of the study. Finally, the analyses

reported here failed to detect evidence that either daily life events, victimization, and/or perceptions of the extent of crime locally or substantially alter respondents' attitudes.

We are confident that our analyses provide a more definitive assessment of both the measurement properties of popular survey items and the substantive properties of the underlying attitudes they are presumed to reflect than has heretofore been available. Once again, though, we stress that this has been possible only because we have been able to simultaneously model measurement and substantive processes. Conventional regression analyses of the individual survey items would yield results strikingly different from those reported here. Such analyses would be dwarfed by measurement errors, and attitudes toward crime and criminal justice would have appeared relatively volatile and virtually unaffected by socioeconomic differences among respondents. Our findings underscore the importance of maintaining the distinction between attitudes and its operationalization not only in our theoretical musings but in our statistical models as well.

Finally, the results we have just reported do not bode well for the usual kinds of survey measures used in criminal justice evaluations. What we have found is that the underlying dimensions reflected in common survey items are extremely stable even in the face of important events to which they should respond: victimization, contact with the criminal justice system and the like. Thus, it is very unlikely that these underlying dimensions will be sensitive to changes in criminal justice policies. In other words, evaluations of criminal justice programs in which citizen attitudes and assessments are taken as important outcome measures will be "biased" toward null findings. In some sense, we have uncovered the worst of all possible worlds: individual survey

items are likely to be too volatile to tap program effects while the underlying dimensions are too stable to tap program effects.

One possible implication is that citizen attitudes as measured through surveys should be discarded or at least complemented with more "objective" measures not relying on the assessments of citizens (e.g., the time it takes for police to respond to a call). Another implication is that it seems necessary to go back to the drawing boards when citizen attitudes will figure significantly in criminal justice evaluations. The traditional sorts of items do not seem to work as intended and perhaps the time is ripe for the development of alternative questionnaire items. At least one productive route would be to target survey items more narrowly on the program outcomes of interest (as we suggested in the last chapter). Another productive route would be to employ more specific kinds of response categories. Thus, it might be more productive to ask respondents, for instance, the number of times in the past month they felt threatened by teenagers in their neighborhood than to ask in general terms whether they feel safe on the streets at night. It might also prove useful to spend some time in the pre-test phase of an evaluation educating respondents about the kinds of questions that will be asked and therefore the kinds of events and experiences to which they should be sensitive.

It also does not seem to us that traditional survey methods are necessarily the best way to elicit citizen attitudes about criminal justice performance. It might be possible to generate more respondent involvement and more better measures if the usual question-and-answer format were at least complemented by other approaches. For example, Berk and Rossi (1977) used card-sorting procedures to elicit respondent judgments about "appropriate" sentences for convicted felons, and these procedures not only engaged the attention of respondents, but appeared to generate a host of useful indicators

of how well the courts are doing. There is also the possibility of using diaries to document the kinds of events that criminal justice programs are designed to affect. For example, it might be useful to ask respondents to record over a week-long interval each incident in which they felt threatened by potential criminal activity (e.g., walking home from the bus stop). In short, there are a number of ways to elicit citizen attitudes about the performance of the criminal justice system beyond the means that have proved popular in the past.

NOTES TO CHAPTER IV

1. Measurement parameters were re-estimated within the full (measurement and substantive) model. The full models are subject to many more over-identifying restrictions than the measurement models of Chapter III (primarily because predetermined variables are constrained to affect survey indicators solely through the unobservable dimensions). Consequently, estimates of measurement parameters within the full model typically differ slightly from those reported in Chapter III. However, in no case were differences large enough to be on any consequence. In principle, estimates from the full model are subject to less sampling variability. However, we have more confidence in the estimates reported in Chapter III. Equations in the full models were estimated sequentially rather than simultaneously, so the potential efficiency gain is likely not to have been realized.

2. The substantive models that we estimated actually contain 21 equations, since the 18 endogenous variables are directly affected by causally prior variables. However, the determinants of those variables are not the focus of this report, so parameter estimates for the corresponding 18 equations are not reported.

3. An additional indicator of police service, "How often did you see a policeman in this neighborhood?" (questionnaire item 43), was included when estimating the substantive model. However this item had virtually no reliable variance and loadings (λ's) near zero. Consequently, it had a negligible impact on the estimation of the substantive equations.

V

SUMMARY AND CONCLUSIONS

If nothing else, we have demonstrated the importance of panel designs and the application of statistical models that allow one to model simultaneously substantive and measurement effects. Specifically, we decomposed covariation among thirty-three survey measures of nine underlying attitudes toward crime and criminal justice, separating unreliability in the individual items from true stability and change in unobservable attitudes. We found each of the nine attitudes to be remarkably stable over the seventeen-week period, even though individual responses to the less reliable indicators were highly volatile. Many of the attitudes became more stable over the course of the study, suggesting that simply participating in the survey may have contributed to the formulation of coherent opinions about crime and criminal justice. At the same time, we found that individual differences in unique "error" components of some items became more stable over time, suggesting that participation in the survey may also contribute to the crystallization of a "response set" or "recall contamination" across panels.

The highly stable underlying unobservable attitudes were subject to sharp regional and subcultural differentiation, although there was far from complete consensus among respondents with identical socioeconomic characteristics. Further, our results suggest that there are specific regional and subcultural "climates" reflected in attitudes toward crime and criminal justice, since respondents reports of prior contact with crime and the criminal justice system failed to account for socioeconomic differences in attitudes. For example, women are substantially less secure about their personal safety than men, and this difference does

not disappear once victimization, perceptions of the extent of crime, and personal contact with the police are controlled. However, the women in our sample are certainly quite aware that women in general are more likely to be victimized by certain crimes. The particular women in our sample need not have experienced high rates of victimization themselves for their perceptions of an aggregate trend to influence their feelings of personal safety. Similar arguments might explain why city and neighborhood ethnicity differences do not disappear when prior contact is controlled at the individual level.

Our study was not an assessment of specific criminal justice programs, but it does have implications for such evaluations. It is perhaps most important to recognize that attitudes about crime and criminal justice may be quite stable and are likely to become even more stable once they are regularly monitored. Further, while the attitudes we measured were not completely insensitive to contact with crime and criminal justice, differences in attitudes between respondents who differ in city, gender, or ethnicity were typically much larger than differences between those who had recently been victimized or affected by the criminal justice system and those who had no such experience. In short, the usual survey measures do not seem to be good candidates for the evaluation of criminal justice programs: individual items are too volatile while the underlying dimensions are too stable.

In this context, we offer the following recommendations.

1. If the standard question-and-answer format is retained, telephone interviews complemented by CATI procedures promise data of at least equal quality to that obtained from face-to-face interviews and will clearly reduce per-unit costs.

2. Survey items should be far more narrowly focused on particular program outcomes of interest. Global assessments founder.

3. The quality of respondent assessments would probably be improved if time were taken in the pre-test phase to educate respondents about the kinds of information being sought.

4. Alternatively, one might achieve the same educational effects with "warm-up" questions before the interview actually began or "throw-away" questions asked early in the interview process.

5. Response categories should be made far more specific and targeted to the kinds of outcome metrics relevant to policy.

6. Traditional survey items should be routinely complemented with "objective" measures of the performance of the criminal justice system.

7. Alternatives to the usual question-and-answer format should be considered such as car sorts and diaries.

8. There are a number of "control" variables that have been neglected not only in this study, but most others. For example, respondent assessments of how well the police are doing first require a judgment about how well police should (or could) be doing.

9. Evaluations of criminal justice programs should routinely consider panel designs and the kinds of statistical procedures we have used here. Under such circumstances, it will be possible to model both the substantive processes and the measurement processes and markedly enhance the accuracy and sensitivity of survey-based evaluations.

10. There are a host of design options implied by our results. In particular, our findings suggest that panels could be spaced a month or two apart without any serious loss in attitudinal information. In addition, one might employ rotating panels in an effort to minimize any biasing

effects from repeat interviews. Finally, telephone screening coupled with CATI techniques might effectively allow researchers to target interviews to specific kinds of respondents who are of special interest (e.g., crime victims) or who are the most appropriate targets of criminal justice programs (e.g., people who use mass transit).

11. An enormous amount of work needs to be done in the development of a theory (or theories) of measurement error. In other words, our measurement models are vulnerable to just the kinds of specification errors that have long been of great concern when substantive models are developed. Our approach was, quite frankly, crassly empirical, and this capitalized to some unknown degree on type I and type II errors.

APPENDIX 1

IDENTIFICATION OF THE MODEL: DISENTANGLING THE COMPONENTS OF VARIATION IN ATTITUDES ABOUT CRIMINAL JUSTICE

In our research we identify four unobservable components of variation in attitudes toward criminal justice: "true" substantive variation (T_j), unmeasured substantive determinants (u_j), and random (v_{jk}) and nonrandom (e_{jk}) error variation. Is it indeed possible to disentangle these unobservable components from a rather limited collection of measurable variables obtained from a panel survey? Wheaton, et al. (1977: 122-124) demonstrate the identifiability of a model virtually identical to ours, and here we present a brief exposition of the identification of a simplified version of our model.

Figure A1 shows a simplified two-panel model with just one background measure and no measured time-specific substantive determinants. Since additional background variables, more panels, and time specific measured determinants provide additional observable data that can be used for computing structural parameters, demonstration of the identifiability of the simplified model is sufficient for determining the identifiability of the full model.

First, we express the observable covariation among the five measured variables, B_1, X_{11}, X_{12}, X_{21}, X_{22}, in terms of the structural parameters. Without loss of generality, we can assume that all measured and unmeasured variables are expressed in a standardized metric with means of zero and unit standard deviations. We shall ignore the structural relationships among the unobservable variables for now, and examine the identification of their intercorrelations instead. It can be shown that the 10 correlations among the measured variables are the following functions of structural parameters:

$$\rho_{B_1X_{11}} = \rho_{B_1T_1}\lambda_{11} \quad \text{(A1)}$$

$$\rho_{B_1X_{12}} = \rho_{B_1T_1}\lambda_{12} \quad \text{(A2)}$$

$$\rho_{B_1X_{21}} = \rho_{B_1T_2}\lambda_{21} \quad \text{(A3)}$$

$$\rho_{B_1X_{22}} = \rho_{B_1T_2}\lambda_{22} \quad \text{(A4)}$$

$$\rho_{X_{11}X_{12}} = \lambda_{11}\lambda_{12} \quad \text{(A5)}$$

$$\rho_{X_{11}X_{21}} = \lambda_{11}\lambda_{21}\rho_{T_1T_2} + \lambda'_{11}\lambda'_{21}\rho_{e_{11}e_{21}} \quad \text{(A6)}$$

$$\rho_{X_{11}X_{22}} = \lambda_{11}\lambda_{22}\rho_{T_1T_2} \quad \text{(A7)}$$

$$\rho_{X_{12}X_{21}} = \lambda_{12}\lambda_{21}\rho_{T_1T_2} \quad \text{(A8)}$$

$$\rho_{X_{12}X_{22}} = \lambda_{12}\lambda_{22}\rho_{T_1T_2} + \lambda'_{12}\lambda'_{22}\rho_{e_{11}e_{22}} \quad \text{(A9)}$$

$$\rho_{X_{21}X_{22}} = \lambda_{21}\lambda_{22} \quad \text{(A10)}$$

In addition, the unit standard deviations impose the following:

$$1 = \lambda_{11}^2 + \lambda_{11}'^2 \quad \text{(A11)}$$

$$1 = \lambda_{12}^2 + \lambda_{12}'^2 \quad \text{(A12)}$$

$$1 = \lambda_{21}^2 + \lambda_{21}'^2 \quad \text{(A13)}$$

$$1 = \lambda_{22}^2 + \lambda_{22}'^2 \quad \text{(A14)}$$

The fourteen equations contain thirteen structural parameters on the right-hand side of the equals sign. Is there a subset of thirteen equations that allows us to compute unique solutions for structural parameters from observable

correlations? The three equations, A1, A2, and A5 are easily solved for λ_{11}, λ_{12}, and $\rho_{B_1T_1}$. Similarly, λ_{21}, λ_{22}, and $\rho_{B_1T_2}$ can be obtained from A3, A4, and A10. Given this information, $\rho_{T_1T_2}$ is determined from A7 (or A8). A11 through A14 can then determine λ'_{11}, λ'_{12}, λ'_{21}, and λ'_{22}, so we now have enough information to obtain $\rho_{e_{11}e_{21}}$ from A6 and $\rho_{e_{12}e_{22}}$ from A9. Since $\rho_{T_1T_2}$ is overdetermined by these equations, the model implies that a restriction must hold among the observable correlations. After considerable manipulation, it can be shown that this restriction is:

$$\rho_{X_{12}X_{21}}\rho_{B_1X_{11}}\rho_{B_1X_{22}} = \rho_{X_{11}X_{22}}\rho_{B_1X_{12}}\rho_{B_1X_{21}} \quad \text{(A15)}$$

Should sample correlations depart from this relationship more than could be expected on the basis of sampling variability, we would be compelled to reject or modify the structural model.

Since all correlations among unobservables are identified, the structural relationships among them (represented by α_1, α_2, β_{21}, γ_{21}, γ'_{21}, θ_{21}, and θ'_{21}) can be obtained from a set of multiple regression-like "normal equations." Thus, it turns out that each of the structural parameters in our simplified model is identified, and the model implies a single overidentifying restriction upon the observable covariation. The full model implies many such restrictions, which allow both global and specific tests of the model. Further, we could generate and test additional restrictions under hypotheses about various parameters in the model (for example, $\gamma_{21} = \theta_{21} = 0$ implies that $\rho_{X_{11}X_{21}}\rho_{X_{12}X_{22}} = \rho_{X_{11}X_{22}}\rho_{X_{12}X_{21}}$).

Fortunately, the maximum likelihood program LISREL (Jöreskog and Sörbom, 1976) allows us to circumvent the tedious algebra. It provides asymptotically efficient parameter estimates, a "goodness-of-fit" measure that can be used to test part or all of the implications of the model, and information that aids in diagnosing possible misspecification of the model.

APPENDIX 2

Wave 1 Questionnaire

RESPONDENT'S NAME _____ STREET ADDRESS _____

CITY/STATE/ZIP _____ PHONE ( ) _____

AUDITS & SURVEYS, INC.
One Park Avenue
New York, N.Y. 10016

_____

PROJECT #4987
March, 1979

QUESTIONNAIRE: CRIMINAL JUSTICE
EVALUATION STUDY

INTERVIEWER'S NAME _____ INTERVIEWER # [ ][ ][ ][ ][ ]

DATE OF INTERVIEW _____ 26-27-

TIME INTERVIEW BEGAN _____ TIME INTERVIEW ENDED _____ TOTAL MINUTES _____ (28-29)

LOCATION # [ ][ ][ ][ ][ ][ ][ ]

(FOR OFFICE USE ONLY): I.D.# [ ][ ][ ]

1. To begin with, I'd like to know something about the makeup of your household. Please give me the first name only of all persons who live here with you. ENTER IN GRID BELOW UNDER Q.1. LIST RESPONDENT FIRST.

FOR EACH PERSON LISTED IN Q.1, ASK:

2. How is (INSERT NAME FROM Q.1) related to you? ENTER BELOW UNDER Q.2.

3. What is approximately (your/his/her) age? ENTER BELOW UNDER Q.3.

4. CHECK SEX UNDER Q.4 BELOW. ASK ONLY IF NOT APPARENT FROM NAME.

Q.1 NAME	Q.2 RELATION	Q.3 AGE	Q.4 SEX MALE	Q.4 SEX FEMALE	Q.5a TYPE SCHOOL PUBLIC	Q.5a TYPE SCHOOL PRIVATE	Q.5b MEANS OF TRAVEL TO/FROM SCHOOL
	SELF	(30-31)	( )32-1	( )-2	(X)	(X)	XXXX 39-
	33-	(35-36)	( )37-1	( )-2	( )38-1	( )-2	40- 47-
	41-	(43-44)	( )45-1	( )-2	( )46-1	( )-2	48- 55-
	49-	(51-52)	( )53-1	( )-2	( )54-1	( )-2	56- 63-
	57-	(59-60)	( )61-1	( )-2	( )62-1	( )-2	64- 71-
	65-	(67-68)	( )69-1	( )-2	( )70-1	( )-2	72- 79-
	73-	(75-76)	( )77-1	( )-2	( )78-1	( )-2	80- 14-
	8-	(10-11)	( )12-1	( )-2	( )13-1	( )-2	15-

(5-7) 025

IF NO CHILDREN 6-17 YEARS IN HOUSEHOLD, SKIP TO Q.6
IF ONE OR MORE CHILDREN 6-17 YEARS IN HOUSEHOLD, ASK:

5. I have a couple of questions about the children in your household who are attending grade school, junior high school or high school. Please tell me for each child whether they attend a public or private school and how they normally travel to and from school.

ENTER SCHOOL TYPE IN GRID ABOVE UNDER Q.5a. ROMAN CATHOLIC SCHOOL IS PRIVATE, ENTER USUAL MEANS OF TRAVEL UNDER Q.5b. BE SPECIFIC. IF RESPONDENT SAYS "BUS", DETERMINE WHETHER SCHOOL BUS OR CITY BUS. IF "PUBLIC TRANS." IS ANSWER, DETERMINE VEHICLE TYPE: CITY BUS, SUBWAY, TROLLEY, ETC.

6. How many years have you lived in this neighborhood? IF LESS THAN ONE YEAR, ASK NUMBER OF MONTHS.

YEARS _____ (16-17)

MONTHS _____ (18-19)

7. Do you own or rent this residence?

OWN ( )20-0
RENT ( ) -1
OTHER - SPECIFY: ( ) -4

_____

8. How do you normally get around for shopping, visiting friends and things like that. Do you normally walk, use an automobile, bus, train, subway, a combination of these or some other means? DO NOT READ LIST. CHECK AS MANY AS ARE MENTIONED.

WALK ( )21-1
AUTO ( )22-1
BUS ( )23-1
TRAIN/SUBWAY ( )24-1
OTHER - SPECIFY: ( )25-1

_____

SKIP COL.26-29

9. We would like to know to what extent people carry insurance on certain possessions. Please tell me if you or anyone living with you owns any of the following vehicles and appliances and if so, whether or not it is covered by insurance against theft or vandalism.

ASK OWNERSHIP OF EACH ITEM, ENTER RESPONSE IN GRID BELOW UNDER Q.9a, IF "YES", IMMEDIATELY ASK WHETHER COVERED BY INSURANCE AND ENTER RESPONSE IN GRID UNDER Q.9b. IF NO TO INSURANCE IN Q.9b, ASK Q.9c IMMEDIATELY: Did you try to get insurance but couldn't?

	Q.9a OWNERSHIP YES	Q.9a OWNERSHIP NO	Q.9b INSURANCE YES	Q.9b INSURANCE NO	Q.9c TRIED BUT COULDN'T GET INSURANCE YES	Q.9c TRIED BUT COULDN'T GET INSURANCE NO
CAR	( )30-2	( )-0	( )31-2	( )-0	( )32-2	( )-0
TELEVISION SET(S)	( )33-2	( )-0	( )34-2	( )-0	( )35-2	( )-0
STEREO	( )36-2	( )-0	( )37-2	( )-0	( )38-2	( )-0
BICYCLE	( )39-2	( )-0	( )40-2	( )-0	( )41-2	( )-0

I would now like to get directly into the area of crime and crime control.

10. In general, have you limited or changed your activities in the past few years because of crime?

YES ( )42-2
NO ( ) -0

11. Do you think that most people in this neighborhood have limited or changed their activities in the past few years because they are afraid of crime?

YES ( )43-2
NO ( ) -0

12. Do you think people in general have been limited or changed their activities in the past few years because they are afraid of crime?

YES ( )44-2
NO ( ) -0

13. Which of the following statements do you agree with most?
Crime is less serious than the newspapers and TV say.
Crime is more serious than the newspapers and TV say.
Crime is about as serious as the newspaper and TV say.
CHECK ONLY ONE RESPONSE.

LESS SERIOUS ( )45-0
MORE SERIOUS ( ) -2
AS SERIOUS ( ) -1
DON'T KNOW ( ) -9

14. Which of the following statements do you agree with the most?
My chances of being attacked or robbed have gone up in the past few years.
My chances of being attacked or robbed have gone down in the past few years.
My chances of being attacked or robbed haven't changed in the past few years.
CHECK ONLY ONE RESPONSE.

UP	( )	46-2
DOWN	( )	-0
SAME	( )	-1
DON'T KNOW	( )	-9

15. Would you say, in general, that your local police are doing a good job, an average job or a poor job? CHECK ONLY ONE RESPONSE.

GOOD	( )	47-2
AVERAGE	( )	-1 ASK Q.16
POOR	( )	-0
DON'T KNOW	( )	-9 SKIP TO Q.18

IF "POOR", "AVERAGE" OR "GOOD" IN Q.15, ASK:

16. Would you say that the police needed improvement in doing their job?

YES	( )	48-2 ASK Q.17
NO	( )	-0
DON'T KNOW	( )	-9 SKIP TO Q.18

IF "YES" IN Q.16, ASK:

17. In what ways could they improve? Give me a yes or no answer to each of the following statements. CHECK YES OR NO TO EACH STATEMENT a THROUGH h.

	Q.17 YES	Q.17 NO
a. More policemen could be hired	( ) 49-2	( )-0
b. Police could concentrate on more important duties, serious crime, etc.	( ) 50-2	( )-0
c. Police could be more prompt, responsive, alert	( ) 51-2	( )-0
d. Police could improve training, raise qualifications or pay, improve recruitment policies	( ) 52-2	( )-0
e. Police could be more courteous, improve attitudes, community relations	( ) 53-2	( )-0
f. Police shouldn't discriminate	( ) 54-2	( )-0
g. More traffic control is needed	( ) 55-2	( )-0
h. More policemen of a particular type (for example, foot car) are needed in certain areas or at certain times.	( ) 56-2	( )-0

ASK EVERYONE:

18. Are there some parts of this metropolitan area where you have a reason to go or would like to go during the day, but are afraid to because of fear of crime?

YES	( )	57-2
NO	( )	-0

19. How about at night, are there some parts of this area where you have a reason to go or would like to go but are afraid to because of fear of crime?

YES	( )	58-2
NO	( )	-0

20. How do you think your neighborhood compares with others in this metropolitan area in terms of crime? Would you say it is much more dangerous, more dangerous, about average, less dangerous or much less dangerous?

MUCH MORE DANGEROUS	( )	59-4
MORE DANGEROUS	( )	-3
ABOUT AVERAGE	( )	-2
LESS DANGEROUS	( )	-1
MUCH LESS DANGEROUS	( )	-0

21. How about during the day - how safe do you or would you feel being out alone in your neighborhood? Would you feel very safe, reasonably safe, somewhat unsafe, very unsafe?

VERY SAFE	( )	60-3
REASONABLE SAFE	( )	-2
SOMEWHAT UNSAFE	( )	-1
VERY UNSAFE	( )	-0

22. How safe do you feel or would you feel being out alone in your neighborhood at night? Would you say it is very safe, reasonably safe, somewhat unsafe, very unsafe?

VERY SAFE	( )	61-3
REASONABLY SAFE	( )	-2
SOMEWHAT UNSAFE	( )	-1
VERY UNSAFE	( )	-0

23. Now I'd like to get your opinions about crime in general. Within the past year or two, do you think that crime in your neighborhood has increased, decreased, or remained about the same?

INCREASED	( )	62-2
DECREASED	( )	-0
REMAINED THE SAME	( )	-1
NOT HERE LONG ENOUGH	( )	-8
DON'T KNOW	( )	-9

24. Within the past year or two do you think that crime in the United States has increased, decreased, or remained about the same?

INCREASED	( )	63-2
DECREASED	( )	-0
REMAINED THE SAME	( )	-1
DON'T KNOW	( )	-9

25. I now want to ask about various sorts of experiences people sometimes have. During the past month did any of the following things happen to you, or to someone else in your household. IF RESPONDENT SAYS "YES", BUT DOES NOT SPECIFY PERSON, ASK, "WHO DID THIS HAPPEN TO?" REPEAT QUESTION "DID THIS HAPPEN TO YOU OR TO SOMEONE ELSE IN YOUR HOUSEHOLD?" FOR SEVERAL EXPERIENCES UNTIL RESPONDENT HAS PATTERN FIRMLY IN MIND.

	Q.25 HAPPENED TO SELF YES	SELF NO	OTHER(S) HOUSEHOLD YES	OTHER(S) HOUSEHOLD NO
a. Got sick enough to go to bed	( )64-2	( )-0	( )65-2	( )-0
b. Won a prize in a contest	( )66-2	( )-0	( )67-2	( )-0
c. Got cheated by a merchant	( )68-2	( )-0	( )69-2	( )-0
d. Received an unusually nice gift or compliment	( )70-2	( )-0	( )71-2	( )-0
e. Purchased a new refrigerator, television set, washing machine, or some other major appliance	( )72-2	( )-0	( )73-2	( )-0
f. Had trouble paying some bills	( )74-2	( )-0	( )75-2	( )-0
g. Had an unusually good trip or vacation	( )76-2	( )-0	( )77-2	( )-0
h. Had an unusually nice time with some friends or relatives	( )78-2	( )-0	( )79-2	( )-0

26. Next, I want to ask about some other types of experiences people sometimes have. During the past month did any of the following things happen to you, to someone else in your household, or to a close friend or relative not living with you? IF RESPONDENT SAYS "YES", BUT DOES NOT SPECIFY PERSON, ASK "WHO DID THIS HAPPEN TO?" REPEAT QUESTION "DID THIS HAPPEN TO YOU, SOMEONE ELSE OR TO A FRIEND OR RELATIVE NOT LIVING WITH YOU?" FOR SEVERAL EXPERIENCES UNTIL RESPONDENT HAS PATTERN FIRMLY IN MIND.

5-7 03C

	Q.26 HAPPENED TO SELF YES	SELF NO	OTHER(S) IN HOUSEHOLD YES	OTHER(S) IN HOUSEHOLD NO	FRIEND/RELATIVE OUTSIDE HOUSEHOLD YES	FRIEND/RELATIVE OUTSIDE HOUSEHOLD NO
a. Got a job	( ) 8-2	( )-0	( ) 9-2	( )-0	( )10-2	( )-0
b. Got a promotion or raise	( )11-2	( )-0	( )12-2	( )-0	( )13-2	( )-0
c. Got fired or laid off	( )14-2	( )-0	( )15-2	( )-0	( )16-2	( )-0
d. Lost out on a promotion	( )17-2	( )-0	( )18-2	( )-0	( )19-2	( )-0
e. Got sick enough to need a doctor's care	( )20-2	( )-0	( )21-2	( )-0	( )22-2	( )-0
f. Got engaged or married	( )23-2	( )-0	( )24-2	( )-0	( )25-2	( )-0
g. Got separated or divorced	( )26-2	( )-0	( )27-2	( )-0	( )28-2	( )-0
h. Got into an unusually serious argument with a spouse	( )29-2	( )-0	( )30-2	( )-0	( )31-2	( )-0
i. Got into an unually serious argument with a friend	( )32-2	( )-0	( )33-2	( )-0	( )34-2	( )-0
j. Gave birth to a child	( )35-2	( )-0	( )36-2	( )-0	( )37-2	( )-0
k. Got threatened with violence	( )38-2	( )-0	( )39-2	( )-0	( )40-2	( )-0
l. Purchased a car	( )41-2	( )-0	( )42-2	( )-0	( )43-2	( )-0
m. Had a child graduate from high school or college	( )44-2	( )-0	( )45-2	( )-0	( )46-2	( )-0
n. Had an automobile accident at home or work	( )47-2	( )-0	( )48-2	( )-0	( )49-2	( )-0
o. Found out that a close friend or relative died	( )50-2	( )-0	( )51-2	( )-0	( )52-2	( )-0

27. Now I want to ask you about crime and whether you have been a victim of a crime. Have any of the following ever happened to you?

IF "YES", ASK IMMEDIATELY:

28. Did this happen during the past year?

	HAPPENED TO YOU Q.27 EVER YES	Q.27 EVER NO	Q.28 PAST YEAR YES	Q.28 PAST YEAR NO
a. Had an attempt to break into your home or car	( )53-2	( )-0	( )54-2	( )-0
b. Had an automobile stolen on the street	( )55-2	( )-0	( )56-2	( )-0
c. Were robbed by force or threatened outside your home	( )57-2	( )-0	( )58-2	( )-0
d. Were sexually assaulted	( )59-2	( )-0	( )60-2	( )-0
e. Were beaten up by a person who wasn't trying to rob you	( )61-2	( )-0	( )62-2	( )-0
f. Had your home damaged by vandals	( )63-2	( )-0	( )64-2	( )-0
g. Had property stolen from your home worth $100 or more	( )65-2	( )-0	( )66-2	( )-0
h. Had property stolen from your home worth less than $100	( )67-2	( )-0	( )68-2	( )-0

29. As far as you know, have any of the following happened to anyone else in your household during the past year.

	Q.29 HAPPENED TO OTHER(S) IN HOUSEHOLD YES	HAPPENED TO OTHER(S) IN HOUSEHOLD NO
a. Robbery by force of threat outside the home	( )69-2	( )-0
b. Sexual assault	( )70-2	( )-0
c. Any other kind of assault	( )71-2	( )-0
d. Had an auto stolen	( )72-2	( )-0
e. Got harrassed or threatened on the way to or from school	( )73-2	( )-0

5-7 04D

30. As far as you know have any of the following happened during the past year to a close friend or relative not living with you?

	Q.30 HAPPENED TO FRIEND/RELATIVE OUTSIDE HOUSEHOLD YES	NO
a. Had an automobile stolen	( ) 8-2	( )-0
b. Had an attempt to break into their home or car	( ) 9-2	( )-0
c. Were robbed by force or threatened outside their home	( )10-2	( )-0
d. Were sexually assaulted	( )11-2	( )-0
e. Were beaten up by a person who was not trying to rob them	( )12-2	( )-0
f. Had their home damaged by vandals	( )13-2	( )-0
g. Had property stolen from their home worth $100 or more	( )14-2	( )-0
h. Had property stolen from their home worth less than $100	( )15-2	( )-0

31. As far as you know, have any of the following happened to you, someone in your household or a close friend or relative not living with you during the past year? IF RESPONDENT SAYS "YES", BUT DOES NOT SPECIFY PERSON, ASK "WHO DID THIS HAPPEN TO?"

	YOU YES	YOU NO	OTHER(S) IN HOUSEHOLD YES	OTHER(S) IN HOUSEHOLD NO	FRIEND/RELATIVE OUTSIDE HOUSEHOLD YES	FRIEND/RELATIVE OUTSIDE HOUSEHOLD NO
a. Called the police for assistance	( )16-2	( )-0	( )17-2	( )-0	( )18-2	( )-0
b. Gotten assistance or help from a police officer	( )19-2	( )-0	( )20-2	( )-0	( )21-2	( )-0
c. Received a traffic citation	( )22-2	( )-0	( )23-2	( )-0	( )24-2	( )-0
d. Arrested for some other offense	( )25-2	( )-0	( )26-2	( )-0	( )27-2	( )-0
e. Appeared in court as a defendant	( )28-2	( )-0	( )29-2	( )-0	( )30-2	( )-0
f. Appeared in court as a witness	( )31-2	( )-0	( )32-2	( )-0	( )33-2	( )-0
g. Spent some time in jail or prison	( )34-2	( )-0	( )35-2	( )-0	( )36-2	( )-0

32. As far as you know, is a member of your household or a close friend or relative not living with you currently employed as any of the following:
IF RESPONDENT SAYS "YES", BUT DOES NOT SPECIFY PERSON ASK, "WHO IS THAT?"

	Q.32 MEMBER(S) OF HOUSEHOLD YES	MEMBER(S) OF HOUSEHOLD NO	FRIEND/RELATIVE OUTSIDE HOUSEHOLD YES	FRIEND/RELATIVE OUTSIDE HOUSEHOLD NO
a. A police officer	( )37-2	( )-0	(.)38-2	( )-0
b. A member of a police department, but not a police officer	(.)39-2	( )-0	( )40-2	( )-0
c. A prison guard or corrections official	(.)41-2	( )-0	(.)42-2	(.)-0
d. A judge, district attorney, clerk or some other position within the courts	( )43-2	(.)-0	( )44-2	(.)-0

33. As far as you know, have any of the following events occurred one or more times in your neighborhood during the past year? IF "YES", ASK IMMEDIATELY: About how many times?

	Q.33 HAPPENED IN NEIGHBORHOOD NO	YES	# OF TIMES	
a. A murder	( )45-0	( )-2	_____	(46-47)
b. Arson, that is, purposely setting a building on fire	( )48-0	( )-2	_____	(49-50)
c. A rape	( )51-0	( )-2	_____	(52-53)
d. An armed robbery of a local business	( )54-0	( )-2	_____	(55-56)
e. A sexual assault on a child	( )57-0	( )-2	_____	(58-59)
f. A serious fight between teenage gangs	( )60-0	( )-2	_____	(61-62)
g. An assault on a police officer	( )63-0	( )-2	_____	(64-65)
h. An assault on a teacher	( )66-0	( )-2	_____	(67-68)
i. Unnecessary force by a police officer	( )69-0	( )-2	_____	(70-71)

34. Think of a scale from 0-10. Zero stands for no possibility at all and ten stands for extremely likely. For each statement I read, give me a rating from 0 to 10. During the course of a year how likely is it that?

5-7 5E

a. Someone would break into your residence when no one is home?	_____	(8-9)
b. Someone would break into your residence when someone is home?	_____	(10-11)
c. Your purse/wallet would be snatched in your neighborhood?	_____	(12-13)
d. Someone would take something from you on the street by force or threat in your neighborhood?	_____	(14-15)
e. Someone would beat you up or hurt you on the street in your neighborhood?	_____	(16-17)

I would now like to change the subject to things some people do when they are concerned about crime.

	YES	NO
35. Have you installed or do you have special or extra locks on doors?	( )18-2	( )-0
36. Have you installed or do you have burglary alarms or security alarms?	( )19-2	( )-0
37. Have you installed or do you have special locks or bars on windows?	( )20-2	( )-0
38. Do you own a dog for protection?	( )21-2	( )-0
39. Do you carry a chemical repellant like tear gas or mace?	( )22-2	( )-0
40. Do you have sport guns such as shotguns or hunting rifles in your home?	( )23-2	( )-0

41. Do you keep a loaded hand gun in your home?
YES ( ) 24-2 ASK Q.42
NO ( ) -0 SKIP TO Q.43

42. About how often do you keep the hand gun in your home - always, usually or sometimes?
ALWAYS ( ) 25-3
USUALLY ( ) -2
SOMETIMES ( ) -1
OTHER (SPECIFY) ( ) -4

I would now like to turn to some general questions about the criminal justice system in this city and the country at large.

43. How often do you see a policeman in this neighborhood? Every day, at least once a week less than once a week, or never?
EVERY DAY ( ) 26-3
AT LEAST ONCE A DAY ( ) -2
LESS THAN ONCE A WEEK ( ) -1
NEVER ( ) -0
DON'T KNOW ( ) -9

44. Some people say the police don't come quickly when you call them for help. Do you think this happens to people in this neighborhood.
YES ( ) 27-2
NO ( ) -0
DON'T KNOW ( ) -1

45. Some people say the police frisk or search people without good reason. Do you think this happens to people in this neighborhood?
YES ( ) 28-2
NO ( ) -0
DON'T KNOW ( ) -1

46. Some people say the police don't show respect for people or they use insulting language. Do you think this happens to people in this neighborhood?
YES ( ) 29-2
NO ( ) -0
DON'T KNOW ( ) -1

47. Some people say the police rough up people unnecessarily when they are arresting them or afterwards. Do you think this happens to people when they are in this neighborhood?
YES ( ) 30-2
NO ( ) -0
DON'T KNOW ( ) -1

48. Do you agree or disagree with the statement that all people are treated the same by the police regardless of race?
AGREE ( ) 31-2
DISAGREE ( ) -0
NOT SURE ( ) -1

49. Of all the crimes that are actually committed in your neighborhood, how many would you guess are ever reported to the police? Most of them, about half, about a quarter, or fewer than that?
MOST OF THEM ( ) 32-3
ABOUT HALF ( ) -2
ABOUT A QUARTER ( ) -1
FEWER THAN THAT ( ) -0
DON'T KNOW ( ) -9

50. Of all the crimes that are reported in this neighborhood, how many would you guess are solved or cleared up? Most of them, about half, about a quarter, or fewer than that?
MOST ARE SOLVED ( ) 33-3
ABOUT HALF ( ) -2
ABOUT A QUARTER ( ) -1
FEWER THAN THAT ( ) -0
DON'T KNOW ( ) -9

51. Of all the crimes that are reported in this neighborhood, how many would you guess lead to someone actually being arrested? Most of them, about half, about a quarter, or fewer than that?
MOST LEAD TO ALL ARRESTS ( ) 34-3
ABOUT HALF ( ) -2
ABOUT A QUARTER ( ) -1
FEWER THAN A QUARTER ( ) -0
DON'T KNOW ( ) -9

52. Of all the crimes that are reported in this neighborhood, how many would you guess lead to someone actually going to trial. Most of them, about half, about a quarter, or fewer than that?
MOST RESULT IN A TRAIL ( ) 35-3
ABOUT HALF ( ) -2
ABOUT A QUARTER ( ) -1
FEWER THAN THAT ( ) -0
DON'T KNOW ( ) -9

53. Of all the crimes that are reported in this neighborhood, how many would you guess lead to someone actually going to prison? Most of them, about half, about a quarter, or fewer than that?
MOST RESULT IN PRISON SENTENCE ( ) 36-3
ABOUT HALF ( ) -2
ABOUT A QUARTER ( ) -1
FEWER THAN THAT ( ) -0
DON'T KNOW ( ) -9

54. Overall, how satisfied are you with the police protection provided in this neighborhood. Generally satisfied, somewhat satisfied, very dissatisfied?
GENERALLY SATISFIED ( ) 37-2
SOMEWHAT DISSATISFIED ( ) -1
VERY DISSATISFIED ( ) -0
DON'T KNOW ( ) -9

Now, I want to ask you some questions about courts, prisons and other criminal justice agencies.

55. Generally, do you feel the courts have been too easy in dealing with criminals, too severe, or do you feel they have treated them fairly?
TOO EASY ( ) 38-0
TOO SEVERE ( ) -2
FAIR ( ) -1
IT VARIES ( ) -3
DON'T KNOW ( ) -9

56. Do you feel programs to rehabilitate criminals -- people who have served time for a crime -- are adequate or inadequate?
ADEQUATE ( ) 39-2
INADEQUATE ( ) -0
DON'T KNOW ( ) -1

57. What do you think should be the main emphasis in most prisons: punishing the individual convicted of a crime, trying to rehabilitate the individual so that he might become a productive citizen, or imprisoning him to protect society from future crimes he might commit?

PUNISH	( )40-0
REHABILITATE	( ) -2
PROTECT SOCIETY	( ) -1
DON'T KNOW	( ) -9

58. Should parole boards be more strict, less strict, or about the same they are now in granting parole?

MORE STRICT	( )41-0
LESS STRICT	( ) -2
ABOUT THE SAME	( ) -1
DON'T KNOW	( ) -9

59. On the whole do you feel that the ruling of the Supreme Court in recent years have given too much consideration to the rights of people suspected of crimes?

YES	( )42-2
NO	( ) -0
DON'T KNOW	( ) -1

60. Some people say stricter gun controls would help reduce the number of crimes committed by people with guns. Does this sound like a good argument in favor of stricter gun controls? or a poor argument?

GOOD	( )43-2
POOR	( ) -0
DON'T KNOW	( ) -1

61. It has been suggested that anyone who commits a crime with a gun be given double the regular sentence. Does this sound like a good idea to you or a poor idea?

GOOD	( )44-2
POOR.	( ) -0
DON'T KNOW	( ) -1

62. Do you believe in the death penalty or are you opposed to it?

BELIEVE IN IT	( )45-2
OPPOSED TO IT	( ) -0
DON'T KNOW	( ) -1

63. Do you think it would be a good idea or a poor idea to hold parents responsible for property damage that their children cause?

GOOD IDEA	( )46-2
POOR IDEA	( ) -0
DON'T KNOW	( ) -1

64. Do you strongly agree, agree, disagree or strongly disagree with the following statement? There are so many loopholes in the law that it is difficult to bring criminals to justice.

STRONGLY AGREE	( )47-4
AGREE	( ) -3
DISAGREE	( ) -1
STRONGLY DISAGREE	( ) -0
DON'T KNOW	( ) -2

Finally, I have a few more questions on your background.

65. Are you self-employed in your own business or professional practice?

YES	( )48-2
NO	( ) -0

66. What kind of work do you normally do? PROBE: IF RETIRED, UNEMPLOYED OR DISABLED ASK USUAL TYPE OF WORK PREVIOUSLY DONE. IF RESPONDENT DESCRIBED SELF AS PART TIME STUDENT OR HOUSEWIFE, SIMPLY WRITE THESE STATUSES IN THE SPACE BELOW.

_____ 49-
50-
_____ 51-

67. Are you currently working full-time, part-time, or are you not working right now. ASK OF EVERYONE - SOME RETIRED PEOPLE HAVE PART-TIME JOBS; SOME HOUSEWIVES MAY TURN OUT TO WORK "PART-TIME".

NOT WORKING	( )52-0	SKIP TO Q.71
PART-TIME	( ) -1	ASK Q.68
FULL-TIME	( ) -2	

IF "PART-TIME" OR "FULL TIME" IN Q.67, ASK:

68. How do you normally get to work? Walk, automobile, bus, train or subway, or some other means? CHECK AS MANY AS APPLY.

WALK	( )53-1
AUTO	( )54-1
BUS	( )55-1
TRAIN/SUBWAY	( )56-1
OTHER-SPECIFY	( )57-1

_____

69. At what time do you normally leave for work? ENTER TIME AND CIRCLE AM OR PM

58-
59-
_____ AM/PM 60-
61-

70. At which time do you normally return from work? ENTER TIME AND CIRCLE AM OR PM

62-
63-
_____ AM/PM 64-
65-

71. Which is the highest grade of school you have completed?

_____ 66-
67-

HAND RESPONDENT THE INCOME FLASH CARD

72. Considering income that you personally received in 1978 from all sources before taxes, please tell me in what income group you belong. Just give me the letter on the card next to your income group. CHECK DESIGNATED BOX UNDER Q.72 BELOW.

	Q.72 INDIVIDUAL INCOME (68-69)	Q.73 HOUSEHOLD INCOME (70-71)
A. 0 - $499	( )-01	( )-01
B. $500 - $999	( )-02	( )-02
C. $1,000 - $1,999	( )-03	( )-03
D. $2,000 - $2,999	( )-04	( )-04
E. $3,000 - $3,999	( )-05	( )-05
F. $4,000 - $4,999	( )-06	( )-06
G. $5,000 - $6,999	( )-07	( )-07
H. $7,000 - $9,999	( )-08	( )-08
I. $10,000 - $14,999	( )-09	( )-09
J. $15,000 - $19,999	( )-10	( )-10
K. $20,000 - $29,999	( )-11	( )-11
L. $30,000 - $39,999	( )-12	( )-12
M. $40,000 - $75,000	( )-13	( )-13
N. Over $75,000	( )-14	( )-14
REFUSED	( )-15	( )-15
DON'T KNOW	( )-16	( )-16

73. Lastly, considering income that all members of your household received in 1978 before taxes, please tell me in what income group your household belongs. Just give the letter on the card next to your household's income group. CHECK DESIGNATED BOX UNDER Q.73 ABOVE.

TAKE FLASH CARD BACK

In addition to the gift we are offering at the completion of the study, it may be possible for us to offer another gift during the course of the study. If this becomes possible we would like to tailor this gift to suit your personal taste. In view of this, I would like to ask you a couple of questions about leisure activities.

4. What is your one favorite hobby or sport? _____ :72-
_____ 73-
_____ 74-
_____ 75-

5. What particular TV show do you enjoy watching the most? _____ :76-
_____ 77-
_____ 78-
_____ 79-

This concludes our interview. Many thanks for your valuable time.

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TABLE 2.1

TIMING OF INTERVIEWS

Wave	Date Interview Began	Median Interview Date	Median Interval Between Interview Date and Preceeding Interview
1.	April 29	May 7	---
2.	May 20	May 27	19 days
3.	June 6	June 13	15 days
4.	June 22	June 30	15 days
5.	July 5	July 14	14 days
6.	Aug. 5	Aug. 15	29 days
7.	Sept. 6	Sept. 14	31 days

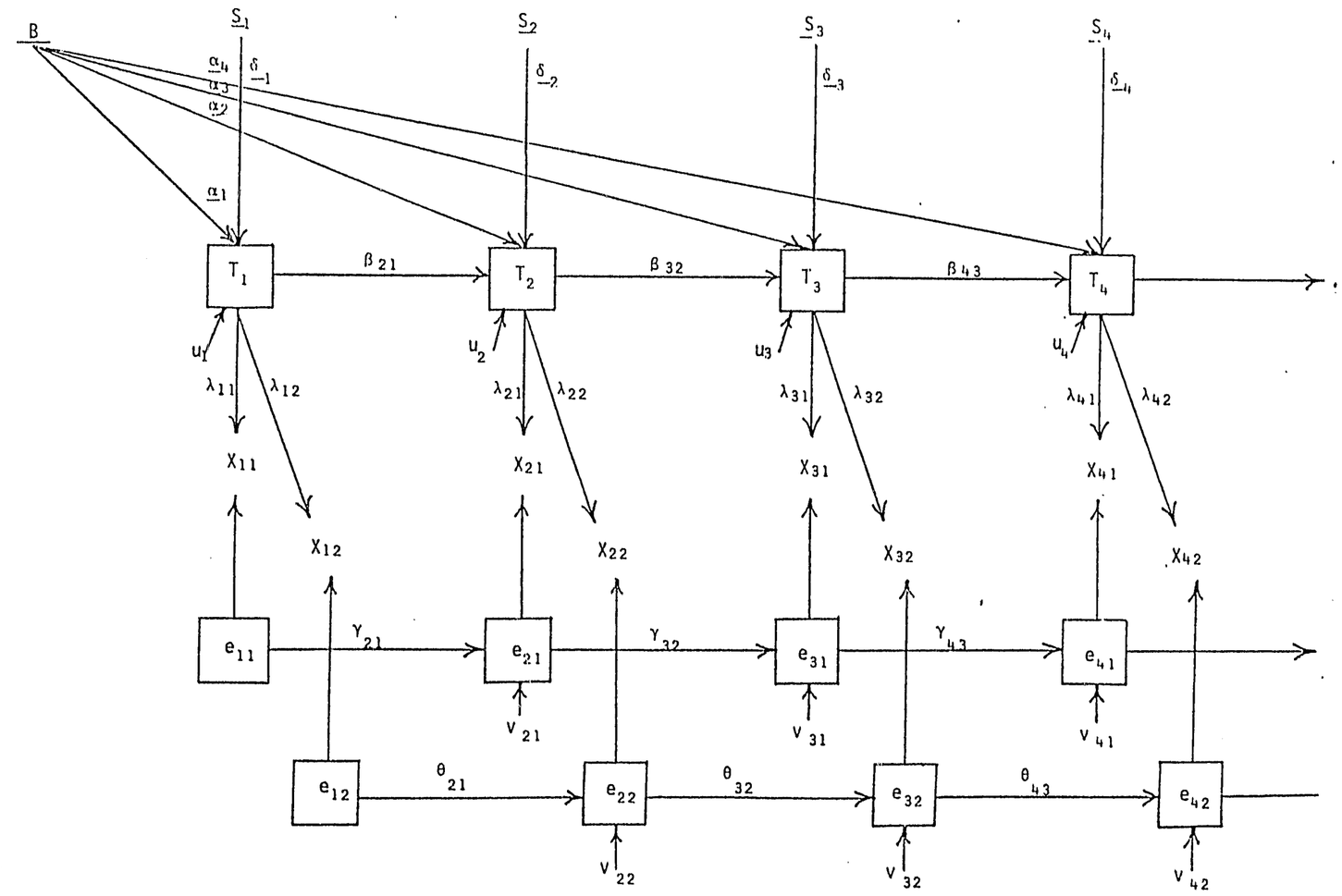

Figure 2.1 - A Multi-Wave Panel Model for Statistical Analysis of Attitudes About Criminal Justice

CONTINUED

1 OF 2

TABLE 2.2

PERCENTAGES, MEANS AND STANDARD DEVIATIONS (IN PARENTHESES) FOR SOCIAL CHARACTERISTICS OF RESPONDENTS: WAVE 1 CASES, WAVE 7 CASES, AND FOUR ANALYSIS SUBSAMPLES

Attribute	WAVE 1 (N = 532)	WAVE 7 (N = 402)	FEAR - 1234 (N = 264)	FEAR - 1467 (N = 255)	CRIM - 1234 (N = 188)	CRIM - 1467 (N = 192)
% LA	25.2	25.1	23.9	25.9	26.2	29.2
% HOU	24.6	21.6	19.3	18.4	19.1	17.6
% WASH	24.8	25.9	28.0	26.7	23.9	24.1
% MINN	25.4	27.4	28.8	29.0	30.8	29.1
% HISPANIC	12.2	9.2	8.3	9.0	10.1	8.8
% BLACK	32.1	32.8	34.4	32.2	35.1	32.4
% MIXED	31.2	31.8	29.2	32.9	30.3	32.4
INCOME (household)	22,741 (14,655)	22,413 (14,182)	23,363 (13,951)	23,046 (14,331)	22,844 (14,798)	22,858 (14,651)
EDUC	12.8 (2.9)	12.9 (2.8)	13.2 (2.5)	13.0 (2.6)	13.1 (2.6)	13.1 (2.6)
AGE	43.0 (16.0)	43.0 (15.7)	41.0 (15.5)	41.4 (15.2)	40.3 (15.0)	41.2 (15.2)
% MALE	48.7	46.2	46.6	45.9	45.2	46.1
CRIME SERIOUS (0-2)	1.40 (.64)	1.39 (.64)	1.42 (.62)	1.41 (.65)	--	--
BREAKIN (0-10)	4.68 (3.18)	4.69 (3.17)	4.74 (3.13)	4.81 (3.15)	--	--
PROPERTY CRIME (0-5)	1.32 (1.39)	1.31 (1.40)	1.34 (1.35)	1.38 (1.41)	1.36 (1.33)	1.34 (1.35)
PERSONAL CRIME (0-3)	.18 (.48)	.17 (.45)	.22 (.53)	.18 (.49)	.11 (.33)	.18 (.45)
COURTS EASY (0-2)	.27 (.52)	.24 (.50)	--	--	.29 (.58)	.27 (.56)
POLICE SAT (0-2)	1.17 (.71)	1.20 (.72)	--	--	1.14 (.71)	1.15 (.72)

TABLE 2.3

ETHNIC COMPOSITION OF WHITE, BLACK, HISPANIC, AND MIXED NEIGHBORHOODS

(WAVE 7 RESPONDENTS, N = 402)

Ethnic Group	(1) Black Neighborhoods (N = 132)	(2) Hispanic Neighborhoods (N = 37)	(3) White Neighborhoods (N = 105)	(4) Mixed Neighborhoods (N = 128)	(5) Total Neighborhoods (N = 402)
1. % Black	81.1	0.0	1.9	15.6	32.1
2. % Hispanic	0.8	86.5	2.9	5.5	10.7
3. % White & other	18.2	13.5	95.2	78.9	57.2
TOTAL	100	100	100	100	100

TABLE 3.1

Attitudinal Measures of Fear of Crime

(Scale Values in Parenthesis)

A. Limit activities because of crime (LIM):

LIM_1	In general, have you limited or changed your activities in the past few years because of crime?	YES (1) NO (0)
LIM_2	Do you think that most people in this neighborhood have limited or changed their activities in the past few years because they are afraid of crime?	YES (1) NO (0)
LIM_3	Do you think people in general have been limited or changed their activities in the past few years because they are afraid of crime?	YES (1) NO (0)

B. Perceived increase in crime (PI):

PI_1	Which of the following statements do you agree with most? Crime is less serious than the newspapers and TV say. Crime is more serious than the newspapers and TV say. Crime is about as serious as the newspapers and TV say.	LESS SERIOUS (0) MORE SERIOUS (2) AS SERIOUS (1) DON'T KNOW
PI_2	Which of the following statements do you agree with most? My chances of being attacked or robbed have gone up in the past few years. My chances of being attacked or robbed have gone down in the past few years. My chances of being attacked or robbed haven't changed in the past few years.	UP (2) DOWN (0) SAME (1) DON'T KNOW
PI_3	Now I'd like to get your opinions about crime in general. Within the past year or two, do you think that crime in your neighborhood has increased, decreased, or remained about the same?	INCREASED (2) DECREASED (0) REMAINED SAME (1) NOT HERE LONG ENOUGH DON'T KNOW
PI_4	Within the past year or two do you think that crime in the United States has increased, decreased, or remained about the same?	INCREASED (2) DECREASED (0) REMAINED THE SAME (1) DON'T KNOW

TABLE 3.1 (Cont'd)

C. Fear areas of city (FC):

FC_1	Are there some parts of this metropolitan area where you have a reason to go or would like to go during the day, but are afraid to because of fear of crime?	YES (1) NO (0)
FC_2	How about at night, are there some parts of this area where you have a reason to go or would like to go but are afraid to because of fear of crime?	YES (1) NO (0)

D. Fear neighborhood (FN):

FN_1	How do you think your neighborhood compares with others in this metropolitan area in terms of crime? Would you say it is much more dangerous, more dangerous, about average, less dangerous or much less dangerous?	MUCH MORE DANGEROUS (4) MORE DANGEROUS (3) ABOUT AVERAGE (2) LESS DANGEROUS (1) MUCH LESS DANGEROUS (0)
FN_2	How about during the day--how safe do you or would you feel being out alone in your neighborhood? Would you feel very safe, reasonably safe, somewhat unsafe, very unsafe?	VERY SAFE (3) REASONABLY SAFE (2) SOMEWHAT UNSAFE (1) VERY UNSAFE (0)
FN_3	How safe do you feel or would you feel being out alone in your neighborhood at night? Would you say it is very safe, reasonably safe, somewhat unsafe, very unsafe?	VERY SAFE (3) REASONABLY SAFE (2) SOMEWHAT UNSAFE (1) VERY UNSAFE (0)

E. Perceived likelihood of victimization (PV):

Think of a scale from 0-10. Zero stands for no possibility at all and ten stands for extremely likely. For each statement I read, give me a rating from 0 to 10. During the course of a year how likely is it that...

PV_1	someone would break into your residence when no one is home?
PV_2	someone would break into your residence when someone is home?
PV_3	your purse/wallet would be snatched in your neighborhood?
PV_4	someone would take something from you on the street by force or threat in your neighborhood?
PV_5	someone would beat you up or hurt you on the street in your neighborhood?

TABLE 3.2

Measures of Attitudes Toward Criminal Justice System

(Scale values in parenthesis)

A. Evaluation of police services (PS):

PS_1	Would you say, in general, that your local police are doing a good job, an average job or a poor job?	Good Average Poor Don't know	(2) (1) (0)
PS_2	Some people say the police don't come quickly when you call them for help. Do you think this happens to people in this neighborhood?	Yes (2) No (0) Don't know	(2) (0) (1)
PS_3	Overall, how satisfied are you with the police protection provided in this neighborhood. Generally satisfied, somewhat satisfied, very dissatisfied?	Generally Satisfied Somewhat Dissatisfied Very Dissatisfied Don't Know	(2) (1) (0)
PS_4	Of all the crimes that are reported in this neighborhood, how many would you guess are solved or cleared up? Most of them, about half, about a quarter, or fewer than that?	Most are solved About Half About a Quarter Fewer Than Than Don't Know	(2) (2) (1) (0)

B. Perceptions of police abuse (PA):

PA_1	Some people say the police frisk or search people without good reason. Do you think this happens to people in this neighborhood?	Yes No Don't Know	(2) (0) (1)
PA_2	Some people say the police don't show respect for people or they use insulting language. Do you think this happens to people in this neighborhood?	Yes No Don't Know	(2) (0) (1)
PA_3	Some people say the police rough up people unnecessarily when they are arresting them or afterwards. Do you think this happens to people when they are in this neighborhood?	Yes No Don't Know	(2) (0) (1)
PA_4	Do you agree or disagree with the statement that all people are treated the same by the police regardless of race?	Agree Disagree Not Sure	(2) (0) (1)

TABLE 3.2 (Cont'd)

C. Effectiveness of Criminal Justice System (CE):

CE_1	Of all the crimes that are reported in this neighborhood, how many would you guess are solved or cleared up? Most of them, about half, about a quarter, or fewer than that?	Most are Solved About Half About a Quarter Fewer Than That Don't Know	(2) (2) (1) (0)
CE_2	Of all the crimes that are reported in this neighborhood, how many would you guess lead to someone actually being arrested? Most of them, about half, about a quarter, or fewer than that?	Most Lead to All Arrests About Half About a Quarter Fewer than a Quarter Don't Know	(3) (2) (1) (0)
CE_3	Of all the crimes that are reported in this neighborhood, how many would you guess lead to someone actually going to trial. Most of them, about half, about a quarter, or fewer than that?	Most Result in a Trail About Half About a Quarter Fewer Than That Don't Know	(3) (2) (1) (0)
CE_4	Of all the crimes that are reported in this neighborhood, how many would you guess lead to someone actually going to prison? Most of them, about half, about a quarter, or fewer than that?	Most Result in Prison Sentence About Half About a Quarter Fewer Than That Don't Know	(3) (2) (1) (0)

D. Perceptions of Leniency in Criminal Justice System (CL):

CL_1	Generally, do you feel the courts have been too easy in dealing with criminals, too severe, or do you feel they have treated them fairly?	Too Easy To severe Fair It Varies or Don't Know	(0) (2) (1)
CL_2	Should parole boards be more strict, less strict, or about the same they are now in granting parole?	More Strict Less Strict About the Same Don't Know	(0) (2) (1)
CL_3	On the whole do you feel that the ruling of the Supreme Court in recent years have given too much consideration to the rights of people suspected of crimes?	Yes No Don't Know	(2) (0) (1)
CL_4	Do you strongly agree, agree, disagree or strongly disagree with the following statement? There are so many loopholes in the law that it is difficult to bring criminals to justice.	Strongly Agree Agree Disagree Strongly Disagree Don't Know	(4) (3) (1) (0)

Table 3.3 Measurement Characteristics of "Fear of Crime" Items, Feat-1234 Subsample (N = 264)

[illegible]able Description (Range[illegible])	μ_{ij} Mean				σ_{ij} Observed Standard Deviation [σ_{T_j} "True" Standard Deviation]				$\sigma_{e_{ij}}$ Error Standard Deviation				λ_{ij} Relative True Score Slope				$\rho_{ii'}$ Reliability				$\rho_{x_t,x_{t-1}}$ - Observed Lag-1 Correlation [$\rho_{T_t,T_{t-1}}$ - "True" Lag-1 Correlation]			$\rho_{e_t e_{t-1}}$ Error Lag-1 Correlation		
(1)	W1 (2)	W2 (3)	W3 (4)	W4 (5)	W1 (6)	W2 (7)	W3 (8)	W4 (9)	W1 (10)	W2 (11)	W3 (12)	W4 (13)	W1 (14)	W2 (15)	W3 (16)	W4 (17)	W1 (18)	W2 (19)	W3 (20)	W4 (21)	W2 (22)	W3 (23)	W4 (24)	W2 (25)	W3 (26)	W4 (27)
LIMIT ACTIVITIES BECAU[illegible]IME																										
LM1 Respondent (0-1)[b]	.43	.39	.38	.42	.50	.[illegible]	.49	.49	.29	.31	.28	.27	1.00	1.00	1.00	1.00	.65	.59	.66	.71	.60	.69	.82	.30	.37	.60
LM2 Neighbors (0-1)	.52	.40	.47	.49	.50	.50	.50	.50	.27	.24	.23	.22	1.06	1.17	1.11	1.09	.72	.78	.79	.82	.62	.72	.77	.20[ns]	.22[ns]	.16[ns]
LM3 People in general [illegible]	.03	.03	.03	.03	.38	.37	.37	.39	.35	.34	.33	.34	.36	.42	.42	.47	.15	.18	.20	.25	.33	.37	.59	.24	.26	.49
					[.40]	[.38]	[.40]	[.41]													[.76]	[.87]	[.92]			
PERCEIVED INCREASE IN [illegible]																										
I1 More or less serio[illegible] media say (0-2)[b]	1.42	1.36	1.32	1.38	.62	.67	.63	.60	.54	.63	.60	.53	1.00	1.00	1.00	1.00	.23	.09	.08	.20	.46	.53	.56	.39	.48	.50
I2 My chances of be[illegible]ized have gone up (0-2)	1.61	1.55	1.51	1.51	.59	.50	.61	.55	.44	.46	.41	.41	1.35	1.81	2.49	1.36	.46	.39	.55	.44	.56	.43	.54	.33	.13[ns]	.13[ns]
I3 Crime in neighbor[illegible] [illegible]reased or decreased (0-2)	1.33	1.33	1.27	1.30	.63	.59	.61	.61	.55	.50	.51	.52	1.50	1.53	1.86	1.26	.22	.27	.31	.30	.49	.61	.70	.35	.54	.59
I4 Crime in U.S. incre[illegible] or decreased (0-2)	1.05	1.79	1.84	1.80	.42	.45	.40	.44	.40	.41	.37	.39	.42	.02	.03	.80	.09	.13	.14	.24	.59	.53	.59	.56	.48	.51
					[.30]	[.20]	[.10]	[.27]													[.89]	[.87]	[.97]			
FEAR AREAS OF CITY																										
C1 During the day (0-[illegible])	.41	.41	.42	.40	.49	.49	.50	.49	.37	.41	.38	.39	1.00	1.00	1.00	1.00	.43	.31	.39	.38	.55	.62	.66	.39	.40	.48
C2 At night (0-1)	.63	.69	.70	.68	.48	.46	.46	.47	.25	.23	.26	.22	1.29	1.44	1.19	1.36	.75	.74	.66	.76	.56	.70	.81	-.19[ns]	.29[ns]	.46[ns]
					[.32]	[.20]	[.31]	[.30]													[.83]	[.80]	[.94]			
FEAR NEIGHBORHOOD																										
N1 This neighborhood [illegible] to others in metropolitan area [illegible]	1.51	1.51	1.50	1.45	.93	.99	.94	.93	.73	.78	.67	.71	.82	.90	.96	.77	.40	.37	.48	.42	.64	.72	.68	.50	.55	.47
N2 Safe in area during the day (0-3)	2.29	2.32	2.33	2.33	.73	.63	.69	.65	.49	.45	.40	.40	-.75	-.66	-.74	-.66	.55	.50	.53	.63	.51	.58	.59	.14[ns]	.17	.13[ns]
N3 Safe in area at [illegible] (0-3)[b]	1.69	1.61	1.69	1.69	.89	.87	.86	.89	.52	.54	.53	.43	-1.00	-1.00	-1.00	-1.00	.66	.61	.63	.76	.66	.73	.76	.20	.34	.38
					[.72]	[.68]	[.68]	[.78]													[.87]	[.97]	[.94]			
PERCEIVED LIKELIHOOD OF [illegible]IZATION																										
V1 Break-in, no-one [illegible] (0-10)[b]	4.74	4.52	4.25	3.57	3.13	2.74	2.87	2.65	2.75	2.25	2.28	2.18	1.00	1.00	1.00	1.00	.17	.30	.30	.28	.59	.73	.60	.55	.62	.45
V2 Break-in, someone [illegible] (0-10)	1.94	1.98	1.83	1.77	2.34	2.28	2.10	2.06	1.94	1.98	1.64	1.57	.97	.75	.80	.88	.29	.24	.35	.36	.51	.62	.67	.26	.55	.54
V3 Purse-wallet snat[illegible] (0-10)	2.98	3.11	3.00	2.55	3.09	2.84	2.82	2.56	1.86	1.55	1.34	1.37	1.87	1.57	1.57	1.54	.62	.69	.75	.72	.56	.70	.70	.26	.36	.33
V4 Robbed by force [illegible] (0-10)	2.63	2.94	2.88	2.41	2.80	2.76	2.55	2.44	1.29	1.08	.96	.80	1.91	1.69	1.51	1.63	.79	.85	.86	.89	.53	.69	.68	-.02[ns]	-.15[ns]	.00[ns]
V5 Beat up on street [illegible]	2.19	2.30	2.25	2.17	2.70	2.41	2.37	2.31	1.39	1.30	1.35	1.06	1.79	1.34	1.26	1.45	.74	.70	.69	.79	.56	.59	.65	.19	.08[ns]	.27
					[1.30]	[1.50]	[1.56]	[1.41]													[.69]	[.62]	[.80]			

Notes:

[illegible]: See table 3.1 for [illegible] description.

[illegible]: Slope for reference [illegible]ator set at 1.0.

[illegible]: Model includes wi[illegible] error correlations between FN_2 and FN_3. Three of the four correlations were not significant.

[illegible]: Adding error corre[illegible] among PV_1 and PV_2 and among PV_3, PV_4, and PV_5 reduces chi-square to 235.2 with 134 degrees of freedom (p=.000)

[illegible]: Estimated coeffic[illegible] than twice its standard error.

Table 3.4 Measurement Characteristics of "Fear of Crime" Items, Fear 1467 Subsample (N = 255)

Variable Description (Item)[a]	μ_{tj} Mean				σ_{tj} Standard Deviation				$\sigma_{e_{tj}}$ Error Standard Deviation				λ_{tj} Relative True Score Slope				$\rho_{ttj'}$ Reliability				$\rho_{x_t, x_{t-1}}$ Observed Lag-1 Correlation			$\rho_{e_t, e_{t-1}}$ Error Lag-1 Correlation			$\sigma_{x'}$	λ'
	W1	W4	W6	W7	W1	W4	W6	W7	W1	W4	W6	W7	W1	W4	W6	W7	W1	W4	W6	W7	W4	W6	W7	W4	W6	W7		
	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)		
1. LIMIT ACTIVITIES b/c FEAR OF CRIME																												
LIM_1 Respondent (0-1)[b]	.43	.40	.41	.42	.50	.49	.49	.49	.27	.26	.23	.20	1.00	1.00	1.00	1.00	.68	.72	.79	.84	.63	.77	.81	.29	.51	.40		
LIM_2 Neighbors (0-1)	.52	.50	.51	.52	.50	.50	.50	.50	.29	.22	.24	.23	.99	1.00	1.01	.91	.67	.81	.77	.70	.56	.77	.78	-.03[ns]	.42	.40	23.1	
LIM_3 People in gen. (0-1)	.82	.81	--	--	.39	.39	--	--	.35	.35	--	--	.39	.45	--	--	.17	.23	--	--	.31	.31	--	.19	--	--		
					[.41]	[.42]	[.44]	[.45]													[.77]	[.86]	[.83]					
2. PERCEIVED INCREASE IN CRIME																												
PI_1 More or less crime than media say (0-2)[b]	1.41	1.30	--	--	.66	.60	--	--	.57	.55	--	--	.71	.55	--	--	.21	.16	--	--	.58	--	--	.52	--	--		
PI_2 My chances of being victimized have gone up (0-2)	1.60	1.56	1.56	1.52	.59	.56	.58	.57	.41	.36	.35	.30	1.00	1.00	1.00	1.00	.51	.59	.64	.73	.53	.61	.68	.03[ns]	.02[ns]	.05[ns]	54.21	
PI_3 Crime in neighborhood increased or decreased (0-2)	1.35	1.32	1.33	1.41	.65	.63	.62	.61	.59	.53	.52	.52	.65	.60	.73	.65	.18	.29	.29	.27	.43	.65	.65	.26	.49	.51		
PI_4 Crime in U.S. increased or decreased (0-2)	1.84	1.80	--	--	.44	.46	--	--	.42	.38	--	--	.35	.58	--	--	.11	.29	--	--	.56	--	--	.51	--	--		
3. FEAR AREAS OF CITY					[.42]	[.43]	[.46]	[.49]													[.88]	[.98]	[.97]					
FC_1 During the day (0-1)[b]	.39	.39	.38	.41	.49	.49	.49	.49	.37	.38	.35	.35	1.00	1.00	1.00	1.00	.44	.41	.48	.49	.47	.68	.79	.26[ns]	.55	.65	9.7	
FC_2 At night (0-1)	.63	.68	.67	.67	.48	.47	.47	.47	.27	.28	.30	.29	1.24	1.20	1.06	1.08	.70	.63	.58	.62	.57	.77	.02	.11[ns]	.65	.65		
4. FEAR NEIGHBORHOOD					[.33]	[.31]	[.34]	[.34]													[.79]	[.84]	[.94]					
FN_1 This neighborhood compared to others in metropolitan area (0-4)	1.52	1.45	1.50	1.56	.93	.94	.96	.88	.72	.71	.73	.63	.80	.77	.81	.01	.37	.40	.39	.48	.63	.70	.69	.45	.51	.49		
FN_2 Safe in area during the day (0-3)	2.29	2.29	2.27	2.26	.73	.65	.68	.67	.51	.43	.47	.46	-.73	-.64	-.66	-.67	.50	.58	.52	.56	.59	.62	.62	.25	.32	.30	64.5	0.5
FN_3 Safe in area at night (0-3)	1.66	1.70	1.69	1.67	.89	.86	.85	.87	.55	.39	.43	.44	-1.00	-1.00	-1.00	-1.00	.63	.81	.75	.74	.67	.76	.01	.12[ns]	.26[ns]	.50		
5. PERCEIVED LIKELIHOOD OF VICTIMIZATION					[.71]	[.77]	[.74]	[.75]													[.90]	[.91]	[.92]					
PV_1 Break-in (some home) (0-10)[b]	4.81	3.61	--	4.41	3.15	2.75	--	2.84	2.63	2.20	--	2.12	1.00	1.00	--	1.00	.27	.36	--	.39	.40	--	.57[c]	.25	--	.42[c]		
PV_2 Break-in, others close (0-10)	2.07	1.87	--	2.39	2.49	2.11	--	2.48	2.04	1.44	--	1.81	.83	.89	--	.94	.34	.49	--	.46	.49	--	.58[c]	.33	--	.20[c]		
PV_3 Purse/Wallet snatched (0-10)	3.11	2.58	--	2.90	3.23	2.59	--	2.67	1.93	1.31	--	1.13	1.55	1.35	--	1.36	.63	.75	--	.82	.47	--	.66[c]	.13[ns]	--	.13[c,ns]	239.2	
PV_4 Robbed by force on street (0-10)	2.78	2.38	--	2.77	2.86	2.39	--	2.33	1.28	.81	--	.82	1.56	1.35	--	1.22	.80	.88	--	.87	.46	--	.69[c]	-.08[ns]	--	-.00[c,ns]		
PV_5 Beat up on street (0-10)	2.28	2.13	--	2.67	2.76	2.31	--	2.32	1.43	.99	--	.93	1.45	1.25	--	1.20	.73	.81	--	.85	.52	--	.63[c]	.13[ns]	--	.19[c]		
					[1.63]	[1.66]		[1.78]													[.61]		[.80][c]					

NOTES:

a: See table 3.1 for a full description.

b: Slope for reference indicator set at 1.0

c: Intertemporal correlation between waves 4 and 7.

ns: Estimated coefficient less than twice its standard error.

TABLE 3.5

Within-Wave Correlations

Among "Fear of Crime" Dimensions

(N = 255)

WAVE 1

	Variable	LIM 1	PI 2	FC 3	FN 4	PV 5
1.	LIM-Limit Activities	--				
2.	PI-Perceived Increase	.30	--			
3.	FC-Fear City	.46	.33	--		
4.	FN-Fear Neighborhood	.44	.48	.52	--	
5.	PV-Perceived Victimization	.42	.53	.42	.70	--

$x^2_{103} = 175.89$ p = .0000

WAVE 4

	Variable	LIM 1	PI 2	FC 3	FN 4	PV 5
1.	LIM-Limit Activities	--				
2.	PI-Perceived Increase	.53	--			
3.	FC-Fear City	.56	.38	--		
4.	FN-Fear Neighborhood	.68	.38	.59	--	
5.	PV-Perceived Victimization	.38	.37	.38	.62	--

$x^2_{103} = 196.05$ p = .0000

WAVE 6

	Variable	LIM 1	PI 2	FC 3	FN 4
1.	LIM-Limit Activities	--			
2.	PI-Perceived Increase	.53	--		
3.	FC-Fear City	.67	.50	--	
4.	FN-Fear Neighborhood	.61	.55	.62	--

$x^2_{19} = 17.39$ p = .56

TABLE 3.5 (Cont'd)

WAVE 7

	Variable	LIM 1	PI 2	FC 3	FN 4	PV 5
1.	LIM-Limit Activities	--				
2.	PI-Perceived Increased	.64	--			
3.	FC-Fear City	.63	.51	--		
4.	FN-Fear Neighborhood	.67	.55	.66	--	
5.	PV-Perceived Victimization	.55	.46	.54	.77	--

$x^2_{61} = 105.39$ p = .0004

TABLE 3.6 Measurement Characteristics of "Criminal Justice" Items, Crim-1234 Subsample (N = 188)

Variable Description (Range)[a]	μ_{ij} Mean				σ_{ij} Standard Deviation				$\sigma_{e_{ij}}$ Error Standard Deviation				λ_{ij} Relative True Score Slope				$\rho_{ii'}$ Reliability				$\rho_{x_t x_{t-1}}$ Observed Lag-1 Correlation			$\rho_{e_t e_{t-1}}$ Error Lag-1 Correlation		
(1)	W1 (2)	W2 (3)	W3 (4)	W4 (5)	W1 (6)	W2 (7)	W3 (8)	W4 (9)	W1 (10)	W2 (11)	W3 (12)	W4 (13)	W1 (14)	W2 (15)	W3 (16)	W4 (17)	W1 (18)	W2 (19)	W3 (20)	W4 (21)	W2 (22)	W3 (23)	W4 (24)	W2 (28)	W3 (29)	W4
1. POLICE SERVICE																										
PS_1 Police Performance (0-2)	1.14	1.32	1.27	1.30	.71	.62	.62	.60	.49	.42	.41	.38	1.00	1.00	1.00	1.00	.50	.52	.53	.60	.66	.74	.77	.40	.47	
PS_2 Police Response Time (0-2)	.93	1.01	.90	.91	.95	.98	.99	.99	.75	.74	.73	.69	-1.14	-1.45	-1.47	-1.52	.37	.43	.45	.51	.60	.69	.71	.41	.45	
PS_3 Police Protection (0-2)	1.41	1.44	1.48	1.47	.69	.62	.57	.64	.41	.45	.35	.43	1.10	.96	.99	1.02	.65	.48	.62	.55	.55	.60	.54	.16[ns]	.17[ns]	
PS_4 % Crimes Solved (0-3)	1.20	1.44	1.49	1.35	1.05	1.10	1.05	1.04	.96	1.04	.98	.99	.88	.85	.75	.71	.18	.12	.13	.10	.54	.68	.71	.50	.64	
					[.50]	[.44]	[.45]	[.47]													[.88]	[.97]	[.93]			
2. POLICE ABUSE																										
PA_1 Frisk W/O Cause (0-2)	.70	.69	.64	.60	.89	.93	.93	.94	.60	.59	.62	.46	1.00	1.00	1.00	1.00	.53	.57	.54	.75	.61	.59	.74	.31	.01[ns]	
PA_2 No Respect (0-2)	.66	.68	.71	.76	.91	.95	.95	.97	.60	.51	.46	.47	1.06	1.14	1.13	1.05	.57	.71	.78	.78	.51	.70	.70	.06[ns]	.12[ns]	
PA_3 Rough Up (0-2)	.68	.66	.70	.70	.90	.93	.95	.96	.44	.47	.46	.39	1.21	1.15	1.01	1.08	.76	.75	.75	.83	.65	.72	.70	.17[ns]	.13[ns]	
PA_4 Treat Races Equally (0-2)	.46	.52	.54	.45	.80	.87	.87	.82	.70	.82	.84	.80	-1.24	-.38	-.35	-.20	.04	.09	.05	.04	.41	.60	.60	.37	.57	
					[.65]	[.70]	[.68]	[.81]													[.80]	[.92]	[.88]			
3. CRIMINAL JUSTICE EFFECTIVENESS																										
CE_1 % Crimes Solved (0-3)	1.20	1.44	1.49	1.35	1.05	1.10	1.05	1.04	.86	.78	.79	.82	1.00	1.00	1.00	1.00	.28	.43	.43	.39	.54	.68	.71	.43	.51	
CE_2 % Arrests (0-3)	1.05	1.21	1.18	.99	1.03	1.14	1.07	1.01	.63	.54	.63	.67	1.45	1.39	1.23	1.18	.60	.78	.66	.57	.50	.65	.55	.14[ns]	.03[ns]	
CE_3 % Trial (0-3)	.77	1.04	.88	.87	1.03	1.13	.97	.97	.52	.62	.44	.42	1.61	1.31	1.35	1.35	.74	.71	.78	.81	.49	.61	.63	-.07[ns]	-.11[ns]	
CE_4 % Prison (0-3)	.43	.51	.57	.55	.81	.84	.82	.86	.56	.64	.52	.53	1.08	.77	.42	1.03	.54	.44	.59	.61	.41	.47	.58	.13[ns]	.11[ns]	
					[.55]	[.72]	[.64]	[.65]													[.71]	[.85]	[.87]			
4. CRIMINAL JUSTICE LENIENCY																										
CL_1 Courts (0-7)	.29	.25	.26	.32	.58	.48	.49	.56	.44	.37	.30	.39	1.00	1.00	1.00	1.00	.42	.43	.60	.52	.54	.61	.60	.34	.43	
CL_2 Parole Boards (0-2)	.25	.23	.24	.25	.51	.48	.52	.50	.42	.35	.43	.35	.77	1.04	1.09	.88	.33	.46	.31	.51	.54	.60	.64	.36	.43	
CL_3 Supreme Court (0-2)	1.36	1.49	1.40	1.44	.90	.86	.92	.89	.80	.75	.78	.76	-1.07	-1.34	-1.14	-1.16	.20	.24	.26	.28	.49	.53	.53	.40	.42	
CL_4 Longnoles (0-4)	3.42	3.32	3.23	3.40	.79	.90	.89	.81	.70	.80	.76	.73	-.96	-1.23	-1.26	-.86	.21	.18	.29	.18	.38	.39	.50	.26	.23	
					[.38]	[.31]	[.38]	[.40]													[.03]	[.81]	[.89]			

NOTES:

a: See Table 3.2 for a full description.

b: Slope for reference indicator set at 1.0.

ns: Estimated coefficient less than twice its standard error.

TABLE 3.7 Measurement Characteristics of "Criminal Justice" Items, Crim-1467 Subsample (N = 182)

Variable Description (Range)[a]	μ_{tj}-Mean				σ_{tj}-Observed Standard Deviation [σ_{T_j}-"True" Standard Deviation]				$\sigma_{e_{tj}}$-Error Standard Deviation				λ_{tj}-Relative True Score Slope				$\rho_{tt'}$-Reliability				$\rho_{X_{t+1}X_{t-1}}$-Observed Lag-1 Correlation / $\rho_{T_t T_{t-1}}$-"True" Lag-1 Correlation			$\rho_{e_{t+1}e_{t-1}}$-Error Lag-1 Correlation			Chi-Square Goodness of Fit
(1)	W1 (2)	W4 (3)	W6 (4)	W7 (5)	W1 (6)	W4 (7)	W6 (8)	W7 (9)	W1 (10)	W4 (11)	W6 (12)	W7 (13)	W1 (14)	W4 (15)	W6 (16)	W7 (17)	W1 (18)	W4 (19)	W6 (20)	W7 (21)	W4 (22)	W6 (23)	W7 (24)	W4 (25)	W6 (26)	W7 (27)	
1. POLICE SERVICE																											
PS_1 Police Performance (0-2)	1.15	1.34	1.34	1.31	.72	.61	.50	.57	.51	.40	.36	.30	1.00	1.00	1.00	1.00	.50	.57	.62	.66	.62	.77	.01	.30	.52	.54	
PS_2 Police Response Time (0-2)	.90	.91	.90	.92	.96	.99	.99	.99	.76	.69	.76	.67	-1.13	-1.54	-1.42	-1.72	.36	.51	.42	.55	.58	.69	.00	.38	.49	.63	
PS_3 Police Presence (0-2)	1.41	1.49	1.43	1.46	.70	.65	.63	.64	.41	.38	.47	.40	1.13	1.13	.93	1.17	.60	.65	.45	.61	.57	.66	.66	$.07^{ns}$	.32	.30	90.7
PS_4 % Crime Solved (0-3)	1.20	1.34	1.23	1.27	1.00	1.05	1.02	1.02	.97	1.01	.99	.98	.92	.68	.55	.62	.19	.09	.06	.07	.47	.67	.64	.44	.64	.62	
					[.51]	[.46]	[.46]	[.43]													[.03]	[.94]	[1.00]				
2. POLICE ABUSE																											
PA_1 Frisk w/o Reason (0-2)	.64	.65	.60	.73	.88	.94	.94	.96	.60	.43	.46	.40	1.00	1.00	1.00	1.00	.51	.70	.74	.82	.58	.81	.82	.27	.35	.26	
PA_2 No Respect (0-2)	.69	.71	.60	.74	.88	.96	.94	.97	.69	.46	.44	.47	1.04	1.02	1.03	.90	.66	.70	.70	.77	.45	.76	.77	$.07^{ns}$	.29	$.06^{ns}$	
PA_3 Rough Up (0-2)	.64	.69	.63	.71	.89	.95	.92	.96	.93	.36	.39	.32	1.31	1.07	1.03	1.04	.86	.87	.82	.89	.57	.74	.87	$.02^{ns}$	-.28	.26	170.7
PA_4 Treat Races Equally (0-2)	.48	.52	.50	.54	.81	.87	.87	.88	.79	.83	.81	.82	-.27	-.25	-.31	-.32	.04	.06	.08	.10	.39	.77	.75	.35	.74	.72	
					[.63]	[.83]	[.81]	[.87]													[.68]	[.92]	[.97]				
3. CRIMINAL JUSTICE EFFECTIVENESS																											
CE_1 % Crimes Solved (0-3)	1.20	1.34	1.23	1.27	1.08	1.05	1.02	1.02	.93	.88	.83	.79	1.00	1.00	1.00	1.00	.18	.27	.32	.38	.47	.67	.64	.42	.57	.48	
CE_2 % Arrests (0-2)	.96	.96	.95	.93	1.02	1.01	.99	.98	.62	.63	.50	.59	1.76	1.36	1.44	1.26	.62	.55	.69	.65	.35	.51	.72	$.10^{ns}$	$-.01^{ns}$	.37	
CE_3 % Trial (0-2)	.70	.87	.81	.73	.99	1.00	1.00	.96	.50	.41	.52	.50	1.04	1.64	1.48	1.32	.72	.82	.72	.75	.39	.60	.62	$-.05^{ns}$	$-.14^{ns}$	$-.16^{ns}$	170.0
CE_4 % Prison (0-3)	.40	.58	.55	.50	.81	.87	.83	.81	.62	.49	.53	.56	1.16	1.26	1.08	.90	.43	.63	.56	.48	.40	.69	.61	.29	.38	.23	
					[.46]	[.55]	[.57]	[.63]													[.50]	[.84]	[.92]				
4. CRIMINAL JUSTICE LENIENCY																											
CL_1 Courts (0-4)	.27	.29	.26	.32	.56	.53	.50	.54	.44	.35	.32	.36	1.00	1.00	1.00	1.00	.34	.57	.58	.57	.42	.66	.70	$.12^{ns}$	.27	.31	
CL_2 Parole Boards (0-2)	.23	.25	.21	.27	.49	.50	.49	.53	.39	.35	.36	.39	.92	.91	.86	.69	.30	.53	.46	.47	.51	.84	.70	.31	.73	.45	
CL_3 Supreme Court (0-2)	1.32	1.46	1.54	1.46	.91	.88	.83	.88	.78	.76	.68	.61	-1.47	-1.13	-1.25	-1.57	.28	.26	.33	.52	.45	.59	.72	.30	.44	.54	146.5
CL_4 Loopholes (0-4)	3.49	3.43	3.27	3.16	.78	.81	.91	.95	.71	.74	.81	.83	-1.06	-.85	-1.13	-1.20	.20	.18	.23	.26	.35	.57	.59	.27	.49	.47	
					[.33]	[.40]	[.38]	[.41]													[.80]	[.95]	[1.00]				

NOTES

a: See Table 3.2 for a full description.

b: Slope for reference indicator set at 1.0.

ns: Estimated coefficient less than twice its standard error.

TABLE 3.8

Within-Wave Correlations

Among "Criminal Justice" Dimensions

(N = 188)

WAVE 1

	Variable	PS 1	PA 2	CE 3	CL 4
1.	PS-Police Service	--			
2.	PA-Police Abuse	-.66	--		
3.	CE-Criminal Justice Eff.	.09	.06	--	
4.	CL-Criminal Justice Len.	-.10	.22	.18	--

$x^2_{97} = 125.24$ p = .03

WAVE 4

	Variable	PS 1	PA 2	CE 3	CL 4
1.	PS-Police Service	--			
2.	PA-Police Abuse	-.66	--		
3.	CE-Criminal Justice Eff.	-.09	.17	--	
4.	CL-Criminal Justice Len.	-.12	.23	.22	--

$x^2_{97} = 148.33$ p = .0006

WAVE 6

	Variable	PS 1	PA 2	CE 3	CL 4
1.	PS-Police Service	--			
2.	PA-Police Abuse	-.66	--		
3.	CE-Criminal Justice Eff.	-.06	.10	--	
4.	CL-Criminal Justice Len.	-.12	.25	.34	--

$x^2_{97} = 153.64$ p = .0002

TABLE 3.8 (Cont'd)

WAVE 7

	Variable	PS 1	PA 2	CE 3	CL 4
1.	PS-Police Service	--			
2.	PA-Police Abuse	-.65	--		
3.	CE-Criminal Justice Eff.	-.06	.06	--	
4.	CL-Criminal Justice Len.	.07	.18	.22	--

$x^2_{97} = 136.90$ p = .005

TABLE 4.1

Measured Endogenous Determinants of Attitudes Toward

Crime and Criminal Justice

Variable	Description (Range)	Questionnaire Items (See Appendix 2)
1. Property Crime, Ever	Of property crimes, how many had happened to respondent, ever (0-5).	163, 165, 173, 175, 177 (Wave 1 only)
2. Personal Crime, Ever	Of 3 personal crimes, how many had happened to respondent, ever (0-3)	167, 169, 171 (Wave 1 only)
3. #Different Crimes, Year	Of 8 violent crimes, how many had occurred in neighborhood, past year (0-8)	33 (Wave 1 only)
4. Others Victimized, Year	Of 5 personal and property crimes, how many had happened to others in the household, past year (0-5)	29 (Wave 1 only)
5. Police Assistance, Year	Respondent was assisted by police during the past year (0=No, 1=Yes)	31 A,B (Wave 1 only)
6. Police Prosecution, Year	Other than traffic citation, respondent was arrested, prosecuted, or jailed within past year (0=No, 1=Yes)	31 D,E,G (Wave 1 only)
7. Minor Bad	How many of the following happened to someone in the household during the past month (two weeks): illness, cheated, trouble paying bills, lost a promotion, argued with spouse, accident (0-4)	25 A,C,F; 26 D,H,N
8. Minor GOOD	How many of the following happened to someone in the household during the past month (two weeks): won prize, received gift, good trip, nice time with friends (0-4)	25 B,D,G,H
9. Major Bad	How many of the following happened to someone in the household during the past (two weeks): fired, seperated, friend died (0-2)	26 C,G,O

TABLE 4.1 (Cont'd)

Variable	Description (Range)	Questionnaire Items (See Appendix 2)
10. Major Good	How many of the following happened to someone in the household during the past month (two weeks): hired, raise, married, birth, child's graduation (0-4)	26 A,B,F,J,M
11. Victimization	Of 8 personal and property crimes, how many happened to respondent during the past month (two weeks) (0-3)	28
12. Crime	Of 8 violent crimes, how many had occurred in neighborhood during the past month (two weeks) (0-7)	33

TABLE 4.2

Reduced Form (Total) Effects of Socioeconomic Characteristics on "Fear of Crime."

(Attitudes are measured in standard deviation units; metric coefficients appear in parentheses.

Exogenous Variable	Limit Activities (LA)			Perceived Increase (PI)			Fear City (FC)			Perceived Likelihood of Victimization (PV)		
	W1	W4	W7	W1	W4	W7	W1	W4	W7	W1	W4	W7
Age (10's)	.002 (.001)	.002 (.001)	.005 (.002)	.007 (.002)	.000 (.000)	.007 (.003)	.020 (.008)	.014 (.005)	.013 (.005)	.004 (.007)	.006 (.010)	.003 (.007)
Gender	-.265 (-.108)	-.476 (-.199)	-.383 (-.169)	-.311 (-.095)	-.451 (-.173)	-.402 (-.171)	-.468 (-.192)	-.609 (-.212)	-.568 (-.223)	-.448 (-.737)	-.485 (-.801)	-.489 (-1.106)
Minn	-.834 (-.340)	-.576 (-.241)	-.736 (-.325)	-.515 (-.157)	-.370 (-.142)	-.188 (-.080)	.005 (.002)	-.103 (-.036)	-.102 (-.040)	-.364 (-.600)	-.296 (-.490)	-.439 (-.994)
Hou	.059 (.024)	.170 (.071)	.009 (.004)	.262 (.080)	.462 (.177)	.571 (.243)	.117 (.048)	.279 (.097)	.224 (.088)	.052 (.086)	.088 (.146)	-.064 (-.147)
Wash	-.592 (-.241)	-.388 (-.162)	-.645 (-.285)	-.469 (-.143)	-.527 (-.202)	-.346 (-.147)	.290 (.119)	.348 (.121)	.329 (.129)	-.193 (-.318)	-.117 (-.433)	-.298 (-.675)
Hispanic	-.216 (-.088)	.359 (.150)	.292 (.129)	-.554 (-.169)	-.130 (-.050)	.038 (.016)	.273 (.112)	.362 (.126)	.349 (.137)	.837 (1.378)	.551 (.911)	.707 (1.599)
Black	.427 (.174)	.519 (.217)	.534 (.236)	-.485 (-.148)	-.109 (-.042)	-.172 (-.073)	-.029 (-.012)	.052 (.018)	.217 (.085)	.491 (.808)	.763 (1.262)	.624 (1.412)
Mixed	.064 (.026)	.019 (.008)	.136 (.060)	-.466 (-.142)	-.242 (-.093)	-.324 (-.138)	.090 (.037	.089 (.031)	.199 (.078)	.360 (.593)	.295 (.487)	.198 (.448)
Income (1,000's)	-.005 (-.002)	-.007 (-.003)	-.007 (-.003)	-.007 (-.002)	-.005 (-.002)	-.009 (-.004)	-.005 (-.002)	-.006 (-.002)	-.005 (-.002)	-.004 (-.006)	-.005 (-.008)	-.003 (-.006)
Ed	-.005 (-.002)	.014 (.006)	.020 (.009)	-.085 (-.026)	-.021 (-.008)	.030 (.013)	.012 (.005)	.023 (.008)	-.008 (-.003)	-.007 (-.012)	-.005 (-.009)	-.042 (-.095)
R^2	.205	.251	.231	.290	.231	.193	.143	.174	.143	.173	.207	.240
σ_ε tric Disturbance ndard Deviation)	.363	.362	.387	.257	.336	.382	.379	.316	.363	1.497	1.472	1.895

TABLE 4.3

Reduced Form (Total) Effects of Socioeconomic Characteristics on Attitudes Toward Criminal Justice. (Activities are measured in standard deviation units, metric coefficients appear in parentheses.)

Exogenous Variables	Police Service (PS)			Police Abuse (PA)			Criminal Justice Leniency (CL)			Criminal Justice Leniency (CL)		
	W1	W4	W7	W1	W4	W7	W1	W4	W7	W1	W4	W7
1. Age (10's)	.111 (.058)	.135 (.062)	.114 (.049)	-.093 (-.061)	-.178 (-.148)	-.164 (-.143)	-.067 (-.035)	.078 (.044)	.151 (.094)	-.184 (-.071)	-.116 (-.048)	-.144 (-.062)
2. Gender	-.114 (-.060)	.011 (.005)	.068 (.029)	.056 (.037)	-.013 (-.011)	.014 (.012)	-.251 (-.131)	-.287 (-.162)	-.167 (-.104)	.369 (.142)	.092 (.036)	.156 (.067)
3. Minn	.425 (.223)	.338 (.155)	.310 (.133)	-.370 (-.243)	-.473 (-.394)	-.575 (-.501)	-.306 (-.160)	-.309 (-.174)	-.217 (-.135)	.689 (.265)	.080 (.033)	-.160 (-.069)
4. Hou	.008 (.004)	.013 (.006)	-.082 (-.035)	-.097 (-.064)	-.229 (-.191)	-.398 (-.347)	-.159 (-.083)	-.310 (-.175)	-.188 (-.117)	.476 (.183)	.429 (.177)	.156 (.067)
5. Wash	.242 (.127)	.212 (.097)	.275 (.118)	-.332 (-.218)	-.600 (-.500)	-.501 (-.489)	-.067 (-.035)	.012 (.007)	.016 (.010)	.447 (.172)	.310 (.128)	.082 (.035)
6. Hispanic	-.940 (-.493)	-.897 (-.411)	-.797 (-.342)	1.287 (.841)	1.343 (1.119)	1.129 (.984)	.500 (.261)	.559 (.315)	.446 (.277)	.481 (.185)	1.074 (.443)	.616 (.265)
7. Black	-1.102 (-.578	- .838 (-.384)	-.655 (-.281)	1.013 (.665)	.912 (.760)	.787 (.686)	.253 (.132)	.507 (.286)	.615 (.382)	.853 (.328)	.752 (.310)	.788 (.339)
8. Mixed	-.351 (-.184)	-.179 (-.082)	.104 (.045)	.315 (.207)	.344 (.287)	.292 (.254)	.377 (.197)	.459 (.259)	.449 (.279)	.078 (.030)	.546 (.225)	.409 (.176)
9. Income (1,000's)	-.010 (-.005)	-.004 (-.002)	-.007 (-.003)	.002 (.001)	-.005 (-.005)	-.000 (-.000)	-.024 (-.011)	-.009 (-.005)	-.019 (-.012)	-.005 (-.002)	-.005 (-.002)	-.005 (-.002)
10. Ed	.097 (.050)	.057 (.026)	.063 (.027)	-.038 (-.025)	.025 (.021)	-.023 (-.020)	.103 (.054)	-.000 (-.000)	.005 (.003)	.075 (.029)	.053 (.022)	.058 (.025)
R^2	.338	.248	.234	.297	.339	.285	.150	.151	.259	.311	.188	.162
σ_ε (Metric Disturbance (Standard Deviation)	.427	.397	.375	.550	.677	.737	.482	.520	.535	.319	.371	.354

TABLE 4.4

Total (Reduced Form) and Direct (Structural) Effects of Socioeconomic Characteristics on Wave 1 "Fear of Crime." Attitudes are measured in standard deviation units.

	(LIM) Limit Activities		(PI) Percieved Increase		(FC) Fear City		(FN) Fear Neighborhood		(PV) Perceived Likelihood of Victimization	
	Total	Direct	Total	Direct	Total	Direct	Total	Direct	Total	Direct
1. Age (10's)	.002	.005	.007	.006	.020	.020	.009	.010	.004	.006
2. Gender	-.265	-.287*	-.311	-.279	-.468	-.453*	-.718	-.689*	-.448	-.389*
3. Minn	-.834	-.734*	-.515	-.420	.003	.059	-.490	-.451*	-.364	-.324
4. Hou	.059	.142	.262	.347	.117	.180	-.165	-.085	.052	.113
5. Wash	-.592	-.498*	-.469	-.403	.290	.344	-.366	-.300*	-.193	-.149
6. Hispanic	-.216	-.196	-.554	-.570	.273	.263	1.014	.956*	.837	.668*
7. Black	.427	.387*	-.485	-.495*	-.029	-.037	.544	.523*	.491	.457*
8. Mixed	.064	-.029	-.466	-.531	.090	.037	.425	.346*	.360	.262
9. Income (1000's)	-.005	-.005	-.007	-.007	-.005	-.005	-.007	-.007	-.004	-.004
10. Ed	-.005	-.009	-.085	-.098*	.012	.005	-.027	-.037	-.007	-.019
R^2	.205	.301	.290	.355	.143	.196	.353	.441	.173	.250
σ_ε	.363	.341	.257	.245	.379	.367	.560	.521	1.497	1.235

*Structural coefficient exceeds twice its estimated standard error.

TABLE 4.5

Total (Reduced Form) and Direct (Structural) Effects of Socioeconomic Characteristicson Wave 1 Attitudes Toward Criminal Justice. Attitudes are measured in standard deviation units.

	(PS) Police Services		(PA) Police Abuse		(CE) Criminal Justice Effectiveness		(CL) Criminal Justice Leniency	
	Total	Direct	Total	Direct	Total	Direct	Total	Direct
1. Age (10's)	.111	.101*	-.093	-.102*	-.067	-073	-.184	-.205*
2. Gender	-.114	-.072	.056	.055	-.251	-.233	.369	.411*
3. Minn	.425	.278	-.375	-.411*	-.306	-.362*	.689	.600*
4. Hou	.008	-.089	-.097	-.091	-.159	-.197	.476	.441
5. Wash	.242	.141	-.332	-.381	-.067	-.090	.447	.377
6. Hispanic	-.940	-1.108*	1.281	1.315*	.500	.427	.481	.390
7. Black	-1.102	-1.158*	1.013	1.039*	.253	.253	.853	.897*
8. Mixed	-.351	-.256	.315	.308	.377	.411	.078	.111
9. Income (1000's)	-.010	-.010	.002	.002	-.021	-.021	-.005	-.005
10. Ed	.097	.101*	-.038	-.037	.103	.105*	.075	.081*
R^2	.338	.418	.297	.323	.150	.172	.311	.392
σ_ε	.427	.400	.550	.540	.482	.475	.319	.300

*Structural coefficient exceeds twice its estimated standard error.

TABLE 4.6

Structural Coefficients of Substantive Equations, Attitudes are Measured in Standard Deviations Unless Metric Coefficients Appear in Parentheses

Pre-determined Variables [illegible]	Mean (Standard Deviation) Fear Sample	Mean (Standard Deviation) P.J. Sample	Illegal Activities W1	Illegal Activities W4	Illegal Activities W7	Perceived Income W1	Perceived Income W4	Perceived Income W7	Fear City W1	Fear City W4	Fear City W7	Fear Neighborhood W1	Fear Neighborhood W4	Fear Neighborhood W7	Perceived Likelihood of Victimization W1	Perceived Likelihood of Victimization W4	Perceived Likelihood of Victimization W7	Police Service W1	Police Service W4	Police Service W7	Police Abuse W1	Police Abuse W4	Police Abuse W7	Criminal Justice Effectiveness W1	Criminal Justice Effectiveness W4	Criminal Justice Effectiveness W7
1 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
2 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
3 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
4 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
5 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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7 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
8 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
9 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
10 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
11 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
12 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
13 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
14 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
15 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
16 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
17 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
18 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
19 Age (10's)			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
20 Gender			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
21 [illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
22 [illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
23 [illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
24 Hispanic			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
25 [illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
26 [illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
27 Income (10,000's)			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Wave 1 Attitude			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Wave 4 Attitude			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R^2			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]			[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Goodness-of-fit: χ^2			[illegible]			[illegible]			[illegible]			[illegible]			[illegible]			[illegible]			[illegible]			[illegible]		
df			[illegible]			[illegible]			[illegible]			[illegible]			[illegible]			[illegible]			[illegible]			[illegible]		

[a]Point estimate of disturbance [illegible] was less than zero

END