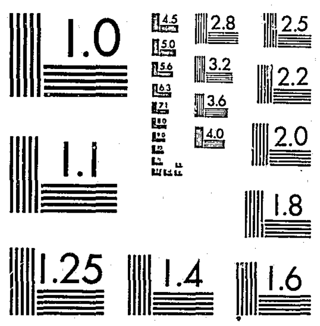

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SOURCES OF ERROR IN SURVEY DATA USED IN CIRMINAL JUSTICE EVALUATIONS: AN ANALYSIS OF SURVEY RESPONDENTS' REPORTS OF "FEAR OF CRIME"

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ABSTRACT

This paper presents preliminary analyses of the measurement characteristics and substantive determinants of seventeen measures of "fear of crime" obtained from the first two waves of a multi-wave panel survey of about 400 adults in four large metropolitan areas. The short-term stability of the underlying attitudes is differentiated from unreliability of measurement and intertemporal persistence in response errors. Substantively, we find that "fear of crime" is modestly related to gender, neighborhood ethnicity, and city, but that contact with crime and the criminal justice system has suprisingly little net influence on respondents' attitudes.

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INTRODUCTION

Survey data has become an 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 Ciarlo, 1975). Evaluations of criminal justice programs are certainly no exception (Empey and Lubeck, 1971; Empey and Erickson, 1972; Lipton, et al., 1975; Lenihan, 1977; Chelimsky, 1977; Lewis, 1978) and if the ongoing investment in the victimization studies is a fair example (Ennis, 1967; Biderman, et al., 1967; U. S. Department of Justice, 1973), 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; Campbell 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.

We are currently engaged in an examination of a variety of response errors in criminal justice surveys which until now have not been carefully studied. Using survey data collected in four large metropolitan areas, we are examining sources of error in public assessments of criminal justice activities: exposure to and satisfaction with the services provided by the criminal justice system (cf. Kelling, et al., 1974). By using a seven wave panel and recent estimation procedures which allow one to work with sets of equations with errors in equations and errors in variables (Jöreskog and Sörbom, 1977), we:

1. Document substantive sources of variation in common survey measures which are 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.

This paper presents a preliminary analysis of subjective evaluations of "fear of crime" obtained from the first two panels of our survey.

CONCEPTUAL APPARATUS

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 (Guildord, 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. In the analysis presented here, we consider a respondent's "fear of crime" and measured by seventeen different attitude items that have appeared in major social surveys.

At any point in time, fear of crime 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. 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 fear of crime (e.g., health, employment, family relations)

B. Attitudes perhaps related to fear of crime (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. Stochastic measurement error.

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 fear of crime, 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 everything 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 (and others) used in criminal justice evaluations (and others) 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. We intend to explore whether either assertion is justified.

RESEARCH DESIGN

As a methodological study, we are far more concerned with collecting data permitting an examination of measurement quality than with collecting data from some substantive population of interest. Moreover, realistic budget constraints restrict the options available. We fully recognize that should our efforts prove useful, the findings will eventually have to 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 have collected data on a seven-wave panel of 400 adults. When all seven waves are analyzed with the statistical procedures described in the next section, we will be able to estimate the impact of a wide range of measurement artifacts. For example, we will be able to gauge the effect of "learning" during multiple exposures to the survey instrument <u>without</u> "matched" samples of naive subjects.

Similarly, we will be able to determine the responsiveness of measures tapping criminal justice concerns to <u>other</u> events in people's lives. Indeed, with just the two waves of data analyzed here we are able to distinguish persistence over time due to correlated errors from true stability of the attitudes underlying the measured reports of fear of crime.

We have obtained data on approximately 100 respondents in four metropolitan areas: Houston, Minneapolis, Los Angeles, and Washington D. C.. Within cities, respondents were stratified by ethnicity of neighborhood: predominately white, black, mixed, or hispanic. Hispanic neighborhoods were sampled only in Houston and Los Angeles.

Data were collected by Audits and Surveys, Incorporated. Their field staff in the four cities conducted the first wave of personal interviews. Responses were obtained from about 550 households in order to ensure a final sample size of 400. Waves two through seven were administered as computer assisted telephone interviews from Audit and Surveys' New York offices. 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 will allow us to examine the effect of the length of time between interviews on the measurement characteristics of responses.

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 respondent's perceptions of the incidence of crime in their neighborhoods.

The measures of fear of crime analyzed here are reported in Table 1, arranged according to five component dimensions: (1) limit activities because of crime (LIM); (2) perceived increase in crime (PI); (3) fear for safety in areas of the metropolitan region (FC); (4) fear for safety in neighborhood (FN); and (5) perceived likelihood of victimization (PV). The degree to which the dimensions are empirically distinct and the relative reliabilities of the different indicators will be examined below. Socioeconomic variables, and measures of victimization, incidence, and contact with crime are described in Table 3.

A MULTI-WAVE PANEL MODEL FOR STATISTICAL ANALYSIS OF ATTITUDES ABOUT CRIMNAL 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 paper, we specify a model for measured reports of "fear of crime," although the format of the model is identical for any other criminal justice measure (see Figure 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 obtained on each of the J occasions;

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

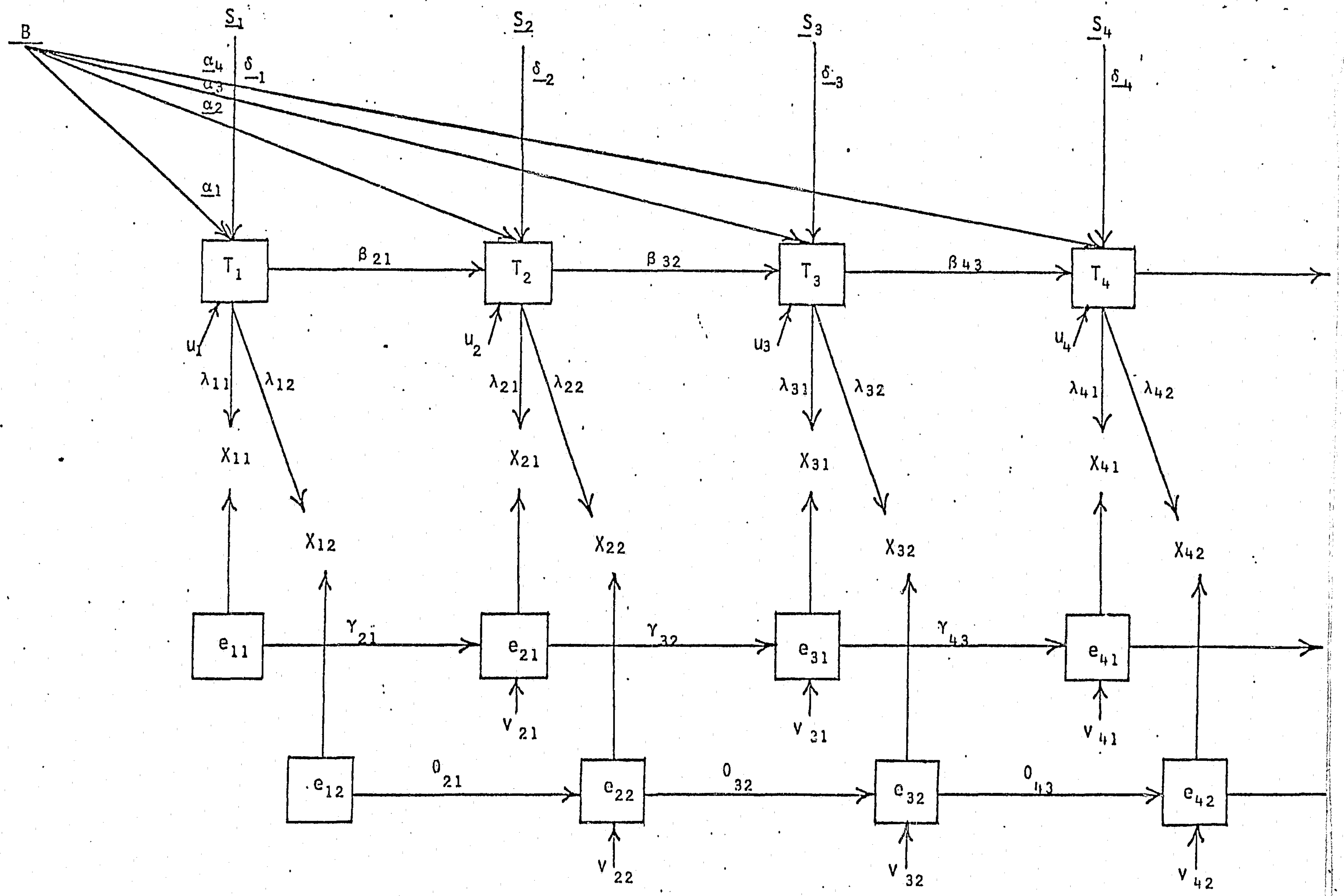

Figure 1 - A Multi-Wave Panel Model for Statistical Analysis of Attitudes About Criminal Justice

The structural equation model is presumed to generate the observed covariation among these measures, so knowledge of the covariation allows us 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 paramters 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 "overidentified" 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 inconsistent 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 below, and the identification issue is discussed in the Appendix.

The Measurement Model

At the jth assessment, measured variation in the K reports of satisfaction with police services, X_{jk}, has a common source of "true" substantive variation, T_j, while each report has a source of error variation, e_{jk}. Specifically,

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

Coefficient λ_{jk} is an increasing function of (positive) correlation between

the error and true components (Bielby, et al., 1977a, 1977b).[2] For each of the K measures, the error term has a systematic component correlated 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:

$$e_{j1} = \lambda_{j,j-1} \, e_{j-1,\,1} + v_{j1} \qquad (2)$$

$$e_{j2} = \theta_{j,j-1} \, e_{j-1,\,2} + v_{j2} \quad (j = 1, \ldots, J) \qquad (3)$$

where v_{jk} is the unique random error variation, and the systematic component is simply a function of error in the same measure at the previous point in time.[3]

The Substantive Model

There are four types of determinants of "true" attitudes about fear of crime, T_j: a vector of measured background factors that vary across individuals but not over time, $\underline{B}$; a vector of measured 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. Inlcuding a term for the mean, μ_j, the "true" component can be expressed as:

$$T_j = \mu_j + \underline{\alpha}_j' \underline{B}_j + \underline{\delta}_j' \underline{S}_j + \beta_j T_{j-1} + u_j \qquad (4)$$

The above equation is a substantive model of the determinants of fear of crime. Embedded within the measurement model of equations (1), (2), and (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 1 presents a schematic "path diagram" of the model described above 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}),[4] and also captures 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.[5] Estimates of the parameters of the measurement equations will provide quantitative evaluation of the quality of the measures 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 theoretically identified factors, and less obvious measurable substantive influences. Furthermore, the estimates of the substantive equation will not be contaminated by the biasing effects of response error. The estimates can then be compared to "naive" estimates that accept the measured reports at face value, in order to assess the magnitude of biases due to response errors.

Maximum likelihood computer programs are avialable (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. In addition to providing efficient statistical estimates, the programs allow one to statistically test whether the model successfully reproduces the measured covariation among $\underline{B}$, $\underline{S}$, and the X_{jk}, and appropriate modifications of the model can be made accordingly.

The specification estimated here is a simplified version of the model in Figure 1. It encompases just two waves of data and includes no measured determinants of fear of crime obtained in the second wave. That is, $\underline{S}_2$ has been omitted, and estimates of $\underline{\alpha}_2$ and B_{21} are actually assessments of "semi-reduced form" effects which include the indirect effects of $\underline{B}$ and T_1 that are transmitted via $\underline{S}_2$. Below, we examine the estimates for the measurement equations first and then interpret the results for the substantive equations.

RESULTS: MEASUREMENT EQUATIONS

The intertemporal observed correlations for each of the seventeen fear of crime items seem to suggest that "fear of crime" is a transient state of mind, showing only modest stability over a period as short as two weeks. Only two of the seventeen correlations in column (12) of Table 2 are greater than .60, and assessments of the same item obtained two weeks apart typically share less than a third of their variance in common ($.33^{1/2} = .57$). However, the correspondence between an underlying attitude held by the respondent, the response noted on the questionnaire, and the scale value assigned to that response is certainly less than perfect. That is, a variety of measurement errors could cause two reports to differ even when the underlying attitude has remained stable over the two waves. On the other hand, to the extent that errors are correlated over time, the observed association will overstate the stability of the underlying attitude. Consequently, the observed correlations in column (12) tell us absolutely nothing about the stability of various dimensions of fear of crime until we can simultaneously take into account unreliability and correlated errors of measurement. Of course, that's just what the measurement model was designed to do, and many of the statistics in Table 2 address the problem directly or indirectly. Column (13) shows that the two-week stabilities of the fear of crime dimensions are substantial--from .72 for "limit activities" (LIM) to .88 for "fear neighborhood" (FN). At the same time, column (13) reveals that the unique or "error" components are significantly correlated across waves for thirteen of the seventeen items. Error correlations are typically attributable to temporally constant response effects, and no doubt such effects are responsible in part for the correlations detected here. For example, the fact that the first "perceived victimization" item (PV_1) has the largest error correlation suggests that on each measurement occasion it sets up a similar response effect for the entire series of items.[6] (To test this hypothesis, we have reversed the order in which the items are presented in the final panel. If a temporally constant response effect is operating, then the error correlation for the "break-in, no one home" item should be substantially smaller across waves six and seven.) Nevertheless, the "error" component of an item is actually just its unique source of variation, orthogonal to the common variance. Persistence in an item's uniqueness across waves could be substantive and not a measurement artifact. For example, this might account for the disproportionately high error correlation for the first "limit activities" item (LIM_1) where the respondent is the referrent. The common ("true") variance for the three items may tap a generalized response towards limiting activities because of crime. But the respondent's report about his or her own activities might have a valid unique component, uncorrelated with the generalized response and reasonably stable overtime. In short, the lines between "true," and "unique" are not always clear in attitudinal studies.

The reliabilities in columns (10) and (11) indicate the proportion of observed variance in each item attributable to the common underlying dimension. Interestingly, the most reliable items are those dealing with the personal safety of the respondent. The least reliable items have the most generalized

referents, e.g., items LIM_3 ("people in general"), PI_1 (crime and the media), and PI_4 (crime in the U. S.). Among the perceived victimization items those that are least personally threatening are reported least reliably. If applicable to other content domains as well, it may be that the most useful subjective social indicates are those that tap some aspect of the personal well-being of the respondent.

The metric error standard deviations [columns (6) and (7)] and true score slopes [columns (8) and (9)] allow for the comparison of the measurement characteristics of the same item overtime or across items measured on the same scale. These estimates will become more important as we add panels to the analysis and examine the effect of "retesting" on response errors. However, comparisons between waves one and two should capture differences in the measurement characteristics of the two data collection procedures--personal interview (Wave 1) and telephone interview (Wave 2). We anticipated that the personal interview would elicit more accurate responses than the telephone interview, and consequently error variation would be uniformly larger in wave 2 (see Bielby, Hauser, and Featherman, 1977a). The estimates provide absolutely no support our expectations,[7] and in fact suggest that the telephone interviews may elicit more accurate reports for the perceived victimization items (PV_1 through PV_5). Perhaps the presumed advantages of face-to-face interviewing are offset by the increased accuracy obtained by pre-coded responses and automatic error checking in computer-assisted telephone interviewing. The potential trade-offs certainly deserve closer examination by those concerned with minimizing costs of large-scale social surveys without sacrificing accuracy.

Finally, the measurement characteristics presented in Table 2 do not address the "discriminant validity" of the five dimensions. Estimating seperate models for each dimension provides no assessment of the degree to which the five dimensions of "fear and crime" are empirically distinguishable from one another. It could be that the seventeen items are all tapping a single generalized "fear" dimension, and not distinct components. To examine this possibility, we estimated a ten factor confirmatory factor analysis (CFA) model of the thirty-four measures obtained in the two waves. The estimates of the measurement equations were virtually identical to those reported in Table 2, even though the CFA model contained many more overidentifying restrictions. Within each wave the five dimensions were clearly distinct, with most correlations around .50. In Wave 1, the largest correlation was. 68--between the "fear in neighborhood" (FN) and "perceived likelihood of victimization" (PV) dimensions. The intercorrelations were slightly higher in Wave 2, with the correlation between FN and PV increasing to .79.[8] In summary, it seems that the underlying components of "fear of crime" are empirically distant and reasonably stable over a short period of time.

RESULTS: SUBSTANTIVE EQUATIONS

Table 4 presents coefficient estimates for the substantive equations. Lines (a) and (b) are reduced form equations assessing the total effects of demographic variables. Lines (c) and (d) add the crime variables measured at Wave 1. Line (e) adds the Wave 1 dependent variable into the Wave 2 equation. While lines (c) and (e) are the structural equations, comparison of lines (c) and (d) are often more interesting, showing whether the effects of predetermined variables replicate across waves. For every outcome, line (e) shows a substantial net stability across waves--from .64 for "limit activities" (LIM) to .95 for "fear neighborhood" (FN). In every case, the standardized stability coefficient is at least 90 percent as large as the zero-order intertemporal correlation [column (13) of Table 2], so the spurious component of the associa-

tion is quite small.[9] The structural equation for Wave 2 also reveals that virtually all of the effects of the predetermined variables measured at Wave 1 are mediated by the Wave 1 outcome. Apart from the lag effects only 4 of the 105 coefficients are greater than twice their standard errors, (not shown) in line (e) for the five outcomes. Since one would expect five to be significant by chance, we will resist the temptation to interpret these effects.

More interesting is an examination of the predetermined variables which have consistent effects on an outcome across waves as revealed in line (a) through (d). In general, standardized coefficients of about .13 or larger in magnitude are statistically significant in our sample (exceed twice their standard errors), so we will concentrate on coefficients that exceed .13 in both lines (a) and (b) or both lines (c) and (d) for any outcome variable.

The socioeconomic variables, education and income, have no consistent significant effects on any of the five fear dimensions (although the income coefficients are slightly below our criterion of .13 for the "fear neighborhood" (FN) and "perceived increase" (PI) items). It seems that the security that can be gained through socioeconomic well-being is captured by the neighborhood ethnicity variables, since those most advantaged live in the homogenous white neighborhoods. Neither working outside the home (WORK) nor tenure status (TEN: renter versus non renter) had consistent effects across waves. We anticipated that those working outside the home would be exposed to more risk and consequently express more fear of crime. Instead, the effects of working are consistently negative though not statistically significant. In future analysis we will interact WORK with both gender and day versus night shift in order to assess the risk factor more directly.

Sex has by far the largest and most consistent effects across all five outcomes. Women express more fear of crime on every dimension, and very little of the gender difference is mediated by the crime variables. The "perceived increase" dimension at Wave 1 (PI_1) is the only outcome for which the effect of sex did not exceed our criterion, and it is the dimension of fear of crime least directly related to personal safety. The pervasive effects of gender suggest that we disaggregate by sex in future analyses in order to exmaine interaction effects.

The four types of neighborhoods--hispanic, black, mixed, and white--are represented by three binary variables. The standardized coefficients reveal that the effects of neighborhood ethnicity are often formidable, but the metric coefficients are easier to interpret. Respondents in black neighborhoods are substantially more likely to limit their activities because of crime (LIM), fear for their safety in their neighborhoods (FN), and feel that they will be victimized (PV) than are respondents living in white neighborhoods (the omitted category). Those in hispanic neighborhoods are even more fearful for the safety while in their neighborhoods (FN) and express a greater likelihood of being victimized (PV) than do those living in black neighborhoods, but hispanics are a bit less likely than blacks to limit their activities because of crime (LIM). Interestingly, while those from hispanic neighborhoods fear for their safety when in other parts of the city (FC), this is not true of respondents from black neighborhoods. This probably is because black neighborhoods are perceived to be most dangerous by all respondents. When a respondent lives in such a neighborhood it becomes his or her referrent for the "fear in neighborhood" (FN) items, but for a respondent not in a black neighborhood, it becomes the referrent for the "fear in metropolitan area" (FC) items.

Finally while residents of black neighborhods express more fear for their personal safety than do those in white neighborhoods, they are less likely to perceive an increase in crime in recent years (PI). Those in white neighborhoods perceive the greatest increase. These might be accurate perceptions

by all groups. It may be that crime is not increasing in the black and hispanic neighborhoods but is being experted to the white suburbs. However, we note that the gap between the white neighborhood and the others increases when the crime variables are controlled (compare lines (2a) with (2c) and (2b) with (2d)).

In many cases, the coefficient for mixed neighborhoods (MIXED) is intermediate between the effects for white and black neighborhoods. Perhaps this accurately reflects the perceptions of the respondents, but further analysis is necessary to disentangle the ethnicity of the neighborhood from ethinicity of respondent. Without race of respondent (which in being obtained in the last wave of interviews) we cannot tell if blacks and whites in mixed neighborhoods feel similar threats to their personal safety because of crime.

We expected older respondents to express more fear of crime, and we detected significant but models negative effects of age only for "fear in parts of metropolitan area" (FC) and "fear is neighborhood" (FN) dimensions. We obtained no strong evidence that older respondents either feel more likely to be victimized or perceive themselves and others to be limiting activities because of crime. However, the effects of age may be nonlinear, with a large negative effect among those sixty or older. We shall explore this possibility in subsequent analyses.

To facilitate comparison of "city effects" across outcomes, the coefficients for city variables have been rescaled and presented in Table 5. Each entry in the table has been standardized for the outcome variable only, so it guages the effect of being in a given city--relative to Los Angeles, the omitted category--in standard deviation units of the dependent variable. The "total" effects are from lines (a) and (b) in Table 4 (controlling demographic variables only) and the "net" effects derived from lines (c) and (d) of the table (controlling both demographic and crime variables). Minneapolis is clearly the most secure city in terms of fear of crime, ranking fourth or third (usually fourth) across all outcome variables. Houston's respondents are the most fearful, ranking first or second (usually first) on all outcome variables. Differences between cities of about .45 or greater are statistically significant, so the differences between Houston and Minneapolis are significant in nearly every case. Furthermore, the differences are attenuated only slightly when respondents' perceptions of the amount of crime and reports of contact with crime are controlled. The findings suggest a possible "climate of fear" in Houston that is not present in Minneapolis.

With the exception of "fear areas of city" (FC), respondents in the Washington metropolitan area feel nearly as secure as those in Minneapolis. As long as they avoid certain parts of the city--which they fear nearly as much as Houston residents--they do not have to live with the more pervasive fear that characterizes Houston. Los Angeles respondents are generally not too different from those in Houston, particularly in the fear of crime they feel in their neighborhoods and the degree to which they see themselves and others limiting activities because of crime.

Overall, the performance of the crime variables was disappointing. Only three of the nine measures had consistent effects across waves for one or more outcome. The number of different property crimes ever committed against the respondent slightly increased reported fear in areas of the metropolitan region (FC) (but not fear in neighborhood). The number of different crimes experienced by others in the respondent's household positively affected perceived increase in crime (PI), and the number of different violent crimes that the respondent reported being committed in the neighborhood over the past year influenced both perceived increase in crime (PI) and fear in areas of the metropolitan region (FC). A number of the crime variables had nonsignificant

effects on fear of crime in the expected direction, and perhaps the collinearity among the items precluded isolating their seperate effects. While few individual coefficients were statistically significant, the hypothesis that the coefficients of the crime variables were jointly zero was easily rejected for each outcome. The nine variables increase the coefficient of determination (R^2) by .08 to .12 for the Wave 1 outcomes and by .05 to .08 for the Wave 2 outcomes. Nevertheless, the fear of crime outcomes are not overwhelmingly sensitive to measures of contact with crime and the criminal justice system. It may be that we have accurately measured all of the variables, and respondents' fear of crime is simply not sensitive to the amount of contact they have with crime, the police, the courts, etc. However it could be that we have poor measures of the crime variables and are consequently underestimating their impact on the five dimensions of fear of crime. The issue deserves careful attention in future research.

The measurement results revealed that items directly tapping personal safety were reported most accurately. We expected the coefficients of determination to follow a similar pattern, with R^2 highest for the likelihood of victimization dimension (PV) and lowest for perceptions of increase in crime (PI). Again, the results did not conform to our expectations. We explained 37 to 50 percent of the variance in the "fear in neighborhood" dimension (ignoring the equation with the lagged outcome variable) but only about a fourth of th variance in the "fear areas of metropolitan region" dimension, yet both dimensions concern the respondent's feeling of personal safety. While we explain nearly a third of the variance in "perceived likelihood of victimization," we do slightly better than that for the dimension linked least directly to personal safety--"perceived increase in crime"--and a bit worse for the dimension "limit activities because of crime." In short, we have no convincing explanation at this time differences across outcomes in the degree to which demographic and crime variables account for variation in the dimension of fear of crime.

SUMMARY AND CONCLUSIONS

We have presented a preliminary analysis of 17 items measuring five dimensions of "fear of crime" obtained from the first two waves of a multi-wave panel study. Our multiple indicator structural equation model documents both the measurement characteristics and the substantive sources of variation in the 17 items.

The five components of fear of crime are clearly distinguishable from one another empirically. The short-term stability of each dimension is substantially larger than zero-order observed correlations suggest, even after taking into account intertemporal error correlation as large as .50. The items reported most reliably are those dealing most directly with the personal well-being of the respondent. In contrast, items with generalized referents, e.g., "crime in the U. S." or "people in general," are reported least accurately.

The substantive equations revealed that gender, ethnicity of neighborhood, and city have the most pervasive effects on "fear of crime." In contrast, measures of contact with crime and the criminal justice system had effects on fear of crime that are modest at best, mediating very little of the influence of demographic variables. Thus, it appears that gender-, neighborhood-, and city-specific climates of fear (or security) exist that are largely independent of the degree to which individual have personal contact with crime, the police, the courts, etc. However, this speculation must be tested against the alternative hypothesis that the effects of crime variables were grossly attenuated due to poor measurement.

Our future work will, of course, add the five remaining waves of data, allowing

us to assess the effects of repeated measurement. Substantively, we are interested in whether the underlying attitudes persist over time through a simple Markov chain, or whether there is a longer term memory or "state dependence" whereby attitudes early in the study directly affect opinions expressed in the last waves. We will also be examining the degree to which contact with crime, the police, and the courts between waves directly changes attitudes in subsequent waves. In addition we will see whether expressed "fear of crime" is sensitive to various major and minor events in peoples' lives (everything from losing a job to going on a vacation) that have no direct link to a person's perceived vulnerability to crime. Finally, analyses such as these will be extended to other commonly measured attitudes about crime and criminal justice--e.g., satisfaction with police, support for the courts, etc.,--to determine whether or not widely used "social indicators" measure anything of social, scientific, or policy relevance.

NOTES

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

2. 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).

3. 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 and all e_{j2} to be similarly intercorrelated (Wheaton, et al., 1977).

4. With more than two measures of the attitude at each point in time, it may be possible to assess whether errors in reports of different measures obtained on the same occasion covary (i.e., correlations between v_{j1} and v_{j2}).

5. By estimating the model for subgroups of the sample, we can investigate whether the amount or pattern of error variation is a function of characteristics of the respondents. For example, we might find that the amount of error variation is larger for less education respondents.

6. Although not reported in Table 2, the measurement model allowed for within-wave error correlations for the two "break-in" items, PV_1 and PV_2, and among the three personal victimizations; PV_3, PV_4, PV_5. Two of the four error correlations were significantly different from zero in the first wave and three of the four in the second wave.

7. Nor is there any convincing evidence that means and standard deviations differ systematically across waves. See columns (1) through (4) of Table 2.

8. We may find that over time, respondents are less discriminating in their responses to different items in the survey. This is one of several types of learning (or "unlearning") effects we will be looking for as we analyze subsequent waves.

9. In fact, the predetermined variables actually supress part of the stability in the "fear neighborhood" dimension. The net standardized coefficient is 108% of the zero-order association.

APPENDIX

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:

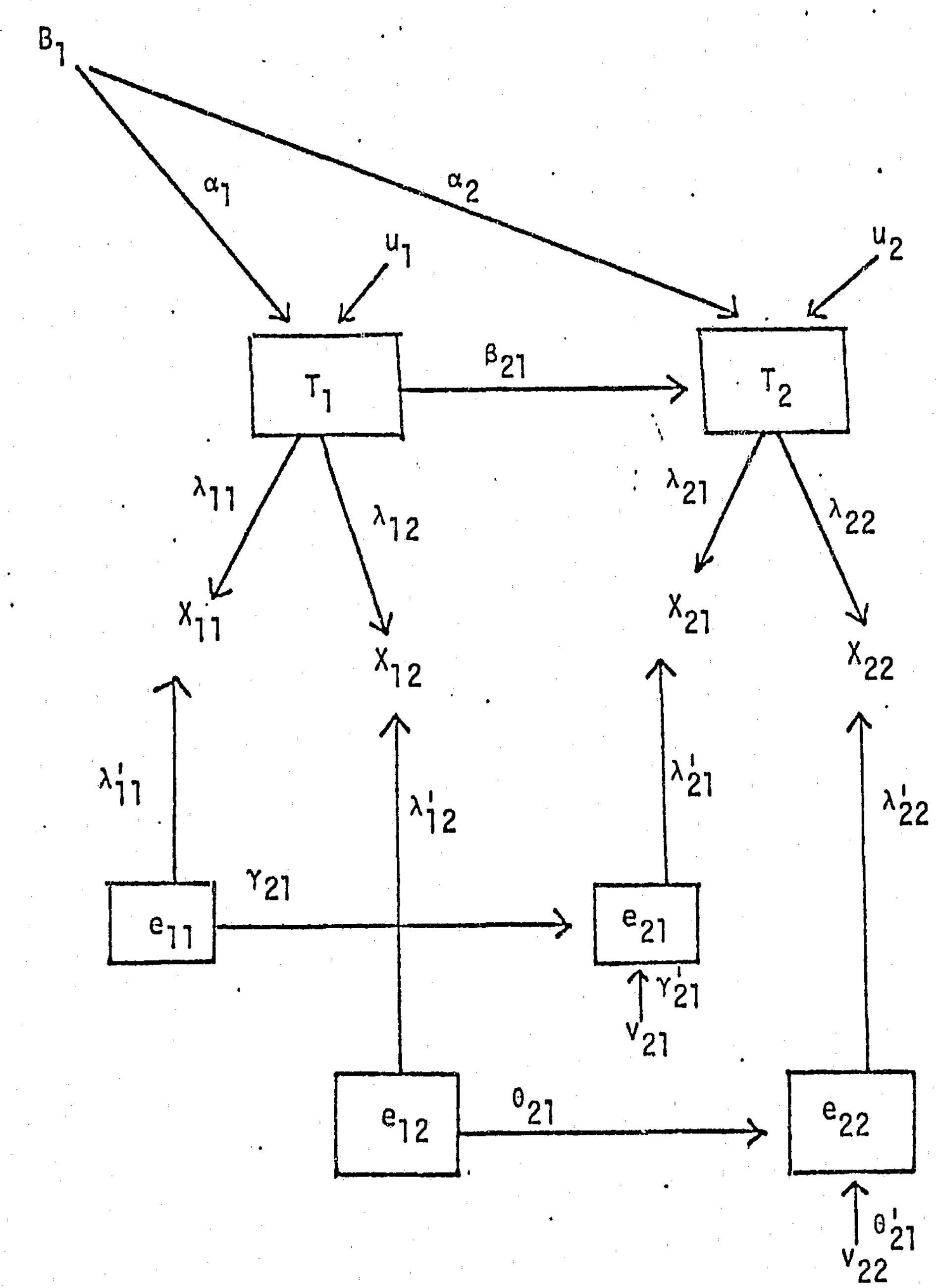

Figure A1 - A Simplified Multi-wave Panel Model

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

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

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

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

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

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

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

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

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

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

In addition, the unit standard deviations impose the following:

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

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

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

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

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:

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

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.

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Table 1. Attitudinal Measures of Fear of Crime

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. CHECK ONLY ONE RESPONSE.	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. CHECK ONLY ONE RESPONSE.	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

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 2. Measurement Characteristics of Fear of Crime Items (N = 329)

Description[a] (Range)	μ_{ij} Mean W1	μ_{ij} Mean W2	σ_{ij} Standard Deviation W1	σ_{ij} Standard Deviation W2	$\sigma_{e_{ij}}$ Error Standard Deviation W1	$\sigma_{e_{ij}}$ Error Standard Deviation W2	λ_{ij} Relative True Score Slope W1	λ_{ij} Relative True Score Slope W2	$\rho_{ii'}$ Reliability W1	$\rho_{ii'}$ Reliability W2	Intertemporal Correlations: Observed ρ_{12}	True $\rho_{T_1T_2}$	Error $\rho_{e_1e_2}$
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
1. LIMIT ACTIVITES BECAUSE OF CRIME													
LIM_1 Respondent (0-1)	.42	.38	.49	.49	.32	.34	1.00[b]	1.00[b]	.58	.49	.58		.41
LIM_2 Neighbors (0-1)	.51	.50	.50	.50	.26	.23	1.14	1.30	.73	.79	.56	.72	.05[ns]
LIM_3 People on general (0-1)	.81	.84	.39	.36	.35	.33	.45	.45	.18	.18	.35		.25
2. PERCEIVED INCREASE IN CRIME													
PI_1 More or less serious than media say (0-2)	1.39	1.39	.65	.65	.56	.67	.86	.60	.26	.12	.43		.36
PI_2 Chances of being victimized have gone up (0-2)	1.60	1.52	.61	.61	.48	.47	1.00[b]	1.00[b]	.40	.40	.55	.82	.40
PI_3 Crime in neighborhood increased or decreased (0-2)	1.31	1.29	.64	.61	.58	.49	.86	.93	.27	.34	.51		.35
PI_4 Crime in U.S. increased or decreased (0-2)	1.84	1.79	.42	.46	.41	.42	.30	.46	.07	.14	.60		.52
3. FEAR AREAS OF CITY													
FC_1 During the day (0-1)	.41	.40	.49	.49	.29	.34	1.00[b]	1.00[b]	.65	.51	.52	.81	.15[ns]
FC_2 At night (0-1)	.66	.71	.48	.46	.35	.34	.80	.85	.44	.43	.54		.33
4. FEAR NEIGHBORHOOD													
FN_1 This neighborhood compared to others in metropolitan area (0-4)	1.52	1.55	.95	1.00	.72	.80	1.10	1.30	.41	.36	.66	—	.50
FN_2 Safe in area during the day (0-3)	2.27	2.32	.78	.65	.55	.46	-1.00[b]	-1.00[b]	.50	.50	.54	.88	.21
FN_3 Safe in aréa at night (0-3)	1.65	1.62	.92	.88	.50	.44	-1.40	-1.65	.70	.75	.70		.21[ns]
5. PERCEIVED LIKELIHOOD OF VICTIMIZATION													
PV_1 Break-in, no-one home (0-10)	4.76	4.35	3.13	2.80	2.70	2.07	1.00[b]	1.00[b]	.24	.46	.55		.45
PV_2 Break-in, someone home (0-10)	2.07	1.91	2.48	2.21	2.00	1.88	.94	.60	.33	.27	.44		.23
PV_3 Purse/wallet snatched (0-10)	3.11	3.09	3.17	2.89	1.91	1.95	1.64	1.14	.63	.56	.54	.78	.22
PV_4 Robbed by force on street (0-10)	2.83	3.02	2.96	2.86	1.82	1.48	1.52	1.29	.62	.73	.56		-.03[ns]
PV_5 Beat up on street (0-10)	2.38	2.49	2.89	2.61	1.85	1.62	1.45	1.06	.60	.60	.58		.20

Notes:
- a: See Table 1 for full description of items.
- b: Slope for reference indicator fixed at 1.0.
- ns: Estimated error correlation less than twice its standard error.

Table 3. Means and Standard Deviations on Predetermined Variables (n = 329)

Variable	Description (range)	Mean	Standard Deviation
1. ED	Education, yrs (6-19)	13.12	2.78
2. INC	Household income × $10,000 (.25-7.5)	2.35	1.45
3. WORK	Work full- or part-time (0=No, 1=Yes)	.62	.49
4. TEN	Tenure status (0=nonrenter, 1=renter)	.24	.43
5. SEX	(0=Male, 1=Female)	.50	.50
6. HISP	Hispanic neighborhood (0=No, 1=Yes)	.11	.31
7. BL	Black neighborhood (0=No, 1=Yes)	.34	.48
8. MIX	Mixed neighborhood (0=No, 1=Yes)	.30	.46
9. AGE	Age × 10 yrs (2.1-8.4)	4.21	1.57
10. MINN	Minneapolis metropolitan area (0=No, 1=Yes)	.26	.44
11. HOU	Houston metropolitan area (0=No, 1=Yes)	.20	.40
12. WASH	Washington, D.C. metropolitan area (0=No, 1=Yes)	.29	.45
13. EVPR	Of 5 property crimes, how many had happened to respondent, ever (0-5)	1.29	1.36
14. EVPE	Of 3 personal crimes, how many had happened to respondent, ever (0-3)	.20	.50
15. YRPR	Of 5 property crimes, how many had happened to respondent, past year (0-5)	.51	.93
16. YRPE	Of 3 personal crimes, how many had happened to respondent, past year (0-3)	.09	.37
17. VICO	Of 4 personal crimes, how many had happened to others in the household, past year (0-4)	.17	.57
18. VIOL	Of 7 violent crimes, how many had occurred in neighborhood, past year (0-7)	1.67	1.39
19. #VIOL	Total number of violent crimes in neighborhood, past year (0-39)	2.81	5.21
20. ACOP	Respondent was assisted by police during the past year (0=No, 1=Yes)	.36	.48
21. PROS	Other than traffic citation, respondent was arrested, prosecuted, or jailed within past year (0=No, 1=Yes)	.09	.28

Table 4. Reduced-Form, Semi-Reduced Form, and Structural Standardized Coefficients--
A Model of the Determinants of Fear of Crime (N = 329).
Metric coefficients appear in parentheses.

[illegible]endent Variable (1=WAVE 1; 2=WAVE 2)		Predetermined Demographic Variables												Predetermined Crime Variables									Lag Endog.		Components of Variati[illegible]		
		ED	INC	WORK	TEN	SEX	HISP	BL	MIX	AGE	MINN	HOU	WASH	EVPR	EVPE	YRPR	YRPE	VICO	VIOL	#VIOL	ACOP	PROS	Y1	R^2	Residual σ_e	Explained $\sigma_{\hat{y}}$	To[illegible] σ_y
(1a)	LIM1	-.031 (-.004)	-.076 (-.020)	.078 (.060)	.084 (.074)	-.129 (-.097)	.058 (.071)	.231 (.182)	.136 (.112)	.036 (.009)	-.349 (-.297)	-.044 (-.041)	-.204 (-.170)	--	--	--	--	--	--	--	--	--	--	.17	.34	.17	.3[illegible]
(1b)	LIM2	-.009 (-.001)	-.065 (-.015)	-.011 (-.008)	.132 (.105)	-.139 (-.095)	.143 (.158)	.249 (.179)	.127 (.094)	.031 (.007)	-.331 (-.255)	-.059 (-.050)	-.184 (-.139)	--	--	--	--	--	--	--	--	--	--	.22	.30	.16	.34
(1c)	LIM1	-.038 (-.005)	-.079 (-.020)	.052 (.040)	.097 (.085)	-.131 (-.098)	.037 (.045)	.208 (.165)	.053 (.043)	.083 (.020)	-.276 (-.235)	.029 (.027)	-.151 (-.125)	.054 (.015)	-.038 (-.028)	.112 (.045)	-.026 (-.027)	.080 (.058)	.068 (.018)	.115 (.083)	.137 (.107)	-.036 (-.049)	--	.27	.32	.20	.3[illegible]
(1d)	LIM2	-.017 (-.002)	-.074 (-.017)	-.018 (-.013)	.137 (.108)	-.138 (-.094)	.118 (.131)	.217 (.155)	.071 (.053)	.079 (.017)	-.273 (-.211)	.004 (.003)	-.138 (-.104)	.106 (.027)	.002 (.001)	.010 (.004)	.028 (.026)	.129 (.085)	.115 (.028)	.003 (.002)	.064 (.045)	-.030 (-.036)	--	.28	.29	.18	.34
(1e)	LIM2	.008 (.001)	-.023 (-.005)	-.052 (-.036)	.074 (.059)	-.054 (-.037)	.094 (.104)	.083 (.059)	.037 (.028)	.026 (.006)	-.096 (-.074)	-.015 (-.013)	-.041 (-.031)	.071 (.018)	.026 (.018)	-.062 (-.023)	.045 (.041)	.078 (.051)	.071 (.018)	-.072 (-.047)	-.024 (-.017)	-.006 (-.008)	.643 (.583)	.58	.22	.26	.34
(2a)	PI1	-.189 (-.026)	-.108 (-.029)	-.091 (-.072)	.089 (.080)	-.111 (-.086)	-.056 (-.070)	-.292 (-.238)	-.147 (-.124)	.067 (.017)	-.234 (-.205)	.026 (.025)	-.200 (-.171)	--	--	--	--	--	--	--	--	--	--	.27	.33	.21	.39
(2b)	PI2	.026 (.004)	-.123 (-.033)	-.119 (-.094)	.206 (.185)	-.172 (-.132)	-.082 (-.102)	-.276 (-.223)	-.205 (-.172)	.119 (.029)	-.142 (-.124)	.099 (.095)	-.177 (-.150)	--	--	--	--	--	--	--	--	--	--	.24	.34	.17	.38
(2c)	PI1	-.198 (-.028)	-.100 (-.027)	-.092 (-.074)	.092 (.083)	-.099 (-.077)	-.068 (-.085)	-.314 (-.256)	-.162 (-.137)	.[illegible]07 (.026)	-.196 (-.172)	.076 (.073)	-.173 (-.148)	.088 (.025)	.023 (.017)	-.000 (-.000)	.017 (.017)	.228 (.171)	.163 (.045)	-.184 (-.137)	.052 (.042)	-.106 (-.147)	--	.36	.31	.24	.39
(2d)	PI2	.005 (.001)	-.123 (-.033)	-.135 (-.107)	.228 (.204)	-.166 (-.128)	-.091 (-.114)	-.293 (-.237)	-.252 (-.212)	.155 (.038)	-.096 (-.083)	.152 (.146)	-.160 (-.136)	.120 (.034)	.065 (.049)	.041 (.017)	-.032 (-.033)	.158 (.117)	.067 (.018)	-.005 (-.004)	.048 (.038)	-.161 (-.222)	--	.32	.32	.20	.38
(2e)	PI2	.169 (.023)	-.040 (-.010)	-.059 (-.047)	.152 (.136)	-.084 (-.065)	-.035 (-.044)	-.033 (-.027)	-.118 (-.099)	.066 (.016)	.066 (.058)	.090 (.086)	-.017 (-.014)	.048 (.013)	.046 (.035)	.041 (.017)	-.046 (-.047)	-.031 (-.023)	-.068 (-.019)	.147 (.108)	.005 (.004)	-.073 (-.101)	.827 (.821)	.75	.19	.33	.38

Table 4, continued

[illegible] Variable (1=WAVE 1; 2=WAVE 2)		Predetermined Demographic Variables												Predetermined Crime Variables									Lag Endog.		Components of Varia[illegible]		
		ED	INC	WORK	TEN	SEX	HISP	BL	MIX	AGE	MINN	HOU	WASH	EVPR	EVPE	YRPR	YRPE	VICO	VIOL	#VIOL	ACOP	PROS	Y1	R^2	Residual σ_e	Explained $\sigma_{\hat{y}}$	Tot[illegible]
(3a)	FC1	-.054 (-.008)	.011 (.003)	-.047 (-.039)	-.000 (-.000)	- .239 (- .190)	.175 (.225)	-.036 (-.030)	.103 (.089)	.170 (.043)	-.059 (-.053)	.021 (.021)	.114 (.100)	--	--	--	--	--	--	--	--	--	--	.15	.37	.15	.40
(3b)	FC2	-.081 (-.010)	.029 (.007)	-.105 (-.076)	.036 (.029)	- .242 (- .169)	.224 (.254)	.062 (.046)	.132 (.101)	.195 (.043)	-.043 (-.034)	.139 (.121)	.133 (.103)	--	--	--	--	--	--	--	--	--	--	.21	.31	.16	.35
(3c)	FC1	-.093 (-.013)	.011 (.003)	-.077 (-.063)	.017 (.015)	- .215 (- .170)	.140 (.181)	-.060 (-.050)	.048 (.041)	.192 (.049)	-.033 (-.030)	.072 (.072)	.115 (.101)	.158 (.046)	-.048 (-.038)	-.130 (-.056)	.093 (.099)	.081 (.062)	.153 (.044)	.030 (.023)	.091 (.075)	-.168 (-.239)	--	.23	.35	.19	.40
(3d)	FC2	-.100 (-.013)	.023 (.005)	-.117 (-.084)	.043 (.035)	- .234 (- .164)	.202 (.229)	.032 (.024)	.083 (.063)	.233 (.052)	.000 (.000)	.193 (.169)	.163 (.126)	.161 (.041)	-.025 (-.018)	-.086 (-.033)	.057 (.053)	.106 (.072)	.130 (.033)	-.011 (-.008)	.096 (.070)	-.050 (-.069)	--	.28	.30	.18	.35
(3e)	FC2	-.030 (-.004)	.014 (.003)	-.059 (-.042)	.030 (.025)	- .073 (- .051)	.097 (.110)	.077 (.057)	.047 (.036)	.089 (.020)	.025 (.020)	.139 (.121)	.076 (.059)	.042 (.011)	.011 (.008)	.012 (.004)	-.013 (-.012)	.045 (.030)	.015 (.004)	-.033 (-.022)	.028 (.020)	.076 (.095)	.752 (.662)	.72	.18	.30	.35
(4a)	FN1	-.116 (-.023)	-.129 (-.049)	-.003 (-.003)	.055 (.071)	- .298 (- .328)	.336 (.601)	.313 (.363)	.266 (.320)	.113 (.040)	-.175 (-.218)	-.049 (-.067)	-.152 (-.185)	--	--	--	--	--	--	--	--	--	--	.38	.43	.34	.55
(4b)	FN2	-.056 (-.009)	-.125 (-.040)	.003 (.003)	.029 (.031)	- .289 (- .266)	.321 (.479)	.265 (.257)	.174 (.175)	.134 (.039)	-.192 (-.201)	-.057 (-.066)	-.108 (-.110)	--	--	--	--	--	--	--	--	--	--	.32	.38	.26	.46
(4c)	FN1	-.153 (-.030)	-.147 (-.056)	-.039 (-.045)	.060 (.078)	- .285 (- .314)	.285 (.508)	.295 (.342)	.171 (.206)	.136 (.048)	-.129 (-.161)	.008 (.010)	-.141 (-.172)	.103 (.042)	.020 (.021)	-.100 (-.059)	-.111 (-.164)	.053 (.057)	.131 (.052)	.183 (.194)	.121 (.139)	-.042 (-.083)	--	.50	.39	.39	.55
(4d)	FN2	-.086 (-.014)	-.123 (-.039)	-.022 (-.021)	.042 (.045)	- .283 (- .260)	.307 (.459)	.260 (.252)	.124 (.125)	.159 (.047)	-.150 (-.157)	-.017 (-.020)	-.104 (-.106)	.076 (.026)	.102 (.093)	-.014 (-.007)	-.043 (-.053)	.034 (.031)	.069 (.023)	.083 (.074)	.089 (.085)	-.126 (-.207)	--	.37	.36	.29	.46
(4e)	FN2	.059 (.010)	.018 (.006)	.015 (.014)	-.016 (-.017)	- .011 (- .010)	.037 (.055)	-.021 (-.021)	-.039 (-.040)	.029 (.008)	-.027 (-.028)	-.025 (-.028)	.031 (.031)	-.022 (-.007)	.083 (.076)	.081 (.040)	.063 (.078)	-.017 (-.015)	-.056 (-.018)	-.091 (-.081)	-.027 (-.025)	-.086 (-.141)	.953 (.797)	.83	.19	.42	.46
(5a)	PV1	.030 (.017)	-.039 (-.041)	-.107 (-.338)	.012 (.044)	- .229 (- .704)	.299 (1.490)	.247 (.798)	.162 (.542)	.088 (.086)	-.193 (-.673)	.010 (.040)	-.140 (-.475)	--	--	--	--	--	--	--	--	--	--	.24	1.34	.75	1.54
(5b)	PV2	-.056 (-.038)	-.099 (-.130)	-.010 (-.040)	.032 (.142)	- .281 (-1.070)	.223 (1.378)	.236 (.948)	.164 (.683)	.092 (.111)	-.152 (-.657)	.073 (.345)	-.070 (-.294)	--	--	--	--	--	--	--	--	--	--	.25	1.66	.96	1.91
(5c)	PV1	.005 (.003)	-.043 (-.046)	-.129 (-.407)	.004 (.016)	- .213 (- .654)	.267 (1.327)	.219 (.709)	.104 (.349)	.123 (.120)	-.146 (-.506)	.066 (.254)	-.111 (-.376)	.094 (.106)	.087 (.267)	-.058 (-.097)	-.026 (-.108)	.047 (.140)	.248 (.275)	-.034 (-.099)	.069 (.219)	-.081 (-.442)	--	.32	1.26	.87	1.54
(5d)	PV2	-.082 (-.056)	-.095 (-.125)	-.025 (-.098)	.032 (.141)	- .272 (-1.035)	.212 (1.309)	.233 (.933)	.137 (.571)	.115 (.139)	-.118 (-.509)	.104 (.495)	-.068 (-.284)	.057 (.080)	.147 (.556)	-.038 (-.077)	-.083 (-.424)	.083 (.304)	.132 (.181)	-.040 (-.146)	.066 (.261)	-.104 (-.712)	--	.30	1.60	1.05	1.91
(5e)	PV2	-.085 (-.059)	-.064 (-.084)	.069 (.269)	.028 (.126)	- .117 (- .445)	.018 (.111)	.073 (.293)	.061 (.255)	.025 (.031)	-.012 (-.053)	.056 (.266)	.013 (.055)	-.011 (-.016)	.083 (.316)	.005 (.010)	-.064 (-.326)	.048 (.178)	-.049 (-.067)	-.015 (-.056)	.016 (.064)	-.046 (-.312)	.728 (.903)	.65	1.12	1.53	1.91

Table 5. Total and Net City Effects on Fear of Crime
Semi-standardized coefficients: effect of city in standard deviation units of dependent variable (N = 329).

	Limit Activities				Perceived Increase in Crime				Fear Areas of City				Fear in Neighborhood				Perceived Likelihood of Victimization			
	LIM1		LIM2		PI1		PI2		FC1		FC2		FN1		FN2		PV1		PV2	
	TOT	NET	TOT	NET	TOT	NET	TOT	NET	TOT	NET	TOT	NET	TOT	NET	TOT	NET	TOT	NET	TOT	NET
HOU	-.11	.07	-.15	.01	.06	.19	.25	.38	.05	.18	.35	.48	-.12	.02	-.14	-.04	.02	.16	.18	.26
LA	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00
WASH	-.45	-.32	-.41	-.30	-.44	-.37	-.39	-.36	.25	.25	.29	.36	-.34	-.31	-.24	-.23	-.31	-.24	-.15	-.15
MIN	-.78	-.62	-.75	-.62	-.52	-.44	-.32	-.21	-.13	-.08	-.10	.00	-.40	-.29	-.44	-.34	-.44	-.33	-.34	-.26

END