Colloquium on Stress and Crime

SUMMARY AND PROCEEDINGS VOLUME I

December 4-5, 1978
Arlington, Virginia

CENTER FOR THE STUDY OF CRIME CORRELATES AND CRIMINAL BEHAVIOR

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Colloquium on Stress and Crime

SUMMARY AND PROCEEDING VOLUME I

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PREFATORY REMARKS BY THE COLLOQUIUM CO-CHAIRMAN

Albert J. Reiss
Yale University
New Haven, Connecticut

Despite decades of effort constructing paradigms to understand crime, we are still groping for some integration of their separate perspectives. Broadly speaking, the biological, psychological, and sociological paradigms ebb and flow as each advances monomorphic explanations of crime. The true monomorphic paradigm that might integrate or obviate these competing paradigms eludes us as each perspective picks and chooses different kinds and forms of criminal behavior for explanation.

Biological and psychological theories display a preference for internal forces as causing behavior, either as motivating or mediating conduct. They likewise tend to characterize what is to be explained as consequences and as states or conditions of persons and events: "violent persons" commit "violent events." Sociological explanations of the same kind of behavior are more likely to explain its consequences, such as the harm done, or examine variations in its frequency, such as the rate of violent events. These explanations are often couched in terms of social organization, as in learning violence with group support, or culture, as in subcultural transmission through a "culture of violence."

Social-psychological theories try to bridge these paradigms as the literature on stress and violence bears ample testimony. And a conference from time to time such as this one on stress and violent crime also challenges the trained incapacities of each partisan group. Though in the papers and discussions that follow each group may still tend to make imperialistic claims for its paradigm, the discussion draws them together by a common reminder that we as yet understand very little about either individual or collective violence in crime and by a common bond that only scholarly paradigms and their empirical pursuit will enhance our understanding. That common bond here included a commitment to explore how promising are paradigms emphasizing the role of "stress" for explaining at least some, if not all, types of behavior labeled criminal.

Still, the common endeavor is not easily tackled when we do not quite speak a common language even when we speak the same words. It is readily apparent, for example, that stress does not mean quite the same thing to scholars with different explanatory paradigms. For some, it evokes concepts of biochemical states; for others, it elicits images of an internal experience of anxiety; for still others, it

calls up a construct of a culturally conditioned perception or a state of social forces that are conflicting or disjunctive, as in producing "strain."

The common language problem is not easily resolved as the concluding discussion often discloses. Much room for dialogue on matters of common interest remains, nevertheless, as the following questions tackled in one way or another in the discussion discloses: What are the major kinds of research designs that may enhance our common understanding of stress and violence, and particularly, what is the relationship of stress to criminal behavior (whether or not it is characterized as violent) or to the behavior of persons who have been labeled criminals (as in prison riots)? What kinds of variables can be manipulated in experiments or other research designs that are also amenable to social choice in public policy and administration? What kinds of populations should be selected to test our paradigms or enhance our understanding of crimes with violent consequences (whether to persons or property)? What are the effects of ethical regulation of inquiry for studying the relationship of stress to violence? What are our ethical responsibilities in different research designs? Where are the conflicting explanations among the competing paradigms? And finally, are there ways we can resolve them by crucial experiments or by longitudinal or other forms of designed inquiry?

If the scenario just sketched conforms somewhat to a reading of the papers and discussion that follows, fortunately one's experience in reading quickly departs from it. For the papers are appropriately judicious, when partisan, and serve more to open opportunities for dialogue around the integration of paradigms than to close it off. Fortunately, too, the dialogue is characterized by conflict as well as consensus, for each contributes to understanding in the long run.

I suspect that each reader of the volume, like the conference participants, casts a chadow that shelters and protects from what a common source might illuminate. Nonetheless, at the margins of the perfect shadow lies the penumbra where some of the light from the common source is caught, no matter how dimly. It would be presumptuous--perhaps foolhardy--and altogether unreasoning and unreasonable for me to ferret out where that penumbra lies for each of you as readers. Each of you, as have I, will discover something cast in a different light in the essays and discussion that follows. Each, also, happily may be influenced in other, albeit subtler, ways--ways that provide the insights and paths by which a symposium such as this one moves the collective effort forward by deflecting

individual achievement into new and productive ways. Alas, our readers cannot profit from those tangible and intangible ways that the personal experience of participation brought something new to members of this symposium. And I have had the special privilege of sharing a delightfully light burden of this conference with Professor Wilkins, Eleanor Chelimsky, and its National Institute and LEAA sponsors.

PREFATORY REMARKS

Leslie Wilkins
State University of New York
Albany, New York

When I was asked to cochair a symposium on "Stress," I first protested my ignorance of the subject. It seemed that I could claim only a decayed knowledge of one aspect of the use of the term, namely that concerned with engineering. Even then my engineering connections date to the time of World War II. On further consideration, however, it seemed that a topic of such wide-ranging scope might, perhaps, be chaired by somebody who, because he could claim no expertise, might at least be unbiased. As it may appear, some bias may show in one or two of my comments. This is because, as I found out, the problems of "stress" are such that no one in the field of criminal justice can claim a lack of involvement.

I mention this because I think that some potential readers of this work may, like myself when first approached, think that "it is of no interest to me." As it proved, this was for me a totally incorrect assessment, both of the topic and of the ways in which it was dealt with my the array of distinguished speakers who presented previously circulated papers. Prior circulation was, without doubt, responsible in part for the extremely high level of discussion and the incisive comments of participants.

The range of perspectives will be obvious from a glance at the Table of Contents. This is because the term "stress" has been regarded as useful by laymen, professionals and research workers in a variety of disciplines. The term comes first, in any technical meaning, from the field of engineering, although there is a linguistic meaning, as in "laying stress" upon a syllable in a word. It is, it seems, the engineering use of the word which has come to be used, through analogy, by workers in psychology, physiology, social work and social research. The use of the concept in the biological and social fields became evident in World War II and it has received considerable attention since that time.

It is gratifying that the Law Enforcement Assistance Administration through the Institute has called together a number of persons who have interested themselves in research centering around the concept of stress, and from quite different disciplines and persuasions in terms of their methods of investigation. The immediate pay-off from the symposium may be difficult to assess, but perhaps more attention should be given to important questions rather than to problems which seem likely to have a quick practical return on investment. The value of scientific activity is often best measured in the level of surprise

it occasions to those in the field, and surprises are not matters which can be estimated well in advance. The symposium, I think and hope, presented some items which will surprise readers! I confess to some surprise.

There is no general theory of stress which is accepted by participants. Rather the concept provides a focus upon forms of reaction which militate against the optimal functioning of individuals in society. Some focus upon the biological aspects--although the physiological reactions which are occasioned by stress are also characteristic of other responses of the human or animal involved. Some focus upon the social aspects of the immediate environment of the individual such as the family, some consider the individual personality factors, while others are concerned with the general environment using macro-economic indicators as a basis for inference. And there are other approaches.

Of course, stress may be "identified" as a "factor" which impairs human functioning in many ways which do not materialize in a criminal response. Nonetheless, it appears that there are many situations which are generated by those phenomena which we call "crime" and which fall within the scope of research workers concerned with problems of stress. The crime victim (as those who are victims of "circumstances") is clearly in a stress situation, but so also is the offender when he is subjected to the punishment inflicted upon him--even although this may well be his "just dessert."

Discussion was not concentrated only upon factors which were believed to have any causal association with stress. Rather it was accepted that stressing situations will occur, with varying frequency and severity to all persons. In addition to attempts to relieve the stress generating situation, persons could be trained in strategies for coping with stresses which they could not avoid. At this point, the practicality of the discussion will be obvious, as well as the difficulties in identifying the appropriate "coping strategies" and working out methods of education and training programs for those who are at specially high risk of stressful situations in the course of their occupation. In this regard the police and prison officers were noted and research involving some of their problems was noted.

I was particularly pleased to be able to share the honour and responsibility of chairing this important symposium with my friend Professor Albert Reiss, Jr.

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SUMMARY

BACKGROUND

The colloquium on "Stress and Crime," whose summary and proceedings are presented in this document, was the second conference conducted by The MITRE Corporation for the Center for the Study of Crime Correlates and Criminal Behavior of the National Institute of Law Enforcement and Criminal Justice (NILECJ). These colloquia were conducted as part of the activities under MITRE's grant from NILECJ to aid in the formulation of an agenda for a program of basic research into the correlates and determinants of crime. The Summary/Proceedings[1] and the Invited Papers[2] from the first colloquium (on the Correlates of Crime and the Determinants of Criminal Behavior) have been published. Publications from this colloquium will follow a similar format, with this document devoted to a summary and proceedings and another to the texts of the written papers.

The colloquium represents one of several approaches used by The MITRE Corporation to obtain information and suggestions concerning possible topics, methods and directions for the NILECJ research program. Other approaches have included: a Delphi-type survey of leading researchers in criminal justice; a selective review of a limited sample of recent literature in criminal justice research and theory; and a review of relevant research funded by federal agencies other than LEAA and the NILECJ. All of these approaches should be seen as alternate ways of tapping expert opinion concerning foci, topics and methodology for basic research into crime.

The colloquium on Stress and Crime was held in Arlington, Virginia on December 4 and 5, 1978. The attendees included 11 invited speakers, three discussants, two co-chairmen as well as representatives of LEAA and MITRE. A list of the participants appears on page 3. Among the invited speakers and discussants were individuals involved with research which has been concerned with the concept of stress in various ways. Some of the participants were directly studying criminal justice problems while the work of others was less directly oriented to crime

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[1] Otten, L. A. (ed.) Colloquium on the Correlates of Crime and the Determinants of Criminal Behavior, Proceedings, National Institute of Law Enforcement and Criminal Justice and The MITRE Corporation.

[2] Otten, L. A. (ed.) Colloquium on the Correlates of Crime and the Determinants of Criminal Behavior, Invited Papers, National Institute of Law Enforcement and Criminal Justice and The MITRE Corporation.

and criminal justice. A wide variety of disciplines and approaches (both methodological and substantive) were represented by the participants.

The colloquium served several purposes. Among them were:

- to present ideas for future research using the concept of stress as a correlate or determinant of crime;
- to introduce specific individuals to LEAA and the NILECJ who may not, in the past, have been known to them, as possible sources of research expertise; and
- to allow interchange among researchers in terms of ideas, methods, findings and theories which would aid the individual researcher and perhaps lead to joint and/or interdisciplinary research efforts.

The focus of this colloquium was determined jointly by MITRE and NILECJ. After considering several topics, the idea of the relationships between stress and crime, a relatively unexplored and interesting area, was agreed upon as a fitting topic for a colloquium. It was believed that stress research covered a wide range of disciplines as well as providing a theoretical focus relating to various types of criminal behaviors and criminal offenders. A position paper providing a rationale for a colloquium on stress and crime was prepared by MITRE and submitted to the Center for the Study of Crime Correlates and Criminal Behavior of the NILECJ. This paper reviewed a few major studies, such as the Midtown Manhattan Mental Health Study, using the concept of stress and dealing with behavioral and physiological consequences of stress. Areas of stress-crime relationships were suggested and directions of potential research delineated. This paper appears in Appendix A.

Several individuals who had written in the area of stress were then contacted with regard to their interest in participating in the suggested meeting. Among those contacted were several individuals whose studies had been examined in the literature review component of MITRE's project. Some researchers contacted in turn suggested others who were working on topics relevant to the colloquium. A distinguished panel of individuals was formed, who represented many disciplines and varied approaches to the concepts of stress and crime, substantively (in terms of specific topics), methodologically and theoretically.

The colloquium lasted one and one-half days, with the first day devoted to presentations by the invited speakers and discussions

stemming from these presentations. The second half-day consisted of a wide-ranging discussion among the participants. Consistent with the major purpose of the colloquium, that is, to provide MITRE and the NILECJ with ideas and directions for future research, most of the papers and the presentations as well as much of the discussion contained both specific and general ideas for research.

COLLOQUIUM AGENDA

Following (see pages 4-5) is the agenda for the colloquium. All listed presentations were delivered and discussion periods took place as indicated.

LIST OF PARTICIPANTS

Albert Reiss	Yale University
Leslie Wilkins	State University of New York at Albany
Marvin Opler	State University of New York at Buffalo
Morton Lieberman	University of Chicago
John Petrich	University of Washington
M. Harvey Brenner	Johns Hopkins University
Murray Straus	University of New Hampshire
John Lion	University of Maryland
Hans Toch	State University of New York at Albany
Robert Staples	University of California at San Francisco
Leonard Hippchen	Virginia Commonwealth University
Jonathan Freedman	Columbia University
Thomas Lalley	National Institute of Mental Health
James Thompson	The Vera Institute
Lynn Curtis	Department of Housing and Urban Development
Homer Broome	Deputy Administrator, Law Enforcement Assistance Administration

COLLOQUIUM ON STRESS AND CRIME
DECEMBER 4-5, 1978

MONDAY, DECEMBER 4--PLENARY SESSION, NORTH II

9:00 Welcome

Blair Ewing
Acting Director
The National Institute for
Law Enforcement and Criminal Justice

9:10 Opening Remarks of the Chairmen

Albert J. Reiss
Department of Sociology
Yale University

Leslie Wilkins
School of Criminal Justice
State University of New York at Albany

9:30 Research Papers

A. Stress and Adaptation

1. Social Stress and Rising Rates of Sociopathy

Marvin K. Opler
Department of Anthropology and Sociology
State University of New York at Buffalo

2. Stress, Adaptation, and Coping

Morton Lieberman
Department of Behavioral Sciences
University of Chicago

10:00 Discussion

10:30 COFFEE BREAK

10:45 Research Papers

B. Stress and Criminal Behavior

3. Criminal, Behavior, Arrest, and Life Change Magnitude

John Petrich, M.D.
Department of Psychiatry
University of Washington Medical School

4. Stress and Assault in a National Sample of American Families

Murray Straus
Department of Sociology
University of New Hampshire

5. The Influence of Economic Stress on Criminal Aggression

M. Harvey Brenner
Operations Research Department
Johns Hopkins University

11:30 Discussion

12:00 LUNCHEON--NORTH I

Police Stress and Criminal Events

Homer F. Broome, Jr.
Deputy Administrator for Administration
Law Enforcement Assistance Administration

1:30 Research Papers

B. Stress and Criminal Behavior (Continued)

6. Organic Determinants of Stress and Violent Behavior

John R. Lion, M.D.
Department of Psychiatry
University of Maryland School of Medicine

7. A Note on Prison Stress

Hans Toch
School of Criminal Justice
State University of New York at Albany

2:00 Discussion

2:30 Research Papers

B. Stress and Criminal Behavior (Continued)

8. Race, Stress, and Family Violence

Robert Staples
Department of Sociology
University of California at San Francisco

2:45 COFFEE BREAK

3:00 Research Papers

B. Stress and Criminal Behavior (Concluded)

9. Biochemistry of Stress Reactions in Crime

Leonard J. Hippchen
Department of Administration of Justice
and Public Safety
Virginia Commonwealth University

10. Crowding, Stress and Crime

Jonathan Freedman
Department of Psychology
Columbia University

3:45 Discussion

5:00 Reception

TUESDAY, DECEMBER 5--PLENARY SESSION, NORTH II

9:00 General Discussion

10:30 COFFEE BREAK

12:00 ADJOURNMENT

OVERVIEW OF THE COLLOQUIUM: MAJOR TOPICS AND SUGGESTIONS

This section, based on the edited transcript of the proceedings (see pages 45-227), summarizes the major themes and suggestions from the one and one-half days of the colloquium. Other research ideas can be found in the papers which are summarized in the following section (SUMMARY OF THE WRITTEN PAPERS) and in the texts of the papers in Volume II.

Definitions of Stress

Several definitions of stress were employed by the participants in their papers and discussions, while others did not specifically try to define stress. These definitions included:

- subjective demands on the person exceeding response capabilities;
- disruptive effects of events on important continually-experienced role relationships;
- exposure to negative social stimuli such as status devaluation leading to internal tension; and
- weighted number and magnitude of selected life events.

The concept of stress was used by some speakers as an intervening construct to integrate or explain relationships between independent and dependent variables while others used the concept to mean the result of certain events, states or conditions. Several participants employed stress as a key concept in their research and theoretical orientation while for others, stress was less crucial; that is, the variables, methods and findings did not appear to necessitate stress as an integrative or operational concept. There was concern expressed by several participants with regard to limiting the stimuli, conditions and states that are labeled stress in order to make the concept meaningful. All states of tension or anxiety or discomfort should not necessarily be considered stressful, nor should all criminal behavior, cardiovascular disease, etc. be considered as indicators of stress. Colloquium participants did not try to agree on a single definition or approach to stress.

Differences and disagreements in the approach to defining and measuring stress were manifested by Professor Lieberman in relation to the Holmes and Rahe method used by Dr. Petrich and Professor Straus in their papers to define and measure stress. The Holmes and Rahe method quantifies stress by using the number and magnitude (needed adjustment) of selected life events while Lieberman's approach views stress as related to those events which produce disruptions in important, ongoing role relationships and/or serious changes in day-to-day activities. Counting the number of life events, even if weighted on a scale of magnitude, without knowing their effects on role relationships and daily activities is not, according to Lieberman, a meaningful way of measuring stress. Professor Reiss questioned whether or not it is the number of events that is related to various adaptations, or whether the influence on various behaviors is better described according to probability models.

Another definitional question raised concerned the use of criminal behavior both as a stressor and as a response to stress (i.e., coping mechanism or adaptation). For example, in the paper by Dr. Petrich, the life event which changed most in the year prior to incarceration was "trouble with the law." In a similar vein one of the discussants (Mr. Thompson) was concerned with looking at the relationship between employment and criminal behavior, and whether criminal behavior was a coping mechanism, an adaptation, a failure of adaptation or another stressor. Can coping or adaptation itself be considered a stressor or source of stress?

It should be noted that both the problems of the possible dual or circular nature of criminal behavior (a result of stress and/or a cause of stress) and the other definitional issues raised earlier were noted and discussed, rather than resolved, during the colloquium.

Methodological Issues

A major theme which was encountered throughout the discussions concerned methodology. The issues raised appeared to deal with methodological alternatives that had implications for many aspects of social science research as well as for the study of crime, stress and the relationships among stress and crime. Three subthemes were discerned:

- Design considerations
- Analysis considerations
- Data sources.

Several designs were mentioned as appropriate for the study of stress and crime as well as for other substantive areas. They included: the longitudinal design; the longitudinal cohort design; multifactor studies of groups in their natural setting; individual (clinical) baseline designs; naturalistic studies of prisons; laboratory simulation studies; natural experiments (quasi-experimental designs); and cross-national studies. The longitudinal and longitudinal cohort approach received a great deal of support as necessary to determine cause and effect and to properly use various statistical techniques. Among the specific comments relating to longitudinal designs were:

- Longitudinal studies permit the development of causal models.

- Longitudinal studies are needed to determine the causal direction of the relationship between early experience of violence and assault by husbands against wives.

- In order to determine the causal direction of the relationship between stress and crime (does stress lead to crime as one reaction or does criminal behavior produce stress) more highly refined longitudinal studies are needed.

- Given a set of general predictors for criminal behavior in a particular cohort group, adding types of stressors to which this group may be exposed to the prediction equation may also allow prediction of the time when this behavior will occur.

- When using a longitudinal cohort, the social context (region, state, city, country) should be taken into consideration. One should simultaneously employ macro-level indicators of a region, state, etc. such as employment, mortality, crime and at the same time relate these indicators to what is happening to the cohort. One can use existing data bases such as the census to provide macro level indicators for geographic areas and/or population subgroups of which the cohort is part.

Although most, but not all, participants agreed with the need for longitudinal studies and longitudinal cohort studies, several other approaches were suggested. They included:

- Naturalistic, multi-factor studies of specific subpopulations (such as minority cultures) and institutions (such as prisons). This would involve

ethnographic approaches. In the words of Professor Opler, "We need to study the real lives of people." These designs would involve close observations, interviews, and other methods and would not attempt to greatly limit the number and types of variables studied.

- Descriptive and statistical studies of the physical and environmental conditions that promote or discourage criminal behavior, especially in urban settings. Professor Freedman believed that this method, although long and tedious, would provide important information for crime control.

- Clinical baseline designs using the individual as his/her own control. This design was suggested by Professor Hippchen to study the effects of biochemical and other biological treatments. He believed this design was necessary because of the complexity of biological factors involved which could not be dealt with through other procedures.

- Cross-national, cross-sectional and cross-cultural comparative designs. Professor Opler suggested comparative studies of high and low crime rate cultures and Professor Brenner suggested that in order to understand what is happening in one country, such as the U.S., comparisons with other countries are needed.

- Simulation types of studies in order to experimentally study the effects of stress in a laboratory situation. Professor Straus suggested the use of computerized "game" situations with a sufficient variety of stimuli and responses to investigate the effects of various stressors on responses in an interpersonal (husband-wife) context.

- Natural experiments using quasi-experimental designs. Professor Freedman recommended that researchers be alert for naturally occurring situations such as the closing of a factory and subsequent unemployment and its effects on crime.

Foremost among methodological issues dealing with analysis considerations was the level-of-analysis problem. The major concerns here involved level of data aggregation which in turn involves the type of units sampled, types of data obtained as well as interpretation.

The large differences in magnitude often found between correlations based on highly aggregated data (such as the relationships, over time, between unemployment rates and rates of many types of social ills) and the correlations between similar variables based on individuals as the unit of analysis are a prominent concern in dealing with units of analysis and levels of aggregation. Some of the ideas stemming from the discussions follow:

- Look at the relationships between highly aggregated data and its explanatory value and how this relates to disaggregated (individual level) data;

- Combine epidemiological and macroscopic approaches with more clinical approaches in studying responses of people to prison environments;

- Variables and classes of variables that may be causal factors are different at different levels of analysis, e.g., those variables which may be explanatory at the level of cities may be different than those using the individual as the unit of analysis; and

- In order to understand why there are clusters of behaviors which occur together such as stress, crime and other pathology, aggregated data using cross-national, cross-ethnic and cross-state types of comparisons are necessary.

The presentation by Professor Brenner concentrated on a multitude of studies showing the correlations of unemployment trends and trends of various social pathologies including rates of homicide, rates of imprisonment and arrest for various crimes, rates of suicide, rates of cardiovascular death, infant mortality, etc. for various countries, states and subpopulations. The relationships were very consistent. Measures of pathology used (including measures of crime such as arrests, imprisonment and offenses known to police) moved up and down (sometimes with a time lag) with unemployment rates over long periods of time and within all geographic divisions. Here Professor Brenner used a longitudinal method, although not a cohort, involving large and different geographic units of analysis and highly aggregated trend data on employment and indicators of pathology.

Although the consistency of the data over time, for different geographic units and for different types of pathology is impressive, nevertheless the data are highly aggregated. The statistical relationships of these economic factors (and their consequent stresses) to criminal or other responses on the individual level are not

determined by these data. Professor Straus provided an example from his data using the individual versus groups as unit of analysis. The correlation between stress level of husbands (or wives) and rate of violence was .9 using grouped data (eight categories) while the correlation between stress level and violence using the individual as the unit was .2. Most colloquium participants who addressed the level of analysis question believed that both the macro and micro level approaches were needed, and that one should not be used to the exclusion of the other.

Other issues concerning analysis were: the "cause of death" problem in which one alternative obviates all others; the use of tests of statistical significance when one cannot determine cause and effect sequences (the longitudinal design was considered essential by Professor Brenner and others to properly utilize significance tests); the use of designs which carefully control for all but one or two variables of interest and thus may prevent the development of "robust" models of reality by eliminating possible important influences; and movement away from the exclusive use of tests of statistical significance to decision-theory models.

With respect to data sources, several participants strongly urged researchers and funding agencies to utilize existing information. Using existing sources would, according to the participants who discussed this matter, save money, provide a greater return on money already spent for data collection and provide a relatively easy way of greatly increasing research payoffs. Specific suggestions included use of the National Victimization Survey to trace the victimization experiences of individuals and families when they move into different environments, the use of census and other existing data bases to add information about the social and economic aspects of different regions of the country to data collected on individuals in longitudinal cohorts, and secondary analysis of "rich" data sets such as the Midtown Manhattan Mental Health Study.

Differential Responses to Stress

A research theme sounded by several of the participants involved investigating differential responses to stress including criminal behavior. Stress was considered to be a stimulus to several kinds of adaptations or coping strategies including depression, criminality, violent criminality, alcoholism, suicide and socially positive adaptations. Responses to stress vary: in content; with time (individuals do not use the same adaptations to stress at all times); by different types of stressors; and as a function of individual and group characteristics. Research questions relevant to differential responses to stress follow:

- What are the coping strategies used by people who become criminals?

- How do subcultures influence the development of coping strategies?

- What kinds of stressors are predictive of a criminal response?

- What kinds of populations, under what kinds of circumstances will respond with one kind of reaction pattern as against another (crime, cardiovascular disease, suicide, etc.)?

- What are the conditions under which stress leads to physical violence, the conditions under which it leads to hypertension, conditions under which it leads to depression?

- Can empirically-based stress theory predict what individuals would react in different ways to intervention such as an employment program?

- What are the intervening factors, both on a macroscopic as well as on an individual level, that influence different types of response to stress?

- What are the effects of different states, nations and ethnic groups as well as age, sex, race and socio-economic status on types of responses to stress brought about by degradation of economic status?

- Is there stability of individual coping strategies over time?

Sociocultural and Ethnic Factors

Several research suggestions by Professors Marvin Opler and Robert Staples dealt with sociocultural and ethnic factors relating to stress and crime. Some questions for research stated by them follow:

- What are the social and cultural influences on the development of sociopathic personalities, especially the more violent type?

- What are the contrasts between high and low crime-rate cultures?

- What are the social and cultural as well as historical factors which make New York City a dangerous crime area in contrast to another very large urban area such as Tokyo?

- What are the conditions under which various subcultural and ethnic groups function with regard to their needs for help in coping?

- What factors control violence among some blacks and encourage it among others when all are exposed to stresses related to status devaluation as a group?

Other research suggestions concerning sociocultural and ethnic factors included the possible effects of multiple social disadvantages (e.g., black women) on stress and crime, and the exploration of changes with respect to crime and violence on the part of women.

Biological Factors

The presentations by Professors Lion and Hippchen focused on biological approaches to looking at the effects of stress on criminal and other forms of behavior pathology. Professor Lion's presentation was mainly concerned with how the brain influences certain types of behavior associated with crime, namely behavior under the influence of alcohol and sexual aggression. With regard to alcohol and crime some of the research questions raised were:

- How does alcohol lead to or play a role in violent criminal behavior, and why is alcohol so ubiquitously implicated in crime?

- How is alcohol linked to brain dysfunction?

- Is there such a thing as a latent criminal who is activated by alcohol?

- What is the relationship of alcoholic rage and brain dysfunction?

- Can alcohol and nonalcohol related crime be distinguished?

- Is disinhibition sufficient to explain alcohol's role in crime?

- Why are other drugs such as marijuana, which are also disinhibitory agents, not so highly linked with crime?

With regard to sex offenders Professor Lion recommended expanded investigation of hormonal treatment and studies to determine the endocrinological parameters of the sexually aggressive patient.

Controversy was raised when Dr. Lion recommended that prisoners be allowed to volunteer for intrusive experimentation including psychosurgery and drug studies, that participation in these studies be made a condition of parole or probation in some instances and that the NILECJ set up an organization to deal with the ethics of criminological research.

Professor Hippchen's presentation emphasized the need for those studying criminals to become aware of the biological literature as it relates to human functioning, especially the literature on biochemical, endocrinological, neurological, nutritional and environmental pollutant factors. These factors are important determinants of how individuals cope with stress. Biochemical and neurological factors themselves can function as internal stressors and lead to nonconstructive ways of dealing with stress. Dr. Hippchen stated that knowledge of biochemical and other biological variables would greatly add to the amount of variance explained as well as to the effectiveness of treatment programs. Among the internal stressors named were food toxicities, food allergies, nutritional deficiencies and hormonal imbalances. These factors along with external stressors are implicated in various forms of failure to deal with life such as failure in the family, failure at school, failure at work, violence, crime and other forms of deviance. Suggestions for research included studying alcohol and drug abuse in terms of biochemical factors such as vitamin and mineral deficiencies, food sensitivities and addiction to refined sugar starting in childhood.

Economic Factors

Economic factors such as unemployment, underemployment and instabilities in the economic cycles, were shown by Professor Brenner to be statistically related to indicators of stress such as various crime measures, deaths by suicide and cardiovascular disease, infant mortality, etc. Data covering several countries, periods of time of up to 65 years and several types of stress-related behavior, including many indicators of crime (offenses known to police, homicide, imprisonment, etc.) showed consistent trends in relationship to economic factors, especially unemployment. Professor Brenner's ideas revolved around stress brought about by negative changes in the economic conditions of nations, groups and individuals which affect various forms of maladaptation (including criminal behavior) and which in turn make it more difficult to cope with other stresses.

Economic factors such as unemployment among black males were also cited by Professor Staples as a source of stress, in addition to status devaluation of blacks and resulting loss of self-esteem. These and other stresses were considered important in producing relatively high rates of official violent criminal behavior among blacks. Professor Opler cited economic disruptions among certain cultural groups as an important factor in disruption of community, neigbhorhood and personal relations which he believes produces individual breakdowns that are minifested in sociopathic behavior (both passive and violent).

Family Factors

Professor Straus's presentation was concerned with serious assaultive behavior between husbands and wives. Using the results of a national survey, he found that the rate of both serious assault by husbands and by wives is related to stress measured by an abbreviated Holmes and Rahe scale. However, the focus of interest was in finding variables that differentiated, among individuals with the highest stress scores, those who did and did not engage in spousal violence. Among the variables investigated were: sociodemographic characteristics, education, importance of the marriage, values concerning the husband's role vis-a-vis the wife, experience of violence from own parents and observation of violence between own parents. These intervening variables may help explain how stress in the family leads to assault. Professor Straus views the family as perhaps the most violent institution in our society and, at the same time, that institution which provides love and support.

Professor Staples in his presentation on stress and crime among minorities touched upon the family as the arena for much of the violence that is influenced by the various stress factors he delineated. Professor Opler mentioned the breakdown in the role of the family in supportive interpersonal relationships as a stress factor leading to alienation and deviant behavior.

Environmental Factors

Another topic of discussion concerned environmental factors as related to both stress and crime. This was the focus of the presentation by Professor Freedman. Environmental factors were the immediate physical, psychological and social environments associated with buildings, streets, other public places, a neighborhood, etc. Professor Freedman expressed the view that careful study of the environmental factors which encourage or discourage criminal behavior had more utility for meaningful crime prevention than studies of large-scale social factors (economic inequality, racial prejudice) or psychological factors (parental behavior, personality characteristics).

Although he did not believe that housing design or crowding consistently produced stress or influenced crime as "main effects," he called for detailed studies of relationships between environmental characteristics and crime. Among the research suggestions made were:

- Situational factors which are likely to encouarge or discourage criminal behavior.
- Relationship between housing type and crime rate within the building as well as the crime rate and location of crime committed by residents.
- Detailed analysis of where, on streets, in schools, houses and in other kinds of environments crimes occur.
- Influence of street design, overall design of cities and how housing design relates to the city and the streets.
- Transportation factors as related to crime.
- Neighborhood organizations, mood of the neighborhood and how the neighborhood is organized, as related to crime.
- Mugging, armed robbery, rape, vandalism and shoplifting should be foci for research since they probably affect day-to-day living and attitude toward the environment more than other crimes in the city.

Applied Research

Another group of ideas discussed can be subsumed under the rubric of applied research, including evaluation research. Ideas were directed mainly toward research on change, treatment and amelioration of problems. Several participants contributed ideas relevant to this topic. Among the research efforts called for were:

- Reconstruction of marriages on a nonviolent basis in cases of battered wives--how can the husband-wife relationship be altered?
- Intervention techniques with sociopathic personalities in a prison population.

- Development of skills to help individuals cope with stress in a socialized and constructive manner.

- Development of training models which could be used with delinquents, criminals and families to teach coping skills.

- Amenability of coping strategies to intentional change efforts.

- Use of the medical concept of triage in criminal justice--who is amenable to treatment and who is not.

- Evaluation of techniques for dealing with symptoms to be carried out at the same time as research involved in looking at causal variables in crime.

- Evaluation of programs using biological interventions along with training programs to aid individuals in coping with stress.

- Study the possibility that an intervention designed to help individuals may itself be a stressor because of its consequences (e.g., an individual may be unsuccessful in or even dismissed from an employment program) and conduct research to identify those individuals most likely to be adversely affected.

- Study the effects of participating in research studies, especially those which may induce or reduce stress.

Stress in Prisons

Professor Toch presented a paper dealing with stress in prisons. He called his orientation a transactional approach. A large number of research suggestions were stated in his paper (see section below on SUMMARY OF THE WRITTEN PAPERS and Volume II). An important point was that stress was not a simple function of architecture, crowding, type of inmates, etc. Different situations are stressful for different people. He thus urged caution in the use of specific standards which are promulgated in order to ease conditions of prisoners (to make prison less stressful thus reducing violence and other negative consequences of stress).

As a result of his studies of prisoners, Professor Toch found that many criminals do not experience stress and that the lack of stress may be involved in criminal behavior as much, if not more, than stress. This

is a very different approach from that emphasized by most other participants. It signifies that it is the lack rather than the presence of stress which may be related to criminal behavior among certain types of individuals.

Ethics of Using Human Subjects in Stress Research

Part of the discussion during the second day concerned the ethics, politics and problems of using human subjects in general, and prisoners specifically. This was especially relevant with regard to performing research on stress which may involve negative stimuli, intrusive and invasive procedures. There was some conflict among the participants about using prisoners and allowing them to volunteer for research, especially with respect to whether or not prisoners are really free to make such choices. Mr. Lalley from NIMH provided the HEW view on use and protection of human subjects. The "rule" of not conducting any human research which may leave the individual "worse off" than before was mentioned by several speakers. Some believed that the procedures needed to conduct research with prisoners, including approval by prisoner committees, was hypocritical in light of the lack of freedom in other aspects of prisoners' incarceration. Conflict between the need to obtain knowledge so as to alleviate problems of crime and the need to respect prisoners' rights was also mentioned. Complete agreement among the participants did not emerge, however. A few of the suggestions concerning the use of human subjects follow.

- Allow prisoners to volunteer for invasive and intrusive studies, including psychosurgery.
- Allow prisoners to participate in research outside the prison as a condition of parole. (In studying violence the base rate of violence in the prison is too low to study the effects of drug therapy.)
- Create institutional procedures to deal with human subjects issues.
- Establish a formal organization to provide leadership for, and to deal with, the bioethics of criminological research.
- Aid in overcoming current procedural barriers for researchers engaged in work involving prisoners.
- Conduct research on assessing risks of participation in studies (e.g., to what extent does interviewing people about stress serve as a therapeutic process?)

- Use animals for stress-inducing studies and humans for stress-reducing studies.

Synopsis

The colloquium was a forum for the expression of several different viewpoints on stress, crime and stress, and crime. The many suggestions for research are contained in the papers (see the following section and Volume II) as well as in the discussions summarized above.

The different methodological approaches and the various processes and variables considered important in understanding crime, stress and the relationships between stress and crime, all probably deserve a place on a research agenda unconstrained by resource availability. In speaking to the differences which came out during the discussion, Professor Brenner stated what may be thought of as a fitting overview of the entire colloquium:

> ...to the extent that there is truth value in the biochemical approach, in the genetic approach, in the organic approach, in the family socialization approach, in the social-environmental approach--to the extent there is truth represented in any formulation--it is logically impossible that they actually compete with one another. Since there is truth in all these, what we require is a kind of analysis of variance design which allows us to take into account all these things.

SUMMARY OF THE WRITTEN PAPERS

"Stress, Adaptation and Coping," by Morton Lieberman

Dr. Lieberman reports on general findings and an approach to the concepts of stress and coping based on a longitudinal study (five year follow-up) of a sample of adult individuals representative of the census-defined population of the urbanized Chicago area. The major focus was not on crime as a response but on mental health--how individuals cope and adapt to stress. Professor Lieberman criticizes other approaches to stress research as being less than adequate to explain: what is stressful; mechanisms by which events and processes become stressful; coping mechanisms; etc. The criticized approaches are: (1) correlating various social factors (demographic and status characteristics) to adaptation; (2) relating various types and magnitudes of life events to physical and emotional illness; and (3) relating single important events (loss from death, separation or divorce, important transitions, etc.) to psychological distress and

adaptation. In contrast to these approaches, Dr. Lieberman and his colleagues concentrate on classes of events and on-going processes which relate to gains, losses or major alteration of roles in the life cycle. He distinguishes two types of events: normative events which are expected and predictable; nonnormative events which are often crises, which although commonly occurring are not easily predictable since they are not built into movement through the life cycle (divorce, losing a job, illness); as well as durable role problems which are often chronic and on-going in one or more role areas (occupational, marital, etc.). Adaptation in the sense of restoring homeostasis or psychological equilibrium revolves around these three types of circumstances.

The major findings of the study were:

(1) The occurrence of life strain (stress) as a result of normative and nonnormative events and enduring role problems, is not randomly distributed throughout the population but varies with social and demographic characteristics. In all the major role areas it is the lower socioeconomic classes, young people and women who are most vulnerable to life strain. The young and lower socioeconomic classes are also most likely to be involved in criminal behavior.

(2) Stress effects as indicated by changes in mental health were most often found for: nonnormative loss events in the occupational role (being fired or laid off, leaving a job for health reasons, being demoted); nonnormative loss in the marital role (divorce, separation and death); persistent role problems within marriage which produced more stress than the normative or nonnormative losses; and intense day-to-day problems in a particular role area, whatever the source of these problems.

(3) In general, the persistent day-to-day problems in the marital, parental and occupational roles produce stress and mental health problems more than normative or nonnormative transitions and crises, although nonnormative crises do have profound effects.

(4) The effects of events on mental health and adaptations are primarily a function of how these events (or ongoing processes like persistent role problems) influence the major roles which individuals

play in their lives. The more profound the role changes in terms of reshaping everyday existence, the greater the stress and need for coping. The events do not act solely or directly on the inner life, but through the reordering of more general life circumstances. Normative or nonnormative events in one role area have effects on mental health as a function of the changes in day-to-day life these events produce in other role areas.

(5) Normative and nonnormative transitions involving loss of old roles as well as entering into new roles affect mental health in terms of the degree and type of problems encountered in the new roles.

(6) Increase in mental illness was found to follow nonnormative crises rather than crises being a function of prior mental illness.

(7) Coping behavior protects the individual and mediates the impact of events by eliminating or modifying conditions giving rise to problems, controlling the meaning of experience to neutralize its problematic character and/or by keeping emotional consequences of problems within manageable bounds. These processes were found to be more effective in the role areas of marriage and parenting and least effective in the occupational role.

(8) Each role area appears to involve different means of coping--there is no one general good coping strategy, each role area having its own efficacious coping strategies.

(9) Coping strategies are as unequally distributed in society as vulnerability to life stress, with those of lower socioeconomic status having fewer effective coping strategies. In another study it was found that different ethnic groups (Irish, Polish and Italian) showed different patterns of adaptation to stress.

(10) The number of individuals in the study with problems who sought help was very similar for different sources of stress. Among those who sought help the predominant sources were

informal contacts. There was considerable variation within the group of individuals who did and did not seek help. One group identified as those who did not seek help had least effective coping strategies, lowest self-esteem, a very unsupportive and unreliable informal network and strong reservations about discussing their problems with others. Most social characteristics such as class, race or age did not strongly differentiate between those who did and did not seek help.

(11) The study was not able to demonstrate that those who sought and received help either through informal or professional systems were subsequently better off than those who experienced similar stressful events but did not obtain help.

The major point of Professor Lieberman's paper was that understanding of stress and adaptation must take place through detailed analyses of how events (normative changes and transitions, non-normative crises or persistent role problems) influence the important, continually-experienced role relationships in a person's life. This understanding is essential, given that the fewer the events and changes, the fewer the mental health problems and the less distress.

The major research suggestions made in Dr. Lieberman's paper are:

(1) Study the changes in everyday roles brought about by various life events to determine their effects on mental health and coping behaviors;

(2) Examine coping strategies used by individuals who engage in criminal behavior if crime is seen as failure to adequately cope with life stress;

(3) Study how stress-mediating behavior (coping) is learned and under what conditions. What are the socialization processes with regard to coping?

(4) Study subcultural influences on coping strategies;

(5) Study the stability of coping strategies over time;

(6) Study as a potentially criminogenic group the psychologically vulnerable group of individuals who have few meaningful community supports, relatively poor internal resources and a reluctance to seek external help for problems; and

(7) Study the extent to which coping strategies are amenable to intentional change efforts.

"Stress and Assault in a National Sample of American Families," by Murray Straus

Professor Straus' paper is concerned with assault and violence within the family. He considers the family the most likely place (with the possible exception of the military in time of war) for an individual to experience violence. Several theoretical arguments are raised to provide a rationale why the family, which is a source of love and support and gentleness, is also a violent institution. Among the factors presented are: differences and conflicts among individuals; the "battle of the sexes;" age differences; incongruence between expectations and realities in the areas of material resources and child rearing; the perceived legitimacy of using forms of violence to get family members to do (or not do) what is believed to be necessary; use of physical punishment of children (associating love with violence, legitimizing hitting for purposes of protection and learning, and associating something of substantive importance with the use of physical force); and the involuntary nature of family membership (often not permitting the individual to leave to avoid violence or situations which produce violence).

Stress is defined as a situation where subjectively experienced demands are inconsistent with response capabilities. Dr. Straus emphasizes that violence or other forms of aggression are not an innate or natural response to stress, and that events or stimuli which produce stress do not, by themselves, produce violence. He hypothesizes that certain intervening factors need to be present in order for stress to result in violence such as husband-wife assaults which constitute the subject of the study reported. Several of these factors have been examined, including: the experience of physical punishment by parents after reaching adolescence; observing parents who hit each other; belief in physical punishment of children; belief that husbands should be dominant; and low socioeconomic status. In order for husband-wife attacks to occur there has to be a set of beliefs and/or a learning history that would lead one to see violence as legitimate and congruent with the marital role and a form of behavior which will lead to desired goals.

Professor Straus and his colleagues conducted a survey of a representative sample of 2,143 American couples. The survey looked at the relationship between stress and husband-wife violence as well as the influence of various theoretically based intervening factors (in addition to stress) on the probability of violent behavior. Stress was measured by a shortened version of the Holmes and Rahe scale. Assault was measured by the Conflict Tactics Scales with

serious assault being defined as any of the following violent acts occurring in the course of a family dispute during the past year: punching, kicking, biting, hitting with an object, beating up, and using a knife or gun.

The following were the major findings of the survey:

(1) Males and females in the sample had similar indices of stress with the exception of the area of occupational stress where the males had a much higher index.

(2) Severe violence against the spouse measured by the Conflict Tactics Scale occurred at the rate of 3.8 per 100 for males and 4.6 per 100 for females during the year prior to the interview. For those reporting violence the median frequency was 8.0 for husbands and 8.9 for wives who reported engaging in violence against the spouse.

(3) The rate of assault increased for both males and females as the stress index increased, ranging from a rate of 1.1 per hundred at the lowest stress level for wives to 20 per hundred at the higest level; and 2.2 per hundred at the lowest stress level for husbands to about 14 per hundred at the highest stress level. The largest increase in violence for both males and females occurred from the next-to-highest to the highest stress level. The curve relating stress and assault rate approximates a power function for women and is irregular for men but generally follows an upward direction (more stress, higher rate of serious assaults against wives).

(4) Spouse assault rate increased with level of stress for various categories of stress with the strongest relationhips for "Spousal Stress" (sexual difficulties, separation or divorce and increase in arguments) and "Economic Plus Occupational Stress."

(5) In order to examine the intervening variables which have been hypothesized as important for violence to occur under conditions of stress, the men in the sample in approximately the highest quartile on total stress were selected for study. Comparisons in the rate of assault against their wives within the high-stress group were made according to high

and low groups on several intervening variables. Among the findings for men in the high stress category was that some groups had much higher rates of assault against their wives than those in the opposite category of the intervening variables. These high rate groups included:

(a) Those physically punished by their fathers and who observed their parents hit each other;

(b) Those believing that physical punishment of children and slapping a spouse were appropriate behavior;

(c) Those for whom marriage is not an important and rewarding part of life;

(d) Those working in low status occupations and achieving low incomes;

(e) Those believing that husbands should be dominant in the marriage and feeling they have achieved that dominance; and

(f) Those who did not participate in unions, clubs or other organizations.

(6) Educational level did not differentiate among highly stressed men in terms of rate of assault against their wives.

Dr. Straus points out that several of the findings listed under "(5)" may be open to alternative explanation especially in terms of causal direction. Some of the belief variables may be justifications of the assaultive behavior rather than their antecedents. Some of the intervening factors which differentiate assaultive and nonassaultive high-stress husbands may also be confounded with socioeconomic status. Professor Straus calls for the highest priority for a longitudinal study to test theories about the link between stress and violence.

"Crowding, Stress and Crime," by Jonathan Freedman

Professor Freedman carefully reviews his own research and that of others on the effects of crowding and density on various forms of human pathology (physical and social), including crime and delinquency. He concludes that the evidence shows no consistent relationships of crowding to crime rates, delinquency rates, health indicators,

mental health, infant mortality, adult mortality, etc. Some studies have shown positive correlations between crime rates and various measures of density (number of people per household, number of people per room and area density) but these have disappeared or have been greatly reduced when other factors have been controlled, e.g., income, education. No consistent results regarding density and crime rates or type of crime have been found in several different large cities in the U.S. and elsewhere. In Dr. Freedman's study of New York City he found very little relationship between density and crime even within income levels.

Dr. Freedman concludes that studies of the relationship between density and crime show that (1) any positive relationships tend to be very small with, at most, 10 percent of the variance in crime rates related to variation in density when other crime-related factors are controlled; (2) many of the studies find positive relationships of some measures of density to crime rates but not with other density measures; and (3) there is no direct evidence to support any link between density and indicators of stress (health, mental health, mortality, etc.) which is supposed to underlie a density-crime relationship. The lack of consistent density-crime rate relationships cannot be attributed to inconsistent measures of crime according to the author. Although there are large variations in how crime is measured and the measures are far from accurate, relationships between crime rates and other variables such as income and ethnic group are large and consistent. This provides some evidence that the lack of density-crime relationships cannot be explained by biases in measures of crime rates. Other evidence that there is little or no relationship between density and crime comes from the decreasing density of major cities in terms of total population, number of people per room or amount of space per person over the past 30 years while at the same time crime rates have sharply increased in these cities.

Studies cited show that people living in higher density cities seem to be as healthy and suffer from no higher rates of mental illness than those in less densely populated communities. None of the usual indicators of stress such as stress-related illness, infant mortality, mental illness are consistently found to be greater in higher density areas. Dr. Freedman does not deny that in certain circumstances crowding can be unpleasant and stressful or that intense crowding may be harmful in many ways but that within the range of densities usually found in this country in both homes and cities, crowding is not a generally negative influence.

Dr. Freedman criticizes the housing-crime (environmental design) studies conducted by Newman and concludes there is no evidence that highrise, large housing developments produce more crime once income and area are equated. He also reports on laboratory studies on the

effects of crowding. In general, when the amount of space is varied there are no overall effects of density (either positive or negative) in terms of efficiency, creativity, aggression, friendliness or happiness, with other factors held constant. There have been some findings showing differential effects of density on males versus females although all studies have not shown the same effects. Feelings of control over the situation, awareness of variations in density and other psychological factors play a role in determining the effect of density in laboratory studies.

Although Dr. Freedman provides strong empirical evidence against any consistent effects of density or crowding on crime or other variables, he does speculate that there are effects on how individuals respond. He believes that under high density one's reactions are intensified in either positive or negative ways. "Under high density, the other people who are present become more important, more salient features of the environment. Their actions are more likely to be noticed and more likely to impinge on and affect the individual...This causes the reactions to the people to be stronger." Intensification of response is not necessarily stressful nor does it lead to negative effects. Responses may be positive, negative or neutral, but more likely to be stronger than under low density. In this manner, density in combination with other factors in a particular situation or in a community may have important effects on relationships among individuals, on aggression and on crime.

Professor Freedman suggests several areas of future research. Among them are:

(1) Analyze neighborhoods and families living under high and low density conditions with regard to the interactive effects of closeness of relationships and density to crime.

(2) Study the relationship between housing type and crime rate within the building and crimes committed by residents. This would involve selecting various types of buildings in many communities, assessing crime rates and obtaining other information for purposes of control (demographic characteristics of residents, building design, length of residence, characteristics of the area in which the building is located, characteristics of the community).

(3) Conduct research to answer the question, "If a building is to be built for a particular population in a particular area what would be the best type of building in terms of minimizing crime?"

(4) Investigate how different people respond to different levels of density. There is some evidence of sex differences and there are perhaps other consistent individual differences in response to density level. Some of the suggested potential factors to measure are life stage of the individual and family situation, The dependent responses to variations in density could include aggressiveness and friendliness, criminal behavior, and indicators of pathology and health.

"Social Stress and Rising Rates of Sociopathy," by Marvin K. Opler

Professor Opler defines various forms of maladaptive behaviors as sociopathy. He uses as examples such behaviors as drug use, alcoholism, assaultive behavior, delinquency, child abuse, family desertion, and specific cultural forms such as amok and latah. He attributes much of this type of behavior (both active and passive) to large social forces affecting individual psychodynamics within specific cultural settings.

An important distinction in sociopathic behavior is made between active types (homicide, assault, rape) and passive types (alcoholism, suicide, skid row syndrome), with the major concern of the public occurring in the active or violent types of behavior. Among the larger social forces Professor Opler cites: the decreasing supportive and economic functions of the nuclear family; increasing urbanization and suburbanization; the loss of importance of the community and neighborhood and increasing emphasis on individualism; individual mobility which decreases family and community bonds; and the general weakening of person-to-person emotional and supportive ties (anomie). Some of these social forces are traced to even larger-scale changes in the American society such as industrialization, the movements to and away from cities, and the loss of economic functions of the family. The self-orientations which make life more impersonal as well as weaken various family and community structures have been shown, in the writings of Hendin, Riesman and Fromm, to be the individual manifestations of these larger social forces.

Dr. Opler cites the Midtown Manhattan Mental Health Study which examined mental health and other types of impairments in life functioning, and lists some of the physical, developmental, familial and relationship stresses which were antecedents of these impairments. He contrasts the impersonal and limited ties of the modern nuclear family with the extended matrilineally related families of the Navajo outfit and discusses the lack, at least until recently, of increased sociopathic and criminal behaviors in Japan although that country has been exposed to economic and urbanization forces similar to those affecting our society. This situation in Japan is attributed to the

continuity there of traditional interpersonal relations and social controls despite fairly massive social changes. Increases in mental illness, alcoholism and crime occurring in formerly primitive cultures have been shown to result from the stress of social changes, including urbanization and the weakening of family and tribal networks and relationships.

The major research suggestions given in the paper are:

(1) Ethnic and cultural family group studies, and

(2) Studies of the actual lives of individuals in their cultural and social settings concentrating on how social dynamics affect psychodynamics resulting in stress and its manifestations.

"Race, Stress and Family Violence," by Robert Staples

Professor Staples is concerned with explaining the over-representation of blacks in official statistics of criminal violence and especially the type of violence which occurs in the family and among friends, relatives and acquaintances. After reviewing some of the statistics concerning violence among blacks, Dr. Staples discusses general socialization factors which may play a part in violence among blacks. He dismisses genetic influence by citing cross-cultural data on African societies showing lower rates among Africans than among American blacks. Some of the possible social-cultural factors relevant to violence among blacks are: role models on television and the movies (black children watch TV more than white children); exposure to violence at an early age including shooting, robberies and rape, especially among lower-income groups; the structure of low-income public housing which is conducive to certain forms of violence; the status-conferral system in the ghetto in which the highest level of esteem and respect is reserved for the best fighter and even for those who have killed somebody; the encouraging of fighting among younger males by older males; and the general violence in American society including violence such as wars sanctioned by the government.

With regard to sexual aggression of men toward women (including rape) Dr. Staples attributes this, in part, to sexist socialization of all men. Among black men this is more pronounced due to racist attitudes and the economic position of many blacks have denied them power and certain symbols of manhood, thus emphasizing sexual aggression as one of the few ways of asserting power and dominance. Other aspects contributing to sexual aggression concern the accepting of white men's attitudes toward black women by black men and the expectations (sometimes encouraged by the coquettishness of the

female) of sexual favors by men from women in the dating situation which is then denied. One theory of black sexual aggression concerns the feeling of powerlessness and reaction to female authority figures (heads of household, teachers). Power is associated with the acquisition of wealth including power to extract sexual favors from women. Since many black men have been denied the legitimate opportunities to acquire wealth, sexual aggression (rape) is often the only perceived access to sex. "For black men, rape is often an act of aggression against women because the kinds of status men can acquire through success in a job is not available to them."

In discussing violence between spouses, Dr. Staples points out that it is probably more common among the lower classes than in the middle and upper classes, and even more common among lower class blacks for reasons also associated with socioeconomic and racial status. He cites beliefs (for example, that physical violence against a wife is natural to keep her in line, or that black wives will seek sexual satisfaction elsewhere if relations are not going well) as causes for spousal violence. Jealousy due to extramarital affairs, which are common in the lower class black community, often leads to serious violence which is considered justified. Black females are often involved as the killers or aggressors in spousal violence. Again, Professor Staples views spousal violence among blacks as a function of their social and economic position. Many black males because of their lack of economic resources (often the woman is the economic provider) are unable to provide for their families and thus have a problem maintaining status in the eyes of their wives and children. Violence as a response to conflict is a method of achieving status and control when the man cannot do so in the role of provider and head of house. Violence by the black male against his spouse may also be a reaction to devaluation of self-worth stemming from feelings of failure and powerlessness which is in turn a function of economic failure and racism. Frustration from lack of power and hopelessness often results in intragroup violence, with the wife the object of attack.

Another area where there are high levels of black violence concerns parent-child violence. This type of violence is concentrated among the lower classes and is related to underclass status and poverty as with the other forms of violence discussed. Among the conditions in which violence flourishes are: large size of lower class black families; small amount of living space; one-parent families where the woman works and must discipline her children when she gets home; and the frequency of the use of physical punishment. The utility and necessity of physical punishment to control children are both accepted; and the use of physical punishment can lead to serious injury of the child. Resources and rewards available to middle class children and

used by parents for discipline and learning are often not available to lower class blacks. Thus, physical methods are used more frequently.

The social position of many blacks involves the lack of economic resources, social status and ability to adequately fulfill role requirements as a breadwinner, head of household and parent. This affects interpersonal relations making violence often the only perceived means of solving conflicts and achieving status and prestige. This combined with status-devaluation, frustration, powerlessness, physical conditions of living, role models and a set of norms that permit and encourage violence explain the relatively high rates of sexual aggression, marital violence and child abuse among lower class blacks.

The research questions stated by Professor Staples includes:

(1) Since all blacks are exposed to stresses associated with status devaluation as a group, what factors control violence in some and encourage it in others?

(2) How do social support systems affect the incidence of family violence among lower-income blacks? Some of the factors that should be explored are the role of the extended family, rural versus urban location and consequences of primary over secondary relationships

(3) What are the effects of social class on family violence among blacks? Does access to certain social values and resources tend to mitigate the need for violence as a form of conflict resolution?

(4) How are sex roles defined in the black community? Does the independent role ascribed to women lend itself to provoking assault by husbands?

(5) How is the parent-child relationship defined? Does the need to exercise parental authority encourage the use of excessive violence toward children?

(6) What are the parameters of two types of family violence less often studied, husband abuse and violence by children toward parents?

(7) Does family violence increase as the rate of unemployment increases? What is the relationship, if any, between the type and status of occupations and the incidence of family violence?

"The Biochemistry of Stress Reactions and Crime," by Leonard J. Hippchen

Professor Hippchen's main thesis is that internal stresses leading to many forms of behavior pathology--including juvenile delinquency, adult criminality and violence--are, in part, due to biological factors such as genetic weaknesses, biochemical brain and nervous system deficiencies, food allergies, toxic chemicals, metabolic disorders, minimal brain leisons, physical handicaps, etc. He also speaks of external stresses to which individuals are exposed, e.g., hostile neighborhood, living in a conflicted family, school failure, poor economic conditions, racial prejudice, uncertainty, inconsistent discipline, etc. Stresses may lead to failure in life's tasks and to the development of abnormal forms of behavior, with delinquency and crime as one possible result of unsuccessful attempts to adapt to stress.

Dr. Hippchen briefly reviews developmental processes which affect growth and functions of the brain and some of the factors which may affect this development: genetic abnormalities (metabolic, chromosomal); maternal diet during pregnancy; vitamin and mineral deficiencies; sensory inputs from the environment; and patterned sensory input (response sets learned in coping with both internal and external stressors). Chemical properties of the brain are involved in learning and memory. Electrical activity and changes in the central nervous system are probably mediated through chemical processes.

With respect to biochemical mechanisms influencing crime, Dr. Hippchen mentions genetic predispositions (chromosomal abnormalities, metabolic errors) which create stress, and stress due to brain damage and abnormal nutritional intake in early development which leads to limitations in later life. He states that sensory inputs (negative social experiences and negative attitudes toward self, others and social institutions) also contribute to anti-social behavior.

A brief review of biological effects on various behavior, learning and developmental disorders (including criminal behavior) is organized around the following factors:

(1) Vitamin-mineral deficiencies and dependencies--optimum levels of molecular concentrates of many nutrients are needed for brain growth and effective functioning.

(2) Neurochemical factors in brain disorders--the effect of neurotransmitters (serotonin) on aggressive behavior, oxygen deprivation, drug and alcohol intoxication.

(3) Environmental pollutants--lead toxicity and hyperactivity, radiation from lights and television related to behavior problems in children.

(4) Hypoglycemia--violent behavior among some hypoglycemics and other physical and behavioral symptoms due to hypoglycemia.

(5) Cerebral allergies and addictions--reactions to food and food additives (hyperemotionality, hyper-aggressiveness and hyperactivity) and addiction to refined sugar leading in later life to alcohol and drug addiction.

Dr. Hippchen sees many of these biochemical variables as negatively influencing behavior, especially that of children. The behavior is a sign of stress brought about by the various deficiencies, pollutants, allergens, addictions, and other neurochemical factors. Social reactions to this type of behavior, e.g., hyperactivity, often produces greater stress and reinforces anti-social ideas and behavior.

Research suggestions stated by the author include:

(1) Conduct a thorough search of the literature relative to the biochemistry of delinquency, crime and related forms of anti-social behavior;

(2) Investigate the biochemistry of hyperactivity in children and violent behavior in youth and adults focusing on identifying interacting factors;

(3) Explore vitamin B-3, B-6 and C deficiencies as well as the effects of deficiencies of minerals such as copper, calcium, magnesium, manganese and zinc;

(4) Study levels of neuroregulators such as serotonin and tryptophan as related to behavior disorders and violence;

(5) Study the effects of heavy metals (lead, mercury and cadmium) and various forms of radiation (from TV and fluorescent lighting) on behavior;

(6) Delineate the metabolic processes related to regulation of blood glucose levels with regard to both hypo- and hyperglycemic reactions;

(7) Study allergy to food and chemicals as they are related to explosive forms of behavior;

(8) Investigate the biochemical basis of addiction to alcohol and drugs focusing on specific cerebral allergens such as refined sugar and nutritional deficiencies, especially those of vitamins B-3, B-1 and C;

(9) Conduct a broad literature search of the genetic basis of anti-social behavior prior to making specific research recommendations in this area; and

(10) Establish a national center for criminological research to explore biochemical and related bases of crime (especially violence) and other forms of anti-social behavior to obtain basic knowledge and improve correction and prevention.

"Organic Determinants of Stress and Violent Behavior," by John Lion

Dr. Lion's paper is concerned with the role of brain dysfunction as an organic determinant of stress and its relation to various forms of criminal behavior, especially violence. He briefly discussed several lines of evidence, which show brain dysfunction to be involved in violence and pathological sexual behavior, such as the following:

(1) Some violent criminals demonstrate EEG dysrythmias, neurological abnormalities and organic dysfunction;

(2) Violent patients, many with criminal histories, show EEG-measured dysfunctions and indications of minimal brain dysfunction. Follow-up studies of children with minimal brain dysfunction show a small percentage who continue to have the clinical indicators of MBD and who demonstrate mood lability and aggressiveness;

(3) Aggressiveness and violence among some individuals with limbic system abnormalities and among some epileptics;

(4) Impairments in brain processes involved in waiting, contemplating, fantasizing and reflecting which may exacerbate stress for some who show labile mood shifts, "hair-trigger" tempers, rage when frustrated and all-or-none assault under minimal provocation;

(5) Effects of alcohol on the brain and the relationship of alcohol to violent criminal behavior--disinhibition, pathological intoxication; and

(6) Effects of hormones on serum testosterone and spermatogenesis and the resulting positive effects of hormone treatment on some men with sexual behavior pathologies show the influence of the brain operating through the pituitary.

Professor Lion cautions the reader about the seductiveness of viewing all crime and aggression as a product of brain dysfunction and its implications for treatment. He also cites the emotional and political reactions that are often provoked by looking at crime and violence from an organic point of view.

Various suggestions for research contained in Dr. Lion's paper include:

(1) Lift the moratorium on psychosurgery;

(2) Allow intrusive and invasive research with prisoners on a voluntary basis, permitting prisoners to participate in field studies as a condition of parole or probation, (e.g., to study the effects of antiaggressive drugs in both a prison and an outside setting where individuals would be subject to stresses not found in a prison environment);

(3) Examine how alcohol leads to or plays a role in crime and aggressive behavior by researching such questions as:

-- Is there such a thing as a latent criminal who is activated by alcohol?

-- Does alcohol influence the aggressive component of crime?

-- Is disinhibition sufficient to explain alcohol's role in crime?

-- Why don't other drugs such as marijuana have similar effects on crime?

-- What is the precise psychophysiology whereby alcohol and violence are linked?

-- How many patients with alcohol-related violence can be identified and distinguished from nonalcohol-related violence?

(4) Study the effects of chemical agents such as progestational compounds on sexual criminals, including aggressive paraphiliacs; and

(5) Define the endocrinological parameters of sexual criminals.

"A Note on Prison Stress," by Hans Toch

Professor Toch's paper deals with the concept of stress as it relates to inmates in prison and to some extent, prison guards. He deals with stress from a transactional point of view, an approach in which stress, reactions to stress and consequences of stress are seen as a function of the individual and the context in which that individual "lives." The situations and stimuli that act as stressors as well as modes of adaptation are different for different subgroups (based on culture, demographic factors, etc.) and different individuals. What is stressful for one person is not for another; aspects of the prison that appear as stress-inducing to an outsider are not necessarily so for all inmates. Dr. Toch considers stress as resulting from an experience of an environment which is perceived as difficult to negotiate and is dependent upon what environmental features will be salient for the individual, what coping skills can be brought to bear and what is noxious or attractive to the person. Thus, it is difficult for an outsider to predict what will be stressful for any particular individual.

In terms of consequences of stress, Professor Toch emphasizes that experience of stress in prison may, for some inmates, aid in rehabilitative efforts and all stress should not be considered negative and something to be avoided. Thus, relatively nonstressful environments may not aid in changing offenders. However, court decisions and other decisions specifying prison standards are generic in nature and do not consider individual differences in what is stressful and how inmates can adapt.

Dr. Toch briefly reviewed literature relevant to the issues of different stressors for various subpopulations of inmates and

differential modes of adaptation. For example: women in prison are concerned more with deprivation of companionship than with deprivation of security; young inmates are more responsive to deprivations of autonomy; inmates of Latin heritage often suffer emotional breakdowns in response to outside family problems; schizophrenics respond negatively to environments containing high noise levels or danger cues; and solitary confinement can produce panic among blacks who are comparatively resistant to other stressors.

Toch then discusses the current interest in prison architecture. Although not discounting the possible effects of architectural design on stress and general adaptation, he makes the assumption that the variables that determine stress include people, activities, relationships, responsibilities, challenges, roles, conversations, food and rest. These variables are not predetermined by the design of the prison although they may be circumscribed by it. Different settings and building designs have differential effects on various subpopulations. A large fortress-like building may be depressing to an outsider but not necessarily so to all inmates of that building. A prison which is generally depressing may have "neighborhoods" in which the features of the total environment are not present. There are settings which reduce experienced stress in the prison that arise spontaneously without staff design.

Among the many research suggestions and questions stated in Professor Toch's paper are:

(1) What are the subenvironments, both designed and spontaneous, which act as stress-reducing enclaves within the prison?

(2) What is the contribution of prison architecture to stress, adaptation, etc.?

(3) What are the differential inmate susceptibilities to hypothetical stressors using contemporary and retrospective (debriefing) surveys, validation of stress through physiological indicators and correlations with personal history?

(4) How is stress ameliorated?--This would be studied by means of experimental variations in stress-reducing programs for stressed inmates;

(5) Can stress be prevented?--Procedures can include the use of prison staff to select stressed inmates and to study the cues used by staff in making such assessments;

(6) Can constructive anxiety be generated to promote social learning in rehabilitative efforts such as furloughs, therapeutic communities, halfway houses and enriched parole programs?

(7) Who are stress-susceptible inmates?--Records of stressed inmates can be used to develop predictors of vulnerability to stress;

(8) Given a shared stressful situation (such as a riot) what are the behaviors generated by stress and what are the correlates of differences in stress-induced behavior? Is such behavior immediate or delayed? How is such behavior related to previous stress experience and to physiological indicators?

(9) Are there variations in prison environments which are correlated with differential stress-inducing properties among equivalent inmates?

(10) What is the relationship of stress patterns to inmate career points, stages of prison adjustment and stages of prisonization? What are the stimuli that provoke prevalent stress experience at each stage? What coping strategies are used by inmates who experience less stress in comparable situations?

(11) How does a past history of being stressed or of succumbing to stressors bear on susceptibility to prison stress? Are there inside-outside continuities and discontinuities? Is special programming indicated for inmates on the basis of stress-related histories? and

(12) Can we develop interconnected typologies of stressors, vulnerabilities, stress-perceptions and stress reactions using demographic, physiological, phenomenological and situational data?

"The Influences of Economic Stress on Criminal Aggression," by M. Harvey Brenner

Professor Brenner presents seven popular viewpoints or theories concerning crime causation, briefly describes them and attempts to relate them to economic factors, especially to disturbances and contractions in the general economy. These viewpoints are: economic loss; relative decline in socioeconomic status as a result of greater gain

in such status by the majority of the population; reduced opportunity in legitimate sectors of the economy; frustration-aggression theory; subcultural deviance in both values and normative patterns as a reaction to lack of socioeconomic integration; differential association theory; and loss of community integration due to urbanization and economic growth. He then discusses rational and irrational behavior models in which criminal behavior due to economic change contains elements of both psychological stress and coping strategies. Coping strategies may be "irrational" or "utilitarian," sometimes translated into violent versus property crimes respectively. Criminal behavior represents one coping mechanism to stress situations brought about by economic factors and may include both irrational and utilitarian elements.

Several measures of economic change, and the mechanisms by which these changes lead to stress, are listed. The change measures are: general economic cycles; economic instability (departures from smooth growth patterns); changes in the structure of economic inequality; changes of the extent to which specific subpopulations gain or lose employment and income during economic cycles (upturns and recessions) compared to the general population; secular changes in income distribution among population subgroups; and secular changes in income levels among population subgroups. Measures of mechanisms by which these changes produce stress are: change in economic well-being; change in relative socioeconomic status of subpopulations; income inequality; proportions of income distributed among various subpopulations; and economic instability (i.e., the degree to which levels of income and employment are subject to fluctuation). These mechanisms are hypothesized to create stress in individuals, in subpopulations and in a country.

In the various studies conducted by Professor Brenner, several economic measures were related to criminal justice data. They included: fluctuations in the rate of employment and unemployment; annual percentage changes in the Consumer Price Index; intermediate range (1-5 years) patterns of national economic growth; differential trends in income and employment among various subpopulations; and differences in income and employment of selected minority groups versus the population as a whole.

In order to overcome the problems in using criminal justice statistics (e.g., underreporting, changes in reporting patterns, differences among jurisdictions in reporting and recording, differential accuracy in reporting from various components of the criminal justice system, etc.) and to increase the reliability and validity of general findings, several criminal justice measures and analytical techniques were employed and related to economic change measures. Among them were: multiple criminal justice indicators (crimes known

to police, arrests, criminals brought to trial, convictions, imprisonments); different types of crime; different geographical and political units (states in the U.S., the U.S., Canada, England and Wales and Scotland); different spans of time; data from outside the criminal justice system such as vital statistics on homicide; and use of different age, sex and racial groups. Among the statistical techniques used were: differential trend analysis; regression versus spectral analysis of time series; bivariate and multivariate analysis; and analysis of the consistency of the relationships between economic and crime trends over all measures, geographic units, time periods and subgroups.

Summarizing a large number of statistical studies Professor Brenner cites the following major findings:

- Three measures of the economic state--unemployment, Gross National Product and Consumer Price Index--explain considerably more variation in criminal justice indicator trends than any one. Their combined effect accounts for more than 90 percent of the variation in trends for many criminal justice indicators and holds for the time from the early 1900's to the late 1960's, but more so after World War II.

- Each of the economic measures has statistically significant independent effects on criminal justice trends.

- Cyclic fluctuations in employment and income show the highest relationships to criminal justice trends, accounting for 40-60 percent of the trend variation prior to World War II.

- Since the second World War, the effects of economic growth and inflation have been especially pronounced with regard to predicting crime trends. These measures together with measures of cyclical economic instabilities including unemployment, often account for over 90 percent of the variation in criminal justice trends.

- Based on regression equations and using crime data for 1970, estimates of the effects of a one percent change in unemployment on various levels of crime produced increases between 2.2 percent for burglary to 8.7 percent for narcotics offenses.

"Criminal Behavior, Arrest and Life Change Magnitude," by John Petrich

Dr. Petrich's paper concerned studies of life changes in the years preceding incarceration for samples of prison inmates, felons in a jail and juveniles in a detention facility. Comparison groups of nonincarcerated juveniles and adults were also used. The method of measuring the content, frequency and magnitude of life events prior to the crime for which the offender sample was incarcerated was based on that of Holmes and Rahe. This method has been used previously to look at the life event antecedents of various physical illnesses and behavior disorders. The instrument used was the Schedule of Recent Experience (SRE) which is a self-report measure of events befalling the individual prior to some point in time, such as being incarcerated or becoming ill. The 42 items on the SRE have been classified into 8 categories: conflict with the law; spouse-related events; family life changes; school changes; financial changes; changes in working conditions; personal events (outstanding achievements, death of a close friend, illness or injury, etc.); and life style changes (items concerned with eating, sleeping, recreation, church, residential changes, living conditions, social activities and vacation). The events on the SRE have been weighted according to the required adjustments needed, i.e., potential amount of stress. The Life Change Unit (LCU) score (the number of events reported multiplied by their weights) was calculated for each individual. The data were also analyzed by computing the mean annual frequency of each life event category for each group.

Major findings include:

(1) No significant differences in the number of life events reported in the three years prior to incarceration between the samples of jailed and imprisoned men although some of the prison inmates were reporting on periods five years in the past (before current incarceration) while the jail group were reporting on events prior to a very recent incarceration.

(2) A comparison of mean life change units (LCU) for a selected sample of 30 (out of 206) jail and prison inmates and a normative group of men obtained from local industry and a television audience (N=21) showed: a lower mean LCU for the incarcerated men than the normative group during a period of three years prior either to incarceration or to completing the instrument (for the normative group); similar mean

LCU's for both groups during the period two years prior to incarceration or to completing the instrument; and a greater increase in mean LCU for the incarcerated sample during the one year prior to incarceration than for the normative group. The incarcerated subsample were under 35 years of age, white, did not have more than a single injury or illness during the past year and did not have more than a technical school education.

(3) The difference between normative and incarcerated samples during the year prior to incarceration was due entirely to one category on the SRE, that is conflict with the law. In most other areas covered by the SRE the incarcerated group showed slightly lower frequency of reported life changes than the normative group.

(4) Incarcerates charged with assault showed a higher number of reported conflicts with the law prior to incarceration than felons charged with murder or property crimes. Otherwise there were only small differences in frequency of life changes reported among the three subgroups of incarcerates (those charged with assault, murder or property crimes).

(5) For the jail inmates the ratio of reported jail terms per reported law violation increased in the two years prior to incarceration; in that period it was greater than one--more reported jail terms than reported law violations.

(6) For the juvenile offenders in an institution, a modified SRE was used. The mean annual LCU increased as the measured offense severity increased (severity was measured as the product of two values, one based on the result of the offense and the other the type of victimization). The nondelinquent comparison had the lowest LCU, while offenders incarcerated for status offenses, acts against public order, acts against property and acts against persons showed increasing mean annual LCU's in that order. Within each of these offense groups, correlations between LCU and offense severity ranged from .7 to .5.

(7) In the prison sample, mean LCU's increased from a period of four years prior to incarceration, reached a peak one year prior to incarceration, and then decreased during the years in prison.

Dr. Petrich's paper has provided some evidence that changes in the life events for juvenile offenders were greater in the year prior to the crime for which they were incarcerated than in a non-delinquent sample. Severity of the crime for these juvenile offenders was related to amount of life change during the one year prior to commission of the crime. For adult offenders as compared to a normative group, the only strong increase in LCU during the year prior to arrest was shown in events involving conflicts with the law. These events may themselves be part of the criminal behavior which defines the offender group. It is thus impossible to separate the events involving conflicts with the law from the criminal act itself and therefore it may be difficult to attribute the criminal behavior to these events in an antecedent-consequent manner.

The chief suggestion for future research was to further investigate the finding of the increased ratio of reported jail terms to reported law violations among the jail sample in the two years prior to being jailed. Dr. Petrich suggests that more detailed measurement of events involved in law violation and processing by the criminal justice system would lead to further understanding of the life events surrounding criminal acts, arrest and subsequent incarceration.

EDITED TRANSCRIPT OF PROCEEDINGS

FIRST DAY MORNING SESSION

INTRODUCTORY REMARKS OF BLAIR EWING, ACTING DIRECTOR, NATIONAL INSTITUTE OF LAW ENFORCEMENT AND CRIMINAL JUSTICE

Blair G. Ewing

National Institute of Law Enforcement and Criminal Justice, LEAA
Washington, D.C.

My name is Blair Ewing. I am the Acting Director of the National Institute of Law Enforcement and Criminal Justice. I want to say a couple of things before we start. I want first of all to thank all of you for being willing to come and assist in the effort that we've undertaken here today on the role of stress, and its contributions to the understanding of criminal behavior. I want particularly to thank the staff of the National Institute who helped to prepare this colloquium, particularly Richard Barnes, Helen Erskine, Patrick Langan, and Bernard Gropper who have helped in putting the colloquium together. I want also to thank Eleanor Chelimsky of The MITRE Corporation and her staff who worked very hard in assembling this group, and focusing the issues for us and with you.

The Institute is particularly interested in this topic as a part of its larger concern with "basic research." Over the last decade of its existence, the Institute has spent a good deal of its time, energy, and money on the exploration of questions related to the efficiency and effectiveness of operating agencies in the criminal justice system. That's been very useful research, in many respects, and helpful to those agencies. At the same time it has not addressed itself to the question of why it is that people engage in criminal behavior, except tangentially and occasionally.

Indeed, that body of research has tended to raise a great many questions about behavior which it did not answer. We have turned increasingly, with the support of the Administration and with the stimulus of the National Academy of Sciences, toward more basic questions, more fundamental inquiries into why it is that criminal behavior occurs, and what it is that we can understand about it.

One might ask: Why not sooner? There are many reasons, and I won't bore you with my speculations on them, but I think it is fair to say that when the LEAA and the National Institute were created by the Congress a decade ago, many people thought that the answers to questions on crime, criminal behavior and criminal justice were simply to be had for the purchasing of them; that if there were dollars, there were also plenty of people around to provide answers. We have had some sobering experiences over the last 10 years, having spent a lot of dollars without finding very many answers.

Indeed, as everybody knows, crime continues unabated, or at least is very largely unabated with some ups and downs on occasion. We have not, I think, solved very many of the problems of the operation of the criminal justice system. So we're still seeking, and we have now concluded--somewhat tardily and long after the academic community suggested that we should have concluded this--that it is essential that we support more fundamental inquiry which will help get at the questions of what causes people to behave in ways that lead to violence, crime, and disorder in American society.

This, then, is a part of that effort, and a very important part. I have spent the last couple weekends reading the papers which you have written and found them extremely exciting and stimulating. I am very much looking forward to today's session at which I intend to be present. I want now to introduce your co-chairmen for the day. I'm sure many of you already know them, Leslie Wilkins and Albert Reiss.

Leslie Wilkins is Professor at the School of Criminal Justice at the State University of New York in Albany, and was previously a professor at the School of Criminology at the University of California at Berkeley. He has written a great deal on crime and criminal justice in this country and abroad. He's a renowned expert. He can tell you what the work "expert" means better than I.

Al Reiss is the other co-chairman. He is the William Graham Sumner Professor of Sociology at Yale University where he was Chairman of the Department of Sociology between 1972 and 1978. He's also a professor at the Institute for Social and Policy Studies and a lecturer at the Law School, and a Fellow at Saybrook College. He has taught at the University of Chicago, Vanderbilt, the State University of Iowa, and a number of other places. Both are distinguished contributors to the literature in sociology, distinguished researchers in the field of criminology, and we are very fortunate to have them a as co-chairmen for this panel.

OPENING REMARKS OF CHAIRMEN LESLIE T. WILKINS AND ALBERT J. REISS

WILKINS: Thank you very much Blair. I'm very glad that you'll be able to stay with us. I am sure the participants would like me to suggest that you might not only stay, but please feel free to really join us in deliberations.

You mention the idea of "expert" which is really purely a technical term that the United Nations did once use for me, but that is now in the long-time past. I now disclaim that title.

Perhaps I may add one word of introduction about Al Reiss, a personal one. It was when I was first drafted into the field of criminology and criminal justice, something like 20-odd years (I won't say how much that odd bit is!) that I first bumped into the work of Al Reiss. That is acknowledged heavily in my first work in this field with Mannheim around the late '50s. It is very pleasant to be sharing the role of co-chairman with him today.

This is not a field I know very much about, but I am interested in the proceedings. I have read the papers, also. I think that the whole field of criminal justice, at the moment, philosophically, if not in any other form, is in a bit of a mess.

We have had papers, books, and so on, criticizing all aspects of the field, and indeed undermining the very basic foundations of a lot of the thinking and a lot of the kinds of support--philosophical support--that criminal justice used to rest upon. It's now being attacked, and to some people's satisfaction, a number of the early paradigms have been totally destroyed.

I am not sure that the destruction of all the paradigms is necessarily complete, but I'm beginning to take the view that it is really now very necessary for us to try to think of other paradigms. We need to become quite inventive, and to look around us more widely than perhaps we have done in the criminal justice field for new paradigms that we could work with. You're all familiar, of course, with the "death of the treatment" myth, as some of my firends would put it and the "just desserts" philosophy.

I don't really think that's a complete and satisfactory paradigm, either. As some of you may know, I was on the committee that produced that report. (I did put a little note in the back that I didn't think they had done much more than rediscover sin!) Perhaps the rediscovery of sin is of interest. Maybe there are

other things to be rediscovered and refocused and looked at again, with the possibility of leading to a breakthrough to some new paradigms that might be a little more successful in achieving a decent society.

There is one think that I suppose, as the chair here, I will be trying to focus your attention on. That is: the relevance of our discussions to the concept of crime.

I, of course, take the view, that the reason that criminal justice has not provided the answers as Blair has pointed out, is that if you're asking the wrong questions, it's rather unlikely that you'll get the right answers.

You may be thinking that perhaps we have been asking some of the wrong questions in relation to the crime field. But whether we find the concept of crime an academically satisfying concept or not, it is certainly one which is very much of concern to the community in which we live.

To some extent, even the most abstract of sciences has some accountability to society. Thus we should try to concentrate or focus upon the idea of "crime," even if this is treated as something which needs to be rethought through, and we should try to maintain that focus in interpreting the papers and in the discussion that arises from the papers.

Perhaps I should call on Al to say a few words just to give him a chance to contradict me, if he wishes to. He might! It wouldn't be the first time. That's one of the reasons we've remained friends.

REISS: Leslie, in his usual modesty, failed to mention in our introduction that it was after I had done my work that I discovered a fellow, or a chap, by the name of Wilkins had done some work much earlier on predicting who in the British Army would apply for a campaign medal after the war, and that indeed he had scooped me.

I just wanted to make one statement about my hopes as one of the co-chairpersons of this conference. I would hope that if, at the close of the session tomorrow morning, we might feel we had dealt with three things, it would have been highly successful from my point of view.

The first is: That we had somehow isolated what we thought were the two, three, four, or whatever, most important and critical

issues in this area of stress and crime. That is to say, among the many that we will talk about, whether we can pick those we think are most important, most important from the standpoint of where we ought to be going in the next two, three to five years.

Secondly, what we think are the two, three, four, five, or whatever, most critical kinds of research studies, or types of research studies, that would illuminate those problems.

Then the third thing, and one that may occasion the most difficulty, is to say: So what if every one of those studies came out right, or came up with some critical piece of information that we think it was designed to--what difference would it make? What might be our next steps? What might we expect the payoff from that to be? What are the consequences of having done that research? I think there is an awful lot to be done, and someone would say it can't be done by a committee or a conference, but that's the direction in which I'd like to press us.

Our division of labor is that Leslie is going to chair the morning session, and I will chair the afternoon and the session on the morrow.

INTRODUCTION

WILKINS: Thank you very much. I think we're ready to begin. Let me introduce our first speaker, Professor Marvin Opler. Marvin Opler is a multi-disciplined person. He is a Professor of Social Psychiatry and Anthropology at the State University of New York at Buffalo. He has been Chairman of the Department of Anthropology at Reed College, and Occidental College, as well as SUNY Buffalo. He has also taught at Harvard, Stanford, the University of California, Northwestern, the University of Hawaii, and at Cornell University Medical College. He was the principal investigator for the Midtown Manhattan Mental Health Research Studies, which dealt with social stress and mental health. These studies were published in a book entitled "Mental Health in the Metropolis" of which he was a co-author. He is the author of very many articles, including articles about impounded people based on his work with interned Japanese during World War II.

So it is a pleasure to invite Professor Opler to present his remarks on the topic of "Social Stress and Rising Rates of Sociopathy."

SOCIAL STRESS AND RISING RATES OF SOCIOPATHY

Marvin K. Opler
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To identify where I come from, I have been a Professor in the Departments of Psychiatry and Anthropology at Cornell University Medical College, at the State University of New York at Buffalo, Tulane, and Harvard. I am also a Professor of Sociology at SUNY-Buffalo. I say this not to elaborate on the very flattering introduction, but to indicate that in my conception, and as you yourself may have noticed in my paper, I feel that we get at the notion of what's going on in the crime world by trying to understand the social and cultural context of the kind of individual who becomes involved in antisocial behavior. "Sociopathic" is another term. An old-fashioned term was "psychopathic personality." We now say "sociopathic," and we divide it into more passive and more active types.

What we are talking about is the uneasiness of the public about the criminal assault, the rape, the child abuse, the wife abuse, the husband abuse, the murders, the intra-family violence in sociopathy which Dr. Marvin Wolfgang has illustrated in his work.

In my city they had a dragnet recently on drugs, and they found high school kids were at the top in the target group. Suicide has increased about 250 percent in the last two decades, in the same youth age group, 15 to 24. Drug abuse and serious auto accidents are predominantly found in the same age group.

As I was writing my paper, the trouble broke in Jonestown's People's Temple in Guyana. They were still arguing whether these people were shot or if they had committed suicide. In either case it doesn't release my thinking from the overwhelming impression that they were people without very good relationships supportive of themselves. They were the kind of people that were dependent on a leader, dependent on this type of contrived and exploited group that gave all of their collected Social Security checks to a common fund and were hoodwinked into believing that they were a genuinely cohesive and protected social group.

In San Francisco, there was another dramatization of what's going on in modern urban America at the very same time--the Mayor and one of the city council members were shot dead only a week after Guyana.

Now, these rising rates and roaring headlines can't be referred completely to genetic and biological stressors. I mention in my paper some hypoglycemic and dietary deficiency factors in relation to certain kinds of stressful behavior. However, you can't explain why, in Japan under the same banners of urbanism and crowding (they're more crowded than we in Tokyo), and on Tokyo railroads and subways this is also dramatized, why they in Japan have so little crime, so little of this assault, so little of our list of violent troubles. Why is it they have police systems that know the people in the neighborhood, and do things such as checking whether people are developing a fire hazard in the block.

I can explain this by knowing that one of the groups I intensively studied was the Japanese, for longer than three years. In Japanese culture, in both village and city, neighborly surveillance takes care of the "buraku," or "block," or the little part of the hamlet; the the fire, or now, the police watch, takes care of their fire hazards, gets around and checks that there is not going to be a fire, or sees if all goes well. This is the kind of police system they have evolved from the buraku fire watch.

I earlier went to a conference on psychological stress, and out of it emanated the book edited by Mort Appley and Dick Trumbull, the Appleton-Century-Crofts Psychological Stress volume, in 1967. At that conference, we were all saying that there are independent variables, large-scale independent variables which lead to all sorts of psychological processes residing in individuals and then within clinically discernible persons to "resulting variables" like sociopathic behavior. These things make the public urge officials to "get tough on crime"; they say "it's scaring hell out of us"; it makes Jules Feiffer do that movie called "Little Murders," where people convert their little city apartments into family fortresses.

In the Midtown Manhattan Mental Health Study we examined the extent of the resultant variable, that is, persons with personality problems large enough to be termed "impairments in life functioning," or psychiatric disorder. One instance of this is, of course, the sociopathic personality.

How much of this, in the Midtown Study, did we find? We found that half the people in New York City whom we studied could have benefited from some halt in the process that was producing the difficulties in their way of coping with life events, their impariments in handling stress, or their maladaptive styles of coping. In looking at this, I find we have had a kind of urbanization different from Japan's. We have had a kind of urbanization that has more definitely

disrupted the traditional strengths of things like family orientation, neighborhood relationships, or the corporate forms of town and village organization still found in Japan.

Later on in my paper, I indicate that Ferdinand Tönnies wrote about Gemeinshaft and Gesellschaft, the first being neighborly, interpersonal and meaningful, warm relationships that were found in family, and neighborhood, and community, versus those that were impersonal and alienating. Some of you may have lived in such corporate community cultures or studied such cultures and subcultures.

I grew up in such ethnic urban neighborhoods in the first stages of my life. Then at age six I went into something which was much less neighborly and culturally toned and more middle or upper class segregated. I've been among native American groups since then, and thereby have had the opportunity to study both Ute and Apache Indians. I found that both the Navaho, and the Apache, and other native Americans are among the poorest people in the United States. Their per capita income is less than Blacks. However, they hang onto their cultural groupings and their practices because these lend meaning and support to their activities.

They don't want termination of their Indian reservations, which the Bureau of Indian Affairs wishes to impose upon them. They want to hang on to their social structures having the functions of their cultural continuities; such as a Navajo outfit. A Navajo outfit is a group of extended, matrilineal kin families. It is like an extended family, but on the matrilineal side. They clear the fields and herd the sheep together. In the outfit, you have not only your mother and father. You have all sorts of uncles and aunts and cousins. You have a wide distribution of both help and authority. You have a wide distribution of interdependency and protection, precept and example. All the dependency needs of human beings are much more broadly shared in such social structures.

In such small but cohesive corporate groups large enough for a human being to comprehend, but small enough to know and be known, you can hold onto something; you have human relationships which are meaningful and intense. I recommend reading Walter Dyk's story of a Navajo, Son of Old Man Hat.

In the city, there is alienation and anomie, which Durkheim mentioned a long time ago. When Durkheim wrote, he wrote on such topics as the division of labor and the elementary forms of the religious life. He also wrote on suicide. These happenings in Guyana, he would call a mass suicide through anomie. It is alienation, it is

self-to-other unrelatedness that leves people so fundamentally weak and powerless. They have few emotional supplies to sustain them.

Up to this point, I have talked about my interest in the sociopathic personality, which can be studied with regard to social factors which affect a human being who gets to be like that; and such social and cultural factors can be studied in terms of high-rate areas, high-rate cultures, or low-rate cultures and areas.

One may take two contrasting cultures and study them. One can find them in the U.S.A. One can locate a Native American group with low crime rates, and an urban group with high crime rates. You would be able to find the social factors or effects which produce sociopathic persons. Similarly, one can, as we did in Midtown Manhattan, make model studies of ethnic groups. They are there for the asking, if you so constructed the study so that you could control a good many other factors. For those very reasons, I was personally interested in Dr. Freedman's paper on crowding. When we did the Midtown Study, we found that Dr. Robert Hyde, a psychiatrist, had long before studied the Boston area and found that population density did not account in any sense for mental disturbance, and he is right.

It is not, as Dr. Freedman shows with elegance,--it isn't the higher density areas, like Calhoun's behavioral sink for Norway rats, that produce a high rate of criminalistic behavior. No, it isn't that at all. The human being is not at all like the Norway rat--and the kind of crowding they had, where they were crawling all over each other, and subjected to such types of environmental conditioning, which were enough to produce increased biting and scratching and thus they no longer were potent sexually when they were adult rats.

Human behavior is not predicted or translatable from these behavioral sink studies. It is very suggestive to assume crowding is bad, but Hyde found out before we did the Midtown Study, and issued a caveat for anyone working in Boston or in New York City or in any of the dense urban areas of the nation, that we should not expect density alone to produce high rates of sociopathic personality. We, indeed, did not find that in Midtown.

Dr. Freedman's results are clear and are very interesting; that density can work on the positive side as well as the negative side, and will not determine the total contours of the personality, i.e., the sociopathic personality that we are trying to isolate and discuss.

Sociopathy can occur in families. Marvin Wolfgang, in his elegant work, finds that there are criminalistic families. Child

and spouse abuse, homicide, suicide, and assault and other forms of aggressive behavior cluster. Various of these modes of behavior will be found together in families.

We found this to be the case in Midtown. We found that not only were individuals sick, but some families were indeed sick. We found that some individuals in treatment, in hospitals, weren't as sick as some relatives whom we studied at home and who were unknown to treatment agencies. Some were more psychiatrically torn apart and torn up at home that those in the hospital or in treatment.

I have just been reviewing Smith's work on the Japanese village. He is at Cornell University, an expert on Japan. The Japanese picture is not like ours, despite their growing urbanism. I mean they have a more recent and particular kind of urbanism. It is the kind of urbanism that does not or has not yet been able to destroy traditional human relationships and ethnic groups. I find, for instance, the values of Puerto Ricans are torn apart when they are in Manhattan. In the Midtown Study, the women coming over had a more sustained employment, and they had enjoyed better job continuity over in Puerto Rico, too. They went into well-organized industries, like the garment trades. Among Puerto Ricans, the women paid the rent. The women were becoming more central in the support of the Puerto Rican family. This does not conform to the values of the older Puerto Rican culture in which the male as provider, the machismo, the male domination cult that goes on in the Spanish-derived culture of old Puerto Ricans was losing ground since men had poorer job continuity and poorer pay in their less organized lines of work.

Nevertheless, because of these disruptions, these changes away from the fact of male provider, there were stretches between ideal cultural values and the actuality of female-dominated households (matri-focal households). Of course, with many problems tearing up the Puerto Ricans' value system, things were confusing to the Puerto Rican kids. They didn't know who they were in the shifting orientations and this value conflict involved the Puerto Ricans' sudden awareness of racism and new sex-roles in New York City. As time went on, these were the kinds of problems that were disruptive.

I don't say any of this is a matter of race, as Dr. Staples points out eloquently. It's idiotic to think that this would be a matter of a combination of the Caucasian, the Indian, and the black that are Puerto Ricans. I know, however, that rapid social and cultural change, under urbanization, can destroy older traditional values.

I also mentioned some other literature in my paper. In Papua, New Guinea, Dr. Burton Bradley, a psychiatrist, finds that crime, mental disorders and alcoholism arise even out in Margaret Mead country, when the trading post towns and cash economy come their way.

This persistent, basic unit of our society, the nuclear family of parents and children, is a major form that is losing its gemeinschaft, community supports. It is this loss that results in the current drug abuse and the host of intrafamily problems current in our society. This weakening is the thing that is affecting not only lower socioeconomic categories, but the upper classes as well.

When I find that the nuclear family is not functioning as it once did, or as it optimally should, I am interested in what social and cultural variables can be isolated as disrupting the system, as causing the trouble.

I, therefore, in my paper, began to look into a passive kind of sociopathic disorder, the skid row person, and the flop houses. I had a student who went to the flop houses and studied the passive-receptive end of the sociopathic spectrum in the New York City area. I had him publish the study in a journal which I edit.

I looked at the other kind, the killing spree kind of person, or violence known in such culture-bound syndromes as "running amok," or "running amok in Malaysia," where this occurs. There is such a thing.

I went to Hawaii when they still had, in the city of Honolulu on Oahu, Hell's Half-Acre. It has been cleaned up now, I understand. It was earlier the reception point for Filipinos and Malaysians who arrived in the last wave of immigration. They came in at the bottom of the socioeconomic hierarchy. I would go down to that area of Honolulu being told by phone there had been a "running amok" episode. Yes, I remember once when there were eight people killed. And the drive, when these people go on the killing spree, is suicidal, since they are killed as they go on the "mad dog run." It always ends in their own death. Also, "running amok" is always a male, running-mad-dog kind of episode, in which one handles the knife (or kris) until killed.

Are there female parallels to this? Certainly. In Malaysia there is "latah." A latah personage is usually a lady in a social cul de sac, a blind alley, with no positive outlet possible in her life. This may be a widow, perhaps with a foreign husband. I remember one married to a Dutch sailor. She was not very well off.

She's invited to go somewhere for tea and she goes. She breaks into this behavior sometimes initiated by a startle reaction such as a loud noise whereupon she puns obscenely, in Indonesian, or in one of my cases in one of the Javanese dialects. This is the kind of illness, in other words, where you have a catatonic reaction with an echolalia, echopraxia, or command obedience induced.

I would like quickly to summarize. I have tried to work on the spectrum from the passive to the more acting out or active syndrome (running amok, or latah for women) and its type of emotional disturbance. In the paper I discuss some notions of where the ethnic group, now called "black" in this country, turned a corner, where it lost its family stability, and where it got caught in this kind of family weakening in urban and other circumstances.

I largely used Herb Gutman's The Black Family in Slavery and Freedom, a very fine historical account of the strength of the black family in earlier times, and a correction of the simple matrifocal family type of E. Franklin Frazier, in terms of the former demonstrable strength of the black family. That strength I think, perhaps, could be regained if we worked out ways, on a neighborhood and community level, to buttress such ethnic and cultural groups as blacks, Puerto Ricans, or Mexican-Americans in regard to the ways in which they can cope with psychological stress arising from family problems which I think is the inner key to the rising rates of sociopathy in some groups.

I believe that all these things are dependent variables, and that the larger social, cultural, economic conditions of human existence are the independent variables which unleash the trouble.

INTRODUCTION

WILKINS: Thank you very much. We now move to "Stress, Adaptation and Coping," a paper by Morton Lieberman. Morton Lieberman is Professor in the Department of Behavioral Sciences, and Psychiatry, at the University of Chicago.

There are many other things I could say about him, but I think he is perhaps well enough known to you, so that perhaps I can save time, pointing out that he is the author of many works and ask him to go right into his presentation.

STRESS, ADAPTATION AND COPING

Morton A. Lieberman
University of Chicago
Chicago, Illinois

Many of my co-participants will have examined the concept of stress, and several have reviewed much of the relevant literature. I doubt that I could substantially improve on these reviews and would recommend them to you as relevant statements about the current status of stress research.

Rather than review what has been competently done by others, I decided to draw on two of my ongoing studies, to demonstrate some of the issues that are germane to the focus of the symposium.

Implicit in my remarks, since I have not directly studied criminal behavior, is a model shared by many social scientists in the stress-coping-adaptation framework. This framework revolves around the concept that some forms of criminal behavior are effective from the perspective of coping strategies that maintain homeostasis for the individual, and some other forms of what could be classified as criminal behavior, such as violence, are often indications of failures in the coping system.

This simple-minded perspective enables those of us who have not directly investigated criminal behavior to at least think about it, hopefully in some meaningful ways.

In portraying the interrelated issues of stress, coping, and adaptation, I will be drawing on a longitudinal study which has been underway since 1972. This study, under the joint direction Dr. Leonard Perlin and myself, began by scheduled interviews, with 2300 people representative of the adult population of the census-defined urbanized area of Chicago.

These interviews had three main foci: the assessment of a wide range of problems and hardships that people experience as workers and breadwinners, husbands and wives, and as parents; second, the identification of resources and responses they utilize in coping with these life strains; and, third, the enumeration of symptoms indicative of emotional stress and psychological disturbance.

In 1976-1977, we did a follow-up study of this sample. We had similar concerns in this follow-up. Before describing some of the findings relevant to our concern in this colloquium, let me place our work in a context.

Along with many investigators, we see the proper work of social sciences to be the illumination of the connection between personal problems and social problems. However, the manner in which the study of human behavior has been traditionally divided among academic turfs masks many of these connections.

There are those who study personal problems, relying on speculation, and drawing connections of these problems to the social milieu of people. And, correspondingly, those who study the structure of society and its institutions and guess about their consequences for adaptation.

Our concern has been to bring together these issues by empirically tracing out the links joining the psychological distress of people to the experiences they have within the context of their lives. In my written paper, I trace out several strands of research, similar to ours. I have contrasted this work to those investigators who have (1) looked at social structural parameters as the major source of variation; (2) the familiar life events style of research typified by Holmes and Rahe's pioneering work on life stress; and (3) the studies on single stress events.

Our framework departs in some significant ways from these three approaches. Our work began by distinguishing two major types of events. One is represented in the gains and losses, or major alterations of roles that predictably occur in the course of the unfolding life cycle. We refer to these as normative events, in order to underscore the expectedness and regularity of their occurrence.

The second type of event we refer to as non-normative. These are often crises that, although they occur commonly, are not easily predictable because they are not built into development throughout the lifespan. Some of these eruptive events may lead to role loss, such as being fired from one's job or being divorced. Other non-normative events, such as illness, are disruptive without necessarily entailing role loss.

In addition to the normative and non-normative events of life, I shall examine persistent role problems. These are not events having a discrete onset in time, but, on the contrary, acquire insidiousness and become relatively fixed and ongoing in daily role experiences. Problems of this order are often chronic low-key frustrations

and hardships that people have come to contend with in their occupation, their economic life, and their family relationships. Richard Lazarus terms these kinds of issues the "daily hassles of life," which I think is a good descriptive phrase for what we are talking about.

It is around these three types of circumstances, we believe, that much of the social experience affecting the adaptation of people is organized. The analysis we present is concerned, in part, with learning the extent to which each of them affects adaptation.

In addition, I shall seek to learn how their effects are exercised. Specifically, I shall be attempting to identify the mechanisms through which events come to result in emotional distress. Is it because important changes always produce an inner disequilibrium or psychic imbalance, or are there different processes that determine the impacts of life events?

We also need to know how life strains are distributed in a population. To the extent that these strains grow of fundamental conditions in the larger social order they are not spread randomly among people, but are likely to impinge on some groups more than others.

Finally, we are interested in the procedures individuals use to reestablish equilibrium in response to such events and strains.

I will ignore the methodological and analytic procedures used to convert the survey data into findings. I might say that the 2300 people we studied matched the 1970 census. They were drawn from age groups 18 to 65, at time one.

The findings that I believe are relevant to our concern in this colloquium have to do with the following issues: the social distribution of stress; the relative impact of different life stresses; how such events come to matter, or, put in another way, the mechanisms through which life stress impacts on adaptation; and lastly, the role of coping strategies in mitigating maladaptive responses.

In this question on coping strategies I will be reporting some of the findings relative to two ways of looking at coping. Coping, in its more usual sense, comprises the strategies that are often internal to the person. Adjustment involves problem-solving through the use of coping or defense mechanisms directed towards the events or the consequences of the events.

However, there is another way of examining coping. That is: how individuals use their social matrix or external resources in order to adapt to stress conditions.

Let me first turn to the social distribution of life stress. Our findings, which have associated the occurrence of normative and nonnormative events, as well as role strains, to such enduring characteristics as age, socioeconomic status, and sex, indicate that the events, transitions, and persistent role problems are not scattered helter-skelter throughout the population. Rather, they tend to be more or less prevalent among groups having distinguishable social characteristics.

The results support the assumption that there is a social epidemiology of major life strains. In all the major role areas of life, it is the young, lower socioeconomic classes, and women who are most vulnerable to the occurrence of life strain. As will be shown later, such life stresses radically affect adaptation. The finding that certain individuals in our society are more likely to endure life stresses is not a surprising one. What is important to our concern here is the observation that groups vulnerable to high life stress are also, at least from the point of view of age and social class, those groups most likely to be involved in criminal behavior.

Obviously, the mere fact of this probablilistic association between life stress and structural conditions in our society is only a beginning in a long chain of understanding the relationship between such stress and subsequent adaptation.

Let me now turn to an examination of the actual influence of life strain on adaptation. The most general finding is that life strains--that is, events, transitions, and persistent problems in the major role areas of life--do indeed affect the well-being of people. There is, however, substantial variation in the magnitude of these effects.

For example, in the occupational role, the various life strains, transitions, as well as the day-to-day issues of occupational strain are some of the most impactful on people's lives.

The other area that is of particular relevance is the fact that if one were to select the particular issue that affects most of our sample in terms of their adaptation or lack of adaptation, one would choose, the marital area as the single day-to-day relationship that most affects peple's well-being.

Overall, an examination of our data suggests that it is the persistent day-to-day effects of marriage, parenthood, and occupational roles that are crucial in affecting the mental health of our population, more than either normative or eruptive events. This is not to say that certain specific events, particularly eruptive crises, do not have profound impact on the adaptation of adults.

In order to more fully understand the relationship between the lives of adults and their adaptation, we turned our attention to the question of how such events come to matter. A rather accepted view of social life and adult development is one of people being psychologically bombarded by a parade of changes. According to this view, change of all kinds imposes an inner need for readjustment. Whenever and however it occurs, it is likely to produce the signs of maladaptation.

The explanation for distress is thus placed on the event itself and its interference with established habits and equilibria. It is our view that events and transitions affect people by altering the more enduring circumstances of their lives. Disturbance is more likely to surface when events adversely reshape important life circumstances with which people must contend over time.

Thus, the event does not act solely or directly on the inner life, but through the reordering of the more general life circumstances. In short, the impact of events is largely channeled through the persistent problems of roles. If an event does not disturb the day-to-day relationships in people's roles, it has very little impact on the adjustment of individuals. It's not the events themselves, but it's the impact of such events through the day-to-day context of people's lives that makes the difference.

What such findings so far suggest that it is not the life events themselves, the crises that people encounter that we need to address in searching for amelioration of maladaptation, but rather the microscopic context in which individuals reside; their lives as workers or nonworkers, the family, and their social connections that make more of a difference than do the events themselves.

It is with these considerations that we now examine the strategies available to individuals coping with life stress. Coping refers to the behavior that protects people from being psychologically harmed by problematic social experiences, behavior that mediates the impact that societies have on their members.

The protective function of coping behavior can be exercised in several ways: by eliminating or modifying conditions giving rise to problems; by perceptually controlling the meaning of experience in a manner that neutralizes its problematic character; and by keeping the emotional consequences of problems within manageable bounds.

The efficacy of coping was evaluated by looking at indivdual differences in response to similar sets of stress and determining the degree to which the person experiences distress according to the coping strategies they employed.

The results indicate that individuals' coping interventions are most effective when dealing with problems within the close interpersonal areas of marriage and child-rearing, and least effective with the more impersonal problems found in the occupational role.

There was no such thing, in the abstract, as good coping strategies. Rather, each particular area of life appears to have its own efficacious coping strategy. This does not mean, however, that there are not good and bad copers. The particular strategies that individuals use in attempting to maintain homeostasis is contingent upon the particular life demands they are facing.

Similar to the findings on the social epidemiology of life stress, coping strategies are unequally distributed in our society, with those of a lower socioeconomic status having fewer and poorer effective coping strategies.

Clearly, the results on coping demonstrate, as we move further away from the specifics of stress to the person's complex reaction to it, the role of such stress in the lives of people becomes less important than the various contextual and personal resource factors. If some forms of criminal behavior can be viewed as the failure to adequately cope with life stress and strains, I would put my research priorities on a more thorough examination of the coping strategies used by such individuals.

Perhaps more important is an emphasis on stress-mediating behaviors. We know a little about them; how they are learned, and under what conditions. We know little about their stability, although we are reasonably certain that they are not enduring personality traits, but rather aspects of people that appear to vary from condition to condition.

Although we know that these coping strategies are not randomly distributed across our population but rather are associated with the person's position in society, beyond broad theoretical speculations we know little about how subcultures influence the development of such strategies.

In a previouly completed study, in which we examined the effects of culture on patterns of adaptation, we found large differences among three subcultural groups, the Irish, the Italian, and the Polish, in their patterns of adaptation. Although all three of our samples were equally adapted, the methods each used for adaptation were distinctive according to their culture. Furthermore, a plausible relationship between ethnographic descriptions of these three subcultures and our findings on patterns of adaptation could be made.

Such work is only a beginning in a complex set of investigations that are required to understand the development of coping strategies and the socialization processes that have shaped them through the formative years.

We also know from this previous research that such strategies are not inherently stable, and that over the adult lifespan, patterns that were adaptive at early ages are not adaptive at later ages. However, despite the prominence and continued interest in the psychology of coping, it is an area of vast ignorance and poorly solved methodological dilemmas.

I was going to go on to talk about the other aspect of coping, that is individuals who don't utilize, under conditions of stress and strain, their own internal resources but external resources, utilization of help, but I'm out of time.

DISCUSSION

WILKINS: I am sure speakers will have an opportunity of inserting various significant items that they've missed in the discussion later on; because we do have time scheduled for discussion.

The floor is open.

STRAUS: I am speaking to the point in Dr. Opler's paper about Gemeinschaft and Gesellschaft. We have a long tradition in Euro-American society of romaticizing the rural society, which simply doesn't hold up under examination. Whether you think of that as rural society geographically or the urban village, a close-knit network can be a network supportive of crime, supportive of almost any human activity.

I am reminded of the point that Dr. Freedman makes about the intensifying effect of crowding.

In the same way, a close-knit network can strengthen one to do either pro-social or anti-social acts. So, I think we have to be rather cautious about seeing a restoration of community, which is putting it in its most desirable aspects, as a major solution to crime.

WILKINS: I'm glad a conflict of views seems to emerge right away. That's excellent. One thing about scientists is that we are not supposed to agree, but we are supposed to be somewhat agreeable about our disagreements, and perhaps able to agree on some way of solving the point of disagreement.

So perhaps if we could concentrate on the research questions involved. What would we need to know in order to resolve the conflict, and specify those as research questions. I think that would be an interesting way of highlighting the positive points of the difference. If it can be specified in terms of what we need to know to resolve the points of difference, this would be a significant contribution.

OPLER: Just to comment in response, I think it's a fair enough point that Dr. Straus is raising. When we did the Midtown Mental Health Study we were studying such groups as Puerto Rican, Italian, Irish, Czech, Hungarian, and German-American. We studied two of the three that Dr. Lieberman alluded to: the Irish and Italian, and though I can't say "Polish," we studied other Slovakian groups, like Czech, both the eastern

kind of Czech, the Bohemian Czech, and the western or Slovakian cultural groups. In Buffalo we continued with studies of Polish, German, Italian, Irish and Puerto Ricans.

Now, they weren't all the same. As a matter of fact, the point in the Midtown Study is that the stage in which the ethnic group's cultural value system and community sustenance were functional depended a great deal on the extent and nature of the changes that they were experiencing in rapid acculturation in New York. That's the whole point of my Puerto Rican discussion: the Puerto Ricans were not simply in rapid cultural change, which is Alex Leighton's simple formula, which I think also is a vague one for commenting on these things, but they were more specifically in the kind of rapid cultural change that is not supportive of their human efforts. We talk a great deal in the Midtown Study about "impairments in life functioning" which was our general or global description of what was troubling people. But that impairment could be at home, at work and in other statuses that all people in our modern society tend to fulfill. A human being has a number of economic and social roles in our society. The marital one is alluded to. The work status was also alluded to. The parental one was alluded to. These are all roles and statuses in a narrower and more particular sense than the socioeconomic status alone. Such statuses are active under the headings of ethnic group roles and statuses.

What happens to people, specifically and spelled out fully is the matter that is crucially at issue. For that reason, in my paper I say clearly we have to have these model studies on ethnic groups to see where they're at, to see what they're actually coping with, and to see how their efforts can either be supported or set back.

For instance, you can have a program for Puerto Ricans that is so maladroit that it doesn't start where they're at, so it doesn't relate to their problems; it doesn't energize their interests, it is meaningless and mindless to them, and "it doesn't grab them," to use the modern term.

When I say "ethnic group," when I say "cultural group," when I say the "Geminschaft relationships," I am not simplifying it down to the Navajo outfit alone. I am using one type of example as a kind of almost early evolutionary example of the type of social structure, roles and statuses that must be studied in modern circumstances in other groups.

We provided some agencies in Manhattan, such as the Northside Clinic where Kenneth Clark was, Planned Parenthood, and other interested groups with advisement, as to the nature of the cultural and subcultural groups, they were dealing with and how they might be best approached. Some of these things worked; they paid off handsomely. Public policy was made more effective at many points.

I had another experience, in the Japanese-American Centers, during World War II. It started out with the social scientist, called Community Analyst, being looked on as a very remote person or type of academic scholar. It ended up with myself on the direct teletype to Washington, D.C., because we knew the social and cultural realities of the Centers. We had been so accurately predictive about what could be expected for a period of three years that Washington could no longer ignore us. We had predicted in social science that there would be a swell in the suicide rates, and there were. We probably could call the shots on things like Guyana, I think, and do so a little bit faster in these kinds of circumstances, because we were watching "impairments in life functioning" in terms of general factors affecting real lives, or impinging on actual human beings.

So, I am not saying turn the clock back, I am saying be aware that the human being requires some kinds of supportive person-to-person interrelationships of the sorts that have largely gone out of style or "out of function" in a good many modern cultural situations. We are losing sight of these human factors today. I don't think getting better surveillance, more police or other developments for police departments, will simply handle the job. I don't think live experimentation on a medical level with criminals in prisons will do the job, either. I think we wasted a lot of time and a lot of funding on such types of things that do not work. That is why I favor studying the actual lives of people in real social and cultural contexts.

WILKINS: Thank you.

Two speakers have caught my eye. Professor Brenner, and then I would like to go back to Professor Lieberman for his response. After those two, we will adjourn for coffee.

BRENNER: On the matter of styles of coping, whether by personaility type or by ethnic group, there are problems here. We observe the same ethnic groups attributed at some historical point to

to be extraordinarily violent; at the moment, blacks in the United States are a very good example. When we look back about 20 years or 30 years we find in the United States in its historiography precisely the opposite view of the American Negro as a thoroughly docile individual.

These things are not stable in time, and it's critical to look at historical circumstances whereby the idea of coping behavior can rather radically change: the point being that coping behaviors are not stable phenomena. All human beings possess a great variety of them. One or another of them may tend to dominate at particular times in the individual's life. An older person will rarely resort to extreme violence, an adolescent male probably will. The older person charged in an aggressive way will react with high blood pressure, a young child with different forms of play aggression, perhaps. The style of response varies enormously with the temporal aspect involved. That's point No. 1.

On Gemeinschaft-Gesellschaft which is often translated into crowding, density or city size there are very great problems with the formulation. We know that the societies in Europe most heavily densely populated for centuries are the ones with the lowest homicide rates, the lowest crime rates on record. The Netherlands is perhaps the best single example. The issue of crowding is extremely difficult when approached from a macroscopic point of view of the Gemeinschaft-Gesellschaft variety.

On the matter of the very fine volume of research that Dr. Lieberman was speaking about, the stress research on individual life change events. The critical thing about much of this work a la Holmes, Rahe and others, is that there's an epidemic character to it. The events are cumulative. If they are not cumulative, we do not seem to observe the kinds of pathological responses. The cumulativity is the issue. The central question is: Why is that cumulative? Why, suddenly, in the course of the experience of an individual do we have one, two, three, four, five--as many as 10 to 20--apparently separate events that occur together? Clearly, the coexistence of these phenomena is not random. Most probably it has a good deal to do with some fundamental changes in the region, the area, the life of the individual in question.

Taking into account these dimensions of change and overall social context; if we're looking at individual persons and their stresses or attempting to evaluate programs which look at individuals having been subjected to prison or whatever, we are probably not going to be able to find much in the way of results unless we look at the overall social context in which these kinds of epidemic phenomena arise.

How might one go about researching these things? Cross-national, cross-ethnic, cross-state kinds of comparisons in this particular regard seem to be one of the more fruitful ways of approaching the general subject.

WILKINS: Thank you very much.

LIEBERMAN: Murray Straus' point reminded me of another mythology that is written in the social sciences, particularly those dealing with mental health, that is, the function and role of one's individual social network as a support system. In our own information, we have not found differences in those areas to be particularly important in terms of adaptation. I am aware of the vast literature in this area. I think it is quite contradictory.

The question of whether one's immediate social support system is a major mechanism of coping is for me, anyway, an open question. When we talk about criminal behavior, support systems is an issue that I've not seen addressed thoroughly.

WILKINS: Thank you very much.

(Brief recess)

INTRODUCTION

WILKINS: We open this session with the paper by Dr. John Petrich, Professor of Psychiatry at the University of Washington, Department of Psychiatry and Behavioral Sciences.

He's been a consultant to correctional agencies in King County, Washington, and a member of the Washington Council on Crime and Delinquency. He's published articles on topics, including psychiatric treatment and emergency care in jails, criminal violence and life change, illness and life change and hyperactivity.

So may I call upon John Petrich to talk on the subject of "Criminal Behavior, Arrest, and Life Change Magnitude."

CRIMINAL BEHAVIOR, ARREST, AND LIFE CHANGE MAGNITUDE

John Petrich, M.D.
University of Washington
Seattle, Washington

Thank you, Dr. Wilkins.

Good morning. The material I would like to present outlines our method of examining life events. You've heard this morning of the method of Holmes and Rahe, which is derived from medical research. I will present the results of a pilot application of this methodology to three criminal populations.

Our material will be more narrowly focused than the previous papers today.

We want to lend support to the adaptational theories of criminal behavior and outline the obvious shortcomings of the method.

In our laboratory, we're intrigued that criminal behavior been observed to fluctuate in time for most individuals. Few violent or criminal patients were continuously criminal and some may experience only a single criminal event in their lives. Some investigators have developed an adaptational model to account for these observations.

In these models, criminal behaviors are viewed as potential adaptive responses to the life situation given an appropriate set of predisposing features.

Table 1 illustrates the Schedule of Recent Experience, one portion of which is our research tool.

A formal approach to the study of life events and their temporal relationship to medical illness was pioneered in our laboratory by Dr. Holmes and his co-worker, Richard Rahe. This began with a study of 5000 medical patients, where the quality and quantity of events which occurred prior to the time of illness were systematically examined.

It was observed that clusters of events requiring change and adjustment preceded the onset of a variety of illnesses, T.B., cardiovascular, skin diseases, etcetera.

TABLE 1

SCHEDULE OF RECENT EXPERIENCE

NO.	SRE QUESTION	MEAN VALUE
1	Trouble with boss	23
2	Change in sleeping habits	16
3	Change in eating habits	15
4	Revision of personal habits	24
5	Change in recreation	19
6	Change in social activities	18
7	Change in church activities	19
8	Change in number of family get-togethers	15
9	Change in financial state	38
10	Trouble with in-laws	29
11	Change in number of arguments with spouse	35
12	Sex difficulties	39
13	Personal injury or illness	53
14	Death of close family member	63
15	Death of spouse	100
16	Death of close friend	37
17	Gain of new family member	39
18	Change in health of family member	44
19	Change in residence	20
20	Jail term	63
21	Minor violations of the law	11
22	Business readjustment	39
23	Marriage	50
24	Divorce	73
25	Marital separation	65
26	Outstanding personal achievement	28
27	Son or daughter leaving home	29
28	Retirement	45
29	Change in work hours or conditions	20
30	Change in responsiblities at work	29
31	Fired at work	47
32	Change in living conditions	25
33	Wife begin or stop work	26
34	Mortgage over $10,000	31
35	Mortgage or loan less than $10,000	17
36	Foreclosure of mortgage or loan	30
37	Vacation	13
38	Change in schools	20
39	Change to different line of work	36
40	Begin or end school	26
41	Marital reconciliation	45
42	Pregnancy	40

The Schedule of Recent Experience includes a wide range of social and personal events related to family, job, religion, health and life style. There are 42 events.

Early work with this tool suggested that it was desirable to weigh the events. In other words, one could quantify the amount of adjustment required. This resulted in another example, which I don't have, called the Social Readjustment Rating Scale, SRRQ. Cross-cultural studies later showed that the SRRQ values could be a reliable index of general consensus about the relative magnitude of life change events.

In the last decade, the methodology of the Scheule of Recent Experience or the SRE, has been widely applied to the study of medical, surgical, and behavioral disorders. Life change in this line of research has been shown to relate linearly, both to the time of onset and to the severity of the dysfunction.

This report summarizes the pilot application of this tool to the study of criminal behavior and subsequent arrest.

The SRE was used to collect data on the quantity and quality of life events experienced in the years prior to arrest and incarceration for criminal behavior. In Table 2 you see three populations. The juvenile population completed a modified SRE for the year prior to the crime for which they were incarcerated. The second group was comprised of males from a county jail sample who were awaiting trial on felony charges. The last group were convicted male felons in state and federal institutions.

You can see the demographic characteristics and the distribution of charges in Table 2.

For the analysis it was desirable to compare the experiences of the incarcerated groups with the normative sample.

I won't go into the details, but we've identified a normative group of comparable age, socioeconomic status, health history, and of course, no criminal history.

One question, of course, that always comes up, are the samples comparable?

The county jail inmates completed their questionnaire within hours or days after their arrest. The prison inmates had gone through the adjudication process and completed their questionnaire

TABLE 2

CHARACTERISTICS OF THREE INSTITUTIONALIZED SAMPLES

	Juvenile Detention[1] (N=334)	County Jail[2] (N=30)	Prisons[3] (N-176)
MEAN AGE RANGE	15.5 yrs (13-18)	25.6 yrs (14-36)	32.7 yrs (21-65)
Sex: Male	77%	100%	100%
Type of Crime			
Status	24%	N.A.	N.A.
Public Order	20%	N.D.	15%
Property	35%	17%	28%
Against Persons	21%	83%	53%
Assault	N.D.	63%	49%
Murder	N.D.	20%	4%

N.D. - No Data; N.A. - Not Applicable.

[1]Kulcsar (1976); [2]Petrich and Hart (1978); [3]Masuda et al. (1977)

a year or up to three years later. We asked the question, was the amount of recall regarding the life changes reported prior to arrest influenced by the passage of time?

Table 3 shows the mean number of events reported by the incarcerated samples for each of the three years prior to their last incarceration.

Jailed felons are compared to a comparable cohort of prisoners. There is no significant difference in the number of events reported by these groups, despite the fact that prisoners reported about a period which in some cases occurred five years previously and jail inmates reported about their life experiences prior to a crime committed in the last week or month.

Figure 1 demonstrates the annual life change experienced by a group of felons and a normative group.

The dashed curve labeled "Normative," is the life change reported by a group of non-ill nonincarcerated factory workers in our community.

Notice on the left-hand scale the magnitude of life change goes from zero to over 400. And on the bottom line we see the years prior to taking the examination or years prior to incarceration.

The life change fluctuates for the normal group from around a mean of 220-225 for the period 2-3 years prior to measurement. There is a small nonsignificant peak in life change in the last year's reporting period for the normative group. We call this a memory effect; that there is a certain impact on recall of recent life changes.

Contrast this, though, to the solid line of the felons N = 30. Here we see two things, one of which is that their life change scores during the baseline levels, in the two and three years prior to arrest were lower than the normative group.

There was almost a five-fold increase in life change in the year prior to arrest for these felons. This was found to be statistically significant when subtracting the so-called memory effect noted in the normative group.

To study further the composition of the life events obtained in the incarcerated sample, the scale was simplified and the life events were grouped into eight areas of activity.

TABLE 3

MEAN NUMBER OF LIFE CHANGES REPORTED IN EACH OF THREE YEARS PRIOR TO INCARCERATION

	1 Year Prior	2 Years Prior	3 Years Prior
County jail inmates (N=30)	18.9	8.2	6.0
Prison inmates (Young, N=74)	14.5	8.5	7.2
	N.S.	N.S.	N.S.

N.S. Not Significant at $p = .05$

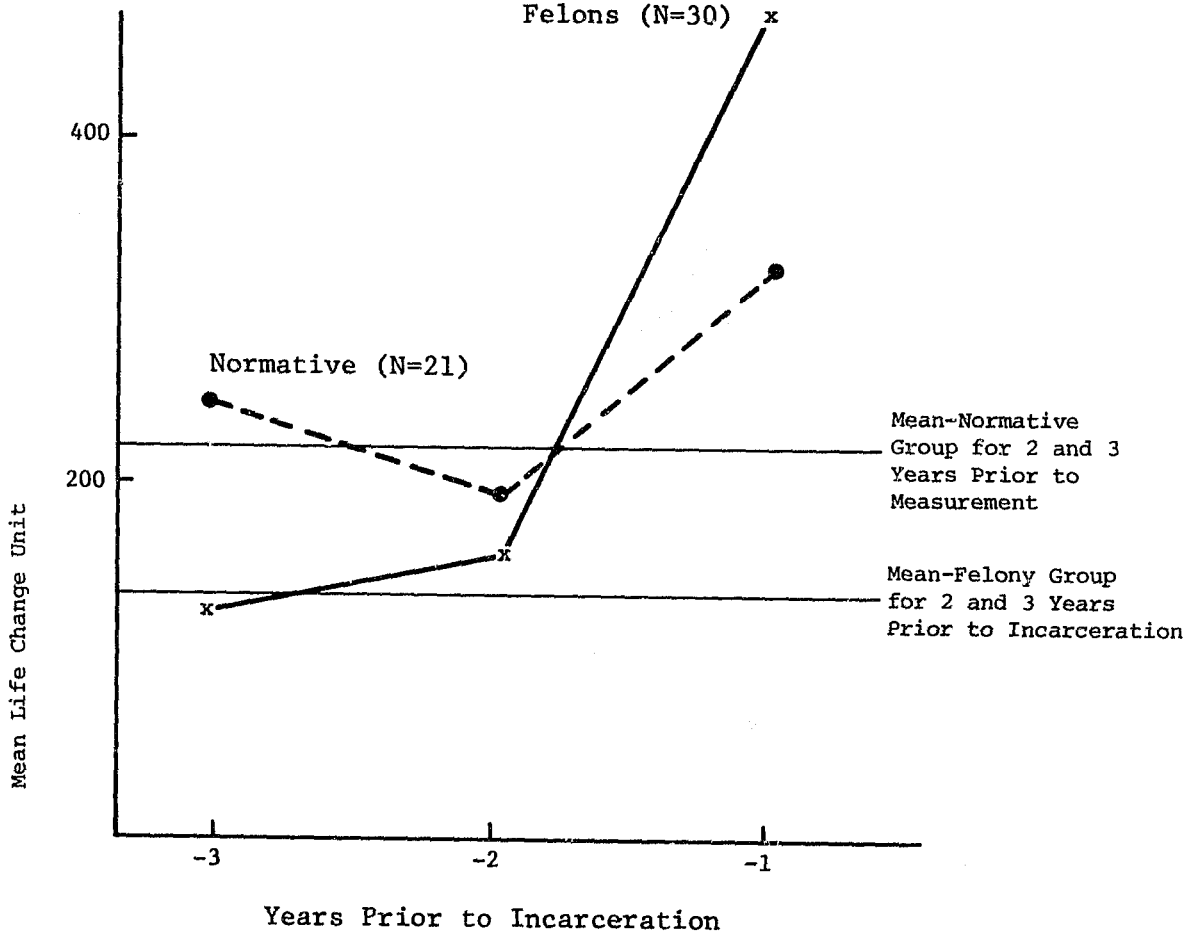


FIGURE 1
ANNUAL LIFE CHANGE UNITS FOR ADULT MALES
SELECTED FELONS AND NORMATIVE GROUP

The scale has 42 items. We grouped them to eight categories. Conflict with the law is on the top of the scale, going down to personal events and life style change at the bottom (Figure 2).

Conflicts with the law included items such as detention in jail and minor violations of the law. The second category, spouse-related events, included 7 items such as marriage, divorce, separation, reconciliation, death of a spouse, argument with a spouse, and on.

Down at the very bottom of the list, the area called lifestyle included such items as eating, sleeping changes, recreation changes, church changes, living conditions, social activities, et cetera.

The mean annual frequency of experience within each activity is shown for the normative males (solid line marked with an "X" in Figure 2). This is a normal male distribution curve for the year prior to taking the test. Notice the very low frequency of conflict with the law and the relatively high frequency of lifestyle changes.

Figure 2 also compares the criminal group with the normal group. The normal group is the solid line called "Normative." The three other lines reflect the criminal groups broken down according to the character of their crime. Assault is the small dashed line labeled "a," murder is the large dashed line laveled "m" and the dotted line labeled "p" is the property crimes.

Figure 2 shows that the reported life changes of the criminal groups were relatively lower than the normal population; yet, in the specific life area dealing with conflict with the law, we see some rather marked differences.

In most of the life event areas, the incarcerated groups reported (fewer) recent life experiences. Conflict with the law is the most conspicuous feature of the recent life experience in these incarcerated groups.

Moreover, conflict with the law is the only life event in which the incarcerated group reported greater recent life experience than the normative group.

The mean annual frequency of experience within each activity area for each of the three years prior to arrest in the jailed sample shows conflict with the law was the most frequently reported area of experience on their Schedule of Recent Experience.

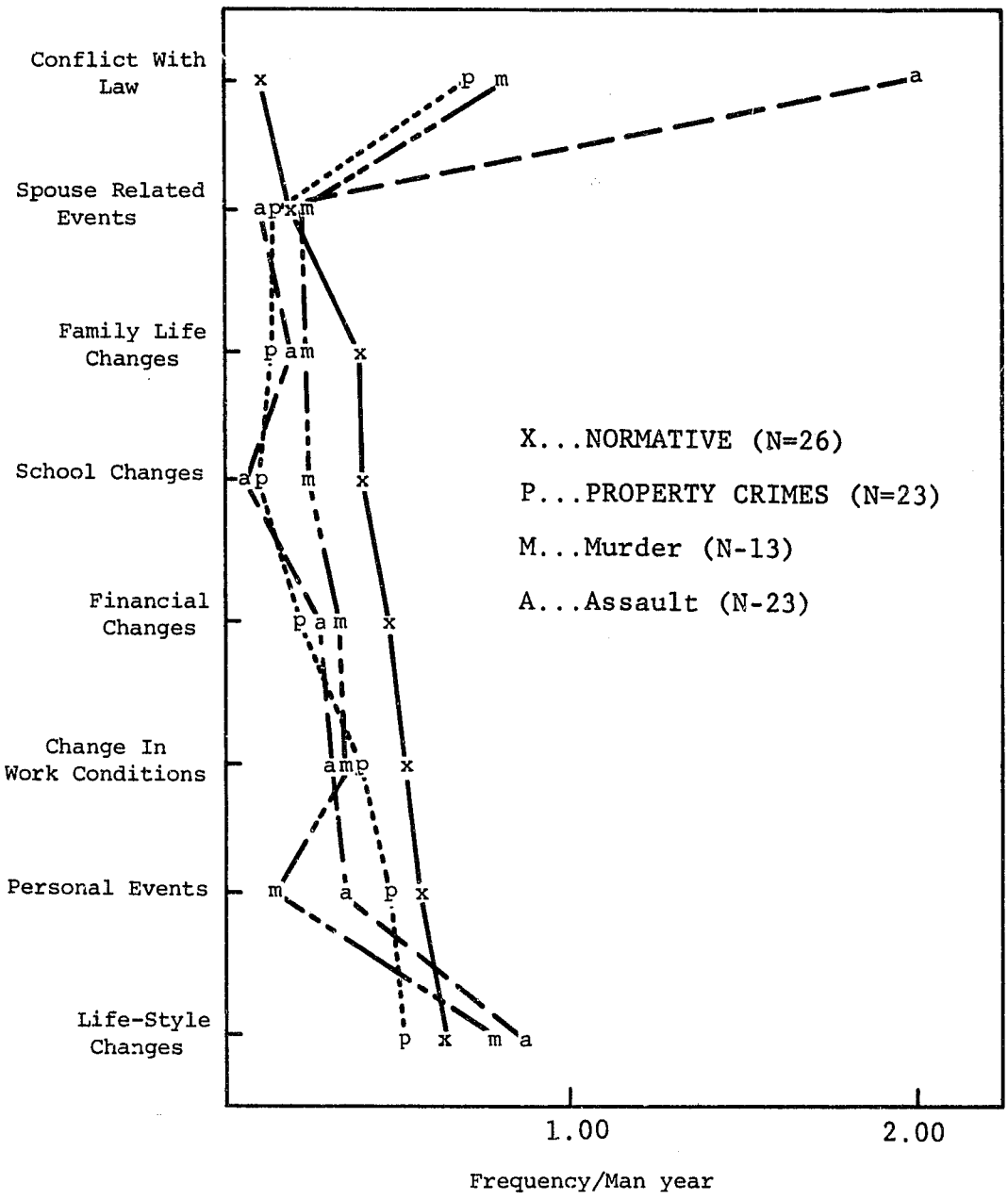


FIGURE 2
LIFE EVENT FREQUENCY PROFILES:
NORMATIVE MALES AND THREE CRIMINAL SAMPLES

So this pattern of lifestyle persisted, at least previous to the arrest for three years.

We now try to examine the amount of life change and the severity of the crime (Figure 3). Analysis of the relatioship of life change and the severity of the crime was possible only with a sample of 334 juvenile delinquents, who were in a detention center in Utah. A measure of crime severity was obtained based on the Utah Juvenile Code. Their schema, codified in law, is that crime severity is defined as the product of two factors, one resulting from the result of the crime and one relating to the type of victimization.

Now there's also the offense. There's a 9-point hierarchy. Death of the victim is at one end and no victim is at the bottom end, and somewhere in between are threats of violence and such.

Type of victimization is another 7-point hierarchy. Multiply these two together and one gets a severity score.

We then plotted the severity score of the various offenses; nondelinquent juvenile group (no offense), status offenses, acts against public order, acts against property, acts against persons against the mean annual life change score. Figure 3 shows a strong positive relationship between the mean severity and the life change score for four groups of juvenile offenders and nondelinquent controls.

Within the group detained for status offenses or acts which are illegal only for juveniles, the Pearson's coefficient between the one-year life change score and the crime severity was .70. Within the group detained for acts against public order, the correlation was somewhat lower, .51. For juveniles detained for acts against property and acts against persons, the correlation between recent life change and crime severity is .59 and .64 respectively.

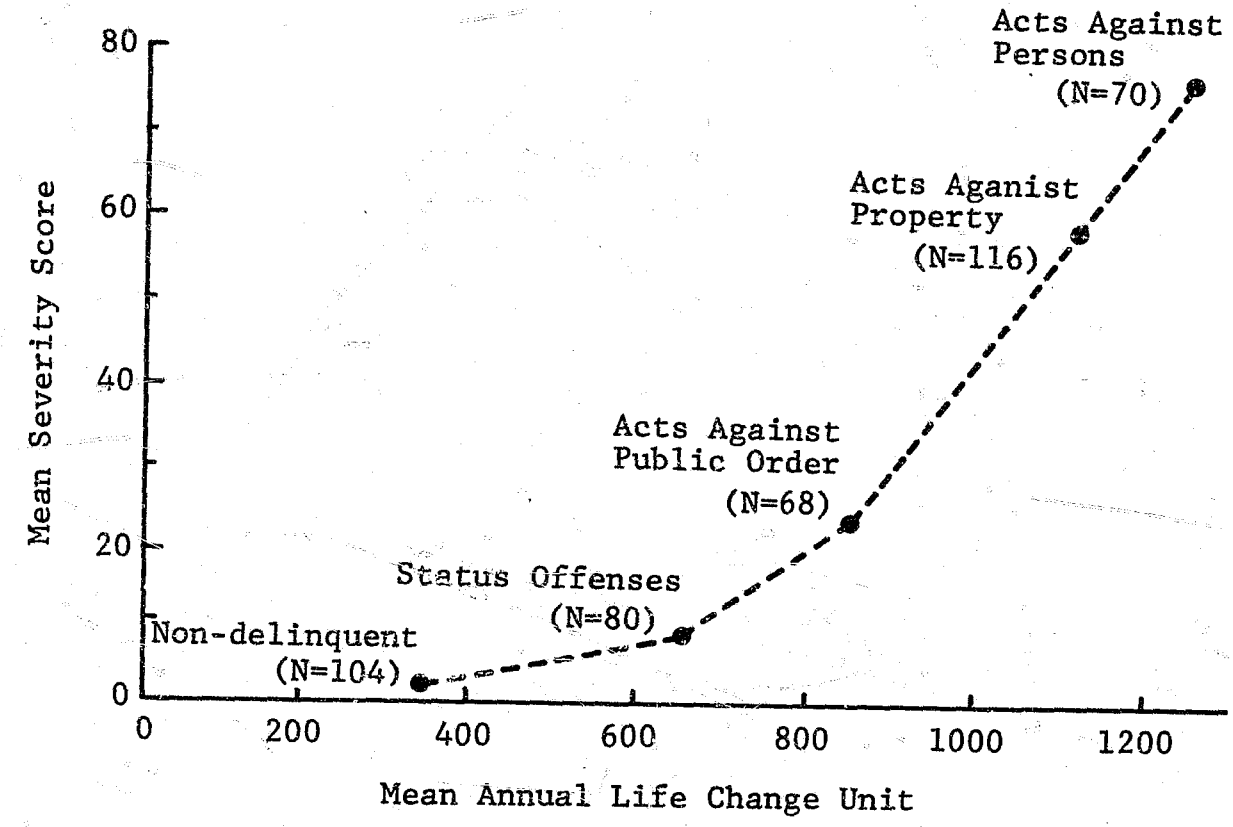
The range of these correlations indicate that between 26 and about 5[illegible] percent of the variation in crime severity scores is associated with variations in the life change score in the year prior to criminal behavior.

Parenthetically, we didn't do this for the older men because we didn't have any systematized way of indicating the severity of the crime.

In the paper that you have, we attempted to look at the life change magnitude and the process of imprisonment. This data has been published elsewhere.

CONTINUED

1 OF 3



From Kulcsar (1976)

FIGURE 3
MEAN LIFE CHANGE AND JUVENILE OFFENSE SEVERITY SCORES

Figure 4 shows that the mean annual life change score for 176 prisoners for each of the five years prior and subsequent to the current incarceration. There is mounting life changes observed here as in the combined jail and juvenile delinquent group.

With three years in prison, though, the life change score is observed to return to a low level observed in the 4-5 years prior to incarceration.

An interesting sidelight is that the lower graph of Figure 4 shows that the time course of this change process is much slower for the oldest prisoner group (46-65 years). In other words, the young seem to adapt to jail as measured by their life change scores much more quickly than the older inmates.

The purpose of this report is to examine, retrospectively, the recent life history of three incarcerated samples using the methodology of Holmes and Rahe. Data adduced from this device suggests that for both adult and juvenile criminal populations incarceration occurs in a setting of mounting life change.

The observed association is quantitatively similar to that observed in both prospective and retrospective studies of illness susceptibility and some behavioral disorders. Behavioral disorders primarily relate to child abuse, addictions to alcohol, drug abuse and suicide.

Data for this comparative study was provided from the criminal offense reports on the residents of a juvenile detention center in Utah and prison inmates in the State of Washington.

Since recent work by Dr. Holmes shows that older people report fewer life change events than younger people, this analysis compares data for similar age cohorts.

It was initially hypothesized that the different time intervals for the reports of life events would hinder the comparison of jail, and the prison samples.

For instance, prisoners recalled the time period which in some cases had passed five years previously; whereas, the jail sample recalled the time period days, or at most, weeks previously.

It was observed, however, that there was no significant difference in the number of events reported by the two samples. If impaired recall reduced the number of life events, the effect was

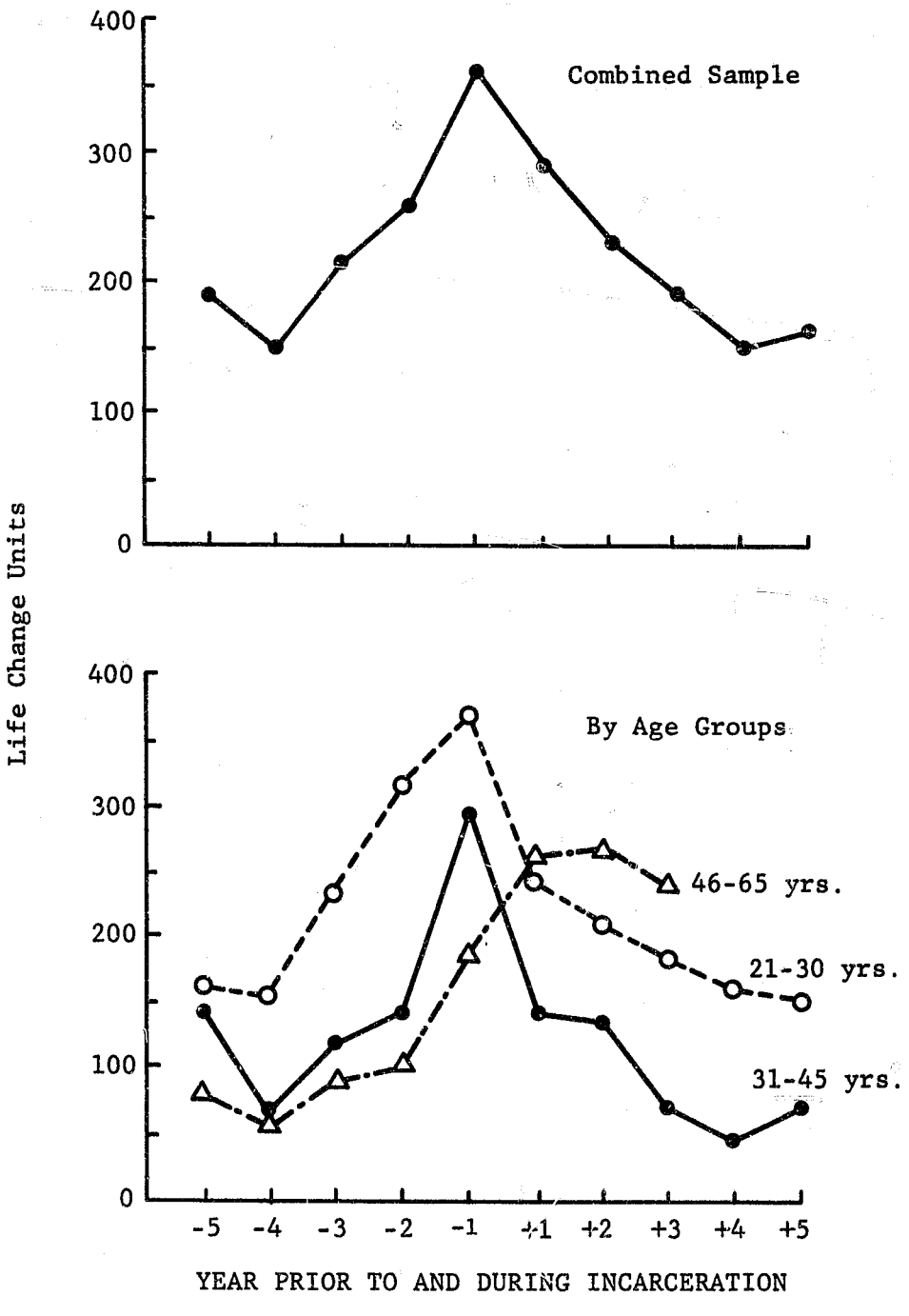


FIGURE 4
ANNUAL LIFE CHANGE UNITS BEFORE AND DURING INCARCERATION

not evident in the period two or three years prior to incarceration. Other workers have observed only a small decrement in life recall.

Jail and prison samples were combined for a selection of young incarcerated men to be compared with a normal nonincarcerated group. The incarcerated men show a significant increase in the life change score in the year prior to their incarceration.

Incarcerated men also reported a lower life change score than normals for a more remote period, in other words, the two and three years past. Analysis of the mean annual frequency of various life events showed that with the notable exception of conflicts with the law, the recent and remote life experiences of the incarcerated men included fewer events in most areas than normative men.

Conflict with the law emerged as the most consistent and outstanding feature of life experiences reported by the incarcerated men, regardless of the nature of their crime, such as assault, murder, et cetera.

For this reason, conflict with the law is further analyzed in its two component events, jail terms and minor law violations. Figure 5 shows that the reported ratio of going to jail per reported frequency of law violations seems to increase especially in the two years prior to incarceration.

Quantitatively, this type of relationship suggests that samples of incarcerated individuals may represent those selected by the criminal justice system for exactly this pattern of behavior in the criminal record.

Quantitatively, the data indicates that the men are reporting jail terms more frequently than they are reporting law violations on our test. This measure is difficult to interpret, but suggests that this particular area of research might be profitably expanded into a more meaningful test of the recall of events for criminal groups.

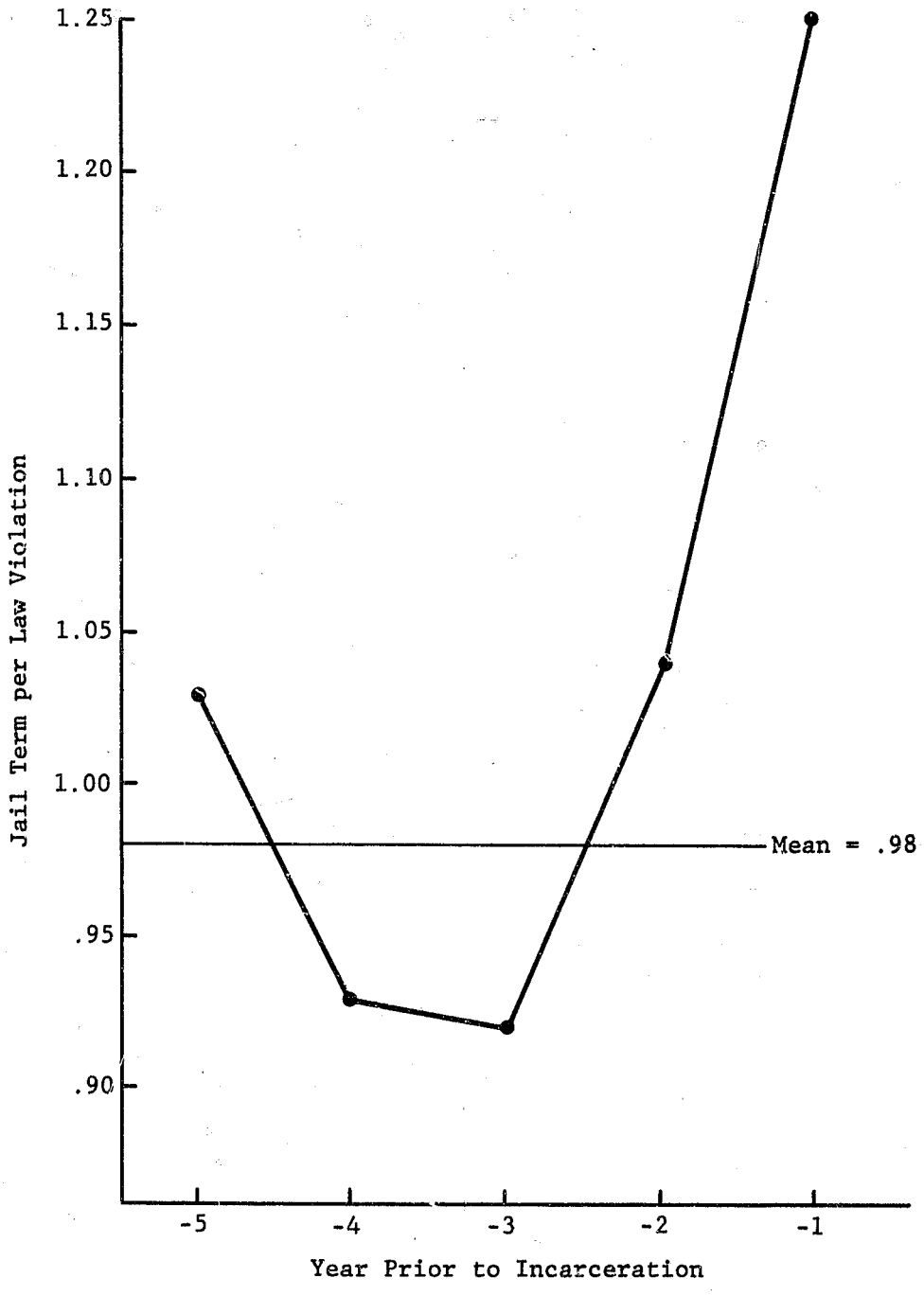
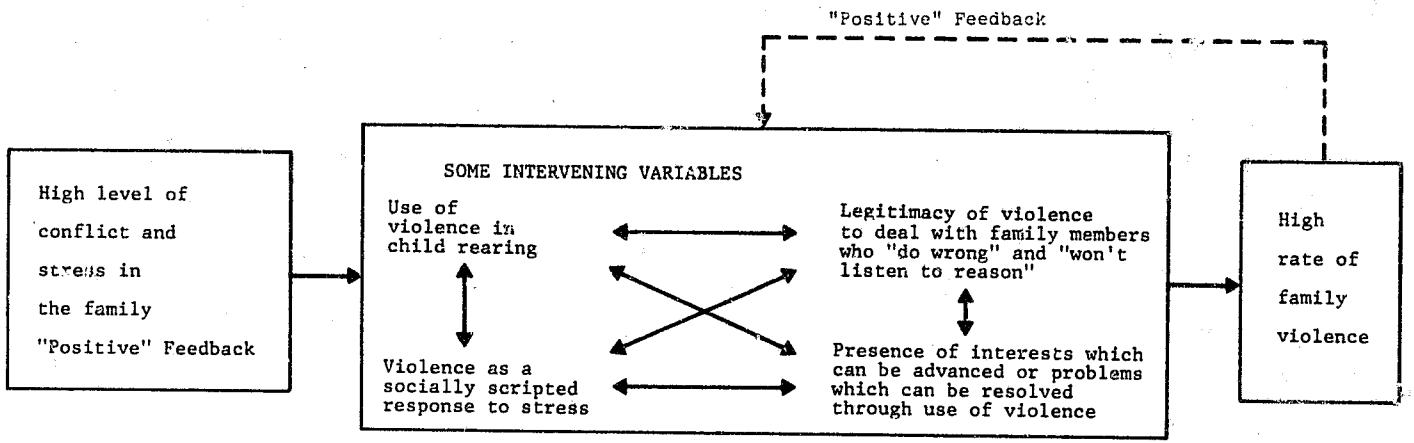


FIGURE 5
REPORTED JAIL TERMS PER REPORTED LAW VIOLATIONS
(N = 59)

INTRODUCTION

WILKINS: If we may move on to the next speaker, Professor Murray Straus, Professor of Sociology at the University of New Hampshire. He's also serving as director of the Family Violence Research Program.

He's recently been the co-editor of a number of books on family measurement techniques, the social causes of husband-wife violence, "Behind Closed Doors" and violence in the American family. He's going to address the topic of "Stress and Assault in a National Sample of American Families."



This diagram is labeled as a "partial model for two main reasons: The most obvious reason is that it includes only a sampling of the intervening variables which could be included in the center box. Second, the model omits negative feedback loops (i.e., deviation dampening process) which must be present. Without them the violence would escalate to the point where the system would self-destruct--as it sometimes, but not typically, does. See Straus, 1973 for a system model of family violence which includes negative feedback processes and other elements of a cybernetic system.

FIGURE 6
PARTIAL MODEL OF RELATIONSHIP BETWEEN STRESS AND FAMILY VIOLENCE

I recommend it for a couple of reasons. One is the methodology. I think they have the best treatment of stressful events that I have seen though I haven't seen Dr. Lieberman's work. They take into account the cumulative nature of these events that Dr. Brenner mentioned, and also the interaction effects. The point, though, is that they relate stress not to violence but to depression to nonviolence, to withdrawal in their sample of 500 London women.

The data that I'm going to present were obtained in a survey conducted in January and February, 1976. Interviews were conducted with a nationally representative sample of 2143 American couples. The aspect of stress which was measured was limited to what can be called "stressor stimuli." The data was obtained by using a modified and shortened version of the Holmes and Rahe life event scale.

The technique used to measure physical violence is one I've developed called Conflict Tactics Scales. This measure consists of a checklist of acts of physical violence.

The respondent is asked about conflicts and difficulties with other family members, and then is asked if in the course of the conflict he or she did any of the items on the list.

The list starts out with nonviolent tactics. It has, in fact, a reasoning scale, then a verbal aggression scale, and then finally, items that make up the physical aggression or violence scale as a conflict tactic. The violent acts, in turn, were deliberately designed so as to permit a measure of the severity as well as the frequency of family violence.

The list of violent acts starts out with pushing, slapping, shoving, and throwing things. These are what can be called the ordinary or normal violence in family life. It then goes on to kicking, biting, punching, hitting with an object, beating up, and using a knife or gun.

The latter group of violent acts is what I'll focus on this morning. It's used to compute a measure of severe violence which is comparable to what, in the case of parent-child relationships, social workers call "child abuse," in the case of spousal violence, feminists call "wife-beating," and criminologists would call "assaults."

The data in the first row of Table 4 shows that violence by a husband against his wife which was serious enough to be classified as wife-beating, occurred at a rate of 3.8 per hundred couples in the year of the survey. Violence by a wife serious enough to be

TABLE 4

INCIDENCE RATES FOR SEVERE VIOLENCE INDEX, OVERALL VIOLENCE INDEX, AND ITEMS MAKING UP THESE INDEXES

Conflict Tactics Scale Violence Indexes And Items	Rate Per 100 For Violence By:		Frequency*			
			Mean		Median	
	H	W	H	W	H	W
Wife-Beating and Husband-Beating (N to R)	3.8	4.6	8.0	8.9	2.4	3.0
Overall Violence Index (K to R)	12.1	11.6	8.8	10.1	2.5	3.0
K. Threw something at spouse	2.8	5.2	5.5	4.5	2.2	2.0
L. Pushed, grabbed, shoved spouse	10.7	8.3	4.2	4.6	2.0	2.1
M. Slapped spouse	5.1	4.6	4.2	3.5	1.6	1.9
N. Kicked, bit or hit with fist	2.4	3.1	4.8	4.8	1.9	2.3
O. Hit or tried to hit with something	2.2	3.0	4.5	7.4	2.0	3.8
P. Beat up spouse	1.1	0.6	5.5	3.9	1.7	1.4
Q. Threatened with knife or gun	0.4	0.6	4.6	3.1	1.8	2.0
R. Used knife or gun	0.3	0.2	5.3	1.8	1.5	1.5

*For those who engaged in each act, i.e., omits those with scores of zero.

classified as husband-beating occurred at an even higher rate, 4.6 per hundred couples.

However, it's important to remember that these data are based on attacks rather than injuries produced. If one uses injuries as the criterion, then wife-beating would far outdistance husband-beating.

Now let me turn to the relationship between stressful life events and assault between spouses. Data plotted in Figure 7 show that the higher the stress score, the higher the rate of assault between husband and wife. For the wives, the curve approximately fits a power function. For the husbands, the relationship shows a general upward trend but is irregular.

Now, interesting as are the findings shown in Figure 7, they do not reflect the theoretical model that I talked about. And I might even say that the data distort the situation because the graphs tend to draw attention away from a very important fact. That fact is that most of the couples in this sample who were subject to a high degree of stress were <u>not</u> violent.

So the critical question brought to light by this fact and by the theoretical model is what accounts for the fact that some people respond to stress by violence, whereas, others do not?

Part of the answer to that question was suggested by the set of variables in the center box of Figure 6. In order to operationalize that theoretical model; that for stress to result in violence, there have to be certain intervening variables, what I did was to separate out the husbands who were in the high quartile of stress. Those are the ones that I'll be talking about.

The high quartile stress husbands were further divided according to each intervening variable. This enables us to see if the intervening variable was, as specified in the theoretical model, necessary for life stresses to result in violence. If the theory is correct, the men who are high in respect to intervening variables will have a high rates of violence, whereas, the men in the low category with respect to the intervening variables will not be more violent than the sample as a whole, despite the fact that they are under just as much stress during the year as the others.

The data for these tests of theory are presented in Table 5; I'll discuss section A, on childhood experience with violence, and then skip to the summary.

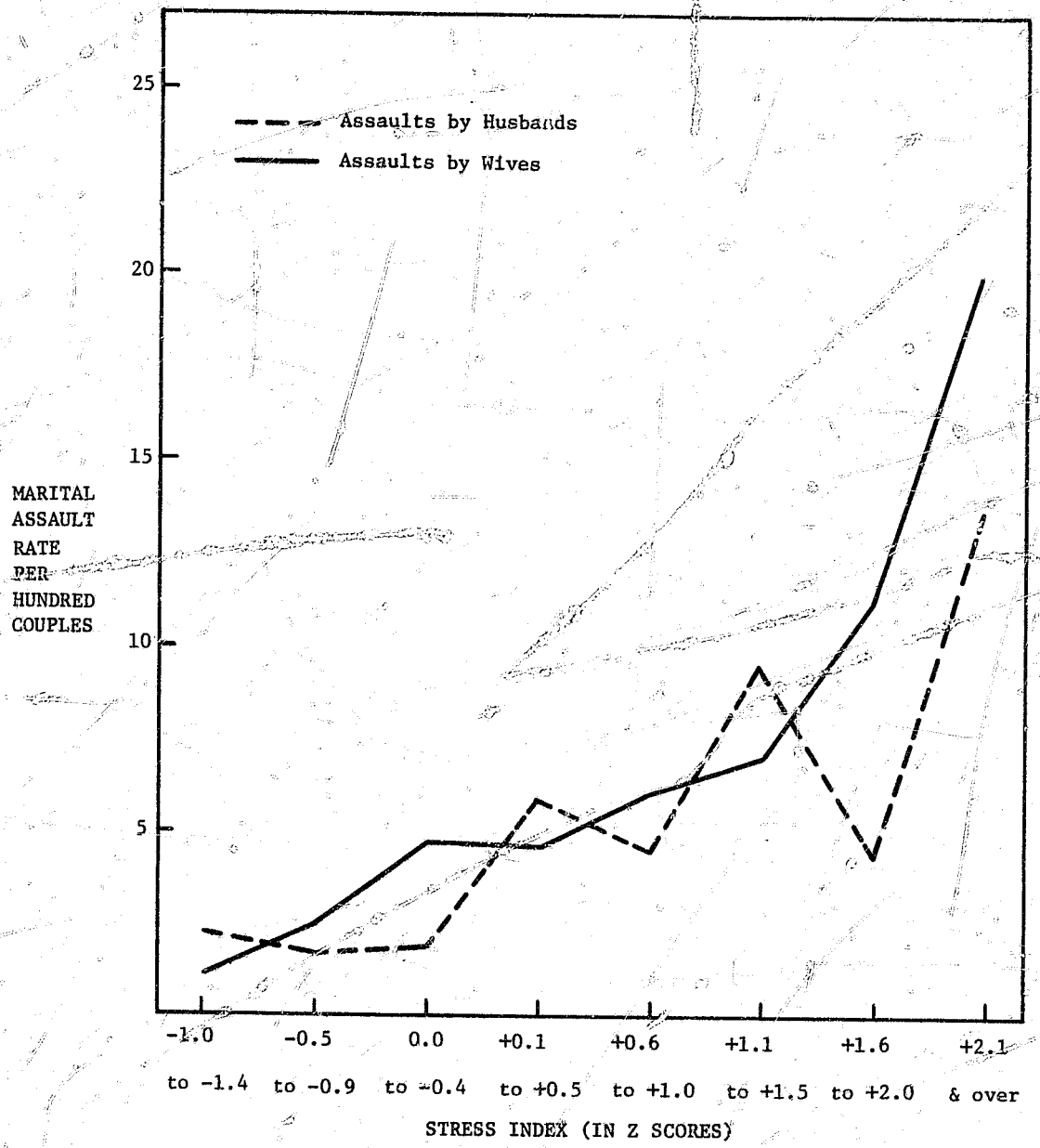


FIGURE 7
MARITAL ASSAULT RATE BY STRESS INDEX SCORE

TABLE 5

EFFECT OF INTERVENING VARIABLES ON THE INCIDENCE OF ASSAULT

BY HUSBANDS EXPERIENCING HIGH STRESS

Intervening Variable	Assault Rate Per 100 Husbands when Intervening Variable was:		N*	
	Low	High	Low	High
A. Childhood Experience With Violence				
Physical punish. after age 12 by mother (0 vs 4+ per yr)	7.1	6.7	85	89
Physical punish. after age 12 by father (0 vs 4+ per yr)	7.4	8.4	81	83
Husband's father hit his mother (0 vs 1+ per yr)	5.4	17.1	167	41
Husband's mother hit his father (0 vs 1+ per yr)	4.6	23.5	176	34
B. Legitimacy of Family Violence				
Approval of parents slapping a 12 year old (0 vs high ½)	5.9	9.9	34	71
Approval of slapping a spouse (0 vs any approval)	2.7	15.0	150	100
C. Marital Satisfaction and Importance				
Marital Satisfaction Index (low vs high quartile)	12.3	4.9	73	61
Marriage less important to husb. than to wife = high	5.9	11.7	17	34
D. Socioeconomic Status				
Education	6.1	5.4	49	56
Husband a blue-collar worker = low	9.2	5.4	284	202*
Income (low = $9,000, high = $22,500)	16.4	3.5	122	113*
E. Marital Power				
Power Norm Index (high = husb. should have final say)	4.2	16.3	71	55
Decision Power Index (high = husb. has final say)	5.2	16.1	58	62
F. Social Integration				
Organizational Participation Index (0 vs 11+)	10.5	1.7	86	60
Religious service attendance (0-1/yr vs weekly)	8.9	5.4	79	56
Relatives living near (0-2 vs 13+)	5.7	11.9	124	118*

*The N's vary because, even though the intent was for the high and low groups to be the upper and lower quartiles, this was not always possible. In the case of occupational class, for example, the comparison is between a dichotomous nominal variable. In the case of continuous variables, we sometimes wanted to preserve the intrinsic meaning of a score category, such as those who with a score of zero, even though this might be more or less 1/4 of the sample. Another factor causing the N's to vary is that the division into quartiles was based on the distribution for the entire sample of 2,143, rather than just the high stress subgroup analyzed in this table. Finally, there are three variables for which the data was obtained from the wife as well as the husband (husband's occupation, family income, and relatives living nearby). The N's for these variables are roughly double those for the other variables because they are based on the entire sample, rather than only on those families where the husband was the respondent.

The first row in Table 5, runs directly contrary to the theory being examined. It shows that men who were physically punished the most by their mother when they were teenagers were slightly less violent under stress than the men who were not, or who were only rarely hit at this age by their mother.

On the other hand, having been physically punished more than on just a rare occasion by a father after age 12 does relate to violently assaulting a wife. Husbands whose fathers hit them the most have an assault rate against their wives which is somewhat higher than do husbands who are under equally high stress during the year, but who did not experience this much violence directed against them as a teenager.

The difference between the effect of having been hit by one's mother versus by one's father suggests that violence by the father against a teenage boy is a more influential role model for violent behavior which the son will later display under stress.

The next two rows in Section A refer to violence between the parents of the husbands in this sample. These two rows show large differences between the husbands who were sons of fathers who assaulted their wives and those who were not. The assault rate by the husbands whose own father hit their mother was 216 percent higher than the rate for men whose father never hit their mother (at least as far as they could remember). So we see again a role modeling effect.

I'll now skip to my summary statement of each of the different sections.

Men who assault their wives under stress believe that physical punishment of children is appropriate behavior much more than do others. They also approve of slapping a spouse under certain circumstances (Table 5 Section B). Their early experience with violence, therefore, seems to have carried over into their present normative stance. However, a longitudinal study is needed to establish whether this is actually the causal direction. This is because people can retrospectively define what is acceptable behavior based on what they actually do. In fact, we have a lot of evidence that this does happen.

Second, men under stress are more likely to assault their wife if the marriage is not an important and rewarding part of their life. (Table 5 Section C).

Third, education does not affect the link between stress and violence. However, low income and low status occupation do, perhaps because these are indicators of additional stresses. (Table 5 Section D)

Fourth, men who believe that husbands should be the dominant person in a marriage, and especially husbands who have actually achieved such a power position, have assault rates from 1 1/2 to 3 times higher than men without such values who are also under stress. (Table 5 Section E).

Let me conclude by saying that in my opinion, human beings clearly have an inherent capacity for violence. They also have an inherent capacity for doing algebra. The capacity is only translated into actually solving an equation or actually assaulting a spouse if one has learned to respond to a scientific problem or technical problem by using mathematics or learned to respond to stress and family problems by using violence.

Even with such training, though, violence is not an automatic response to stress, nor algebra an automatic response to a scientific problem. One also has to believe that the problem is amenable to a mathematical solution or to a violent solution.

The findings presented in this paper show that violence tends to be high when these conditions are present; for example, those whose childhood experience has taught them the use of violence and whose present need to dominate the marriage provides a situation which is likely to yield to such violence. If conditions such as these are present, stress is related to violence. If these conditions are not present, the relationship between stress and violence is absent or minimal.

INTRODUCTION

WILKINS: Our next discussant is Dr. Harvey Brenner, who is Associate Professor in the Departments of Operations Research, Behavioral Science and Mental Hygiene at Johns Hopkins University. He's also the coordinator for health and policy studies in the Metropolitan Department of Planning. His books include: Mental Health and the Economy, and Estimating the Social Costs of National Economic Policy: Implications for Mental and Physical Health and Criminal Aggression. He will talk on the subject of "The Influence of Economic Stress on Criminal Aggression."

THE INFLUENCE OF ECONOMIC STRESS ON CRIMINAL AGGRESSION

M. Harvey Brenner
Johns Hopkins University
Baltimore, Maryland

Thank you, Professor Wilkins. I must say just to begin with I'm really quite impressed with the growth of high quality stress research of the kind that I've seen in the papers and heard this morning. I think it may well represent a major new venture in criminology resulting in some rather hard and fast material, which we can all use rather soon.

At the same time, there have been a number of traditional methodological problems in this field, that perhaps given the high quality of the research, it is now time to look into more carefully. As has been pointed out by virtually every speaker, the pioneers in this generation of research into stress seem to run along Holmes, Rahe, Masuda, et cetera, style of life change analysis. This is very important in psychiatry, psychosomatic medicine, and now in criminal justice.

There is one very profound methodological problem in the use of those materials which perhaps most of the discussants are familiar with. For those who aren't, it is simply that the causal direction of the relationship is virtually impossible to discern. For example, as several of the speakers have pointed out, the great variety of life's stressful events are associated with criminal behavior in whatever category. The question becomes which is causing what? Is it that the kinds of behavior associated with the criminal label are in turn productive of life's stresses (in the Holmes and Rahe scale, problems with the law is itself a series of stress events), or is it that the life events are predictive of the subsequent behavior of a criminal psychopathologic sort. This is a profound problem that is not yet solvable with this set of techniques. We require much more highly refined and longitudinal and other types of research to get at the causal connecitons. As of the moment, it is virtually impossible to discern the direction, and because that is impossible our tests of statistical significance are subject to a variety of faulty associations. Quite beyond that, it is almost impossible to determine the importance of the relationships because of the problems of analysis of variance, given the causal connections.

Now we have a very, very long history in criminological research of cross-sectional work, as well as in stress research in the medical

and psychiatric fields, which has associated problems of virtually every type of human deviance of any serious sort with lower socio-economic status. We've had at least a half century of this kind of work in the United States, in Britain, and elsewhere in Europe, much of it very convincing. I'm convinced by it, for one.

The same type of methodological problem has been prevalent. Is it that lower socioeconomic conditions (deprived conditions) are productive of the pathological behavior? Or indeed, is it that people with pathological personalities who are ill, or who are psychiatrically disturbed, don't do very well in society, and as a result, don't make it economically as their age cohorts or other socioeconomic groups do? I do not find this latter argument terribly persuasive, but it has been sufficiently powerful over the years that, methodologically, it requires a strenuous attempt to deal with it. If we do not deal with it, we shall not be able to establish the causal connections involved here, which are fundamental, but we shall not be able to substantiate our tests of analyses of variance and the most fundamental of our tests of statistical significance.

How then to manage this kind of problem?

The basic issue is: where we have individual persons who are able to control or influence, in any way, <u>both</u> the life stress situations in their lives and their criminal behavior or depression or suicide, or mental problems (where the same individuals are capable of influencing both), it is virtually impossible statistically to discriminate which aspect of those changes or which attribute of that personality is the influencing factor at a single point in time, no matter how large the sample, no matter how many units of analysis are involved, no matter how many geographic regions. Even in longitudinal research over a period of two or three years, the problems do not vanish at the individual level, simply because the individuals, themselves are able to influence the outcomes.

I believe, and am committed to the position, that it is, indeed, these life events that precede the phenomena we have been observing. This is my own view, which I think can now be demonstrated in a rather microscopic way, which also has immediate policy implications for criminal justice, for national policy in the economic area, and in the stress-medical-psychiatric area as well.

One type of solution, then, involves finding stress situations which are not under the control of individuals. Now at first blush, this may seem a very difficult matter. Because there are clusters of events in people's lives, stress events, which tend to precede these pathological outcomes, for example, in the criminal justice

area, we simply cannot subscribe to a random notion of clustering. We have little epidemics of stressful phenomena that are occurring in the lives of these individuals. We must look at some set of instigators, or stimuli, of those epidemic movements in individual lives. What then can make for radical changes of many different types in individual's lives?

One of the most powerful known, of course, is an alteration in the economic status, in social position; a degradation of self-esteem, in social position, which very easily can be seen to lead to many different types of stresses themselves, and to the inability or the lessened ability of any individual to deal with the kinds of stresses which affect him. Thus, new stresses arise, where, otherwise, there would not have been stresses.

> (Editor's Note: At this point Professor Brenner presented and briefly discussed a large number of graphs which demonstrated the relationships between trends in economic indicators (primarily unemployment) and trends in various indicators of medical-social pathology (infant mortality, deaths from cardiovascular disease, admissions to mental hospitals, suicide, etc.) as well as trends in various criminal justice indicators (crimes known to police, homicide, admissions to state and federal prisons, known heroin addicts, etc.). Most of these graphs which were presented have not been made available to MITRE, although some have. A transcript of Professor Brenner's remarks at this point without a reprint of the graphs will not add much to the reader's understanding. For that reason MITRE has taken the liberty of summarizing the main points and incorporating several of the graphs that were available to illustrate these points. The transcript will continue with Professor Brenner's statement following the showing of the graphs.)

Professor Brenner's basic hypothesis is that many aspects of social pathology (including criminal behavior) cluster and that this clustering is due to stress emanating from economic causes. Using a wide range of medical-social pathology and criminal justice data (from the U.S., Canada and the United Kingdom), Professor Brenner demonstrated consistent relationships over long periods of time (up to 65-70 years) between measures of economic well-being (chiefly measures of unemployment) and indicators of medical-social pathology reflecting stress and indicators of criminal behavior. These relationships showed that as economic indicators decline medical-social pathology and crime increase. There were time lags in several of these relationships, e.g., declines in economic well-being (employment, per capita income) preceding increases in

medical-social pathology or criminal justice activities by several years. In order to best demonstrate the general relationship, the ordinates of the graphs were often transformations of the raw data. In addition, several of the relationships were shown by using percent changes in the economic, pathology and crime indicators over 2, 3 or 5 year periods.

As illustrations of these basic relationships using criminal justice data, Figures 8a-d compare the trends in unemployment (with the scale reversed--lower values equal higher level of unemployment) against the trends in homicide between 1912 and 1968 for white and black males age 25-29 and 55-59 in the United States. Using another indicator of crime, Figures 9a-d show trends in unemployment (again with the scale reversed) and trends in crimes known to police (robbery, larceny, housebreaking and shopbreaking) in the United Kingdom between 1915 and 1970. Figures 9 a, b and d depict changes over 3 or 5 year periods. Figure 10 uses still another indicator of criminal behavior--number of prisoners received into U.S. state and federal institutions convicted of larceny for 1925-1952 in terms of percentage change over three year intervals (unemployment scale reversed).

These examples as well as the many others used by Professor Brenner in his presentation provide evidence of a long-term negative relationship, on an aggregate level, between economic well-being and indicators of medical-social pathology and crime for the United States and other countries.

Professor Brenner also reported regression equations using the various criminal justice and medical-social pathology measures as dependent variables and economic indicators as independent variables. The equations produced results that were able to account for over 80 percent, and in some cases over 90 percent of the variation in the crime and medical-social pathology trends. Table 6 shows estimates generated by the regression equations of the effects of a one percent increase in the unemployment rate on changes for various crimes and state prison admissions using the 1970 data base. The cost implications of criminal justice or medical-social pathology increases have also been calculated.

BRENNER: To summarize: What we are finding as the previous papers have indicated, in the new brand of stress research, is that once we are able to identify one of the more important stimulators of the set of epidemic-like events that result in these clusters, we can with a macroscopic analysis for states, for cities, for countries, produce equations that will allow us to understand the fluctuations over time in the prime indicators, regardless of the type of indicator we used, which is very important in this field.

FROM TESTIMONY OF DR. HARVEY BRENNER BEFORE THE HOUSE SUBCOMMITTEE
ON CRIME, SEPTEMBER 27, 1977

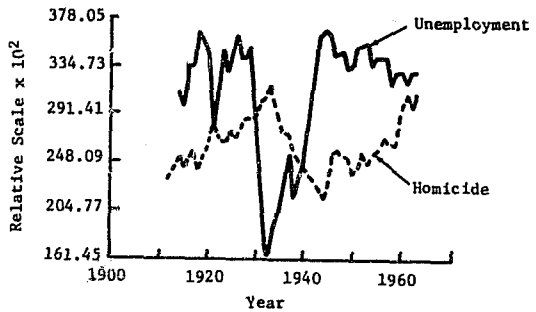


FIGURE 8a
U.S. HOMICIDE MORTALITY VS. PERCENT UNEMPLOYMENT
INVERTED WHITE MALES AGES 25-29 (RATED)—
YEARS 1912-68

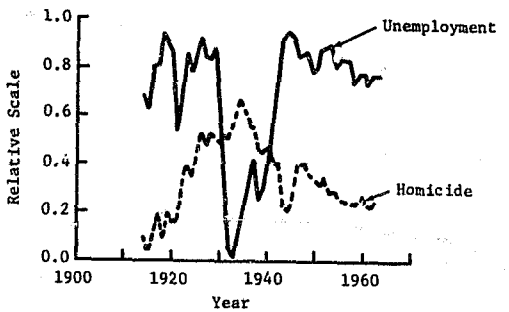


FIGURE 8b
U.S. HOMICIDE MORTALITY VS. PERCENT UNEMPLOYMENT
INVERTED NEGRO MALES AGES 25-29 (RATED)—
YEARS 1915-68

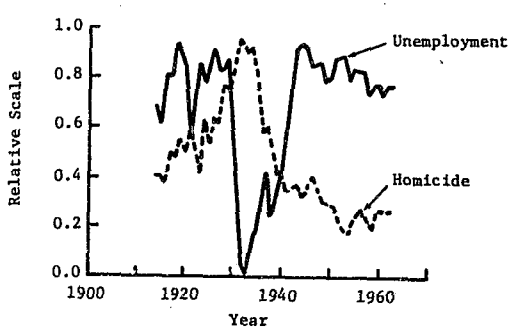


FIGURE 8c
U.S. HOMICIDE MORTALITY VS. PERCENT UNEMPLOYMENT
INVERTED WHITE MALES AGES 55-59(RATED)—
YEARS: 1915-68

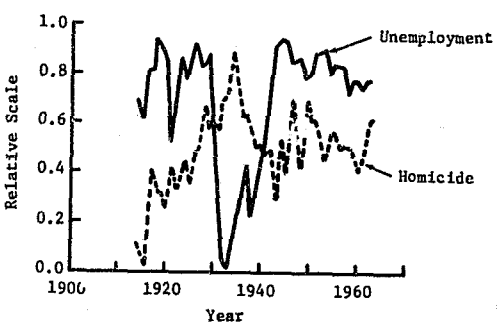


FIGURE 8d
U.S. HOMICIDE MORTALITY VS. PERCENT UNEMPLOYMENT
INVERTED NEGRO MALES AGES 55-59 (RATED)—
YEARS: 1915-68

FROM TESTIMONY OF DR. HARVEY BRENNER BEFORE THE HOUSE SUBCOMMITTEE
ON CRIME, SEPTEMBER 27, 1977

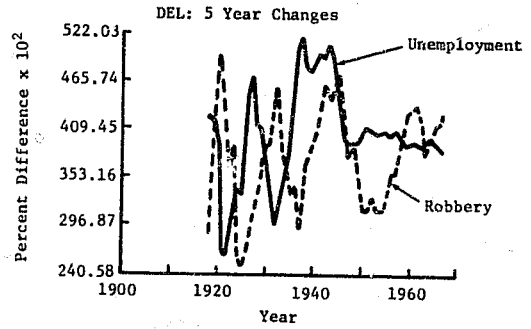


FIGURE 9a
U.K.: CRIMES KNOWN TO POLICE (RATED)
TOTAL ROBBERY VS. PERCENT
UNEMPLOYMENT INVERTED—
YEARS: 1915-70

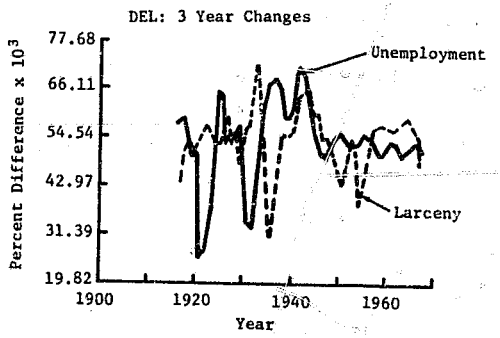


FIGURE 9b
U.K.: CRIMES KNOWN TO POLICE (RATED)
TOTAL SIMPLE & MINOR LARCENY VS. PERCENT
UNEMPLOYMENT INVERTED—
YEARS: 1915-70

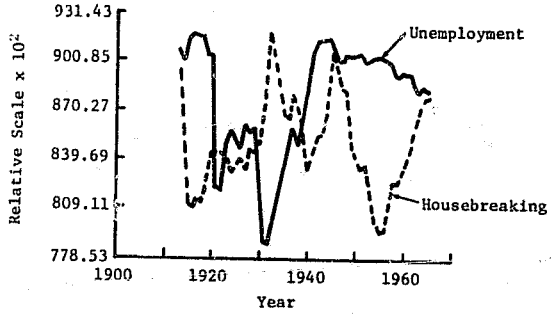


FIGURE 9c
U.K.: CRIMES KNOWN TO POLICE (RATED)
TOTAL HOUSEBREAKING VS. PERCENT
UNEMPLOYMENT INVERTED—
YEARS: 1915-70

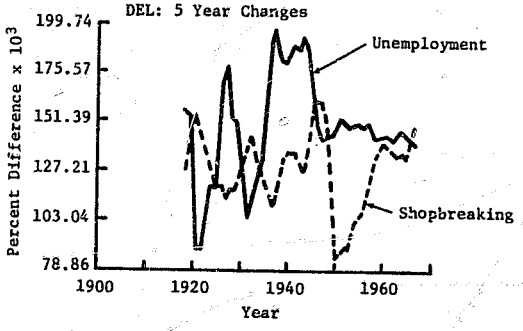
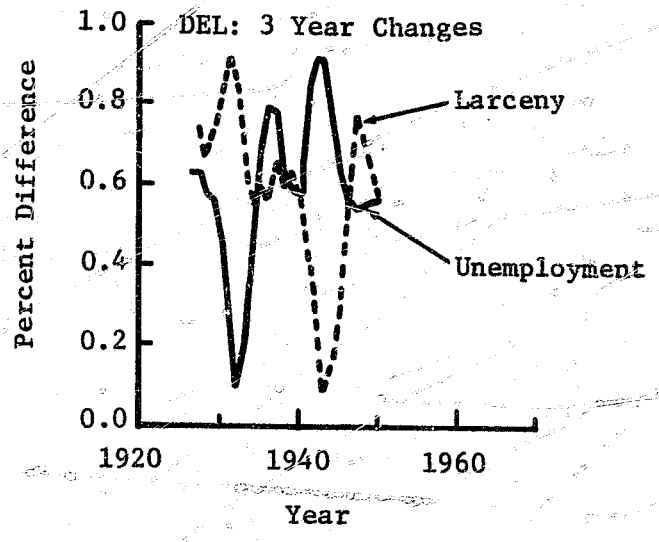


FIGURE 9d
U.K.: CRIMES KNOWN TO POLICE (RATED)
TOTAL SHOPBREAKING VS. PERCENT
UNEMPLOYMENT INVERTED—
YEARS: 1915-70



FROM TESTIMONY OF DR. HARVEY BRENNER
BEFORE THE HOUSE SUBCOMMITTEE ON CRIME,
SEPTEMBER 27, 1977

FIGURE 10
U.S. PRISONERS RECEIVED INTO STATE & FEDERAL INSTITUTIONS (RATED) MALES LARCENY VS. PERCENT UNEMPLOYMENT INVERTED— YEARS: 1925-52

TABLE 6

ESTIMATES OF THE EFFECTS OF A 1-PERCENT CHANGE IN UNEMPLOYMENT RATES ON CRIME INDICATORS

(Based on the population of 1970)

Selected criminal statistics	Incidence of criminality related to a 1-percent increase in unemployment (1)	Total incidence of criminality in 1970 (2)	Incidence of criminality in 1970 related to a 1-percent increase in unemployment in 1970 as a proportion of total 1970 criminality (3) (3=1÷2)
Embezzlement	5,123	85,033	0.060
Robbery	6,740	118,419	.057
Burglary	8,646	385,785	.022
Larceny	23,151	832,624	.028
Narcotics	40,056	468,146	.087
Homicide	648	16,848	.038
State prison admissions	3,340	67,304	.050

As you know, there are great problems with the credibility of data in criminal justice. It doesn't seem to matter what the type of data is that we use, the same relationship reappears.

The more important implications are:

That the evaluation research that has gone on in this field up to the present time has not taken into account the circumstances of the cities of the United States, for example, and the states, as well as the regional impact of economic and industrial change, which are obviously critical to any proper evaluation of what goes on in penal or other criminal justice activities as well as the subsequent behavior of offenders. A very, very important idea, simply because millions upon millions of dollars have been spent in this area with the result that to this day, we have equivocal results as to what impact our programs in criminal justice have as an effect upon criminals, or former criminals.

Clearly, the effect that the programs have is very largely a function of the experience of these men and women after they leave the situation (prison, jail, etc.), what the experience of the city, and the region, and the country economically happens to be.

Finally, as to the notion of future strategies, in line with some of the earlier speakers, I think it's important to identify on a macroscopic as well as an individually based level, what are the intervening factors that seem to move one set of individuals toward a criminal response to these types of stress versus another group, the cardiovascular group, still another group, the depression group, and still another group to the use of alcohol.

I suggest that when we look at different cities, different states, different nations, we will observe that there are different proclivities to respond in different ways by ethnic type, by age, by sex, by race, by socioeconomic status.

These can be identified as the previous very fine paper by Dr. Straus pointed out, but now can be identified as well on a population basis, to indicate what larger categories of persons seem to respond, and under what conditions, with different types of response mechanisms to stress with the key, in this instance, being on criminal responses.

DISCUSSION

CURTIS: The questions would be addressed to Murray Straus and Harvey Brenner, and they would be on the full policy implications of what they say. I think their papers are relevant, though a number of other ones are also. I speak as someone who is implementing the first crime prevention program in public housing for Secretary Harris, a program in which there is going to be employment strategies, tenant organization and crisis intervention, and a lot of other things that are relevant to what people are doing today.

I ask the two speakers: What, really, are the policy implications? It seems to me, especially from Murray Straus' paper, that there are two levels of policy. At the micro and clinical level, the implication is that for those people whose response to stress is violence there are some kinds of therapy, some kinds of clinical interventions, that might allow them to cope better.

The positive aspect of that is it helps them get through the day. I think of the poor minorities in the ghetto, in public housing for example, this is often very important. The negative side of it is that, for minorities, we don't have very good modes of therapeutic intervention, and this doesn't address underlying causes.

At the other level, the macro economic level, we're addressing basically structural changes in the society. That's a positive thing, if one sees those as causes.

The negative aspect of that is that those structural changes are the most difficult to undertake, politically. Also, we don't really know some of the relationships.

One of the conservative criminologists who I am constantly battling recently said to me, "Boy, I really want to get Brenner's data, because I am going to show that it's all explained by a couple of outliers."

Perhaps the best illustration of the fact that it's still difficult to demonstrate a relationship between structural conditions and the kind of outcomes we're interested in came out in recent hearings on income maintenance experiments.

It was shown that across all ethnic and racial groups--an increase in income, maintained income, resulted in a considerable rate of family dissolution. Divorce rates went up significantly.

There were two effects they saw. One was a reduction of stress. Another one was an increase in independence. That created all kinds of debates, which probably will lead to an outcome that we will not have guaranteed annual income.

I would like to just raise the question of what the concrete policy implications are, if any, at this stage of stress research?

WILKINS: You raise some very important questions there. I take it you don't want the answers right now, before lunch?

(Whereupon, at 12:00 noon, the meeting was recessed, to reconvene at 1:30 p.m., this same day.)

FIRST DAY AFTERNOON SESSION
INTRODUCTION

REISS: Well, I think we'll get started with the afternoon session. I have asked the participants to shift to a different way of handling the papers this afternoon.

Very briefly, what I have asked them to do, is to assume that all of us have read the papers and, therefore, try, insofar as possible, to shift to what they think might stimulate discussion this afternoon. We shall try to have more time for discussion.

So, I am quite mindful of that, and I am not going to take very much time leading into the afternoon session.

Our first session this afternoon is going to discuss organic determinants of stress in violent behavior, and a note on prison stress. And the first topic will be led by Dr. John Lion, who is a Professor of Psychiatry and Director of the Clinical Research Program for Violent Behavior at the Institute of Psychiatry and Human Behavior, at the University of Maryland School of Medicine. Dr. Lion has written on the subject of aggression and personality disorders, among others. He recently was coeditor of the book entitled "Rage, Hate, Assault, and Other Forms of Violence."

ORGANIC DETERMINANTS OF STRESS AND VIOLENT BEHAVIOR

John R. Lion, M.D.
University of Maryland
School of Medicine
Baltimore, Maryland

Thanks, Dr. Reiss.

I will attempt to summarize my paper, to foster discussion.

Basically, I dealt with four things. I first reviewed some old thoughts on the link between brain dysfunction and violence. These are very old. I called it the "seductive hypothesis." There have been some more recent studies showing that there is some degree of correlation between what's called "minimal brain dysfunction," a term that's used for children, and violence. This association makes it possible to identify a subpopulation of aggressive criminals who may have the hallmarks of impulsivity, excessive aggressiveness, mood lability, explosive rage outbursts, and who may be treatable with pharmacologic regimens.

Having said this, I make you aware of the policy implications of this which I learned from the psychosurgical experience. The psychosurgical data did shed light on the biology of violence, but it was such a controversial topic that eventually public policy ground it to a halt. There now exists, as you know, a moratorium on psychosurgical research, which is really too bad, though I think in time that it will be lifted.

We do need good research work on violent patients, using special techniques for the assessment of brain dysfunction. We do need prospective studies using pharmacologic regimens for the treatment of a subpopulation of highly aggressive, impulsive, paroxysmally violent criminals.

There are many obstacles to this kind of research. HEW has issued regulations prohibiting research on prisoners unless it is of direct benefit to them. It always has struck me as paradoxical that society wishes to control criminal behavior but prohibits clinical investigations of those very individuals who commit the crimes and who are, according to new public policies, warehoused for a fixed determinate sentence.

I thus make a policy recommendation, having just made a research recommendation, that we allow prisoners to volunteer for well-monitored

invasive or intrusive studies. I am sure this is unpopular, but I know no other way to further clinical progress. I would point out to you that all this needs to be put in perspective. Many of you will scream that we have enough biological research on prisoners and that we should bring this matter to a halt. I would point out to you that we spend millions on studies of what I call "noble diseases": heart disease, cardiovascular research, cancer research. Take cancer, for example. Despite the vast sums of money spent on cancer, limited gains have been made in its control. We've lengthened the survival times of some cancers; there are some remissions of certain leukemias that are possible. While the monies continue to be spent, most progress has come in the area of prevention.

Comparable funds are not spent, nor are they viewed as spendable in proportion to the magnitude of the problem with regard to crime or criminals or prisoners.

I would point out that even if we do allow prisoners to be used for research, it may not work because a prison is, after all, a fairly nurturant milieu and base rates of violence do drop in prisons. Criminals who are very violent on the outside often become more manageable on the inside, and the base rates of violence drop to proportions which are difficult to measure in some instances.

In our own work in assessing an alleged anti-aggressive drug, we found that we could not use institutionalized patients who had seemingly high base rates of violence, because once they were brought into the house, their base rates of violence dropped and there was nothing to measure.

I therefore make a second policy recommendation to complement the first: namely, that we allow prisoners to participate in outside research programs as a condition of probation.

Now, with regard to substantive issues for research, I point to two; first is alcoholism. I make the point that the relationship between alcohol and violence is accepted as a truism and neglected as a phenomenon. We have such things as pathologic intoxication, extreme rage aggravated by alcohol. There is existent unexplained evidence in the literature linking such phenomena with brain dysfunction. It needs further exploration. We know very little about why alcohol is so ubiquitously implicated in crime.

The questions I have are: Is there such a thing as a latent aggressive criminal who is activated by alcohol, or does alcohol specifically affect the aggressive component of crime? Why are other drugs, such as marijuana, which are also disinhibitory agents, not so ubiquitously linked with crime?

These are questions that still need answers. They are very obvious, but they still have not been satisfactorily answered.

There are other public policy issues that go along with these questions: Can we identify alcohol-related crime and distinguish it from nonalcohol-related crime, and, if so, should we formulate programs to coercively administer, say, Antabuse, much as Methadone is used? These are strong public-policy issues. Should we put some pressure or ask the help of the pharmaceutical industry to devise long-acting alcohol antagonists or agents which react adversely with alcohol, much along the lines of narcotic blockers and narcotics antagonists. Again, very stong social sentiments are mobilized by this.

From the alcoholic, let's turn now to the sexual criminal who has some cerebral mechanisms controlling the particular drive state of aggressiveness and sexuality. The paraphiliacs, the global term for sexual criminals, are compulsively driven to sexual behavior.

We know from extensive work in Europe, predominantly, that chemical and surgical castration certainly helps a definable population of paraphiliacs, particularly the aggressive ones. In this country at the present time, experimental programs utilizing newer hormonal agents, the progestational agents, are very few. For example, most of the work at Hopkins by Money and Blumer and Spodak contain populations of several dozen, whereas the series from European countries are several hundred.

The work in this country is lagging. This is surprising. On one level, we have deep social concerns with respect to sexual aggression, and on another concern about behavior control. We know very little about the endocrinologic parameters of the sexually aggressive patient and I suggest that we need more basic research along these lines.

I have identified three areas of research: brain dysfunction, an old concept, a logical one to look at, a dangerous one to look at primarily because it implies that violence and brain dysfunction may be linked. This is a dangerous concept, and humility is required. We don't want to think that violent people should be candidates for psychosurgery.

I have mentioned alcoholism, the ubiquity of the phenomenon and the corresponding lack of phenomenologic knowledge. I have mentioned the use of hormones to control sexual aggressiveness.

And I would want to say that my final proposal would be that the underwriters of this colloquium consider the establishment of

a formal organization to provide leadership for and to grapple with the bioethics of criminologic research. I think that's very much needed, provided that that organization sufficiently articulate the issues and disseminate the issues to the lay public and to the consumer, the prisoner. Only if that is done is there a possibility for creative options in the handling of violent criminals.

Thank you.

INTRODUCTION

REISS: We will have our second presentation before discussion. Our next presenter is Professor Hans Toch, who will speak on "A Note on Prison Stress."

Professor Toch is a member of the faculty at the School of Criminal Justice at SUNY, Albany. He is a social psychologist. He has had a rather wide-ranging set of publications, and his books have dealt very much with the nature of violence, more recently, violence and social control and violence in prisons.

A NOTE ON PRISON STRESS

Hans Toch
State University of New York
Albany, New York

Let me say, that one of the general statements I would make about stress is that it is very hard to infer what happens in the mid-range of stress, whether we are talking about communities or institutional settings, from extremes, unless we happen to be interested in extremes. This is a very relevant kind of distinction to keep in mind when talking about prisons, because much of the thinking about prisons comes from people thinking about extreme situations in prisons, such as extreme uses of segregation, or extreme congestion and overstimulation.

Now, the difficulty is that when we are dealing with extremes, by and large we obliterate individual differences or group differences, except for people who are extremely good adapters or copers.

I am reminded, for instance, of an experiment, a real life experiment, we ran in my state in 1821. On Christmas Day, as a present to the troops, they put all of the extreme, the hard core offenders into indefinite solitary confinement, with certain kinds of additional rules, such as no talking, no lying down, and so on.

They had to suspend that experiment in 1823, because a disproportionate number of serious illnesses and fatalities in the New York prisons originated in that small group of confinees. They had several rather messy situations, such as people banging their heads into the wall, and jumping from tiers, and becoming schizophrenic.

It is inviting to deduce from this that one ought to sharply limit the use of solitary confinement, and it is obviously true, in the extreme. And yet, some recent research in Canada by a psychologist named Suedfeld has demonstrated that moderate uses of solitary confinement are found to be constructive and regenerative by some inmates. The same thing holds with crowding.

I think there is a separate case to be made for prisons which are so overcrowded that inmates are stacked six deep, as opposed to your average prison, with its average amount of crowding, in which Dr. Freedman's statements are much more apt.

Now, in my paper I certainly made a big pitch for what I call a transactional approach to stress and its research implications.

I think a good example of what that sort of approach means is Dr. Freedman's statement, "Any particular person may be happier in one type of place than in another. Some may thrive in cities, where others will wilt. Some will prefer the excitement of high-density city living, while others prefer the relative peace and quiet of the country." When you look at the prison situation, that kind of consideration certainly comes to the fore.

There are two major pieces of prison stress research that are currently underway. One of them is taking place in Texas and is funded by LEAA. The other is taking place in Massachusetts out of Yale and is funded by NIMH. Both have a kind of nontransactional flavor, but in both, if one looks very closely at the data, there is evidence of substantial group differences.

In the Yale study, for instance, the stress indicator, which is high blood pressure, seems to vary with all kinds of subjective feelings of depression and isolation and so on. You have your clinically depressed persons very much at the foreground of the experimental group, and yet the emphasis here is on crowding.

In the Texas studies they are determining that there may be threshold differences in tolerance for crowding, and they are looking for a paper and pencil instrument to find such differences.

Now, those would be examples of what I would consider to be necessary emphases in stress research. The one-to-one correlation between stresses and indicators invariably highlights substantial differences in reaction, including differences in which one man's stress is another man's desirable milieu.

This is true, for instance, when it comes to the level of stimulation, where we discover that some people find privacy extremely important, whereas others react adversely to isolation, find this understimulating and are happier in a more crowded setting.

I think Lazarus and his colleagues have most consistently stressed the need to combine the kind of epidemiological and macroscopic approaches that we have heard examples of here, with more clinical approaches in order to get a complete picture.

That always strikes me as the most important challenge in this field. That is, I don't quite know how one links macrodata with microdata. It takes a tremendous amount of skill, but I think it's necessary.

Now, the practical implication of the epidemiological approach is that you have to produce large-scale social change, which generally speaking is not under our jurisdiction, in order to ameliorate stress.

Hence, that kind of approach leads in some instances, to a pessimistic stance. We cannot affect unemployment rates. The President can't, in fact, as we've seen. But, with a more clinical approach, there is no reason, as Professor Suedfeld pointed out, why institutions cannot be designed so as to provide a range of stimulation levels with the environment being matched to the chronic or immediate needs of prisoners. I think the same point holds for other stress-related variables.

I would also like to point out that in my estimation one of the difficulties with a stress concern, and I have seen this in other problem areas like suicide where suicidologists have shown this tendency, is it makes stress the enemy. In suicide conferences invariably death is the enemy, and in stress conferences, stress is the enemy.

I am not sure stress is necessarily undesirable. In fact, I mentioned a couple of instances in my paper where stresslessness to some extent is a kind of stultifying experience. If one wants to promote change and one regards as beneficial situations where there is no stress or minimal stress, one discovers that one is supporting a variety of defense mechanisms which, in a sense, keep people from changing.

It is interesting that in one article on prison guards they deplored the fact that prison guards are in a situation in which they cannot do what they think is right and therefore they are stressed. Now, I shudder to think what would happen if some prison guards I know could do what they thought was right.

I think that the process of stressing, that is, the kind of intervening variables that Dr. Straus mentioned, the kind of coping processes or noncoping processes that Professor Lieberman alluded to, are necessary emphases of research. It is especially important to look at the processes of adaptation, as well as of nonadaptation, because they have practical implications in terms of building coping processes into noncopers.

Lastly, with regard to the present stress issue, I think it is important to keep the political context in mind. Now, I think the political context in the area of prisons involves, among other

things, two forces. One is the legal framework which is now being defined, having to do with the issue of what's an 8th Amendment violation, in terms of cruel and unusual punishment. This means, in part, what is more stressful than society is willing to tolerate?

That question, incidentally, goes way back, because the 8th Amendment, which was adopted in 1776, actually dates to the English Bill of Rights of 1688. So that type of concern is an old one, although it is beginning to become increasingly more rigidly, or carefully defined, in terms of looking for what is potentially stressful in confinement.

Prison administrators, therefore, have the legal system looking over their shoulders in terms of stress issues. Unfortunately, the legal system tends to want its questions answered generically. That creates another important gap between social scientists and legal researchers, which somehow has to be bridged, in terms of leading people to an understanding of the fact that those definitions that judges or standards-setting bodies have to make have to be more sharply pinpointed.

Now, the opposite political force has to do with what I call the "country club fallacy" in my paper, which involves those people who look to the prison system for indications that prisoners are made too comfortable.

The concern here is not unbiased. Whereas the 8th Amendment forces are spearheaded by environmental psychologists and prison moratorium advocates, so the "country club" forces are spearheaded by people who are at the right of the spectrum and who want our prisons to be as problem-free as possible and who want, basically, a warehouse in the prison setting.

I think the social scientist has to wend his way very carefully between these political forces and it adds to the burden.

Thank you.

DISCUSSION

REISS: Thank you for your presentation, and it is open for discussion.

BRENNER: The last set of comments had to do with the very important consideration of the set of political Bill of Rights issues that have to do with prisons, on the one hand, and the very interesting contrasts with the earlier discussion that, of course, we are unable to effect some of the more major social change phenomena, like changes in the economic situation, which affect crime.

From the standpoint of the Bill of Rights and the charge of the Constitution of the United States Government, there is a protection of citizens rights, which is the basis of a police force, which is the basis of the criminal justice system, which is the basis of much of the legal system of the country. It is the duty, of course, of the government to protect citizens.

Similarly, economic policy is made every day. It is the subject of much of the headlines every day in our newspapers. Clearly, we as a nation, do and can affect the garden variety of economic decisions. It is as much in our capability, very often more in our capability than affecting smaller units such as an individual prison system, largely under the constraints of a budget of an individual state.

TOCH: Let me just say that I don't think that there are any advocates, or very few advocates of unemployment around. The issue with most of the indicators we regard as undesirable, is that we seem to have a fair amount of difficulty dealing with them. So, it becomes a matter of how do we deal with the impacts of these situations, which translates the question to a more clinical question.

Now, generally speaking, I have been impressed all of my academic life with the difficulties, in practical terms, of dialogues between sociologically and socioeconomically-oriented approaches, and more psychological, clinical, or sociopsychologically-oriented approaches.

The kind of data that you presented points us to structural factors, whereas people like myself look at the impact of the immediate environment on the individual.

Although we preach a confluence between those kinds of orientations, in terms of actual theory and in terms of actual research approaches, those kinds of combinations seem to be very hard to achieve.

I think you ended your paper on a very optimistic note. In terms of wishful thinking, I am with you. In terms of anything that us feeble individuals can do, I kind of wonder how you go from the type of model that you have described to the type of concerns which us lesser mortals have?

BRENNER: Let me illustrate how this is normally done in medicine. Epidemiology, as we have heard mentioned many times in the conference, is the type of discipline which points to macroscopic approaches, for example, in occupational health and infectious diseases.

One of the best known cases is, of course, something like smallpox, where the massive public health view and approach is the one that actually deals with the problem. Now, let us say that there is a nation somewhere that has a limited budget, as all nations do, to deal with given problems, say smallpox. How should it spend its budget?

Should it increase the level of training of its specialists in smallpox? Should it increase its medical and nursing manpower? Is that the way it should spend its dollars? Or, should it increase its budget toward prevention, thereby very possibly eliminating, as the World Health Organization has virtually done, the source of the disease from the face of the world altogether?

Now, there is a dual approach, and a necessary dual approach; that smallpox exists when it is not prevented, is obvious. It is humane, and proper, and necessary to expend the budget on repairing the damage done by the illness, whatever the social or biological source of disturbance. This is correct and necessary.

Therefore, the two approaches do not conflict, but are necessary policy objectives of a given government and must be undertaken jointly.

WILKINS: My experience with time series has always been very exciting at the time, and 10 years later I have been very embarrassed with the results.

Let me just make one comment on the time series analysis. Is it possible that the productivity of those who remain employed increases when unemployent rates are increasing? If so, perhaps the measure that one has is that the police are not immune to that general increase in their productivity, when they see the unemployment rate rising. The suspicion arose in my mind when I noticed particularly the very close fit between the possession of housebreaking implements by night and that sort of thing (in Professor Brenner's data).

I know the British data very well, and I know this is a very good measure of police activity, but very poor as a measure of criminal activity.

BRENNER: Presumably that measure of police productivity would have also a great effect on the homicide rate of the country, involving vital statistics, too. I imagine we must assume a rather interesting view on the part of police against the public generally in the United States, Canada, and the United Kingdom.

FREEDMAN: I will change the topic, just slightly. I think Professor Toch mentioned that research is being done on prisons, and particularly on crowding in prisons. I think, as he mentioned, it is a perfect example of how research in this area, which is a very emotionally laden area, becomes politicized.

As he said, there are two groups of people who are doing research on the effects of crowding in prisons and are obviously determined to demonstrate that crowding is bad for prisoners. Most of them are environmental psychologists, which I am sorry about, since I guess I am one of those also. It is very hard in a brief time to describe just how political this research is and how it is misinterpreted, but I will give you one very brief example.

One of the studies that is most widely cited purports to show that crowding is bad in prisons, because under crowded conditions, presoners have more health complaints. In fact, what the study has done is compare prisoners in single cells with prisoners in dormitories, and say that the prisoners in dormitories are more crowded than the prisoners in the single cells, which, in fact, they are not in terms of square feet,

but, of course, they are in terms of number of other prisoners who are present, because in single cells they are alone.

At the end of a series of articles on that subject, the conclusion is, therefore, that crowding or high density, under these circumstances, is bad for prisoners. The problem is that this is quoted elsewhere by people who haven't read the article very carefully, but have read the abstract.

When someone else comes along and says, "Oh, but all they really did was look at the difference between dormitories and single cells," it is sort of lost in the verbiage by that time, and no one knows what's happening.

I think it is just one minor example of the difficulty that these psychologists have in studying kinds of issues that are so complicated and involve both emotions and politics. There is a political position on this; whether it is good to have high density or bad to have high density, whether prisoners are being mistreated or well-treated.

I am sure that in doing research on whether there are physiological correlates or genetic correlates of crime or intelligence, one also becomes terribly involved in politics. I am sure those of you who are in the area know much better than I that doing research on these kind of issues requires tremendous care in interpreting the results, almost as if you need a neutral person to interpret the results of everyone who has done the work.

REISS: I want to respond to this and to what Dr. Lion mentioned. How do you go about doing research on these questions and where might strategic opportunities lie?

For example, in the case of the use of Antabuse in crime, Werner Goldschmidt, a Danish anthropologist-lawyer has done a lot of work on it in Greenland. Most crimes are alcohol-related, and the typical punishment for it is coerced daily administration of Antabuse. It doesn't seem to have a very marked effect on recidivism.

It's worth looking, cross-culturally, at situations where there are made-to-order treatments.

Secondly, in health management, I have a post-doctoral student, Nancy Shaw, who has been studying health management in women's prisons, as contrasted with male prisons.

It's very interesting that in women's prisons they use drugs much more to manage the women inmates, even though everybody believes they are, quote, "less aggressive" than males.

She's looking at the variation in the use of tranquilizers and other drugs to control, to manage the inmate population. That appears highly sex related. It's a fascinating question as to how that came about, and whether there are any differences as a consequence of using that kind of health management.

The third strategic one mentioned is that there is a group of people to whom hormones have been administered systematically, albeit recklessly, and that's transexuals. At the present time there are a very large number of transexuals in this country. I have a post-doctoral student who is studying transexual surgery. It's nothing short of a major scandal in the medical profession, particularly in psychiatry and in surgery, as to how one can develop a set of practices that are administered wholesale and no one is certain of their consequences.

What I'm saying is thus: That there are made-to-order, strategic opportunities to look at some of these matters. We don't have to wait for the experiments to take place.

LION: I agree that there are some "natural," experimental conditions that would allow study. Again, politics enter, though.

For example, with hormones, I suspect that it would be possible to identify a subgroup of compulsive paraphiliacs who are at risk from the stress of their compulsiveness and to administer hormones, but the social policy issues, again, are overwhelming.

I guess the most open, direct way of dealing with this would be to have LEAA start a demonstration clinic.

The politics of such a clinic would really dictate that it be university-based, since the funding source would always be suspect. Literally, the funding would have to be channelled through the university, which is a polite way of saying "laundered," to avoid the stigma associated with "behavior control" in the criminal justice system.

It's very, very difficult, but certainly there is a population that exists that offers potential for research.

PETRICH: I wanted to raise a method question, as well, as long as we're on methods.

So many of our subjects that we study are individuals who have been in trouble because of their behavior. We look at the association of life events and the behavior, but what about those who weren't caught?

Dr. Straus mentioned his household survey. I think that's a very important methodologic point that we should look into a little more. For example, some of the data that Brenner presented with regard to "cases," in quotes, known to the police, but in some way not detained, and some of the screening work with hypertension, and ulcer disorders in air traffic controllers and other high-stress occupations. We think that some of our life-change data may correlate more with going to the doctor than it does necessarily with having some particular illness. I think that is a confounding problem we have in this whole line of research.

THOMPSON: To Dr. Lion, my ignorance is nearly total in the area of "minimal brain dysfunctions," but I am concerned. Do you believe that an adequate body of research, from samples of the general population, find correlations between violent behavior and various symptomatologies that you've described, in terms of prisoner studies? Or are we in a situation where we've been able to study a certain small population and we really have no way of locating those findings in the context of other populations?

LION: The latter. Adequate, no, desirable, yes. You'll never get an adequate knowledge of it.

TOCH: I just want to belatedly concur with two points about the deceptiveness of indicators of stress. It does seem to me that the problem with concentrating on undesirable behavior, as ipso facto evidence of stress, is almost as deceptive as concentrating on stressors with the assumption that they would automatically stress.

It seems to me that the whole issue having to do with crime denoting stress, and of using volunteering for medical attention to denote stress, simply points to the need to look at those behaviors more carefully to see whether indeed that assumption is met. My hunch is that assumption very frequently is not met.

FREEDMAN: One of the issues that seems to have been mentioned in passing, but never gotten hold of very much, is just what we mean by "stress." Psychologists, physiologists, and everyone else, has been talking about this for years, and years, and it's clear that we don't have an adequate, simple definition.

It seems clear to me that "arousal" is not "stress"; "conflict" is not necessarily "stressful"; and as Professor Toch said, the ideal state is not lying in a warm bed of water with virtually no stimulation. Exactly what the ideal state is, we don't know, but it is certainly not a situation with no stimulation. This is a conceptual issue of tremendous importance when we talk about the relationship between stress and crime, or stress and anything else.

Because if we make "stress" a very, very broad concept where it includes practically everything, then it becomes almost meaningless. If we narrow it down to the few situations that we are pretty sure are stressful, like extreme pain, being caught in a rush hour traffic, and a few others, it's not clear that we are left with very much.

We think of unemployment as being stressful. It's quite possible that for some people it's a delightful state. Some people choose not to work. Some people would like to work, but don't. We often have disappointment in our lives. Is disappointment stressful?

I think it's very, very tricky, particularly when we're trying to look at the relationship between stress and crime. In some of the life indicator studies it looked as if, except for having some sort of police incidents, those people committing crime have fewer stressful incidents in their lives. What does that mean? If the only stressful incidents that they have are contacts with the police, you are not really saying anything. In fact, you are saying the opposite of the original notion which is that people who commit crimes are the ones who have less happening to them, not more happening to them. I think it gets terribly complicated, and somehow someone has to deal with it at some point.

LALLEY: John Lion's fine and provocative paper did hit, I think, on two policy issues which, as the only representative of HEW, I do need to address very briefly here.

One is that the HEW regulations do not prevent clinical research or research on prisoners entirely. Rather, they do limit it to that research which can be of benefit to prisoners in their

immediate condition. So we have here a question of competing values. One is what the researcher would like to know and the other is the policy notion that the prison situation is inherently so coercive that voluntary consent by prisoners is suspect, with the result that research ought not to be supported which simply uses prisoners as captive subjects.

The other question is allowing probationers to participate in research as a condition of probation. I read the word "allow" as having more of a connotation of "requiring" them to participate in research as a condition of probation, or of holding out the enticement of probation if one participates in such research.

We are thus dealing with some very controversial and sensitive policy issues. I think the value of this particular part of the discourse is that, to the extent that the National Institute will be getting into more and more of the stress area, it will be getting into some thorny human subjects problems the moment it moves away from aggregate data such as Harvey Brenner has been using.

Consideration may therefore need to be given as to whether the Department of Justice wishes to consult the guidelines which HEW has developed in the human subjects area.

Secondly, the National Institute of Law Enforcement and Criminal Justice may need to create institutional procedures to deal with human subjects issues similar to those which exist in my own Institute and Department.

For example, when we receive applications that involve research on human subjects, we require that a university or other institutional board review the adequacy of the plan for protection of human subjects, but we do not take that as sufficient that all these issues have been satisfactorily addressed.

Unfortunately, university and other institutional boards sometimes give a rather cursory review. So that in addition to the university's own responsibilities with respect to human subjects issues I submit that the federal agency also has a very important responsibility which is inherently a staff responsibility.

REISS: What you are saying, Tom, is that the bottom line is that HEW feels that it can reject any proposal on human-subject grounds.

LALLEY: That's a good point.

REISS: That's an interesting bottom line and one which you'll never forget as an agency. It has surfaced most recently and has become very controversial.

LION: I run our Institutional Review Board at the University of Maryland. While I make the direct policy recommendation that the LEAA or the National Institute establish its own bioethics board, I take immediate issue with the power such a board would have to regulate my own research. It is a troublesome area.

LALLEY: We do not regulate research which you may be doing on your own or with other sources of funding. We do regulate our own funding.

LION: And that regulates my research.

REISS: It is interesting to me that the government is willing to assume the role of regulator but none of the liabilities that go with it. That is to say, if it approves a piece of research and there are damaging consequences, it still wants the university and the investigator to assume the liabilities.

My point is: That increasingly we don't recognize the right of government to exempt itself from liabilities. So you can't have it both ways. You can't both control us and force us to take all of the liabilities. You understand the legal point?

LALLEY: I understand the point. I'd like to have an example, sometime, of what problems have been caused.

REISS: I would be glad to supply them.

BRENNER: Just a quick response to the question with regard to some of the data we are presenting on the life stress changes with respect to different samples of people's criminal behavior.

There was one outstanding area on that last chart, or the next-to-the-last chart (of Dr. Petrich), of a particular category of stress, life change, that increased coincidentally,

perhaps not as dramatically as the involvement with the law. And it's "financial." Is that correct?

PETRICH: That was one of the ones that changed, but not quite as dramatically. It's economic, yes.

LIEBERMAN: I think we have not defined "stress," and we have been using "stress" here to mean "life change units," by and large. What strikes me about the data that's been presented so far today is that we've talked to the point that it is not these kinds of stresses that result in the kind of behavior that we're interested in.

As I understand your data, in terms of the two empirical studies presented this morning with some visible data, suggested that it is not the stress, per se, but down the line somewhere what happens with the stress that makes a difference.

I feel that the whole notion of relating life-change units to any kind of behavior, whether it be criminal behavior, illness behavior, is a relatively bankrupt kind of research strategy.

I think we've seen enough examples in the literature in various ways that this is not the place to go. It does not really deal with the issue in which I think we're interested. There are modest correlations of about .2 in the literature between life-change units and illness behavior, psychiatric illness particularly, which accounts for very little of the variance and are explainable by many, many other factors, perhaps including the instrument itself.

We notice, when we saw the criminal population presented, that these are people not in various role areas and therefore are going to have less life-change units in many, many areas than a normative population.

For example, it's been shown over and over again that there is a 25 percent drop in life stress over the life span. Does that mean, in a sense, that stress goes down? No, I think it simply means that people aren't occupying certain roles. So I am making a pitch; this is not the area of research, at least with that strategy, that we should be going.

REISS: You've got to have a work history to have stress.

LIEBERMAN: You've got to be working to have "work stress" on the instrument.

REISS: It accounts for some of the difference between prisoners and delinquents. Murray, (Strauss) since you offered a definition of "stress" in your paper, if not fully operationalized, do you want to enter this discussion at all?

STRAUS: Research on stress illustrates the all-too-familiar distinction between one's intellectual commitments and one's operations. In the paper I defined "stress" as existing when the subjectively experienced demands of a situation disagree with a person's response capabilities. This can be in either direction, many demands or too few. So that's the way I see it, and want to measure it in the future. I believe Dr. Lieberman has measured it somewhat along those lines. However, I did not.

The operations I actually performed dealt only with "stressor stimuli," on the assumption that, on the average, those things did represent a situation where the response capabilities were different than the subjectively perceived demands. I think that, on the average, it is true. The trouble with "on the average" is that's where the large amount of unexplained variance typical of the social sciences comes in.

I would like to add a more general consideration of variance explained. I grew up in the scientific tradition that distrusts a correlation larger than .3--that wonders if there might not be tautology, or some measurement artifact at work whenever there are large bivariate correlations. This distrust is based on the fact that we do not subscribe to single causal factor theories. Therefore a correlation of .30 is about all the variance one can expect to explain with any <u>single</u> factor. Consequently, when Dr. Brenner reports single factors with coefficients of .7, and ends up with multiple R's of .98, most of us are either envious or distrustful.

My view is that his figures are unintentionally misleading to researchers like myself who deal with <u>individual level</u> correlations because all of the individual to individual error variance is suppressed. For example, in Figure 7 (p. 56) of my paper, the correlation between the polynomial regression

line and the observed means for wives is .92. That's taking aggregate-level data. Seemingly this explains about all the variance in the assault rates for wives. On the other hand, if the correlation is based on the 2143 individual cases, then it's only .21.

I want to emphasize that the point is not that one method is right and one is wrong. It has to do with what you are explaining. These are two aspects of the truth. We have to understand both aspects. We have to understand what the general trend is (the aggregate level correlation). We also have to understand that that general trend leaves an awful lot of individual variance unexplained.

WILKINS: This indicates that we should perhaps be moving away from the idea of "significance," and toward the idea of "estimation" and a "decision base" for our analysis, rather than mere correlational significance, which doesn't really get us very far in terms of any practical decision theoretic approach. I guess this is not the right forum to debate econometric and other kinds of analysis. Maybe sometime it would be useful for those of us who would like to fight on those issues to do so.

INTRODUCTION

REISS: Our next paper is by Professor Staples of the University of California at San Francisco. He has previously been on the faculty of Fisk and Howard Universities, and received his doctorate from the University of Minnesota. He has published widely on topics of the family, particularly, on black sociology and black family structure. His books in that area include, The Black Woman in America, Introduction to Black Sociology, and The World of Black Singles.

His specialization in family sociology is the black family, and today he is going to present a paper on "Race, Stress, and Family Violence." Dr. Staples.

RACE, STRESS AND FAMILY VIOLENCE

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Thank you.

In speaking on the topic of race, stress and family violence, while I don't want to be drawn into the argument over what is stress, particularly since I'm not sure how crucial a variable it is in explaining much of the criminality in this country, I do think that in the area of family violence, it probably is more significant than in other types of criminal activity.

If I have come up with any kind of definition, I would say that it's the exposure to negative social stimuli, which then lead to internal states of tension within an individual, which then can become the precipitating force for violent behavior.

I think the relationship between racial membership, stress and violence, particularly within the family, would seem to be fairly clear. I am struck by the fact that while I've some data on family violence, I have very little on stress. Theoretically, I can work that out and will try to do so in order to give you some idea of the relationship between stress, race, and family violence.

First of all, just let me start by saying that as in other areas of socially defined criminal activity, blacks are very, very overrepresented in the official statistics related to family violence. My former professor, Murray Straus, has been working on more recent data in the area of family violence, which, unfortunately, was not available to me at the time that I wrote this paper. We find that blacks constitute almost a majority of those commiting acts of violence. At least at one time, I guess the plurality of violent crimes consisted of violence between intimates, friends, relatives, spouses and lovers.

I'd like to first try to work out, theoretically, the relationship between being black in what is still generally a racially stratified society and the stress forces that come into play for those individuals who are defined as subordinate members of the racial strata and its relationship to family violence.

We have no real data or studies on how stress comes into play in racial relations. I think it's particularly important to realize that we're in a state of flux, where race relations are much more

fluid now as compared to the past, that there would be an increase in stress factors. By stress factors here, I mean the problems that result from being a member of a racially devalued group, as compared to being a member of a racially prestigious group.

The black individual has been living in this society, particularly in the last ten years, where they've gone from being almost an institutionally defined inferior group, to being one that is supposedly now on an equal status with members of the white majority. The fact is that you have some discrepancy between the changes in that status and actual practices and roles and values that continue to exist. The black individual is probably being forced to cope in a much different way with his racial status than he was in the past.

To try and operationalize this in some way, I recall the writings of James Baldwin in the early '60s, when he talked about blacks being in constant danger of being locked behind the gates of paranoia. By this we mean that they were never very clear in terms of the actions of whites toward them, which ones were racially motivated, which ones were expressions of a continued attitude toward them as inferior, or which ones were more or less just the normal gestures and actions that people normally experience in this society.

Psychologically, this creates a great deal of mental imbalance and to a certain extent, stress. Much of this will probably be expressed via some kind of act of violence. It's fairly obvious, too, that those acts of violence are not equally distributed throughout the black community. We might expect stress in this period of change in race relations, in our fluid society. To the extent that the Black's expectations for treatment are based on this change in status from an inferior to an equal, and to the extent that he encounters gestures and actions that one might interpret as treating you as less than equal, and to the extent that one is bothered by this and it creates certain internal states of tension, this will bring about stress. The stress will probably have to be either withheld, drawn within, or manifested in some form.

This psychological stress is compounded by other forces which tend to devalue the black's self-esteem, how you are regarded as a person, both subjectively and objectively by signficant others. Then we might expect to find even more expressions of violent activity.

As an example, there is a fairly high rate of unemployment in the black community. According to unofficial, that is nongovernmentally sponsored surveys of unemployment among black males,

approximately one out of four is unemployed at some time during a given year.

So you've got a fairly large number of blacks, particularly males, who would be subject to stress factors related to being members of a racially devalued group and having that status compounded by being unemployed and the lack of self-esteem, both objectively and subjectively, that results therefrom.

In terms of family violence, the first thing I wanted to dismiss as a cause are genetic factors, which would indicate or imply that there is anything inherent in the black group that predisposes them toward violence. On the basis of the cross-cultural data from African societies we find, for instance, that their rates of violence are considerably lower than Afro-Americans, as well as for the general white population of this country.

That raises the question as to why the fairly significant difference in black and white violence rates. You find blacks in certain categories of violence comprising almost 60 to 70 percent of those who are arrested and convicted. Not that those official figures reflect, necessarily, the real incidence of violence, but it does reflect that fact that it's much more likely to be brought to the attention of the authorities. The cross-cultural evidence indicates that it is nothing that is particularly unique to blacks as a group.

Recently looking at the data on crime, it seems predominantly a lower class phenomenon in the sense that very few middle class blacks are involved officially in criminal activity in the sense of being arrested or convicted.

If we had fairly reliable data on criminal activity broken down by class and race, we might find it actually lower for middle class blacks in comparison to middle class whites.

In having dismissed the genetic argument as a basis for understanding black violence, we have to look at what are some of the reasons behind the disproportionate number of blacks arrested and convicted for violent behavior.

One of the most important ones, obviously, is the centrality of violence to blacks in America during the 300 years of slavery. The basic anchor of the slave system was the fact that whites controlled the organized means of violence, and ultimately that was the reason that people accepted slavery more than anything else.

The other is that black culture is probably much more physical than white culture, and this, of course, has both its positive and its negative aspects.

In the positive sense it probably accounts for the much larger portion of blacks who perform in star roles on athletic teams; they dominate boxing as a sport, and so forth.

On the other hand, violence is much more likely to be used as a form of conflict resolution in a culture which is so physically oriented.

In a sense, blacks are socialized into violence probably much more so than whites. Again, this is particularly true of lower class communities and part of it is simply a matter of violent behavior being part of survival skills. That is, within certain lower class black communities, the only way a child grows up to manhood is through being able to defend himself agains attacks from his neighbors and peers.

Also, within that same community, there is a status conferral system which elevates the person who is capable of being a good fighter, of exercising violence with a certain amount of skill, as a fairly positive figure in the black community.

I looked at specific aspects of family violence, taking some liberties with the term. Including sexual aggression, again we find that blacks are overrepresented in the figures on rape, although the underreporting of actual cases of rape makes me extremely reluctant to make any sort of definitive conclusion about that phenomenon.

I would imagine that less than 10 percent of the actual rape cases are brought to our attention. Despite the fact that rape is perceived as an interracial phenomenon in this country, there are indications, both in terms of informal reports as well as offical studies, that the majority of rapes that occur are intraracial: that is, both the victim and the aggressor are members of the same racial group.

The relationship between stress and sexual aggression is not as clear as it is in marital assault or child abuse. Probably the relationship I see best coming out of this particular pattern are those rapes that are a result of what one writer called misfired attempts at seduction. Within the ordinary dynamics of the dating game, where partners are negotiating over whether sexual relations would take place or not, the dynamics could create some sort of

frustration in the male, particularly in these days of sexual liberation.

This then crosses that thin borderline between forceful seduction and rape, which is what caused the feminist Germaine Greer to label seduction as rape. That's the primary relationship I see between stress and sexual aggression. I think the case is somewhat clearer in terms of marital conflict. I go back to what I said in the beginning about the stress factors that come out of being a member of a racially devalued group and that sort of stress being the precipitating factor toward violence. The person who is most likely to be exposed to that would be an intimate or friend, who, first of all, would be a more accessible target, and secondly, would be a safer target than attacking a stranger.

For the black male who encounters a great deal of frustration in terms of the negative social stimuli that come from his status as a racially devalued person, and the kinds of experiences he would have, particularly in an interracial setting, this then could have its expression in terms of the domestic violence that occurs.

In terms of child abuse, you have a similar set of forces operating with regard to stress resulting from being a member of the devalued racial group, as well as other stress factors such as the number of children (the fact that probably twice as many black families have four or more children than in white families), and the kind of environmental problems which this creates. As a result, we find that child abuse, for instance, is much higher, at least the officially reported child abuse, among the black community than the white community.

REISS: Thank you. We will take a 15-minute coffee break.

(Brief recess.)

INTRODUCTION

REISS: We are beginning our last of the presentations. We have two papers. The first will be presented by Leonard Hippchen, and the second by Jonathan Freedman. The first on "Biochemistry of Stress Reactions in Crime," the second on "Crowding, Stress and Crime."

Professor Hippchen is currently on the faculty of Virginia Commonwealth University in the Department of Administration of Justice and Public Safety. He has done considerable work in the area of delinquency and criminology. Most recently he has edited two books, one a Handbook on Correctional Classification; Programming for Treatment and Reintegration, and a second on Ecologic-Biochemical Approaches to Treatment of Delinquents and Criminals.

Dr. Hippchen is in the process of developing two research programs dealing with biochemistry set in a correctional facility and in the forensic unit of a hospital. He will now try to thrust us in the direction of the biochemistry of stress reactions and crime.

THE BIOCHEMISTRY OF STRESS REACTIONS AND CRIME

Leonard J. Hippchen
Virginia Commonwealth University
Richmond, Virginia

Thank you, Mr. Chairman.

First, let me say that I think that the concept of stress is very useful to help us look at crime from a behavioral viewpoint, because crime as a social definition is too limited a concept to do justice to this phenomena.

I think stress is important in that we recognize it as not only inevitable, but possibly as a necessary part of human existence. Stress in relationship to human development may be something that we should look at much more carefully than merely as an interference with human development or proper social functioning. I tend to think of stress as a positive feature in human life, because I know of no great accomplishments among humans who have not only faced stress but have overcome stress. I advocate a very positive look at stress and what it can offer to help us understand human development.

Likewise, we can look at many forms of failure to adapt, or failure to use stress constructively, as it relates to human development or social functioning. We can look at crime or delinquency, school failure, child abuse, many other kinds of problems that we are facing today from the viewpoint of poor adaptation or a low level of adaptation, certainly not one conducive to a higher level social functioning, or even a higher level of individual functioning.

If we assume that the highest levels of individual functioning would also correlate with higher levels of social functioning, we get away from this problem where many people think that crime is an adaptation to stress, one way of coping with stress. Stress can be a desirable factor in criminal behavior. I've talked to many criminals in many prisons and delinquency areas who felt that their behavior was very good for them personally, even though it may not have been socially useful. Personally, it was the best way they could see of coping with a very frustrating situation. Differentiating between the value of stress from a social viewpoint and from a personal viewpoint is very important.

We have spent a great deal of time talking about the social/cultural factors of stress, and these are undeniable. I think we have overlooked, to a large extent, however, the tremendous volume of research in biochemistry and related fields; neurology and those

fields that deal with the neurological functioning of the brain and the nervous system, such as endocrinology. There are thousands of studies that give us an indication of how individuals react to stress from a biochemical or neurological viewpoint. It's what I term "internal stressors" that I am putting the emphasis on in this paper, not at all discounting the social and environmental factors.

In fact, one of the major social or environmental factors that is emphasized here has to do with pollution and various forms of contamination that we find in the environment; the water, the air, the soil, the pollutants that are put into our food, the poisons that are put in as preservatives. All these are environmental pollutants. These external toxins impact upon the functioning of the mind and the other parts of the body, the endocrine system, in particular, to disturb homeostasis. I think we have overlooked their impact on how an individual may react to various stresses.

A very important area is alcohol and drug abuse. Whether these are looked upon as a way that an individual tries to adapt to stress or whether we look at alcohol and drug abuse in terms of their crippling of more effective or constructive behavior, certainly looking at alcohol and drug abuse anew in terms of environmental or internal biochemistry is very important.

This would be a public-policy issue. I think it would be very vital that we take a look at drug and alcohol abuse as it seems to relate to vitamin and mineral deficiencies, and particularly food sensitivities and addiction to refined sugar.

The idea is that the addictions originate from a basic addiction in early childhood, if not infancy, to refined sugar, which tends to set up cravings which then may later be satisfied by the use of alcohol, drugs, smoking, and so on. Cravings in themselves are serious stresses.

We should look at the internal factors relating to stress. We talked earlier about rape, sex offenses and drives. These people are driven by hormonal deficiencies or possibly other nutritional deficiencies or dependencies, some of which may be genetic, some which may be related to early childhood feeding practices.

Certainly, we know that the nutritional status of people worldwide, and in the United States as well, regardless of socioeconomic groups, whether you're employed or not employed, whether you come from a broken family or not; the nutritional status of the typical child in the neighborhood school is at a very low point.

The problem is stress as it relates to school failure, internal stress as it relates to sensory impairments which make it difficult to function in a school setting. This failure to cope with school or the failure to cope in the family may lead to child abuse. A lot of violent behavior may be related to food toxicities, food allergies, nutritional deficiencies.

We haven't looked at these areas. I feel that it's very important for us to begin to consider which of the number of internal stress items that I have listed in my paper could well be important contributing factors to failure in the family, failure in school, failure in the work world, failure in many areas which may relate to crime or various forms of either antisocial behavior or maldevelopment. I like the idea of stress as a concept because we don't have to deal just with crime. We can look at those factors that may interfere with the individual constructively handling stress.

Now, as far as additional public policy is concerned, I would recommend that research be aimed at studying nutritional deficiencies, cerebral allergies, hypoglycemia, food allergies and addiction, brain lesions, etc., and how this relates to human functioning; how they interfere with human development; and in which ways they correlate with criminal behavior as opposed to other forms of behavior.

Most of the studies that have been conducted up to this point indicate that there is a much higher element of these types of deficiencies and disorders among the criminal and delinquent population or school dropouts, than among successfully functioning humans. I think it's a very promising area for future research.

Thank you.

INTRODUCTION

REISS: Our last speaker today is Dr. Freedman, Professor of Psychology at Columbia University, Ph.D. in psychology from Yale University. He also taught at Stanford University. He is interested in a wide variety of areas in psychology such as memory functions, compliance, crowding and population density, as well as environmental psychology.

His most recent books are entitled: Crowding and Behavior, Happy People, and Introductory Psychology. He also is coauthor of a book called Deviancy: The Psychology of Being Different.

CROWDING, STRESS AND CRIME

Jonathan L. Freedman
Columbia University
New York, New York

Thank you.

I will assume that my paper is available to everyone. I will just repeat in about one sentence the gist of it, which is that if you look at the research that has been done over the past 10 years or so on the effects of crowding on people, what we find is that high-density living or being in a high-density setting is not necessarily harmful to people. In fact, it is sometimes beneficial, sometimes harmful, and often it's neutral.

Perhaps more important the particular effect it has depends on other factors in the situation. Almost all of our effects are interaction effects between level of density and something else. We are very far from being able to specify these "something else's," but almost every study that gets any effects at all gets this pattern.

In the paper, I suggest one mechanism or effect of high density, which is that it intensifies people's responses to the other people that are present, making a response that is positive under low density more positive under high density; a negative response under low density more negative; and whatever the response would be to the other people becoming stronger under high density.

I don't think that is, by any means, a full account of the effect of density on other people, but I do think it's one of the things that happens. Intuitively, those of us who live in cities, I think, are aware that the high density of people in the city makes their presence more intense and our responses to them and to the social situation more intense than living in the country where the density is lower. That is not necessarily good or bad, but I think it is a fact.

So much for what the paper had to say in terms of the effects of crowding.

Being in the enviable position of being last, I would like to talk mostly about what I see as one line of research on which we might focus. Let me just say that I am not a criminologist, and have never really studied it except insofar as it related to crowding

research and to the deviancy research, so I am a naive outsider, to some extent.

It seems to me, though, that when you study criminology, you study it for at least two reasons. One of them is to understand human behavior, in general, and personality development and what influences behavior, using criminal behavior as just one example. That is the goal of some of the research that's been discussed here, or at least one of the goals.

Another goal of studying criminology is much more applied, and that is to try to reduce crime in the immediate future. That second goal is not contradictory to the first goal, but it's quite a different goal. I think if you are focusing on the second goal, it seems to me that in many cases you employ quite a different strategy.

Assuming for the moment that LEAA is interested primarily in the second goal, it seems to me that research which tries to explain why people become criminals, while not hopeless, is unlikely to produce anything that we will be able to implement in terms of reducing crime.

The tendency to become a criminal, as has been stated here many, many times, although not exactly in these words, is due largely to factors such as: economic considerations; racial discrimination; a sense of hopelessness (no one has mentioned that, but it probably is true); cultural values in the society; being or not being a member of some criminal subculture in this society; family discipline; the way your parents acted toward each other and toward yourself; and perhaps some specific kinds of stresses that occur in your life.

I'll add to that biochemical and genetic factors. I don't know to what extent they play a role, but certainly, there is some possibility.

Those are all probably important factors in producing a tendency to become a criminal. I think it is very hard to find additional ones that would explain substantial amounts of the variance. I believe that there are subtle factors that will explain the additional variance, but I also believe that it will be very, very hard to find them.

Even if we find and spell out all these factors very clearly, it will be very difficult to do much about them in terms of action in our society. We live in a democratic society. There are tremendous pressures against doing such things as telling parents how to

raise their kids, telling people not to experience stress, eliminating poverty and racial discrimination. Of course, we're all for that, but we certainly don't have to sit around here and tell other people that it's important to eliminate poverty and racial discrimination.

I personally get a sense of hopelessness in terms of these kinds of issues. Yes, if we could reduce poverty or racial discrimination or make parents raise their kids better, we would probably have a tremendous impact on the amount of crime. But, we're not going to do that kind of thing, and I don't think research is going to help us very much. Those are major societal changes that are desirable but are hard to implement.

Having said that, I think that we can focus on other kinds of factors, and ask quite a different question: What are the factors in a situation which are likely to encourage or discourage criminal behavior? We are now dealing with a population that contains some people who are likely to commit crimes and others who are likely not to commit crimes. However, these people exist in some real world. What can we do in that real world that will lessen the likelihood of crime being committed?

I think much of this involves environmental factors, or at least this is where environmental factors play an important role, and environmental factors can be changed. Now, you can't change the climate, which is an environmental factor, but you can change buildings.

If I were doing environmental research or suggesting research to be done, I'd ask questions such as: What kind of housing encourages or discourages criminal behavior, and for what kind of people? It may be different for different kinds of people. I think that's a very complicated question. We don't have the faintest idea about it, but it's answerable. It's not that difficult. It takes some money and some time, but we can answer that. At least we can answer it for our society; the answers might be different for another society. If you found out that particular kinds of housing structures encourage crime for particular kinds of people, you could try not to have those kinds of people live in those kinds of housing structures. I don't think that's so difficult.

I would probably be more interested, although I think it's more difficult to do, in what can we do to streets to make them safer.

I know these are simplistic questions, but I am not sure that anyone has ever really tried to answer them, except to say put up some more lights. Yes, that probably helps, but we don't know anything about what streets are dangerous and what streets are safe.

It would be a simple although an incredibly tedious matter, to collect detailed statistics on where on the streets crimes occur, and for schools and for houses and for other kinds of environments such as for department stores, and so on. I think those are the kinds of questions we can answer.

I would also say, and this is a separate issue, if I were going to do research on crime, I would focus on those crimes that people in the cities are most concerned about, and those, it seems to me, are mugging, armed burglary, and to some extent, rape. The crimes that change our way of living, that change our attitude toward our everyday existence. Wife-beating and husband-beating and child-beating and murder in the family are very serious social problems. But it does not change your day-to-day life unless you happen to be in one of those families; and it's not what is making people move out of the cities or worry about what's going on in our social structure.

I think we can focus on those kinds of crimes, and they also are just the kind of crimes that are most likely to be affected by the environmental factors I just mentioned. I doubt that homicide is affected by it very much. I do think that mugging is affected by it.

I would be interested in studying crimes that have been virtually ignored because they're not serious enough, such as vandalism and shoplifting. I once did a study on going through red lights in New York City. Those are very minor crimes, but in some way they affect day-to-day living even more than mugging. There's absolutely no evidence for this, but I believe that the existence of a high rate of <u>those</u> kinds of crimes produces an atmosphere of lawlessness, I hate to use that word, in society, which encourages the other kinds of crimes.

All I am saying is that for my money I would put it on looking at factors that affect the likelihood of crime being committed in a particular setting, and, to the extent that we can, describe those factors. I don't think it's going to be simple answers. I think it will be different for different kinds of people, perhaps different for different cultural settings and then maybe we can do something about it.

DISCUSSION

REISS: We now come to the point of returning to the discussion, and maybe with Lord Russell we may be reminded when he said if he had one word to take to a desert island, it would be the word "but." And I suspect we've come to the "but" part, and the caveats and the reservations we will place on things that we have said up to this point.

I want to offer an observation in response to something Dr. Freedman said about the trickiness of researching environment and crime. I did a paper over a year ago in relation to money LEAA gave to Westinghouse for which they were supposed to have done a series of papers on the environment and crime. One of the things that I found in doing that review apropos of lighting and crime, was that everybody seemed to find a consistent result, which is of the following sort: that 25 percent of the crimes occurred in front of lighted buildings, and 75 percent the rest of the way around which were not lighted, which showed that lighting was important.

I did a simple calculation. I said a building has four sides, the expected rate would be roughly 25 percent per side. And that seemed to me perfectly reasonable that it was a chance occurrence.

I still think these issues are tricky. We can fall into those traps so easily. I don't mean to say "but" and discourage it, but it is a very tricky area.

EWING: Let me say, also, the National Institute has over the last half-dozen years or so sponsored a very substantial amount of research on just the kinds of questions that you have raised; that is, where does crime occur? what do you do in the streets? how do you locate what kinds of crimes occur in what circumstances?

I wouldn't say that we've answered all the questions that you've raised, but there has been a very large body of that research.

You list Oscar Newman's book in your list of citations, and we're involved in sponsoring some of his work, and we're still involved in the sponsorship of some of his work. They've gone

most recently to an approach which goes well beyond what Westinghouse has been doing, into asking questions about the relationship between neighborhoods and their characteristics both in terms of block as well as neighborhood characteristics, and the kind of crimes that occur and the kind of responses that occur in those neighborhoods. I say that not in order to suggest that we shouldn't follow your advice, but rather to suggest that we have been indeed pursuing that.

I think it is probably the case that one of the steps that ought to be taken is to attempt to synthesize that body of knowledge in some fashion, and that, too is something we want to pursue.

HIPPCHEN: Let me say there are numerous studies that indicate that when you improve the nutritional environment of a mother and father and child, you can reduce the incidence of violence and disorder in that family. When you improve the nutritional environment, the internal environment, of a child in school who is having learning difficulties, you have a high incidence of corrective behavior in a school system. In dealing with delinquents, when you improve the nutritional environment, the person's behavioral functioning is improved considerably. It's been demonstrated that recidivism rates also can be reduced.

There are very practical public-policy aspects to nutrition and it relates to helping individuals to improve their coping skills and constructively handle stress. The whole concept of supernutrition has been well demonstrated to improve the person's ability to handle both physical and psychological stress.

REISS: One of the problems with saying that you can improve functioning which then has some effect on crime is that clearly, anything that improves the condition of everyone has something to do with everyone. We did a little exercise to estimate how much of the nonmilitary federal budget could be related to delinquency and crime. You can drive that estimate up to about 60 percent of the nonmilitary federal budget by the time you look at everything that has to do with school health and on and on and on. I hope that we can focus ourselves a little more than that for this stress-crime discussion.

That's just to say that if our discussion can zero in on a level where we're talking with greater specificity.

LION: I think it's necessary to distinguish between research and social policy. An example is gun control. You can research it

to death, the issue ultimately lies in social policy; whether or not to control guns, and to affect crime that way.

To this extent, I think LEAA is under constant stress as to what to do with all the criminals. And I bring up the issue of the social policy of triage, which is a viable policy in medicine. The medical profession deals all the time with the concept of "triage." It should do more of it. It relegates some patients as "hopeless," sometimes lets them die, and it sees some as treatable. I would like to see a criminal justice system grapple with this concept. Who is amenable to something and who is not?

OPLER: When I was listening to Dr. Freedman's remarks about the tremendous extent of the problem, and his feeling that you couldn't enter into it, I recall that while we were doing the Midtown Study, we were doing research on the neglect of cases or on nonintervention. In our little community of Yorkville, very close to the New York Hospital, there was a grassroots group of women who were concerned about the rising rates of crime and delinquency in their little community. They began to have meetings. They were concerned about bicycle thefts at first and all sorts of seemingly minor things. They had heard about us doing some kind of a community health study, and they indicated they wanted to do something about their problems. I was very impressed by them, and very interested in them, because we were soon busy doing community ethnic group studies albeit in an informal manner.

Now one of the things I want to say about a conference of this sort is that it should indicate a great many things useful for public policy. I don't mean things that have already been on the books, or have been past unworkable practices.

There is a great deal of backlogged research on sociopathic behavior, on criminality, on stress which I know is not made use of, and I don't think it should be made use of merely in conferences. Having been through a highly successful multi-million dollar study in New York and feeling that a great deal was uncovered about stress, I don't see why agencies of this sort can't begin to reutilize a lot of existing material screened by experts who had something to do with producing this original material.

There are a number of other things I have heard in the conference. For instance, I was interestd in sugar intake.

Well, in Puerto Rico, the poor kids in the slum areas are given sugar cane to chew on.

HIPPCHEN: It's not refined.

OPLER: It isn't refined; that's true, but it peaks an interest in refined sugar.

HIPPCHEN: That's the difference.

OPLER: They also have in this community the curious practice of getting the young men steeled to be full to the hilt of machismo, and so on, and to be hardened and ready for the turbulence of life, by beating them with sticks. The game is to see who can stand it longest. In that community they were doing some things to try to get these kids "toughened up." They used the same kind of broom sticks that the Puerto Rican kids in New York use when they played their street games of baseball or stickball. I think it's had a lot to do with development of excellence in baseball among Puerto Ricans, perhaps, in this instance.

In other words, I don't go for a kind of nihilistic attitude that you can only merely continue to do piecemeal research without social and cultural studies.

I think there's a lot of backlogged research that goes beyond, let's say, the matters that Jonathan Freedman is interested in. I like his paper, and some of his more optimistic remarks very much. But I'm saying that it isn't a matter of lighting the area of this or that building. It isn't a matter of deciding that we're going to intervene in one tiny thing, but that we're not going to put our money on something else perhaps more relevant to the actual lives of real people.

There was a gentleman present at the morning conference who said: What does this add up to in public policy? He said it, after Harvey Brenner had given his report indicating the close correlation with what we in the Midtown Study called "SES," the economic variable, and crime or delinquency. Harvey did it with unemployment. I think if you meat-axe programs like CETA, we're going to pay for it in things related to prisons, and higher crime rates, and so on.

I think an agency like the Law Enforcement Assistance Administration has a lot to contend with, a lot to confront, and to be concerned about. I think the public is equally concerned.

I think it's up to social scientists and behavioral scientists to make a contribution, albeit modest, by helping them with their large-scale problem.

I have a student who now is studying no less a matter than campus security. We have three campuses in Buffalo in our particular segment of the state university. I have another student on a university fellowship who is studying people that we have moved out of the hospitals to the community as people are ejected from psychiatric wards and put back in communities and on the streets. That, by the way, has at times produced a problem because it's put pressure on what used to be called "state hospital institutions" and now are called, euphemistically "Psychiatric Centers," in New York State. Many people are turned out who are getting insufficient care, who are scrounging around to exist. But they make an impact on us other guys when one kills, let's say, five college students living near the hospital. They are discharged against advice of course, against medical advice.

I am interested in some other problems that came up here. They were discussions of stress, in terms of the guideline problems, and I think we went completely off track, because it's a rule in psychiatry, it's a rule in human research I think, not to harm people. The simple, ethical rule of: Don't stress humans needlessly to produce a result. All through the Midtown Study we observed confidentially. We certainly did not damage anyone and this was because there were clear guideline principles then. I'm just saying that we <u>knew</u> this as a professional group.

But that's stress <u>from</u> the research. I feel that's quite a different thing from stresses in society I wouldn't pass off as too global, that there are people in the community, and that real factors indeed affect their human lives.

I think this kind of research will simply have to get closer to the actual lives of actual human beings, and until it does, I think it will fall short of certain very practical goals. I think for that reason that it can't always be a simple one-factor kind of research.

For instance, in some of the discussion I felt I didn't know what some conferees thought the family was. I felt we weren't defining the fact that an <u>extended</u> family, a highly integrated body of a number of people, is different, let's say, than a 2-person married couple, which relates to the question of whether there was some assaultive behavior within the group.

There are so many things we do know. We know much. Child abusers, for example, were child abused themselves. There have been films about it. There have been TV shows. Let's not kid ourselves; it's out, it's known. It's been proven by any number of discrete studies.

I think, on the other hand, that there are whole areas that must now be gone into about the dwarfed relationships in the sociopathic kind of personality. This does bother Jonathan Freedman since he's bothered by the muggings, and concerned about the rape. I've forgotten your list, exactly, but there were certain things that you felt were offensive, probably the things that no longer allow us to go and walk in a park like New York's Central Park after a certain hour of twilight.

Our lives have changed in our American cities. I'm saying, again, they have not changed that much in Tokyo, and it's certainly a big city, and it's got its parks, and it isn't too far to the discovery of why the difference is so great. I am suggesting, again, that the research related to the social and cultural variables that function is very important.

I commend, for instance, Dr. Staples for giving us a paper in which though he called it "race," I call it an American "subcultural group." The black in the U.S. isn't like the West African black. I know the general public can only swallow the much abused term, "race." I'm trying to provide examples of the enormous problems that do exist. There are large amounts of research and data that are backlogged and we need resources for dealing with it. I'm suggesting that agencies go out and tap the people who are sitting on very relevant data.

REISS: I think we got your point: Large-scale tapping of natural resources.

STRAUSS: Along the lines of tapping the people and how they experience these events, I think this is a very important issue that all of us face who want to investigate stress and crime. I would personally like to hear more from Dr. Lieberman about the methods that he has used to go beyond the simple listing of stressor events, to get at things such as how people experience the demands that are made on them, or whatever approach he's taken to go beyond the stressful events list, per se.

LIEBERMAN: It seems to me we started to examine what we've done today and ask the question: what difference does this all make in regard to social policy?

In our own research, what we have been doing is to discriminate between the events that impinge upon people's lives. If you recall in my remarks today I discriminated between those that are normative, which essentially is saying these are expectable events, and crises which maybe happen with larger frequency but are not predictable in the life cycle. To me, the issue of stress is that stress ceases to become a meaningful term after a while.

Your own work suggests this, that it isn't the stress itself, but what people do with it that makes a great deal of difference. The difference in our study and some of the ones presented today is, we have a longitudinal study that has two points in time so far, five years apart.

We know what people were like 5 years ago, before any events or any crises occurred to them. We know how they were operating in their various role areas. The way we've carved up people's lives, in a sense, is to look at them in terms of the economic role, occupational role, marital and parental roles, the major role areas, and if they're not married, the single role.

I think the linchpin, in terms of what we've been doing, is to look not so much at these events which we start from in our equation, but to people's lives, their daily living within the major role areas. What's the occupational role like for them?

We've developed a series of very specific behaviors within all these role areas, and have been able, in one sense, to demonstrate that it isn't the events themselves that impact on people's adaptation, as much as these events alter the conditions of daily life that then affect the adaptation.

To give you one illustration of this kind of thing; we studied a number of individuals who in our study have been laid off, fired, demoted, or unemployed in the 5 years subsequent to our first round of data colelction.

There is, as one would expect, a rather substantial association between that kind of life event and mental illness. However, if we look more closely at it, if unemployment or demotion did not affect the marital relationship of the person, the day-to-day marital relationship remained the same as it was 5 years ago within some parameters, there was a 50 percent reduction in the association between unemployment and mental illness.

If you keep on going with this kind of chain and put in a measurement of coping strategies, i.e., how do people deal with the marital relationship, you soon find that almost all your variance is in these intervening variables, rather than the events themselves. I don't know if this answers the question, but that is essentially the strategy we've been using.

TOCH: I think this is, in some respects, a very suggestive approach because of a point that Dr. Freedman raised earlier, which I think I also hinted at this morning. It has to do with the actual implication of what leads to optimism and what leads to change possibilities.

I think the mainline definition of "stress," which is in Dr. Lieberman's paper, is the discrepancy between the challenge of the situation and one's coping strategies coming into the situation. Pursued chronologically in terms of "what next?" leads to a full range of problems, ranging from people who continue not being able to cope on the one hand, to people who deny the reality of the situation and, in a sense, do not even attempt to cope (one of the ways where nonadaptation or failure to react to potential stress can be quite destructive in terms of adjustment), to, the mid range, in which all kinds of adaptive reactions tell one about options which people can exercise.

Now it is at that stage that I think very useful action possibilities enter the picture. Let us think in terms of what one can do if it is assumed that certain kinds of behavior produced by stress are undesirable, whether in the area of crime, or mental illness, or elsewhere. The issue may be of providing support or some kind of rearrangement of the environment for people who don't seem to have the tools to cope when facing discrepancies between their past experience and the current challenge or to build some kind of coping competence into people who seem to meet those situations inappropriately, whether they are policemen, or prison guards, or inmates, or offenders, or pre-offenders. This seems to me the kind of area where there are immediate action implications which don't require redoing the social structure.

We can intervene, and maybe even rearrange the physical environment and what Dr. Hippchen referred to, the chemical environment and so on. This could be part of the tangible impact possibilities.

LIEBERMAN: The fact of the matter is if you took the stress adaptation model probably the most variance is accounted for in terms of people's coping strategies.

The question is whether we can "teach people," and I use that with quotes around it, or develop certain kinds of skills in people. Certainly they're not genetic, these coping strategies. We know they're related to cultural conditions.

I have never seen a proposal suggest that school systems should teach people better modes of coping. It's somewhat far-fetched to me.

REISS: The juvenile delinquency area is filled with thousands of proposals to teach coping strategies.

LIEBERMAN: Coping strategies, how successful are they?

REISS: You can put ghetto kids through the survival training but as a result of the early "American Soldier" findings, it turned out counter-intuitively. The more educated survived better, broke down less in combat than the kids who came out of the blackboard jungle.

I believe that has something to do with coping strategies. There's a whole body of work going on out there, but it's badly evaluated.

LIEBERMAN: I was curious as to how it works.

WILKINS: It seems to me that there is perhaps some possibility of relating these two divergent perspectives. I heard the phrase "events which impinge on people's lives." Well, those events are not only generated by other people, they're also generated by situations, environments, organizational structures, and so on; not merely by other people.

It may be easier perhaps to work on certain of those other elements than perhaps to concentrate the whole of our thinking only upon people. Certainly people affect people, but I'm also certain things other than other people affect people. I am very bothered about the excessive simplification which really says: "If we had no criminals, there wouldn't be any crime. Therefore, concentrate everything on the offender."

We've been trying this, not since the LEAA, but we've been trying it for thousands of years, and it hasn't been terribly successful. I don't think I have to prove that.

By comparison, with all the work that's gone into trying to deal with crime by dealing with the sinner--the offender--very little, comparatively, has gone into looking at the other elements of the situation. And by the "other elements," I mean, the environmental factors, and also, of course, the victim.

We haven't adequately even got a list describing the other possible elements. We've got a much better catalog for describing personality traits than we have environmental characteristics, even as a list to begin with. Not surprisingly, because comparatively we put very little weight into that.

There is one other thing that bothers me somewhat here, and of course, this may derive from a rather isolated finding. It seems to me that we have quite often, implicitly, in the back of our minds, assumed that the victim set and the offender set are two different sets of people. They aren't all that you know. The victim set and the offender set, if we may trust the data from PROMIS in its Witness Cooperation Study, shows that the intersection was 27 percent. The person who appeared today in the dock, as the accused, at some other point stands a .27 probability of appearing as a witness, normally being, in that case, the victim.

This is an incidental finding. We can't, perhaps, be sure about this yet. But, it seems to me very clear that any assumption that offenders are over there, and victims are over here, entirely different easily discriminable sets of people, is becoming a rather seriously suspect assumption.

If I may just make one point on Japan--I know that country reasonably well. In addition to socio-cultural factors, there are structural and organizational factors in Japan, too.

We cannot, obviously, import their sociocultural background, but maybe there are certain elements of the structural organizational system which might be worth looking at. Now, when I say this, I am not saying that we shouldn't look at offenders. Clearly, whether we like it or not, we are going to have to do something about offenders. The public is going to make sure that we do something about offenders. This is extremely important. I think, perhaps, it might be useful if we could detach our thinking from discussing what we are going to do about offenders from what we think we are going to do about crime. No matter what we do about offenders, we are not going to make a pimple's worth of difference to the crime rate.

So, if we are going to deal with crime, we are going to deal with crime. And that, of course, is going to be extremely difficult. It is going to be difficult, because people like the very situations that generate crime. Gambling is a crime; people want to gamble. That's only one example.

REISS: I wanted to follow up on something myself. It has to do with the emphasis on the longitudinal design and sorting out its problems. A longitudinal design can be a cohort design but it need not involve a cohort design. Essentially, what you have, Morton (Lieberman), is a single cohort which you are following. I think it may not even be cohort. In any case, in a cohort design, we think of three important determinants.

One is historical time, as a determinant. A second is aging, as a determinant. And a third is cohort composition, as a determinant. Unfortunately, it is true that in any such model, if you have two, the third is determined. Therefore, you have a horrendous problem of identifying the model.

Setting aside the problem of identifying the model, there is one issue, I think, which links a number of papers, including the whole question of looking at aggregate correlation and individual level correlation. That is, in a cohort design, it is quite possible that looking at, for example, unemployment and its effect on the individual level might be quite different in a period of high unemployment rates than it is in a period of low unemployment rates.

I would call to your attention that there are a series of studies on the family in the depression, which were done in Austria and in this country, which were trying to look at the effects of unemployment on the family in a period of high employment. It is quite possible that the effects look quite different in a period in which that unemployment rate is very low. Similarly, the effect for groups such as blacks, is probably different when they have high unemployment rates in a society in which there is relatively low unemployment.

It is only by having longitudinal cohort designs that you can separate out these effects. That is what I would say to LEAA. We know those are very expensive designs, but, nonetheless, science is expensive. In the long run we just have to have cohort longitudinal designs to answer these questions.

HIPPCHEN: Let me say that, in addition to having studied criminal behavior for many years, as a criminologist, I have also been involved as a correctional psychologist for many years in trying to correct delinquent and criminal behavior. Let me assure you that I have a lot of evidence that criminal behavior can be corrected to a very high degree.

There are two major approaches that I am confident that on any day, or at any place, can be demonstrated on a controlled experimental basis to correct behavior. There are two modes of approach I have seen to be effective time and time again. One is nutritional therapy, the second is values training. These are the two that I have found to be the most effective, and I think it can be demonstrated anywhere at any time. I have set up projects over the last 10 years with delinquents, where the rate has been completely wiped out, where the judges that have been meeting in Juvenile Court every day and on Saturday morning now only meet in Court half a day a week, because the delinquency rate has been decreased. The adult crime rate has also been decreased.

What I am trying to say is that I have plenty of evidence and have personally been involved with at least a dozen projects, where we have been able to demonstrate that you can change offender behavior from antisocial to prosocial behavior up to the level of 80 percent at least. So it is not accurate to say that this behavior can't be corrected.

FREEDMAN: I didn't mean to imply in my little speech that LEAA was not supporting the kind of environmental or descriptive research that I was talking about. On the other hand, I think, as was just stated, the emphasis and the most amount of effort and money has been on the other work. I think, in fact, your (Mr. Ewing's) example illustrates it very nicely.

You said that you had supported the work of Oscar Newman. Now, Oscar Newman's work has gotten a tremendous amount of publicity. I think part of the reason it got so much publicity, in addition to the way it was presented, is that there are very few studies of that sort that came out. Yet it is generally agreed by most people that it is far from an acceptable study. To do an acceptable study of that sort is a fairly massive operation, but not as big as the kind of studies that Al (Reiss) just described. It's much, much cheaper, because you don't have to look at a time series. You don't have to follow it. It is very expensive. If the money and effort were divided a little more proportionately, you wouldn't have to realy on a relatively small-scale study like Newman's, where he picked a

very few buildings in one city and all the problems with it, but could rely on a series of studies by various investigators of the kinds of problems that, although they are tedious, are amenable to research very easily, much more so than the longitudinal kind of work. Not only in this field, but in all of the efforts by psychologists and sociologists and everyone else to study the factors that produce particular behavior patterns or different personalities using longitudinal studies, are very tedious. They are very expensive; and they are, typically, not terribly successful, which is not to say they shouldn't be done. They should be done, but there are so many problems with them that the probability of a good payoff is small.

REISS: I would disagree that the kind of studies you are talking about should not have longitudinal or cohort designs. In fact, I have been trying to argue that if we utilize the National Victimization Survey to follow up people who have moved, we will see them in changing environments, changing housing types, and so on. Thus, Lynn Curtis won't have to spend a lot of his money which he is going to spend, trying to find it out with a design that will not answer those questions. Tying onto the National Victim Survey would be a very sensible way to really enhance enormously our knowledge of relationships to victimization when people move into different environments and among housing types.

I say that, in part, because I have a co-opted audience here. I think that we haven't done half enough in trying to answer additional questions with that victimization survey to pay off on the big investment we have in it. This is a good example to really get some payoff with the cohort panel design.

OPLER: I just wanted to follow up a while back on Dr. Wilkins remarks, which I thought were excellent, and on comments that Dr. Hippchen made, too, which I think were useful. At what point do you intervene? People are noticing a lot of things labeled delinquency and crime hitting younger and younger age groups. Suicide is hitting younger; drug use is hitting the younger; alcoholism is burgeoning again among youth.

I would just like to give two examples that are separated in time. One is some of the experiences in New York at the point when gangs were formed and became popular. You could think of this in the Puerto Rican and black communities. The gangs had names; they involved themselves in violence. A lot of this was lurid headline stuff. There was little interposition of

real research in regard to this. I am talking about a kind of action research, where you are involved in the community as it is functioning.

Let me give the contrary example. When I was in Los Angeles at UCLA, I began doing Mexican-American studies. The story was just the same. The younger people who owned nothing were saying this is their turf, and these were the boundaries of gang areas, and they were constructing this kind of feeling although they notably owned no land at all.

An effort was made to send a monitor, some very excellent contact people who would work with these young individuals. We did this. This was during a period when there was a certain amount of police harassment of these groups; and there were some problems that would probably have erupted on the scale of New York City if we had not taken such steps.

They had this particular kind of contact point in the community clubs, in the basements where the kids got together. They found out that there were a lot of positive elements in regard to the gang group functioning, such as the reaching out in these adolescent and youth period years for other people to help on a neighborhood basis.

It's a very sharp contrast in results to what happened in New York City. The one went to hell in a hand basket, and people were getting cut up or they were getting poked with sharpened unbrellas, knives, and so forth in New York; and the basic street gang was made most positive in Los Angeles.

BRENNER: I would just like to support a comment you made, Dr. Reiss. It seems in almost every one of these epidemiologic-style fields virtually the only possible way to ascertain casual direction, even with the most highly refined investigative tools and sensitive instruments, is with a mechanism that allows us to observe what is occurring before and after particular phenomena, which we label as independent variables (before) and as dependent variables (after).

They also allow us, incidentally, to do simultaneous modeling, and very often I suspect in this particular field, this is the problem. It is quite likely if we believe the scaling devices of Holmes and Rahe, that certain kinds of criminal activity do represent a stress outcome, but also represent a stress itself, particularly when there is involvement with the law, as appears on their scaling devices.

Now, let us say it is both. If it is both, we require the kind of modeling and the kind of data that allow us to see it as both. A singular cross-sectional analysis, even for a broad range of time, with the samples collected over a year, will simply not be sufficient to do that. We will never escape from that causal box, particularly when our problem is compounded with dual and simultaneous causality, as it most likely is if we are to believe the stress researchers.

I would like to suggest one further thing again in very strong support of the longitudinal model. It isn't necessary in the longitudinal approach with a singular cohort, to remain with the life experience only of the cohort. As Dr. Reiss was saying, it is taking a situation, for instance, of unemployment. The individual unemployed during a period of nationally or regionally high unemployment may well behave quite differently than under conditions of a rather different rate of unemployment. The social context, the regional context, etc., should be taken into consideration. How does one do that? One employs simultaneously, somewhat macro-style indicators perhaps of a region, a state, or a city, as well as (and at the same time as) one takes into consideration the cohort itself.

We have a couple of existing cohorts of the Wolfgang variety, for instance, in which some of that may be possible. But certainly in view of the total cohort work which is being carried on, this would add tremendously and quite inexpensively to the kinds of interpretations that are possible.

REISS: He (Wolfgang) only studied a single cohort. A lot of people forget that.

THOMPSON: I am concerned about the theoretical status of stress variables, coping variables, adaptations. If you take something like street crime for example, the National Institute is at least under the impression that they've asked The Vera Institute to do a 5 year longitudinal study of street crime related to employment.

If you take unemployment as a stress variable, what is street crime, a coping variable, perhaps an alternate income-generating behavior?

Or is it an evidence of the failure of adaptation? Where that is the case, we have begun talking to young men in New York City. It certainly is the case that after failing in employment, they do encounter great numbers of problems in their family situations, often graphically described to us in terms of common law wives beginning to look cross-eyed at the individual because he is around the house all day.

Then there is also addiction; there is alcohol use and so forth, where in a truly longitudinal picture, it seems to me, one's earlier coping efforts become additional stressors. If you really follow people, periodically, over several years, when we are not talking to them only once a year, but hopefully will be in contact with them much more frequently--it doesn't seem as if your discussion, Dr. Lieberman, gives me quite the theoretical handle that at first I thought it did. I don't know how to unravel what I am getting from these informants in terms of the theoretical constructs in your stress model.

LIEBERMAN: We have not been studying criminal behavior. Our study was not concerned with criminal behavior. The bottom line essentially was homeostasis, namely, the maintenance of a certain level of functioning over a five-year period.

In a sense, this avoided what I think you are raising, which is a scientific issue and a value issue. We didn't study this kind of thing, so we didn't have this problem that you are raising.

As I have heard people talk today, and thinking about the kind of behavior, the bottom line behavior you are speaking of, I am not sure where I would place it. Obviously, some criminal behavior is quite adaptive in the model I think about, in terms of homeostasis, and some is not. I am not sure where at this point, how I would sort these things out. In other words, what I am saying is I was able to avoid that issue by not studying that kind of problem.

THOMPSON: If you talk to unarrested offenders, people who are continuing to commit crimes, and they come in and talk, they will certainly prefer to be employed. At least that is what they will tell you. They'll even give some indications of job search efforts, but they're very sporadic; employment is very intermittant. So what you get is a mixed strategy of some employment, some crime, increasing cumulatively, increasing chances of an arrest, leading to detention and perhaps also incarceration after the sentence and then back to the cycle

again, with perhaps accumulated deficiencies because of the earlier experience.

There is a cascading system, in other words, of advantagement or disadvantagement. I don't think that the longitudinal approach really helps that much to unravel this. It certainly helps in the sense of getting rid of the criminal justice system-oriented effects, which you are looking at.

LIEBERMAN: I think what longitudinal studies enable you to do, and leaving aside the cohort issue, because I think that's added and realistic complexity, is it permits you at least to begin to develop causal models. That is what I think is the important part of longitudinal studies. The question you are asking is, how do you categorize this behavior?

THOMPSON: That's right.

LIEBERMAN: I don't know. I mean, literally, I have not given it any thought, because in our research model we hadn't been facing that. We copped out because we studied a random sample of people whose criminal behavior was relatively infrequent. We took a position on adaptation that suggested that as people were able to maintain homeostasis under conditions of stress that was successful adaptation. It is a researcher's way of handling the problem. I am not sure it is the best way, but that is the way we did it.

EWING: I wanted to say a couple of things about some of the observations that have been made this afternoon, and be very brief about that.

In the first place, I want to respond to something that Dr. Opler said, which is that he urged us to be systematic about tapping the knowledge that exists and build on that, as we develop our research agenda in this area and in other areas for the future.

The purpose of this session is not to be a one time event in which we simply get together and talk about your papers, but rather to build our understanding so that we can then begin, with your help in the future, to develop a research agenda that makes sense in this and other areas.

A couple of other comments that I would like to make, too. One is that Dr. Freedman made the observation or inferred, I believe, that LEAA's interest is largely going to be in

the area of immediate action, or in research that is largely applied. We are, of course, interested and have been interested and largely operated in the applied area.

This effort, as well as a variety of others is our first venture into a different kind of field. While the social and policy implications are of great interest and significance to us, and while the short-term payoffs always are of interest to any government agency, we are also now of the view, (and so is the Department of Justice) that the National Institute ought to be pursuing some sort of balance between more fundamental and more applied research. We are moving very strongly in that direction.

Indeed, the legislation that is proposed for the new National Institute of Justice makes that quite explicit. So, we are looking at fundamental kinds of inquiry as well. This leads me then to comment on the observations about longitudinal research, which is also cohort research.

We have asked Marvin Wolfgang to pick up on one piece of research, that is originally an NIH cohort, a perinatal cohort. Helen Erskine, who is here, is the project monitor. If you want to pursue what is going on in that project, you might want to check that with Helen. This involves basically six cohorts over a period of six years, and that goes back to 1959. We are picking these cohorts up now at this point in time. That has certain problems, of course, but we are interested in that and we are interested in the sponsorship of longitudinal cohort studies in the future. We are also interested in pursuing a wide variety of other kinds of research, as well. While our budget is not unlimited, it's certainly adequate at least this year and we think next year, to support a fairly broad range of research strategies, including, of course, applied research and including the testing of ideas in specific settings and sites in a limited way.

There are far fewer limits on what we can do than perhaps was the case in LEAA's and the National Institute's past. We appreciate very much your assistance and advice to us on what the range of research that might be done and what specific research might be done, in what order, and with what level of importance.

REISS: Thank you, Blair.

(Whereupon, at 4:45 p.m., the meeting was adjourned, to reconvene at 9:00 a.m. on Tuesday, December 5, 1978.)

SECOND DAY MORNING DISCUSSION

REISS: We welcome Bob Burkhart of LEAA who joins us today. We're glad to have him with us. This morning, as you know, is devoted to discussion. And as I indicated yesterday at the outset, I thought it could be and should be a time when we try to gather together what we think are the important issues in this area in which research needs to be done, and to try and point toward the quality research that might be done to answer those problems (or arrive at the point where we think maybe it's not worth it).

Let me suggest two criteria of why it might not be worth it at this point. If we think that in the end the amount of variance going to be explained is going to be so terribly small, I think one would need some compelling arguments as to why one would want to tackle that problem at this particular point.

The other criterion is the one I suggested yesterday--which would be a sort of "so what if we knew this; where would it take us next; what would we be able to do with it?" In other words, let us try to focus upon a kind of cumulative research program. I had a few topics that I thought we might chew on for a while before opening it to the kinds of suggestions that have occurred to all of us in one form or another. The first topic I have here is labeled "ethical problems" in doing research in this area. Unfortunately, I wasn't aware of the fact that Dr. Lion was not going to be with us this morning. He is the one who raised that question, and in fact, made several concrete suggestions.

If I may just refresh you on his concrete suggestions. One of his suggestions was that a formal organization be established dealing with the bioethics of criminological research, or in other words, a group or an organization be brought together to attend to those problems.

Another series of suggestions which he made were for the kinds of research that he thought we ought to be allowed to do, which at the present tends to be closed off. He mentioned in that connection three topics: one was psychosurgery research in relation to violence; the second was to allow prisoners to volunteer for invasive and intrusive studies; and the third was to allow prisoners to participate in research as a condition of release, that is to say, as a basis for parole.

HIPPCHEN: I'll start things off this morning. I do feel that it is important for the individual involved with research to be fully aware of the probable consequences of participation. Where there's a particular possible harm involved to the person, such as brain surgery in relation to violence, which I think is a questionable technique because there are other means of dealing with these problems that are much safer, we should always try and opt for the safer route and explore those areas that are fruitful but not harmful to the individual. I would be concerned, even with the person's consent, particularly if there's any coercion involved, in the case with prisoners. If the person's free will is operating, then I would say the person is fully aware and they have some degree of the freedom of choice; then it would be acceptable. But I don't believe that it would be in the best human interest to operate on any other basis.

TOCH: I just want to express a thought in connection with this issue, that freedom-of-choice cuts both ways. It's quite true that prison volunteering isn't the same as volunteering in the outside community. Yet it seems to me and to other people that it is paternalism which tells an inmate that he cannot exercise his choice, given the system of constraints. That he in a sense becomes incapable of dealing with the pressures that operate on him, to say that under no circumstances ought he to be permitted to participate in activities which I as an outside observer don't think I would like is a noxious position taken to the extreme, certainly. I'm not sure where you'd draw a line.

I don't see that I have a right to tell an inmate he cannot participate in a study. And I'm not even sure that the parameters are easily examined.

Supposing that, as Dr. Hippchen said, there are side effects that may be undesirable. Supposing it's a cure for cancer. Now, leave aside the mercenary chemical companies lurking in the background; and this isn't just paranoia, they are out there, trying to make money.

If I, as an inmate, want to work off some of my guilt, if I want to contribute to humanity, and do something meaningful, I say, "I want in, I'd like to take your cancer pill. I've heard everything you've told me, but I want to take it. I know I'm going to feel nauseous and there are unknown after-effects, but I want it." I don't see that the government has any right to tell this guy, even if we suspect it's

expiational, or he wants to influence his parole board or whatever, "You can't do that," simply because of the prevailing ethic in the social sciences, that classifies prison as a coercive environment, while the rest of the world presumably isn't.

I think it has to be thought through. I don't know where I stand on this issue. There is the freedom-of-choice issue on both sides of that fence.

STRAUS: Perhaps there might be procedures worked out that would give the prisoners the kind of opportunity that Dr. Toch is talking about, under minimal coercion.

I've never done any research with prisoners, and I'm not familiar with that literature, but it seems to me that if you ask publicly for volunteers, there's a certain kind of pressure. On the other hand it might be possible to structure the thing so that prisoners can make their decisions privately, that might put a different cast on it.

Perhaps someone here can comment on whether there's been any systematic thought on this, or indeed any research, or whether such research might be a desirable sort of methodological research.

THOMPSON: Murray has a very good point. If you simply deny the parole committee access to information about participation, you would remove the very, very strong sanctions that we are concerned about. You would end up with genuine voluntary participation.

I believe that, from some exposure with prison populations, that you would in fact still get volunteers under those conditions and yet there would not be the question of why was the volunteer forthcoming.

TOCH: Except that there are other examples. First of all, prison is a very boring place; and there's money.

THOMPSON: But those are the kind of incentives that also would work with the civilian population.

TOCH: Except that many of the experts would claim that it is qualitatively different when you've got somebody locked up.

REISS: There is a literature on this. The National Commission for the Protection of Subjects in Biomedical and Behavioral Research made certain recommendations and decisions with respect to prisoner research. One is the HEW regulations on the question. The new LEAA regulations, as I recall, don't speak specifically to prisoner research. It seems to me what is at issue here is that particular population, the prison population.

Secondly, we're talking about something like stress research, where the possibility exists of certain kinds of interventions. It's an experimental situation, in which one might be "creating stress," and so there are all those kinds of conditions in addition to those that inhere just because it's prisoner research.

Thus, psychosurgery poses, it seems to me, a particular issue. And I guess there is a kind of bottom line there for me. While I feel as Hans (Toch) does on the question of the paradox of the freedom to choose, where the consequences could be permanent dependency on the state as in some of the earlier psychosurgery--so that for the rest of one's life I've got to take care of you at taxpayers' expense--then I think the state has a special interest in the consequences of that research.

TOCH: Shouldn't there be more of a humane reason for having a special interest, not only that the guy costs money?

REISS: Humane reasons are always paradoxical. I feel that way, too. But nonetheless, my humanity is not necessarily that person's sense of humanity, or the family's sense of humanity. There is a kind of bottom line from the standpoint of how do you justify the state's intervention. I'm saying the problem arises from why the state is intervening in the first place. It is a further constraint on freedom which is not already imposed.

TOCH: Mr. Chairman, the Constitution, as I remember, gives the state police powers that entitle the state to protect the health and welfare of its citizens. Now, it seems to me, the welfare of a subject of research can come under that heading.

If we are talking about situations in which informed consent is impossible, because the parameters of the problem are not available to the person who is consenting, then maybe we don't need to hunt around for elaborate reasons to say, "No." Psychosurgery, it seems to me, is a situation that would fall under

that heading. If it does, I don't think anybody can consent to psychosurgery.

REISS: That's another matter. Then you are saying it's forbidden not because people are prisoners, but because it's forbidden.

TOCH: No, because the effects of psychosurgery are sufficiently unspecifiable.

REISS: That's right. It should be forbidden for anyone, not because it has anything to do with being a prisoner.

TOCH: Yes. I would say the State has a right to generally forbid procedures like psychosurgery and electroshock if the State acts like it should act.

REISS: Well, it's one thing to say the State does that because it believes no one can make an informed judgement, which is a peculiar kind of thing, because at the same time the State is saying, "We're making an informed judgement. The consequences are such." If the State thinks it can make an informed judgment, then how can it deny an individual citizen the right to make a similar informed judgment?

THOMPSON: Yesterday I asked Dr. Lion if he was satisfied that similar kinds of research had been done on other kinds of populations. It seems that he was by no means satisfied. It seems that this also gets to the ethical question. If this research were on a comparative basis with volunteers from air traffic controller populations, police or military populations, or whatever theoretically interesting populations were available, and then also some were coming in from prison populations, one would be much more confident about the voluntary character of the participation.

Now, the danger seems to me that LEAA sponsorship might lead researchers exclusively to identify prisoner populations as targets of interest. I think LEAA might be able to help in this matter by exploring interagency sponsorship of this kind of research.

We certainly know enough about the plea-bargaining process, the court disposition process, the sentencing process, to suggest that the prisoner population is not something which is defined in a homogenous way in all jurisdictions. And in a way, that somehow plays into the interest in this kind of work. I am concerned that we are talking exclusively about

prisoners. Why not other populations at the same time? That would, I think, also help the ethical problem.

FREEDMAN: I'm certain that's true. But I think there are two points: One is that some of the research is specifically concerned with prisons and prison populations. If what you're interested in is the likelihood that a prisoner, under a particular regime or from a particular prison, is going to go out into the world to commit another crime or get into various kinds of trouble or experience certain kinds of stress, it doesn't do you any good to know about air traffic controllers. You have to know about prisoners, because that's what you want to study. You want to study prisons.

Regulations say you can't study prisons except under very, very limited circumstances because we don't want to interfere with the lives of the prisoners, because they are not free to say "no." This may make some kind of sense ethically. On the other hand, you're basically saying you can't study prisons. And since prisons are terribly important in our judicial system and important from the point of view of the prisoners, as well as from the point of view of everybody else, it just doesn't make sense to rule that out. That's one point.

The second point is that there's no question that the prison population is a special population. And they have been used to do research that no one else would want to take part in. They've been used because you can offer them something that you couldn't offer anyone else, which is that you could offer them freedom. Or you could offer them special consideration that you couldn't offer anyone else. And now we have said in recent years, "You can't do that." That doesn't really give them a choice, because they are under such restrictions and such restraint that the rewards in some sense are too great; the pressures are too great.

I'm not sure that I agree with that, but I certainly understand the arguement. But, as Hans (Toch) said, what you want to say to a prisoner is, "We can offer you an enormous reward. We can offer you freedom. It is to the State's good, we think, in this particular circumstance to offer you freedom in exchange for your participation in the following program. What do you say?"

Now, the argument from the negative point of view is: the prisoner has to say "yes," because the pressures are too great. Perhaps. Obviously, you need some sort of control

on that. On the other hand, here is someone who is in prison against his will. And that's not so nice either. And maybe you are saying "Do this and you will no longer be under restraint,"--not psychosurgery, which some people don't want to allow under any circumstances, but experimental programs: drug testing, psychological testing, some things that no one else or very few other people would like to take part in.

The prisoner will do it because you can offer the prisoner something you can't offer anyone else. Maybe you should give the prisoner a choice. It's obviously terribly complicated and politically probably not possible at the moment. But it's not something that you could make a strong agrument against.

HIPPCHEN: One other aspect of this problem I have some personal experience with, is that there's a general tendency during this time for the rights of prisoners to be of such concern as to almost stop all research. I've had one project that's been held in abeyance because the state legislature is supposedly discussing this problem and considering a law, and the Director of Corrections has said he won't look at any research until this is clarified from the legislature's viewpoint.

I have another project that's in the works. I've been informed that there are seven committees just within the state, not saying anything about the federal government, that are going to have to review all of the consent forms. And at each level they will have to decide whether this is dangerous or not to the criminal. It certainly has a way of discouraging research by the time you get through all the layers of committees and boards that have to approve the proposal at the state level, to say nothing of the federal level as well.

TOCH: Mr. Chairman, the consent-form issue reminds me of another not terribly subtle thought. It's certainly a fact that two populations are linked by reviewing bodies as especially sensitive populations; prisoners and children.

REISS: There's more than that. There are fetuses, too.

TOCH: Well, you can define those as children. The fact of the matter is that the presumption here is pretty similar. They are both nonautonomous populations. Yet the people at NIMH who say, "Don't do any research with prisoners, unless you know you can assure them that it's for their good," also say "You've got to have in every prison or prison system a review

committee which has to include prisoners." Now, that's paradoxical, I think.

On the one hand, the assumption is these people can't use their intelligence and their willpower in deciding whether to participate. Then, those same people get put on the review bodies where they, in a sense, review your application. Now, that logic somewhat bothers me.

I think that anybody with an IQ of better than 60 can justify doing almost anything, in the social sciences, to an inmate in terms of, "It's going to improve your lot, if not immediately, then 20 years from now. All of the stuff I am going to be gathering by way of information is ultimately going to be plowed back into some sort of decision, if those decision-makers just listen to me, and it's going to benefit future generations of inmates." I can't think of anything that I would possibly want to do in prisons that I couldn't justify this way. I think that stimulates hypocrisy, frankly. I have got, in a sense, to come on like a sleazy car salesman and say, "Look, I am interested in the impact of total institutions in some way and I assure you that future generations of inmates are going to be grateful for your answers to my silly questionnaire."

REISS: Well, I want to close and move on to another topic in just a moment. Let me just make one observation about how difficult it may be to control. I was at a meeting recently, on terrorism, outside of this country, in West Germany. There are imprisoned in West Germany people associated with the Baader-Meinhof Gang. A psychiatrist from this country was attending this particular meeting who had wanted to interview one of the terrorists and had written in advance, asking the terrorist for permission to interview him. The terrorist had refused. When he got to Berlin, the psychiatrist again made inquiry, and the prisoner refused. He then contacted the chief of the West Berlin Police and asked him to intervene with the warden of the prison to be able to interview this prisoner. The Chief, in the best German fashion, took him to the prison; they got the warden; and the warden went and got the prisoner and brought him into the room and sat him down and allowed the psychiatrist to make his pitch. Finally, the prisoner agreed and was interviewed for literally a little over seven hours, with the German police chief present and acting as interpreter. The psychiatrist reported all of this, saying he was so elated he couldn't describe to us how elated he had been by this whole experience. At which point I just wanted

to run out of the room. I couldn't believe it. Those things do happen. We don't protect human subjects, German human subjects, from American psychiatrists, to be sure, and ordinarily anyone is allowed a "free" ten subjects in exploratory research.

So hypocrisy runs through the system. You can interview German terrorists if you just have enough chutzpah about how to go about it. I recognize the problem. It seems to me that we have a number of important features here. Leonard (Hippchen) is saying something which, I must confess I haven't thought as much about as some of the other issues, and he's saying how it discourages investigators. That is something, I think, we ought to think about very seriously, because if LEAA is going to talk about a research program in this area, then at the outset they ought to think about attracting people to do certain kinds of research that we think is important, research on prisoners. If the net effect is going to be to discourage most people from doing research on prisoners because procedural barriers are too great, the government itself may have to take some role in running interference on those procedural barriers.

That's not out of the question. I think LEAA, as a matter of fact, has taken a kind of leadership role and NIH in the drug area, in trying to get special exemptions for certain kinds of research. We think this is an important area. It seems to be one of the problems for LEAA to talk about special, even congressional, authorization to do certain kinds of research which are not covered by the ordinary criteria.

LALLEY: I just want to remark, as a general proposition, that to the extent one turns a research program more towards fundamental issues of human behavior with a view to intervention and perhaps prevention, the more one gets into, in our jargon we call the "human subject" issues.

I do agree sometimes that there are these layers and layers of bureaucracy which are not essential, but I do think many researchers have not faced up to these issues before, and perhaps for the first time are going to have to think through some of the implications of the rights and welfare of subjects whom they previously regarded as passive types of populations from whom they can collect information as they will. Even so, some of the issues that we face are problematic, e.g., the issue of informed consent, of being fully aware of the risks involved in serving as a research subject. How can you be fully aware of the risks that may be involved before you even

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begin participating in research? Ought there to be procedures that allow you to stop participating at any time, and so forth and so on?

We tend always to come back to prisoners as the epitome of the human subjects problem. I don't think necessarily they are. I think children are very much involved here. So, also are patients in therapeutic relationships. For example, research is going on right now to try and understand what is meant by "informed consent" in a doctor-patient relationship when you are the doctor and you are asking a patient whether he or she wants to take drugs that you think are indicated, but which could have adverse side effects. What is the quality of that consent when the whole notion of the white gown impels the patient to trust in you because you know best. Human subjects issues like these are difficult. We'll have them, I guess, for our lifetime.

GROPPER: I'm Bud Gropper, of the National Institute. I think if one wants to have guidelines for research, you can think in terms of the prison as a microcosm of the outside world. With regard to the ethics of using human prisoners to study stress, for conceptual purposes, you may be interested in stress-inducing conditions and also stress-reducing strategies, techniques, etc.

There are difficulties insofar as using animal subjects and colonies as analogs for humans, but at least we can go in both directions with them. With rats, or what have you, we can study stress-inducing conditions. I think we're ethically limited and we should be ethically limited about doing anything to induce stress in human beings. We already have enough of that. However, it hobbles us conceptually, insofar as we have to take existing stressful situations and to try to work on the other half of it, stress-reducing. It's a weaker strategy, but ethically I don't think we want to in any way encourage research that would explore means inducing additional stress in human populations. If anything, we would want to confine it, although it's difficult conceptually, to the other half, to the stress-reducing part of it.

KEISS: There are the questions of what kind of rules you impose and there are what I call the 'Morris Rules." Norval Morris was the first person I know to introduce them into the literature. A legal criterion you might have is the rule you just enunciated. That is to say, you can do anything so long as it's no worse than what is now being done. That's a standard legal rule.

I worry a little bit about the argument that you can find substitutes for these, for two reasons. One is: What we want to know, above all, is the prison a place of punishment? That is to say, there is only one way you can really do that in the long run, and that is to look at people inside prisons and what prisons are doing. And, in effect, you can't necessarily allow people even to volunteer for that question.

Secondly, Hans Toch said that for some violent people, prisons tend to have, in his judgment, an effect of reducing their violence. They were violent on the outside but they never commit any violent acts in prison. We want to understand what it is about that environment that does that. Nonetheless, we do know also that some people do commit violence in prisons and they may be people who didn't on the outside. And we're only going to understand that, in my judgment, by being able to study that in vivo.

Therefore, the argument is that some questions we want to understand precisely in that context. We will understand incarceration as a form of punishment only by understanding incarcerated people. Now, this relates to a more general point: When is consent required of people who are in organizational settings? If you want to change the educational system, if you're going to allow every teacher and every parent and every child to constrain you as to whether you can study learning in the classromm, let me assure you as an employer you're not going to get very far in changing anything. So, there are very important general issues here about human populations in relation to organizations. I submit that one has to treat the organizational relationship, in general, different from the way one treats the nonorganizational relationship.

BRENNER: I guess the most general point is that we do not have as thoroughly embedded into our legal structure as we should, mechanisms for safeguarding the public on a variety of fronts; be it the massive market research that is done, which we're all subject to on the telephone and in other ways; the extensive and often very, very important biomedical research that's done in many different areas requiring human subjects in vivo; and the kinds of research involving criminological issues that we're now discussing.

There are not now means formally embedded into our legal structure for safeguarding people, for assuring that analyses

will occur only at certain levels of aggregation, that individuals' identities are not open to pursuance, with criminal sanctions themselves. Violation of such things ultimately (if we are as a society to truly pursue this and we are violating individual prerogatives and rights otherwise guaranteed to them under law) should be subject not to the civil code but to the very criminal code which we're investigating. It is a large subject. As the institutes of health and criminal justice find themselves in greater and greater difficulties over these ethical issues perhaps the only way to rationally proceed is with some connection to the general law.

More generally, as Tom Lalley was saying yesterday, we are confronted with many very fundamental and serious problems, especially in the prison area where we have captive populations, as well as in the military or with regard to individuals subject to the influence of physicians and the wearing of the white coat, as Tom and Al (Reiss) pointed out.

What does one do in such circumstances? It's very difficult in the short run. In the short run there is another way to go, which at least in sociology, as far as I know, and economics and political science, is much more and more heavily used than previously. It is to more extensively use publicly available data of the census variety, of the public-opinion variety, already gathered; to use the available data bases in a more thorough and a more extensive analytical way, linking the data bases now available to us. This would really involve comparatively minimal cost to the government and taxpayer.

But the research yield is often enormous because of the large volume of subjects that are thus made available. Mortality data, imprisonment data, data which in this field bear on general expenditures for manpower and a variety of other things in this field are publicly available at this moment. At least many of them are. It is terribly, terribly wasteful in a society that has already spent such a great treasure on the accumulation of this information not to make use of it extensively.

Another point directly following from that is simply this: Typically, people in the biomedical fields, the bench laboratory researchers, are observing (this is especially true in the cancer area, for example) that work conducted under laboratory conditions, the most highly and deliberately controlled laboratory condictions is (because they're so highly and deliberately controlled) generally and frequently inapplicable to human situations and to natural life-history situations.

It has been our tradition, the philosophy of science tradition of John Stuart Mill and others, to control for variables by removing them from the analysis. To the extent that we do that, we do not allow ourselves to observe their influence. Even where their influence may be overpowering, we purposely shut them out. We go to all kinds of lengths to categorically remove them, and we are quite successful in that.

So successful are we at it, very often, that many of our students in social science find themselves with very little variance left to explain. Much more serious is that we are unable therefore to construct, what we are now given as more or less gospel in our social sciences, fully robust models of what actually happens.

What does that mean? It means that some crucial variables are left out of our analysis. To the extent that any crucial variable is left out of an explanatory analysis, we are given to either overestimate or underestimate the importance of one of the set of variables that we insert as having some causal implication for what we wish to explain. Very often, the signs would be the opposite of what we intended them to be simply because we have not allowed for these variables.

In a word, then it's simply that we have not taken advantage of the natural life-history information that is available. This is an enormous national treasure which incidentally, if we are to interpret things for the sake of policy, we need normally to take into consideration.

REISS: The same problem is applied in getting at that robust information. Those of us who tried for years to get the criminal careers file from the Federal Bureau of Investigation know what frustration you can have in getting at robust information.

TOCH: This has raised a new thought. I do think it's true that we ought to do more secondary analysis of all kinds of depositories of data. I agree with this. It may be useful for certain types of inquiries, such as the ones that Dr. Brenner is wedded to, but it just so happens that for some questions of the kind that concern some of the rest of us, that is not the most appropriate type of data. Some of us really have to go out and talk to people because that's how we operate, because we're working at a more mundane, clinically centered level.

I do think that last point of Dr. Brenner's about getting full-blooded situations is particularly relevant to this general issue because those full-blooded situations are very often available to us in institutions like prisons, hospitals, and schools, which are controlled in both the negative and positive sense. That is, they're natural laboratories. With all the artificiality they're more natural than artificial, compared with other sources and other places where we could work. More of the universe is available to us, and, therefore, it would be a pity to have those settings foreclosed at the very moment when we urge our students to go out and be relevant.

Now, the point about organizations appeals to me, because it is another one of those little paradoxes that come up here. NIMH says, "Be very careful when you go out there into that prison, to make sure that that inmate signs six forms and that that inmate-staff review board approves those forms so that when you finally get in there to ask that inmate the question, his presence has been legitimized." And yet, that same inmate not only has been forcefully put into a prison but he gets put into classrooms and vocational shops, he gets marched from one side of the place to another. He can go to a few places with little chits; he can't go to the rest of the place.

REISS: He can't pick his own doctor.

TOCH: His whole life is circumscribed, except in one area: namely, research. Now, that's rather curious, and I might say I personally have difficulty with a priori distinctions such as, "do things that can ameliorate his fate but don't do things that can conceivably add to his burden," because of the presumption that this is predictable.

In terms of many of the papers we have seen at this conference, one of the prime facts with regard to the stress issue and many other issues is that there are going to be all kinds of reactions by all kinds of people to anything that you do. I personally would find it very difficult to pre-specify an intervention or research strategy along a dimension such as amelioration or addition of stress. If anybody can do this, I say "good luck to them," but I don't see how they'd go about it.

REISS: We can go on discussing this for quite a while, and we've got about half a dozen other topics that we probably ought to devote ourselves to; we probably spent more time on this than we should.

STRAUS: Can I make one point on this? Probably everyone here has had research subjects that have benefitted from being respondents. We have also all had the experience of subjects who have not benefitted and who have felt some pain or discomfort. My experience has always been that the former groups vastly exceeds the latter. But there is obviously a high possibility of bias in that perception. I think this would be an important issue to research, i.e., assessing not just the risks but the benefits of participation. I would like to see some very systematic research on the extent to which interviewing people about stress serves as a therapeutic process, even though that isn't the purpose of the research. I can think of many people who have told me that it has helped them, but we need systematic investigation.

OPLER: In the Midtown Study, we were studying stress and studying it very naturalistically in some ways. There was a questionnaire. Sometimes it took 8 hours. I insisted that we record everything including the material that wouldn't be coded in terms of our coding process and pumped into our computers.

There are two points about stress: One is whether the research adds stress to the situation of the individual. I think that's the crucial ethical point. In psychiatry we don't believe in that. I think any guidelines can really sustain such a point. But it should not inhibit research.

And I think another dimension that we are studying here is violence. Now, I have had students who are studying violence in schools, and the violence outside the school is reflected right inside the school. There are teachers who have to be braced against being thrown out of the second story or tossed out of the room. That's the kind of violence in schools now, in some schools.

I have colleagues who teach at Attica. You've all heard about Attica. They go in and they teach literature. They're professors of literature. I remember teaching out at Santa Cruz, California and the case of the Soledad brothers was a big issue out there at some prison, I can't remember the name of it. But I remember a professor of English going out there

and teaching literature and having a lot of human contact with the prisoners inside that California prison.

I have a student named Frank. He is one of the guards at a correctional institute near our city. He is forever doing studies in a criminological vein and follow-up studies on prisoners, and very compendious studies, 80-page studies, charts and graphs and all. He comes to class and shows slides of the rape areas in the prison, slides of the areas where there is all sorts of violence inside the prisons. Let's not kid ourselves about it. There is rape, there are assaults. There are fear situations. People are preying on other people inside prisons. The violence in the outside society is reflected in a lot of the institutions that Goffman called total institutions. Sometimes I would say even in the school, certainly in the prison situation.

I don't see why research cannot be conducted, and I am talking to Brenner's point really, in a highly naturalistic setting, where the researcher is an ally finding out the truth about some things, about kinds of stresses and their effects on individuals. I think some research could be entry points.

My student, Frank, will bring a portfolio besides his slides with all the weapons that are collected in prison by himself and other guards. It's not a pleasant place. There are all sorts of knives and all sorts of transformations of forks and spoons into a prison array of weaponry. I think we'd be utterly naive around here to act as if the researcher comes in and by studying so-called stress and violence in the situation, is himself promoting the stress and violence in the situation.

I would like to see the guidelines so worked out that they protect the individual at all crucial points. All of my students know how to do that. I have students who are out in the community studying mental health patients or ex-patients, and they're willing to collect the 6 or 8 forms. That isn't the point. Those students are sincerely interested in finding out what is troubling these people out in the community, how they're not making it, how they're not coping, how they're reacting, whether they're getting better or worse, and how they're scrounging around to live.

BRENNER: Just a point in relation to what Hans Toch was saying. There's no intention on my part in any way about curtailing the fundamental work that results from person-to-person interviews. There are no perfect methodologies.

We have available to us a variety of procedures, some more naturalistic of the kind that Dr. Opler was talking about and some involving experimentation, which is becoming extremely difficult to do under ethical considerations.

I don't think any one methodology deserves an imperialistic reign over any other. I think what is required are several attacks, very often on the same kind of problem, from different points of view. Where the resources are available in a natural setting to handle them we should study, with as much as possible an insight, what typically and usually goes on, so that we may afford ourselves the possibility of making generalizations.

REISS: I want to shift now to another topic. Let me say that there are at least two big topics that I think we need to address ourselves to for the rest of the morning. The two big topics, as I see them, are one that might be called where the promising explanatory variables lie in this area. We've had different kinds examined here. We've had Leonard Hippchen, John Lion and others talk about the biochemical, genetic, etc., as a set of variables. Then we have the crime opportunity and environmental kind of variables, whether it be density of housing or others. Then we also have the factors concerning transmission within the family that Murray Straus is talking about. We can mention other classes as well.

We ought to talk about doing research, not simply in terms of explaining variance, but with regard to where are the payoffs likely to lie in terms of these sets of variables, that I think ought to be helpful to MITRE and LEAA.

The second big area is research designs, the level of analysis problem, the cohort longitudinal design problem and so on. Are there particular kinds of designs where we think payoff is more likely to come than with some other designs, or what are the kinds of issues that need to be dealt with in terms of the level of analysis problem, the aggregation versus disaggregation problem? The explanatory variable problem is open for consideration.

BRENNER: I don't know that we have been able as yet in these fields to conduct the kind of research which will enable us to identify which of an array of major variables is more important than another. This is a function of the level of analysis. Comparing states or comparing cities or comparing

countries might require very different kinds of variables than comparing individuals within a country, within a state, within a prison, within a school. At each level we include or exclude a variety of variables. One of the classic cases is the socioeconomic one, in which a method of controlling for social class is by looking at a group, a social class group, and excluding from the analysis the others. Well, we thereby miss the contribution which that particular variable adds to the situation, and what are we left with? We are left with the variance that is attributable to the phenomena that occur within that particular, let us say, class setting. Let's put it this way: The answer to the question, it seems to me, lies very differently at different levels of analysis. The things that explain cross-cultural and cross-national differences are often quite different from those that explain them at the organizational level, and certainly at the individual level. Having said that, there is one outstanding principle that I think needs to be taken into consideration in the discussion. That is, to the extent that there is truth value in the biochemical approach, in the genetic approach, in the organic approach, in the family socialization approach, in the social-environmental approach--to the extent that there is truth represented in these formulations--it is logically impossible that they actually compete with one another. That is logically impossible, because we have stated as a given that there is truth involved in these. Therefore, we require a kind of an analysis of variance design which allows us to take into account all of these things.

Insofar as we are able to take into account all of these things simultaneously, and only insofar as we're able to do that, shall we ever be able to assess the relative importance of any one of them; and that means any single level of analysis at all, from the cross-national, cross-cultural, down to the level of the individual.

REISS: Agreed. But there are, of course, strategies as to where we begin. We can't do them all at once.

HIPPCHEN: Let me say that in my paper I made two recommendations in this biochemical area about which I feel there is sufficient data to support further research. First of all, I think that a much more extensive review of the literature needs to be completed, because this subject cuts across so many different fields, so much research. I'd estimate that at least 5,000 studies have been conducted within the last couple of years

that very few people know about, because they reside in isolated journals. There's some very important work going on.

The second thing, in the applied area, is to go through a period of exploration, exploratory analytical studies to help us to identify, on small pilot bases, variables that look very promising, comparing noncriminal and criminal populations in particular.

A third phase of this research would be a clinical applied model approach that would utilize baseline designs in the study of these phenomenon. We're not that skilled in the social sciences in general with using baseline studies. From a methodological viewpoint we should begin to identify particular types of cases and use baseline approaches to test causation theories. Particularly in the biochemical area it appears that the number of variables that are involved are so vast and the individual nature of variables is so peculiar to each person, that unless one does more of a clinical case study approach using the baseline methodology, I do not feel that we will make sufficient progress in identifying critical clusters of variables.

REISS: Let me play an adversary role for just a moment. Suppose I said that in biochemical and gentic research, that the possibility that any of the theories would imply a specificity, a criminal-noncriminal differentiation, is very unlikely. Given the high diversity of different kinds of offending, it is unlikely that you could explain that diversity in terms of any kind of theory, any single kind of social science theory.

I grant you it's conceivable that a certain kind of genetic theory might explain it. I'm saying what is the possibility that current genetic theory or current biochemical theory would imply that kind of specificity. If it doesn't then ought we be betting on it.

HIPPCHEN: Well, it certainly does. In fact, we have not only a research history in the biochemical area, we have an applied area of clinical practice.

REISS: Let me be specific. One of the few things that I see in the genetic area is that whole terrible literature on XY chromosomes and criminality. It's largely, at least as I understand it, been discredited by geneticists as well as experimentalists. I don't know another thing in that whole literature on genetics. Is there? It may be my ignorance.

HIPPCHEN: Yes, there is a very extensive literature.

PETRICH: There is more.

REISS: What? Does it have that level of specificity in it?

PETRICH: I'm not sure about the level of specificity.

REISS: I'm asking for the specificity, that's where we want to take off.

OPLER: I agree with Dr. Hippchen's general point, which is that literature search is important, because of the computerized and data bank situations we now have. We have an outfit called Information Dissemination Services in our Health Sciences Laboratory. I edit a journal called the "International Journal of Social Psychiatry," and I'm constantly checking to see if some paper that I've received is old hat. So I distinctly and definitely feel that Dr. Hippchen's point is very useful. Those of us that are at a university can really utilize this kind of review.

I wanted to talk also about a related point. A particular research project does not necessarily have to be isolated, a thing by itself. Let me mention that Yale study since Al Reiss is here. The Yale study went on until NIMH and we tended to correct them, but it was not a study of prevalence of illness at all, it was just a study of people in treatment. We were out in the community studying people both in treatment and untreated people, people who were never known to psychiatric agencies or even ministers or psychologists or somebody else. And it grew to be my perception, and I recorded it in the Symposium on Social and Preventive Psychiatry, way back in 1957 when we were starting the Midtown Study. It was called, "Epidemiological Studies of Mental Illness." It's about 40 pages long, and it's methodological. It suggests that you can do staged research or you can have people at different institutions inter-digitally relating their research. I think we do all too little of this on the American scene.

But Midtown was five studies. Our first study was of the communities there. What were we studying? We studied them practically ethnographically and we studied the background of these communities. Let me just indicate how this can be shared. The next this was a questionnaire, stage two. The next thing was famly studies selected from questionnaire

bases. We went on to select from out of the samples, specific variables and phenomena to study. We got down to studies of people, studies of schizophrenia by cultural group, things of that sort. I'm saying we had family studies, we had community studies, we had survey studies.

REISS: It's what some people would call shotgun approach, don't miss a bet.

OPLER: It's a scaffolding approach. You can't do the one until you do the other. You know what your target population is or your problem is. Though it took about ten years to do that, I think it was worth it. I don't think it'll ever be done again.

TOCH: First, I'd like to concur that there is a need for multi-pronged studies of this kind. They are expensive, but obviously ingenuity ought to be exercised to get different types of variables that show up through these various methods.

Returning to the issue of specificity, I think that this is one that you find in all kinds of guises and it poses unresolved problems. What brought it to mind most recently is a paper I reviewed for a journal, which I had a very tough time with, because it is dealing with a subject close to Dr. Hippchen's concerns, and that's the place of learning disability in delinquency causation.

It turned out that this is an excellent example of how hairy the specificity issue is. There are surveys that show that there is a disproportionate amount of leaning disability present in delinquent populations as opposed to public school populations. At least on statistical grounds, one could say, whatever delinquency is and whatever learning disability is, part of the variance is common.

But that poses even more complex questions than it answers. The next set of issues has to do with what type of contribution to what type of delinquency does what type of learning disability make. Is it a question of actual cognitive deficiencies? Is it experienced failure? Is it self-esteem? Is it peer group pressure? Is it the system itself that treats differently individuals who have been labeled or diagnosed or tracked, or whatever? It seems to me that it's really important to leave those sorts of issues open as much as possible.

REISS: Let me try to see if I can bring this to a little more focus. There's a great tendency for we academics to open the world to paradise and look at every conceivable factor. And Harvey (Brenner) made it in his pitch: that you don't know because the model may be lacking the crucial variable, and that's our stock in trade. But that's not the kind of stock in trade that is quite LEAA's stock in trade. I think we're here for another purpose. Are there at the present time, in our judgment, some areas where we think the payoff might be better than in others? What I'm trying to tease out of us is where do we think the bets are? We're not going to make the final determination. We're still going to sit back and say, oh, but there are all those variables out there. We'd be out of business tomorrow, if we knew it all.

HIPPCHEN: I did want to say that the biochemical area is fairly new for criminologists. Three years ago I presented the first paper on the subject to the American Society of Criminology, and it wasn't too well received. Just a few weeks ago I chaired two symposiums on this subject at the Criminology Society and it was well received. The criminologists are beginning to understand and become interested in the subject.

One of the reasons I am suggesting a literature search and a period of analysis for teasing out the important variables is partly for reasons of self-education. We in this field are not aware of this literature. I think that until we become aware of the literature, as a society of criminologists, we are not going to be in a position to advise LEAA or anybody else.

I think we have a responsibility to inform ourselves about an area that has shown extremely critical promise. Most studies that I have seen, regardless of what other variables that you add to your equation, if you add the biochemical aspect it will double the correlation. It will double any correlation that you have if you add the biochemical element to it.

I know it's a critical variable. I can specify certain specific variables. I would like to challenge criminologists to join me and to do the background research I have done, to study the literature. I'll admit it's painful because you have to get into areas you've never heard of. You have to reeducate yourself. You've got to become

knowledgeable in areas you have no business becoming knowledgeable in which suggests, again, a team approach. There's no reason why sociologists can't work with psychologists and biochemists and neuropsychiatrists and so on. We're used to working independently. I think we're living in an age, particularly, of an explosion of knowledge in many areas. Biochemistry is the fastest-moving science that we have today, and criminologists are not familiar with this literature.

FREEDMAN: I'm willing to make some guesses, also, probably from the strong standpoint of ignorance, which always helps in these things, since I don't really know that much about criminology.

Let me first clear up something that was a misunderstanding of what I was arguing yesterday. I suggested that we focus on those kinds of factors that will prevent the likelihood of a crime being committed, rather than what produces a criminal. Probably because I misstated it, this was interpreted by some people as meaning that LEAA should support only applied research as opposed to basic research. I don't believe that at all. What I meant was that I felt that this could eventually and fairly soon have a better payoff in application. But the distinction I'd like to make is that we do not know enough about environmental factors or, for that matter, any other factors, to enable us to choose pilot projects, sensibly. Maybe we do with some. Generally, if LEAA wants to say let's try a pilot project on such and such, probably with intuition and some experience they might pick something good to try and it would work.

But I think concerning these environmental factors we know very, very little. What I was suggesting is that we go out and collect what would be largely descriptive data on what kinds of things lead to the likelihood of crime being committed. I'll make some guesses also, since that is what we're here for. My guess is that housing is not important. I would like to see a housing study done, a really good one, primarily to discount the Newman mythology. Not because I think housing is going to be important; at least it won't be important as a zero order explanation. It may be important in interactions, complex interactions with kinds of people and kinds of communities. I doubt very much that there would be main effects of housing.

Research done by Michaelson in Toronto and by us in New York City and by people in England already show that there are no

simple effects of housing on crime rates or anything else for that matter. I'd like to see that done, although I don't think there would be a payoff. I would like to see research done on street design, design of cities, in the sense not so much of the kind of housing, but how the housing design relates to the city and relates to the streets. Transportation is probably a good issue. These are very, very basic environmental factors, which play an enormous role in crime rates. Also, and I know LEAA is doing some of this, we should look at neighborhood organizations, the mood of the neighborhood, kinds of committees in the neighborhood and how they relate. I would put my money there, basic research to begin with, and my guess is that you'll find out some things that will then lead to applications.

BRENNER: On the matter of specificity, which was raised. In the general stress research that we've seen in a number of different fields, and partly along the lines that Murray Strauss raised yesterday, but more generally as well, the focus might well be on the specificity of reaction rather than on whether or not we see more in the way of criminologic style reaction patterns.

Rather, take a variety of coping behaviors, and see which kinds of life events, (if that's the kind of scale one uses, or another kind of scale to look at stressors) seems to be predictive of a reaction pattern that is along the criminological style. To take Dr. Freedman's approach, which environmental precipitants would tend to move us along this way, which would tend to move us along another way? Are there ethnographic, are there sociologic, are there circumstantial phenomena that make for specificity of one reaction pattern or another? Now, on the matter of levels of analysis, this is relevant here in just one way. The levels of analysis point in this context is that there will almost certainly be different variables identified at different levels as circumstantial or predeterministic of the differential patterns. We will observe at the national level, for example, that the northern Europeans are more likely to suicide than to homicide under any given condition, whereas the Mexicans are more likely to homicide and very rarely to suicide. This is because of the level of the development of the country and because of the high level of Catholicism.

REISS: Let me see if I understand your first point. Your first point is more along the line that Dr. Lieberman's paper was getting at yesterday, looking at the coping strategies. What are the intervening life events, and how they are responded to? Is that it?

BRENNER: It approaches that and goes a bit beyond that. For example, in the voluminous material that I presented by slide, we saw some reactions to changes in employment patterns were suicidal, some cardiovascular, some homicidal, some different types of crimes. But why that kind of variance? What kinds of populations, under what kinds of circumstances, will respond with one kind of reaction pattern as against another? I suggest the level of analysis will have a lot to do with that.

OPLER: Could I elaborate that with a specific point? I put in the title of my paper "sociopathy." I wanted to focus on a more violent aspect. Talking from a psychiatric point of view, my candidate for study is sociopathy and the violent strain. That's simple enough. We all know that cases are mixtures.

Son of Sam was a paranoid schizophrenic so classical you can't find anything more classical, except he shot up people. He had a strain of sociopathy, also. There was a myth about the sociopath or the psychopath that you couldn't study them. I happened to be at Cornell Medical School at a time when Meletta Schmiderberg began to study and to work with them and improve them. It was claimed that you couldn't do anything with a sociopathic personality. She found out that she liked to do it.

I observed the point that she felt she could find the strain in different admixtures of cases that related to violent outcomes and could deal with these. They were generally young people, too, which I think is a positive point, if we could work out some methodology.

I'm talking from the vantage point of psychiatry at this moment, and I'm saying we should study the sociopathic personality, which may be an admixture of other diagnostic categories. Diagnostic nomenclature is a messy thing. Sociopaths were once called psychopaths. It's gone through its vicissitudes. There's something there that can be studied; the violent outcome under stress that some people show when they break down. I wish we would study it. We have a lot of data. I have a lot of Midtown data. But I think there are all sorts of fresh ways of studying it.

TOCH: This antisocial personality disturbance issue reminds me of a point which has been bothering me all along in terms of the topic and the heading of this conference. There is one feature of the antisocial personality that makes it extreme. This is the fact that the anticocial personality disturbance, which is probably more prevalent among car salesmen than in prisons, includes a tremendous capacity for stresslessness. That is,

the fact that the experience, the perception, and the reaction just don't follow from the stimulus.

OPLER: It's a spectrum.

TOCH: I know, but I think that raises the more general issue of: are we, through a heading like "Stress and Crime," presupposing that the payoff is in the direction of finding positive correlations? My hunch is that insofar as there is an area here and assuming that crime is a very heterogeneous universe, there is probably more of a payoff in the direction of finding a contribution of nonstress experiences to crime.

It comes back to some of the longitudinal studies of parolees. And I think Mr. Thompson is probably encountering similar experiences. The most disheartening finding is that you have all these people with all these hypothetical problems, who somehow seem to have developed a capacity not to face their problems and not to respond to them as problems, and to sidetrack their adaptive or maladaptive, quasi-adaptive behavior, in such ways that a criminogenic situation exists. I would suspect that the capacity to avoid stress is a research concern that one ought to put in the forefront, as opposed to stress.

The other recommendation that comes to mind is something that was mentioned yesterday in passing, and that's if you want to discuss stress, if you want to look at stress, how about the victims? It would seem that's where a very prominent stress situation exists, the stress of the crime victim, the stress from the criminal justice system at every stage of the game. These stresses maybe can be ameliorated by slight changes in the system, like those exemplified by the treatment of rape complaints and witnesses but which apply to all kinds of people who come in contact with the criminal justice system.

REISS: I'm going to suggest we take our break and continue this a bit after. It seemed to me at one point that it would be a bit whimsical that Marvin (Opler) and Hans (Toch) were suggesting that the ideal person to study was Evel Knievel as the classical sociopathic personality. The other example, not quite whimsical, is that I sometimes upset the students in my criminology class by taking the movie "In Cold Blood," beginning it in the center after the arrest and playing it to the end, and then ending the movie with the crimes. It is very upsetting because the students don't like to be left with the victims; they would much rather be left with Truman Capote's ideal man at the end. We aren't used, in the movies

and television and the media, to being left with the victims at the end. We must always be left with the contrite offender. It's a paradoxical thing. The police get very upset in the criminal justice system at this. The judge always sees the nice cleancut lawyer-presented defendant, and that's not the way the cops remember him. The world is different, and it's something that we need to think about in this whole area. What kind of perspective ultimately we impose upon this reality. Let's take a break and come back in about 15 minutes.

(Brief recess.)

REISS: I want to give us every opportunity to explore the correlates when I suspect that we have probably almost a general agreement on the different strategies of research in a methodological sense, so I need less time for that. Most of the remarks have been directed to how people get into statuses, how people become "X" and yet, we're also faced with the age-old problem as to how people get out of statuses or kinds of behavior.

Coping strategies are looked at from the standpoint of the failure of the coping strategies. We're always looking for what gets people into criminal or deviant statuses. Yet, everybody has a sense that somehow an awful lot of these people end up in some other boxes as they age. If you go back in the drug studies and look at a few cohort studies that have been done, going back to the early Lexington ones, one of the things you find is, because the death rate tends to be high among drug users, not only from drug-related causes, but almost for every cause of death, they disappear, in part because they have much higher death rates.

It's a very tricky area in which to do the kind of "getting-out" studies, because of the selective attrition that occurs in these populations. I sense that we haven't talked very much about that kind of study. In some ways, it might be quite illuminating.

STRAUSS: I might say that we found that the same process applies in families. That is, in cross-sectional studies for each succeeding year in marriage, the violence rate goes down. But that's primarily because those marriages that are violent terminate more often than others. When we're done retrospectice studies, the results show that the level of violence in families does not decrease, but rather, continues, and in many cases increases. Sometimes it starts even before marriage and continues right through the marriage.

That brings up the question of how people cope with or control violence in the family. I think, the most widely used method is to terminate the marriage. But is there any way of reconstructing the marriage on a nonviolent basis? This is something that I think is urgently in need of research. A good site for such research are shelters for battered wives. Here you have a whole population of women who were beaten up by their husbands. Rightly or wrongly, they almost always want to preserve those marriages. They want to because there was something they found valuable in it and still find valuable in it though they don't want and cannot tolerate the violence to continue.

It follows that one of the research priorities for violence in the family is to investigate how people who currently have a physically violent relationship can end that without ending the marriage.

Does this have anything to do with stress? One aspect of the program in a shelter for battered wives in Portland, Maine suggests that it does. That shelter has a person attached to it to work with the husbands. It's one of the few places that do. These men have many life stresses. The fellow who does this is a sort of walking hot line. He just makes himself available to the men, to try and help them cope with those stresses, on the principle that that is going to reduce the level of violence.

On the other hand a very important issue for research on stress and violence was raised by Harvey Brenner. I think it may be worth repeating. We know from his data, and we know from other data, there's no necessary direct link between stress and violence. Since that is the case, we need to find out conditions under which stress leads to hypertension, the conditions under which stress leads to physical violence, the conditions under which it leads to depression. If we could identify those conditions, it would advance our understanding not only of the instances in which stress results in criminal behavior, but in which it leads to other unwanted types of behavior.

REISS: This is also keeping with Marvin's (Opler) suggestion on looking at the sociopathic personality.

HIPPCHEN: I think an imporant area to consider is this area of skills in coping with stress. If we assume that this kind of behavior, antisocial behavior, whether it's in the family, school, juvenile court or prison, is a failure of the individual to constructively cope with stress, then the identification not only of the factors leading to stress, but the skills

that could be develcped to help the individual to cope in a more socialized manner with these stresses is a very important kind of knowledge that we need.

In this area, I would hope that we would use our imaginations as well as the literature to develop a series of models. I particularly have been working with what I refer to as values training. There is certain literature now coming out of Harvard and Washington, D.C. here at St. Elizabeth's Hospital which suggests that the values training area is very important in aiding a person to cope with stresses that cannot be done so without the values training. I'm suggesting the development of training models which would train delinquents, criminals, family members and so on in coping skills so that they would not be overcome by stress.

FREEDMAN: One of the things I'm struck by is when we talk about coping and stress I think of a young person growing up in New York City, trying to cope with stress. It is not only that different people cope differently, it's that different situations we would generally group under the term "stressors" affect people entirely differently. So, for example, you will have someone who finds being in school very stressful. He's doing poorly, he can't stand it, it's boring, it's not a pleasant place; and he copes with that stressor by not going to school. He's coping in some sense, coping poorly from society's point of view. From his point of view, he may be coping okay.

Other people don't find that stressful at all. They like to be in school or they do well in school. Now, that same boy, it could be a girl these days, goes out on the street. Now, most of us would find the street life in the central city much more stressful than being in school, and I'm sure that even for these people it's stressful. But they cope with that stress in an entirely different way, and probably don't even think of it as stressful. That's the way they live.

And when we talk of stress in general, we have made the important point that it's how you cope with stress that's important. It's overly simplistic to think that there are styles of coping for an individual that apply to all stressors. They will cope with one stressor by ignoring it or denying it or running away from it. They'll cope with another stressor with violence or with being aggressive or with being a criminal. And it's obviously and extremely complicated, not a simple, situation. That's why I think adding up how much stress somebody experiences probably

isn't going to tell you very much about whether or not they're criminals. It's which stressors they respond to in which ways.

REISS: I suppose that research on behavior in extreme situations says that at a certain point all of us disintegrate under certain kinds of external environmental conditions.

STRAUSS: But it doesn't say what we do when we disintegrate. I think that's Dr. Freedman's point. For example, in the research by the two English sociologists, Brown and Harris, they find that lumping all stressful events together does not prove to be very explanatory. Rather they find that the kinds of stresses which lead to depression involve loss of social relationships, such as the death of a close person or an estrangement from a close person or loss of a job. Other types of stressors are not highly correlated with depression.

REISS: I think that's what is at issue, basically. As I look at the literature there are two approaches taken subsequent to the fact that there are stressors. One is that it is the numbers (of stressors) that makes the difference. There is a whole body of research that has tried to convince us that there is a convergence on a number and everyone will search for some kind of correlation. The second approach we were given in Lieberman's paper which implies that it is the particular tracking you take. For example you have to be exposed to work and that work provides stress, and then there are certain conditions related to that situation which leads to a particular coping strategy. At least that's what he's searching for.

It seems to me the suggestion here is that we need to see whether in fact it's simply numbers that make a difference. The other may be that there is a kind of stochastic process or at least it's accidental, in the sense of what stressors are available at a particular time.

BRENNER: There are really two central issues. One deals with particular combinations and interactions of stress. Items A, B, and C maybe lead to one picture, but a different interactional pattern may lead to another picture. That's one general issue. As important, however, is the following: If we're looking at something like cardiovascular disease, for which we have a rather extensive epidemiology, we can identify half a dozen fairly well known risk factors. Any textbook on the problem will identify those for us. It will identify, in other words, those persons who under ordinary circumstances would react with that kind of a pattern.

What we don't know from that literature interestingly is when they will react. We do know that, given certain background factors, given the smoking, the obesity, the drinking, and whatever else, that a particular pattern (cardiovascular disease) is likely among such individuals. Add to those kinds of things a particular group of stresses and you may have the general type equation that would predict or explain for such a population of persons, (a) that they were reacting in a particular way, i.e., cardiovascular disease, and (b) when it is likely that they would react.

The same is possible extending that analogy to criminological or depression responses, where we want to make that kind of comparison. Say we take a population, based on our predictor studies of background factors, of persons who tend to be found with criminal behaviors. Understanding that kind of basis for prediction in the population, we then again add the same or a different grouping of types of stresses and we may possess the kind of equation that will also identify the time as well as which populations are appropriate, and so across the board with other comparisons.

REISS: Your second point has important methodological problems. It is a bit like the cause of death problem. If you die of one, you can't die from the others. And that's a terrible problem here. You can have hypertension, alcoholism, drugs, on and on, altogether. And that kind of mix means that, if you die from one you aren't going to die from the others. If you die from suicide, you aren't going to die from homicide, and so on.

So the contingencies in this are very critical, and it has, from an epidemiological model standpoint, all the problems that are inherent in the cause of death problem. So death rates change because of other death rates.

BRENNER: Understood. That's fine, Al, and I agree completely.

REISS: It's hard to disentangle at that level.

BRENNER: But the implication must be that to answer this kind of question, at a minimum what we need is the more highly refined epidemiologic studies of criminologic behavior as another type.

REISS: We certainly need to record all these conditions as they occur together in given individuals, as well as their disaggregation.

BRENNER: And only then, perhaps will we be also able to make sense of the stress impact.

REISS: Right, I agree.

OPLER: In my paper and in other writings, I've already mentioned, these three types I've talked about, the types of disorders that are related to the central nervous system through the involuntary system. I'm following Franz Alexander who talked about hysterias where the voluntary nervous system (CNS) is involved and then the involuntary nervous system which are by and large the psychosomatic-related disorders; cardiovascular, hypertension, etc.

In my paper I also suggested a classification that starts with the passive sociopathic problem. The Skid Row kind of thing and alcoholisr were the most ready examples of that. In our city, for instance, we have an institution that receives Skid Row bums in the winter season. They came like the birds and they'd go into this place, and when the weather was better, they'd go out again and go on to Skid Row. They have a kind of passive adaption.

There's a third type that I referred to in my paper, and that is the active, acting-out strain in sociopathic behavior; that is not passive, that is acting out, that is explosive, that is violent. For instance, some people are involved in rape rather than in drunk, drink-yourself-to-sleep, patterns.

I think these classifications should be studied as such. They tend to be ways and styles in which people are already coping with stress. They're out there, the stress is out there, they have had long histories of coping with stress. In Midtown, for instance, we were very much interested in relationships between childhood mental health and adult mental health. There are relationships in a longitudinal sense that also can be strained out of studies. I'm saying again, you can study the types of so-called criminalistic behavior. It isn't all violent. Some is. And it isn't all psychosomatic. Some are.

TOCH: I just want to say the actuarial approach has one qualification. It is not at all inconceivable, which is why the extreme stress issue comes up, that in terms of this homeostasis concept, there are qualitative changes in the stress spectrum which transcend this additive model and introduce modifications. For instance, I'm not sure we are right in saying that, given

a sufficient extremity of stress, everybody succumbs. One of the interesting sidelights of the extreme stress literature, the concentration camp literature, for instance...

REISS: You mean like Bettelheim.

TOCH: With Bettelheim, certainly you have differences between middle class and Communist concentration camp inmates. But even with Bettelheim and Frankel, it became obvious that in concentration camps there were adaptive modes. In studies of people on icecaps and in submergibles, it became obvious that people do mobilize all kinds of coping resources. And, incidentally, that analogues of criminal behavior in these situations are qualitatively different. Concentration camps, for instance, have cannibalism. We had inmates acting like guards. These are criminal behaviors, stress induced. I feel the fact of the matter is, extreme stress produces extreme reactions. Those extreme reactions are not simply quantitatively different nor the stress simply added quanta to existing stress. I think they are breakdowns of the quasi-stable equilibria that had existed in the routine stress situation, that produce adaptive modes that are qualitatively different.

That's why I would prefer, if we're dealing with garden variety behavior, for the paradigms to include garden variety stresses. I do think that one point that was raised earlier today is important. I would think that there is a tremendous payoff, although it makes it difficult for LEAA because of its subject matter and focus, to study analogues in a variety of situations. Now, I don't know how one could go about this if one has a criminal justice emphasis and one has a hunch that many of the studies would take you from a criminal, crime-related population to noncrime related populations. These studies may involve schools, for instance, neighborhoods, in which you aren't just dealing with other people subjected to stress (control groups), but in which you see different styles of adaptation to common stresses.

CURTIS: Murray Straus and other people have suggested the need for new kinds of coping therapies, which I underscore. Bob Staples, in the paper yesterday talks about high stress and high violence in minority populations. It is interesting that minorities, poor minorities, don't tend to use therapies. There are a lot of reasons for that we might talk about. They range from the mundane, lack to money, to some people talking about values that don't provide them the desire to ask for therapeutic intervention. I'm reminded of a New York Times Sunday Magazine article this

last weekend on the black middle class, and one of the people who they interviewed said: "My God, we're even going to shrinks now. I mean, what's happening to us." Suggesting that just wasn't done among lower class minorities.

LEAA is starting a family violence program which might be able to address this, and HUD is getting into crisis intervention and conflict resolution in public housing. Most of the populations are minority. To me, this offers a very good environment in which to do evaluation research, which I don't think has been discussed enough here. I don't know if that's because it's seen as less sophisticated than applied or basic research. But we are going to proceed in this important area. I'd just like to ask people generally, and Bob Staples in particular, about their ideas on minority therapies relating to stress in particular, and on how to better research it.

REISS: Bob, do you want to respond to that?

STAPLES: Actually, I don't find the situation of minorities in relationship to stress and crime and violence lends itself to simple or brief explanations. I think that being a racial minority in a racially stratified society is inherently stressful, and, as I pointed out in my presentation yesterday, it really comes down to a question of the forms in which that stress is manifested. The fact that there seem to be fairly strong demographic variations, even within the minority community, as to how that stress is expressed, how in particular it comes to the attention of the authorities by arrest and conviction.

I am somewhat concerned about that, particularly in light of the earlier discussion here about the ethics of the research in prisons and so forth, primarily because of the demographic character of the prison population. I think the question that arises is why procedures that have potential for harm are done among fairly powerless and poor victims. The fact of a captive population always raises the question as to the voluntary nature of their participation.

Other than that, I'm not sure I can give you an easy or brief description of the therapies or the solutions to this problem, other than to raise the questions that relate much more to minorities, particularly since they constitute such a large portion of the criminal population that's been under discussion.

OPLER: I recently saw a black review in my town which had the name "I Can't Cope," and it was really satirical. It was a commentary along the lines of Dr. Staples' remarks just now, I think, insofar as the feeling goes. They were saying, in a lot of the songs, that they coped a hell of a lot. They had a lot of experience with coping under difficult and extreme circumstances.

To turn to something else that was just mentioned here, in the International Journal we published about four articles by Hans Luchterhan. He corrected Bettelheim to quite an extent. He studied as no one else has studied the concentration camp survivors. Now, I have had some experiences in the Japanese-American centers, where they threw the Japanese and kept them in for about three years during World War II. So I know what a kid glove version of a concentration camp is. I know what it does to suicide rates. We made all sorts of predictive studies in that center, and the predictions were pretty clear and they are in the Library of Congress, if anybody wants to look them up.

Luchterhan's findings were that the people who survived best in the concentration camp did precisely what Bettelheim denied. Bettelheim had the Freudian formula that you identify with the aggressor and you get by. Well, that's pretty hard to pull off in a concentration camp, and I have always wondered about it from my own kid glove concentration camp experience, studying Japanese Americans. I would say that the other formula is correct; the people that can keep on their feet and keep their heads clear are, of course, the ones that have had previous experiences in coping and dealing with difficulties.

Now, there are some people that we find who have committed offenses against the law who haven't been able to do that. I think that's what we're commenting upon when we talked about the so-called criminal element. Earlier, Dr. Toch said that there are some that are sort of smooth, that are in the con man category. Well, we know about those people, too, from the sociopathic literature. They delight, as a matter of fact, in fooling the victim. But they're not in this assaultive or highly acting out and harm-the-other person category.

I think all these various subtypes can be dealt with. What intervention techniques, crisis intervention or therapies are appropriate, is a very considerable and important question to which an agency of this sort should always address itself.

That is, how do you work with certain types in prison populations? Why not work with them? Why not study them on a totally voluntaristic basis without adding stresses to their lives, but in relating to them? I think it is possible to do so.

I know when the wonder drugs came into the market and people were studying schizophrenics, some of the studies showed that responses to the inert saline solution or placebos of that sort were very high, because these people were having contact more than they did in the average psychiatric center with caretakers, with intervenors, with doctors, nurses and so on, on a programmed, study basis.

I think this calls for research. I think it's worthy. I think it's defined. I think it's specific. It answers the questions which Dr. Reiss wanted us to answer. And I would applaud such efforts, but suggest that they be targeted at some things that we already know about, some things that we can define and get at.

THOMPSON: I'd like to go back to something I said yesterday that tied into the Morris Rule and tied into the idea that you can only die of one cause. The Vera Institute in 1972, started a supported work program for ex-addicts. We followed that program and research over three years. It is now being replicated in about 14 or 15 jurisdictions. Lucy Friedman directed that research and would be able to convey the following with more accuracy:

Something like one-third of the supported work participants dropped out of the program in the first year of the program. Their crime rates, as recorded by police rap sheet data, were considerably higher than those that remained in the program. Now, I don't suggest we know the causal directions here, whether the crime led to withdrawal from the program or failure in the program led to frustration and crime. It's clear to me, however, not being a social psychologist, that one would have an extraordinary difficulty deciding what behavior was due to stress, what was coping, what was expressive behavior as a result of frustrations.

To unravel that, even in a relatively simple controlled setting, would be very, very difficult. This poses a problem for the stress concept, and one which those of us who work in applied settings, as is true at Vera, need to be educated about. We need your help, those of you who are working with the theory, to help us unravel this complexity.

The reason that it applies to the Morris Rule, of course, is that it is likely that employment is a stress-inducing experience for some ex-addicts and school dropouts. In effect you take a population whose long-term employment level is about 30 percent, (we know that from the control group), and give them all a job on the first day of a program, and then many participants live through the experience of losing that job. This leads to disruption of role relationships and life events indicated by the various scale items that we reviewed yesterday.

Clearly, you have brought stress into program participants' lives. I don't think those kinds of action programs can be accomodated to the Morris Rule. On the other hand, the question arises of "dying of more than one cause." That is to say, it's also likely in a longer-term setting or just in terms of the complexity of what's going on on the street, as against institutional or program settings, that other stress would have arisen. That of course, one cannot know about except by inferring counterfactually. One can certainly try to document this better, in terms of control groups. The problem is not a simple one even in terms of assessment of the amount of damage that's done to the third of the group that drop out.

I also should add that another third stay in the program on a long-term basis, go into unsupported employment, continue to have income, more stable marriages, etc. That is, there is good being done and there is damage being done, probably. We don't know in advance who, of course, will be affected in which way. This would be an area in which an empirically based stress theory would be greatly helpful to tell us how to monitor these efforts better.

TOCH: One addendum to this is that of accentuating the positive. That is, the same studies that provide those clues also provide clues to the opposite. For instance, in the Lucy Friedman study, those individuals who were engaged in human services work which was more meaningful to them did comparatively well. Also, the married men did comparatively well. Quite obviously, there are issues here having to do with what is a meaningful support system and what builds coping competence, which are just as important as clues to stress. And I think we have to attend to those issues as well, and maybe more.

THOMPSON: That's one of the tactics that I'm taking up in Vera's employment and crime longitudinal research, trying to identify types of employment situations, the quality of work and so

forth, that would affect the criminogenic or crime-averting qualities. I might say that here on this issue, that the traditional manpower literature is far better at tabulating employment rates and labor force participation rates and so forth than it is in characterizing employment in these kinds of terms, as being stress-inducing or not. The problem is something that Harvey Brenner was mentioning. In a high-risk, inner city, younger population with relatively low levels of so-called human capital, the variation of employment, which is available in natural settings, is quite reduced, so that it's very hard to analyze quality of employment.

You mentioned human services. Those were actually impressionistic findings in the Vera research, because very few participants were able to be in that kind of employment setting. So we do have a problem of finding enough variation in employment, and also being able to measure that variation in terms of stress-relevant characteristics, rather than income and longevity of employment and so forth.

REISS: You also have the problem that there is relatively little life history or work history data to look at movements across different types of situations. That is, the work history data have not generally been geared toward that kind of problem. It seems to me for the kind of thing that you're talking about, you'd want to look at changes in work history as well.

It always struck me, along that line, that the so-called fire insurance programs for minority youths in cities were exactly the most disastrous kind of program you could imagine. That is, to pay kids for the summer and not pay them the rest of the year. Nothing makes people more stressful than to get them used to a lifestyle and then take it away from them three months later.

There are all sorts of things like that that go into an idea of the support system.

I want to pull together one observation I had in the last day and a half, and that is that we have avoided the problem of minority and sex composition in this area. That is to say, except for Bob Staples drawing our attention to a critical aspect of this in the minority context, there hasn't been anyone here who has reminded us of the critical difference here within our own society between men and women.

What I want to suggest is that we need designs which particularly explore the conjunction of these variables (black women, for example). It is not simply that the way one looks at the minority problem is usually in terms of black men, but that we want to look at whether multiple minority status have separate and conjunctive effects.

Are there significant changes underway in the society with respect to the crimes of women and violence on the part of women? There are some very knotty problems, and I think that should be a part of our research agenda. Some money ought to be set aside for that. And as I say, the conjunction of race and sex is, I think, a critical aspect of this, both theoretically and empirically.

Well, I want to shift for just a few minutes to the other question, one on which we may have a fair amount of consensus and summarize my notes. That question has to do with research design. It seems to me that we see at least five types of design problems. One, which Marvin (Opler) has reminded us of repeatedly is what might be called the multi-causal model being studied best in a community situation. That design permits one to assemble certain macro-data, while exploring the micro problems in some detail.

A second design which we have not discussed very much, is the one Lynn (Curtis) called our attention to, the evaluation research design. I take it, one reason why we haven't is because we thought of this as a basic research program, one that would be illuminated less by evaluation research designs. Nonetheless, it remains one to think about.

The third is the longitudinal design and particularly the longitudinal cohort design, and of how such designs might illuminate a whole series of problems simultaneously. Let me put it this way: I always have great visions that the National Crime Survey one day will be like the Current Population Survey in the sense that there will be monthly cross-section supplements that investigate selected issues. The longitudinal cohort design would be one in which we follow the people in households that leave as well as those that replace them in a given location. That's an ideal design.

The next problem was what we call a level of analysis problem. I think there's general agreement that we have to pursue this problem of the relationship between the highly aggregated explanatory system and how that relates to the disaggregated

data. It's not a question, as Murray (Strauss) said yesterday, of one versus another. Both are aspects of the reality. One is more oriented toward explaining the trend and changes in it and, the other toward a micro-causal system.

Then, finally, there is what someone called the counterfactual problem, which I call the problem inherent in the epidemiological model, that not all alternatives can occur for any individual. It occurs in the form of the death rate; you can only die of one cause which in turn has consequences for the death rates from all other causes. And it will occur, in a sense, when we start looking at those coping alternatives and the consequences of them, not only because there are two death rates, namely suicide and homicide, but because some of the others wouldn't appear very likely to be related to one another.

Those are the model problems as I see them. There are two critical problems at this point. One might be called the levels of analysis problem, since there is confusion about what a macro-explanatory versus a micro-explanatory system covers and what are their implications for social policy and social change.

And the second is, if we think of the causal problem as central, the longitudinal designs will illuminate that problem best.

That's my summary.

HIPPCHEN: I would recommend that there be two major approaches to the problems. One would be a causal type of research and the other would be applied research, where there is an attempt to ameliorate the symptomatology. In other words, if a person is not reacting well to stress, some models can be developed to help this person cope with stress or develop more socialized types of behavior.

We may not know all the causes, but we may be able to develop techniques for helping the person to cope more successfully. And this is where the evaluation model, I think, comes in. We need this two-pronged approach: one, looking at causal variables; but, on the other hand, I don't think we can afford to wait until all the variance is accounted for. I think we should move definitely into applied models where we're dealing with symptoms more and trying to ameliorate these symptoms, and testing models with the evaluation type of applied research.

REISS: Any other final last words on these questions?

THOMPSON: On the issue of levels of analysis, one thing we can do that we don't do is to control the kinds of imagery, the causal imagery that we use in discussing aggregate data. In the economics profession, the Chicago School and so forth, almost exclusively verbal descriptions of aggregate findings are couched in terms of "choices" between alternatives--tradeoffs, cost-benefits, etc.

That imagery never suggests the presence of indirect linkages between, let's say, unemployment and crime, as would be revealed through such things as stress on the family unit as a whole, followed by crime among some members of the family who are not in the labor force. There's not any suggestion of anything other than a direct, mechanical, knee-jerk reflex between the unemployment status and crime behavior.

That imagery is in no sense justified by the data which is being looked at. Yet, the policy implications that come from the data analysis really rely more on the imagery, on the verbal accounting or the theoretical agenda, than it does on the model. I think especially policymakers who review this research or are exposed to summaries of it, as well as researchers themselves, need to be much more careful in their rhetoric, if not in their data analysis.

FREEDMAN: There's one design that you didn't discuss very much, and I guess that's because it's almost never done. It occurs to me that we could do what we might call quasi-experimental designs, or be prepared to do them. For example, if you want to study the effect of unemployment on the crime rate, you can set up a study to look at the effect of unemployment and wait for a situation in which people are unemployed or in which, in fact, the whole unemployment rate suddenly drops, and you then step in.

In the longitudinal studies, you run into these people who got unemployed, but there, although you may get at the causation, there's still the problem that when a few people are chosen to be fired, they may self-select themselves. So it's hard to know.

But if, for example, a factory closes and you can then look at 500 people in that town and immediately say, well, does the crime rate go up in town the next week? It's not a true experiment, but it's a quasi-experiment. If you have the notion you will look around the country for these kinds of studies (I don't think that they're easy to do) but one thing we know is that you can't do them if you're not prepared to do them ahead of time.

BRENNER: I think one central question on the issue of level of analysis is to what populations you wish to generalize, either as a matter of decisionmaking or generally as a matter of contribution to knowledge. If, for example, you wish to know something about the United States, it may be necessary to make comparisons with other countries that afford you the opportunity of understanding how it is, if at all, the United States differs as a country from other places. That's virtually the only way you're going to know about it. The same is true on a city basis, or on a regional basis, or on a state basis.

So the general point is: the level of aggregation is specific to the character of generalizability that one wishes.

Now a second point is that it is very possible that the types of variables that would be operative causally at different levels of analysis will be different from one another. We can even observe that on the small-group level. The factors which are important to interaction in a two-person group are different from those in a three-person group are different from those in a five-person group, are different from those in a crowd, let alone an organization, a family system, a country and so on.

Following that, in my opinion, it is necessary to understand what the usual predictors, the normal factors are, that tend to influence a particular outcome, say homicide, at a particular level of analysis, if one is to get the understanding of what any additional contributing factor happens to be. There is a set of factors, a kind of epidemiology, to put it another way, at each level of analysis which is predicated to a specific kind of outcome.

The outcome must be made as equally specific on an aggregate level of analysis, of whatever size, as it is in comparisons using individual person-based analysis.

STRAUS: I think we shouldn't give up on the idea of true experiments, including those under highly controlled laboratory situations. Even with the consciousness that we now have about the rights of subjects, and concern for inducing stresses, it is possible to carry out laboratory experimental studies of how people respond to stress. There was a whole flurry of research in the 1950s on this, which we could profitably look at again, as well as some new experimental paradigms that are possible.

For example, I am working on a person-computer game as an experimental paradigm for doing experiments on physical violence

in the family. I obviously can't have couples come in my laboratory and hit each other. But I can get individuals involved in a game in which the computer plays the role of a spouse. These are very engrossing, very involving. In fact, students and faculty tie up our computer, because they're playing Star Trek and other games which involve stresses. People seek out stresses to a certain extent.

If there are sufficient response alternatives in the game or in some other experimental paradigm, it offers the possibility of finding out a lot about both the kinds of stresses, circumstances, resources and alternatives that lead to either a violent response, a depressive response, or a normal coping response to stress.

REISS: I'm going to stop here. There are just a few concluding odds and ends. First, Elly (Chelimsky) has a few things to say.

CHELIMSKY: I would just like to thank everybody for having come to this and having lent us your wisdom in these areas that are, you know, very, very difficult for us to sort through.

REISS: I think we certainly ought to thank Addie Normandy for having done such a nice job with the arrangements in taking care of us so well, and Elly (Chelimsky) and the LEAA staff for hosting this and giving us an opportunity to address this very exciting topic for the past day and a half.

(Whereupon at 11:55 a.m., the meeting was adjourned.)

APPENDIX A

A COLLOQUIUM ON STRESS AND CRIME

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Prepared under Grant Number 78-NI-AX-0053 from the National Institute of Law Enforcement and Criminal Justice, Law Enforcement Assistance Administration, U.S. Department of Justice.

A colloquium on the topic of Stress and Crime is being sponsored by the National Institute of Law Enforcement and Criminal Justice to take place December 4th and 5th at the Sheraton National Hotel in Arlington, Virginia. It is the second in a series of colloquia attempting to identify the basic research which needs to be performed in connection with the Institute's effort to illuminate and to better understand the correlates of crime and the determinants of criminal behavior. The MITRE Corporation, through a grant from the National Institute, conceived and organized this colloquium.

It appears that the use of the concept of stress as a correlate or as a causative factor in the etiology of criminal behavior holds great promise. In order to explore this promise there are at least three major questions which must be addressed:

- What are the theoretical and operational definitions of stress?
- What are the empirical relationships between stress and criminal behavior, crime rates, other antisocial deviance? and
- What are the theoretical linkages between factors that create stress and the adaptations that we label crime or deviance?

The concept of stress has been defined in several contexts: biological, psychological and system. In general, definitions have included perturbation, imbalance and adaptation components. The idea of stress proposed by Selye was focused on biological systems and was defined as a syndrome thus including the response (adaptation) as part of the definition. The response, called the General Adaptation

Syndrome, was a sequential process by which the body reacted to stressors (external and internal stimuli which upset the homeostatic balance). Disease and other maladaptations were interpreted in terms of the General Adaptation Syndrome.

Broadening the stress concept and extending it into the social and psychological realm, the Midtown Manhattan study defined stress as "the environmental force pressing upon the individual." Another author, Engel, defined stress as:

> ...any influence, whether it arises from the internal environment or the external environment, which interferes with the satisfaction of basic needs or which disturbs or threatens to disturb the stable equilibrium.

Stressors encountered in varying life situations may range from military service to unemployment to divorce or to the death of a loved one. The Midtown Manhattan study isolated fourteen stress factors which the authors found to be important in mental health outcomes. These were:

(1) Parents' poor physical health
(2) Parents' poor mental health
(3) Childhood economic deprivation
(4) Childhood poor physical health
(5) Childhood broken home
(6) Parents' character negatively perceived
(7) Parents' quarrels
(8) Disagreements with parents
(9) Adult poor physical health
(10) Work worries
(11) Socioeconomic status worries
(12) Poor interpersonal affiliations
(13) Marital worries
(14) Parental worries

If one accepts the idea of stress as involving stimuli which upset or threaten to upset a state of balance, it follows that all individuals, groups and larger systems (such as organizations) experience stress. However, it is probably important to discriminate between stimuli that require major adaptations (stress inducers or stressors) and those to which the individual or group may adapt easily, that is: without major change. Any living organism or system is constantly adapting to internal and external stimuli, and not all of them can be considered as stressors. Therefore, if stress is synonymous with all adaptation, the concept is probably not very useful in dealing with crime and other deviant behavioral adaptations.

It is axiomatic, but important to note, that the stimuli (or conditions) as well as the adaptations to these stimuli vary from individual to individual and group to group. Among the major factors which condition both the responses (adaptations) to stress inducers as well as the determination of what those stress inducers will be for a given individual or group are, first, what the Midtown Manhattan study referred to as "endowment" (that is, the organism's biological inheritance, experiences and learning, prior adaptations, cognitions, etc.) and second, the context in which the organism (individuals, groups, organizations) exists. Thus, in the study of stress and crime (as one adaptation) one must seek both commonalities and individual differences in both stress inducers and adaptations.

The colloquium will explore, from different perspectives and perhaps varying definitions, the relationships between stress and criminal behavior. Given certain definitions of stress inducers, can criminal behavior be related to the type, degree, and extent of these stressors? If such empirical relationships are discovered, does the concept of stress (including criminal behavior as an adaptation) add to the ability to theoretically explain and predict such behavior, and/or to develop policies for dealing with crime?

Several important empirical questions concerning stress and crime may be raised:

- Are individuals and/or groups who commit more criminal behavior more likely to be subject to stimuli defined as stressors or stress inducers?
- Are certain stressors more likely to produce criminal behavior as part of the adaptation than others?
- What are the characteristics (biological, historical, cognitive, social) of individuals and groups that influence the determination of which internal and external stimuli will be stress inducers and demand major adaptations?
- What are the characteristics of individuals and groups which differentiate types of adaptations (criminal/non-criminal) to stressors?
- What are the external conditions (context) which influence the types of adaptations?
- What is the relationship between criminal behavior (perhaps including other forms of antisocial deviance, such as drug abuse or alcoholism) and other adaptations (e.g., physical disease or mental illness)?

The answers to these and other questions require a vigorous program of research as well as re-analysis and re-interpretation of

already existing data and theoretical formulations. It is hoped that the colloquium may be a start in providing the answers as well as suggesting needed inquiry.

If and when definitional problems concerning stress are solved and clear empirical relationships established between stress and crime, a further problem remains. This may be stated in terms of developing the theoretical linkages which provide an explanatory basis for the relationships discovered. For example, one may ask what are the biochemical, neurological and psychological processes (cognitive, emotional) which lead from the presence of stressors to the criminal behavior? How does a stimulus (or condition) become a stressor? The theoretical linkages are important in that they would provide a basis for using the concept of stress as an explanation for criminal behavior. Without these linkages, it may be more useful to consider the stress inducers themselves (poverty, biological abnormalities, special events such as loss of a job or intrafamily problems) without necessary recourse to a stress concept to explain criminal behavior.

It is hoped that the colloquium will address the definitional, relationship and theoretical questions with regard to stress and crime, as well as methodological problems. The latter include how to measure stressors prior to and apart from the response (adaptation), and how to determine the extent to which non-stress-related factors tend to facilitate or inhibit criminal behavior.

One possible way to organize a research agenda in the area of stress and crime--depending, of course, on the results of the colloquium--might be along the following lines, directed:

- at specific individuals in high-risk (that is, highly stressed) groups (these would be basic studies of the determinants of crime involving, for example, juveniles, low SES subjects, recidivists, violent offenders, criminal psychopaths, and also, police, military, etc.)

- at specific groups (crime-correlate studies targeting such groups as poverty-level families, families headed by a single parent, families with violent behavior, children of criminal parents, subcultures such as groups which have high crime incidence, etc.)

- at specific geographic locations (for example, high crime rate areas of cities, specific housing projects, places with certain ecological-architectural characteristics, neighborhoods with small stores, high risk businesses, etc.)

- at specific offender groups (violent offenders, arsonists, white-collar criminals, repeat offenders, career burglars, armed robbers, etc.)

- at specific variables (autonomic nervous system reactivity, early interaction with parents, demographic characteristics of neighborhoods, early signs of deviance, sanctioning practices of the local jurisdiction, etc.)

- at processes (relationship between school performance and delinquency, how deviant behavior is learned, what are the psychophysiological responses of offenders under stress and in interpersonal situations, how does the role transition from youth-student to adult influence criminal and other deviant behavior, etc.)

- at specific offenses (violent offenses, embezzelment, burglary, rape, etc.)

A further specific possibility might involve a replication of the Midtown Manhattan study, in an effort to isolate stress factors important in criminal behavior and to analyze the varying response to those factors (much as the earlier study did with regard to mental illness).

Speakers at the colloquium will represent a variety of disciplines and approaches. Among the topics to be presented are: biochemical and neurophysiological factors in crime and violence; stress and assaultive behavior in the family; stress among prison inmates; crowding, stress and crime; and life events and crime. We believe that from these presentations, as well as from the discussions to follow, the colloquium can lead to meaningful research ideas which may be able to shed light on the etiology, maintanance and prevention of crime.

END