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Colloquium on the Correlates of Crime and the Determinants of Criminal Behavior

PROCEEDINGS

March 30-31, 1978
Arlington, Virginia

CENTER FOR THE STUDY OF THE CORRELATES OF CRIME
AND THE DETERMINANTS OF CRIMINAL BEHAVIOR

NATIONAL INSTITUTE OF LAW ENFORCEMENT
AND CRIMINAL JUSTICE
LAW ENFORCEMENT ASSISTANCE ADMINISTRATION

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PROCEEDINGS

Edited by

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FOREWORD

When Helen Erskine first approached me about the Colloquium on the Correlates and Determinants of Criminal Behavior, I was delighted to hear that LEAA was interested in sponsoring such a venture. But when she asked me to chair the meeting, I was somewhat apprehensive, for an interdisciplinary mix of productive scholars usually involves difficult dynamics of social interaction. A meeting of persons even from the same discipline commonly produces personality clashes, positioning efforts within hierarchies of displays of erudition, provocative insights, new perspectives. By adding the ingredient of varying disciplines, intellectual gaps could promote more conflicting commentary, even ideological and major methodological differences.

Yet, partially because I long ago committed my professional interests to efforts to promote interdisciplinary research, rather than only multidisciplinary oblique assaults on a phenomenon, and partially because I have participated in several prior interdisciplinary meetings abroad and engaged in research involving at least a duet of disciplines, I was pleased to accept the challenge of this Colloquium.

The background papers to be presented in advance were comprehensive in scope, high in quality, written by respected members of the research academy. The oral summaries of these papers were succinct and set the stage for the dialogue of disciplines here recorded as the proceedings. The titles of the topics and the names of the writers appear elsewhere. I wish only to comment on the character of the proceedings in order to entice the reader to peruse both volumes carefully.

The papers stand as independent contributions from the research experience of each writer. They represent some of the best thinking available on the assigned topics. They may be read before or after the volume of the proceedings.

There can be little doubt that the dynamic flow of interaction is most exciting in the transcript of the proceedings. Everyone who has been to colloquia of this or similar sort knows, during the process, whether there is tension, polarization, elevated intellectuality, mutual respect, effervescence. I became aware, early on, that this was a conference characterized by most of the virtues. We were not seeking consensus for its own sake, but an amazing amount of agreement did occur about research priorities. More than muted polarities occurred here, which is why I strongly encourage my colleagues in all disciplines concerned with deviance and crime to take time from busy schedules and their piles of papers to spend an evening with these volumes.

None of my earlier fears about such a Colloquium found ground in this meeting. We were a symbiotic group, mutually respecting, mutually interacting.

Criminology and criminal justice, however similar or overlapping their interests may be, converge around the meaning of pure, basic or fundamental research. These volumes both enrich and inform our desire to develop the relationships between the internal and external influences on human behavior that lead to criminal aggression and violence. These volumes represent an effort to begin the process that provides interlinkages between physiology and psychology, sociology and the centers of the brain, learning disabilities and the limited repertoire of articulated responses to frustration, blocked goals and limited opportunities.

MARVIN E. WOLFGANG
UNIVERSITY OF PENNSYLVANIA

SUMMARY

This colloquium was convened by the National Institute of Law Enforcement and Criminal Justice's newly created "Center for the Study of the Correlates of Crime and the Determinants of Criminal Behavior." The primary objective of the colloquium was to have the assembled researchers delineate the most promising trends in basic work on criminal behavior and to identify data needs and future research directions; these findings would then contribute to the formulation of a research agenda for the Center. The MITRE Corporation is assisting the Center in developing this research agenda.

Dr. Helen Erskine of the National Institute was instrumental in planning this colloquium. She selected the topics and invited the speakers. The two-day meeting, held in Arlington, Virginia, on March 30-31, was chaired by Marvin E. Wolfgang, Professor of Law and Sociology at the University of Pennsylvania. Participants in the colloquium were researchers from a variety of disciplines, including criminology, psychology, sociology and medicine. Their affiliations cut across academia, federal government and state agencies. Perspectives represented reflected ideas that have generally been agreed upon as contributing understanding and insight into the etiology of criminal behavior. The colloquium agenda covered a broad range of correlates and determinants of criminal behavior: environmental influences (such as overcrowding, neighborhood stress, family life), dropping out of school, early childhood deprivation, psychophysiology, psychopathy and the role of drugs and alcohol.

Each participant was asked to prepare a paper for advance distribution to participants and discussants, thus providing prior familiarity, facilitating group discussion and encouraging the generation and synthesis of research ideas. During the colloquium itself, only summary presentations of the papers were given, freeing a greater period of time for the exchange of views and the generation of recommendations.

Introductory Remarks

Mr. James Gregg, the Acting Administrator of the Law Enforcement Assistance Administration, and Mr. Blair Ewing, the Acting Director of the National Institute, delivered the opening remarks at the colloquium. They both stressed the same important idea: the colloquium was the start of a truly unique venture for the National Institute of Law Enforcement and Criminal Justice, reflecting a new and significant priority of the Institute--the support of fundamental inquiry into the correlates and causes of criminal behavior. Both speakers indicated that it was, in their minds, the beginning of a long-term commitment to pursue basic research that would help dispel the fog surrounding criminal behavior by synthesizing findings to enhance the state of understanding of criminal behavior. What might hopefully result in the course of Institute support of such basic research would be the development of a highly discriminating classification system of criminal behavior types that would allow for offender-population size assessment, an indication of the prevalence of criminal activity within any one classification group, an etiology of criminal behavior within a specific classification category and diagnostic tools relevant to each category. The speakers viewed the colloquium as an opportunity for the Institute to receive the advice and counsel of renowned researchers in the development of the basic research agenda incorporating in this way the contributions of many inter-related disciplines.

Summary Presentation of Papers

To facilitate discussion, participants and their work were grouped by common threads of inquiry. Loosely defined, the three groups were labeled as Group A--psychophysiology, Group B--effects of drugs and alcohol and Group C--biosocial influences. While there was overlap and several papers could have been placed in one of two groups, the assignments were thought of as the most appropriate in ensuring productive discussion.

Group A

Four papers were presented under the rubric of "psychophysiology." The presentor and titles were:

- Sarnoff Mednick --You Don't Need a Weatherman
- Robert Hare --Psychopathy and Crime
- Kenneth Moyer --Physiological Determinants of Human Aggression; and
- Russell Monroe --Episodic Dyscontrol in Criminals.

Seeking to focus attention on the prevention (and not the treatment) of criminal behavior, Dr. Mednick began his presentation by drawing a parallel between medical science research efforts to control certain diseases and social science research attempts to control crime. In both cases patchworks have been found--drugs to reduce suffering or improved management and security techniques for criminal justice administration; in neither case has the needed learning taken place to prevent the occurrence either of the disease or of criminal behavior.

He reported on several studies he has conducted in Scandinavia that indicate: (1) that there is evidence of a small genetic contribution to the etiology of criminal behavior and (2) that nervous system differences between criminals and non-criminals are not the result of exposure to the criminal justice system but could be used to predict such exposure. He suggested that by looking at the interaction of these biological factors and social variants, some individual characteristics can be used to predict criminal behavior. To develop the predictive value of this interaction effect, he recommended a longitudinal study involving an intensive assessment of first offenders, tracing their future involvement in criminal activities, determining what measures are predictive of continued criminal behavior and then a follow-up with a replication of the measures and a test of their ability to predict. With the establishment of the ability to predict criminal behavior, it would then be possible to work on prevention.

In summarizing his paper on "Psychopathy and Crime," Dr. Hare highlighted the need for a stringent definition of the personality type known, among other labels, as "psychopath," particularly if this concept is to be useful in understanding criminal behavior. This point was made all the more salient by comparing the results of using his criteria for determining psychopathy and those results obtained by using the DSM-III criteria.* This latter set he described as too liberal, leading to too large a population being defined as psychopathic. With more restrictive criteria (such as his own) for determining the application of the label "psychopath," Dr. Hare suggested the concept can be useful in understanding criminal behavior, as there is a known relationship between psychopathy and future criminality. Additionally, this criminal behavior is quite consistent over time. Further, he discussed some of the apparent biological traits common to psychopaths, among them abnormal amounts of slow-wave activity in EEG recordings, a slower skin conductance response recovery than for non-psychopaths, and poor electrodermal conditioning but good cardiovascular conditioning.

Dr. Moyer summarized his paper on "Physiological Determinants of Human Aggression," by presenting his general model of aggressive behavior: special neural mechanisms which, when fired in the presence of a relevant target, result in aggressive, though not necessarily overt, behavior. Drawing on animal studies and some work with humans, Dr. Moyer discussed the use of lesions and implanted electrodes to control both the aggressive neural system and the neural system responsible for sending inhibiting impulses to the former. Further, he discussed the hereditary influence on aggressive behavior (as individuals

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*American Psychiatric Association Diagnostic and Statistical Manual do inherit neurological characteristics such as thresholds) and the effect of other biologic factors (e.g., hypoglycemia, testosterone levels, allergic reactions) on the production of aggressive behavior. Finally, he pointed out that in addition to physiological means of curbing aggressive behavior (lesions, electrodes, drugs), all biological determinants of aggressive behavior are subject to the learning process, and that, therefore, through manipulation of that process, aggressive behavior could be affected.

The last presentor in Group A, Dr. Monroe, reviewed his work on a two-dimensional classification of criminals based on: (1) a dyscontrol scale (high and low) and (2) EEG abnormalities in theta waves (high and low). The classification produced four groups: epileptoid, hysteroid, inadequate psychopath and pure psychopath. The various group characteristics (e.g., thinking capability, motor skills, neurologic signs, interpersonal relations, etc.) and intergroup differences were briefly pointed out. He stressed the value of the classification in terms of its prognostic, diagnostic and therapeutic implications and the merit of using a multi-dimensional approach as a research strategy.

Several specific suggestions were offered by the participants in Group A as areas which, if pursued, could lead to greater understanding and to potential treatment and prevention techniques for criminal behavior. All stressed the value both of longitudinal and interdisciplinary studies. Subject areas of suggested study included the early detection of high-risk individuals, left/right hemispheric differences and their relationship to crime, the positive and negative affective systems and how they relate to an individual's interactions with the environment (with implications for preventing aggressive behavior) and determining the characteristics of differing criminal populations in multi-dimensional classifications.

Group B

The papers under discussion in Group B were linked by a common concern with the relationship of the use of drugs and of alcohol and criminal behavior. The three topics presented and their presentors were:

- Richard Blum - Toward a Developmental Approach in Criminology: Clues from Drug Studies;
- Jerald Bachman - Delinquent Behavior Linked to Educational Attainment and Post-High School Experiences; and
- Lee Robins - Alcohol and Crime in Veterans.

Summarizing from drug and school studies conducted over 10-15 years, Dr. Blum discussed the process of drug use and criminal behavior as a means of self expression, and stressed the importance of looking at developmental sequences and epochs, beginning with grandparental values, conduct and criminality of parents and finally focusing on the subject generation. Through such a sequential process, he suggested that it is both possible and profitable to identify transition points, variables associated with each period, and the etiological consequences of the developmental periods. Finally, he submitted that, in adolescence, it is possible to see the emergence of attitudes that relate to kinds of drug use and the importance of this derives from the belief that a child involved with extreme drug use is at higher risk of becoming delinquent.

While focusing on the causes and consequences of dropping out of high school, Dr. Bachman's talk also examined the relationship of drug/alcohol use to delinquent behavior. The main conclusion of his work, however, is that the behavioral differences (i.e., delinquency and alcoholic consumption and smoking) that one finds among groups with differing amounts of educational achievement were present prior to arriving at these varying levels of achievement. Further, he indicated that it may be unemployment and not dropping out of school that contributes more to delinquent behavior, as the level of aggression for the unemployed was well above average. Based on his conclusion that delinquency comes before dropping out, he suggests that remedial education efforts should begin prior to high school, as should future research in this area. Finally, he pointed out the need for developing a better employment eligibility criterion than a diploma.

The final topic in Group B, concerning alcohol consumption and deviant behavior of veterans, was presented by Dr. Robins. One of the problems, she pointed out, in studying alcohol and crime is deciding if one is looking at the effects of intoxication, where one might do something illegal when intoxicated, or the effects of alcoholism, where the illegal or abnormal act could occur during a period of alcohol deprivation. Thus, she suggested the need for tight criteria in assessing the relationship of alcohol and crime. Based on her work, Dr. Robins drew several conclusions. One, specifically relating to veterans' alcohol use, is that there is a strong correlation between daily heavy drinking and being arrested; however, daily heavy drinkers were more deviant before they went into the military service. Two, the absence of heavy drinking is a good predictor of not having an arrest record. Third, that if a subject drinks <u>and</u> has a predisposition to deviance, the deviance is more likely to get expressed; without that predisposition, it is possible to drink heavily and not get arrested. Thus, she concludes that while alcohol may be a convenient way to identify individuals at high-risk of arrest, very often the alcohol consumption is not the major cause of an individual's being in trouble.

Participants in Group B suggested some specific areas for future work. Again, as in Group A, an emphasis was placed on the value of and need for longitudinal studies. Some ideas for future research were: to work on the identification of developmental epochs, specifically designed to determine antecedent and correlated variables for distinguishing deviant paths or syndromes with specific outcome criteria; and to sort out the impact of the social environment and experiences on the development of deviant behavior.

Group C

Contributions of biosocial factors to the development of criminal behavior were the common thread of subject matters discussed in Group C. The three presentors and papers were:

- David D'Atri - Psychophysiological Responses to Crowding in Prisons;
- Lorraine Perry - Urban Families and Assault: A Framework for Research Focused on Black Families; and
- James Prescott - Early Deprivation and Criminality.

Dr. D'Atri looked at the physiological and behavioral effects of the prison environment on inmates by expanding upon an original effort to replicate with humans the studies of crowding and blood pressure done with animals. He conducted a study which looked at the effects of crowding (defined by prison housing type), duration of stay, and attitude (toward prison guards, sense of security, and privacy). The data suggest that blood pressure was initially high (due to anxiety), dropped in the two weeks following entry (the result of habituation) and rose as time progressed (a possible reaction to crowding). Dr. D'Atri pointed out that those with extreme views--either positive or negative--had higher blood pressure (due either to overt or suppressed hostilities) than those with intermediate perceptions. The impact of entering prison, as a producer of anxiety, was further reinforced when Dr. D'Atri compared the scores of prisoner psychological well-being against the scores of others recorded in an anxiety study by the National Health Interview Survey. Finally, a note of caution was provided--that blood pressure, as a state variable, is an index of reactivity and not a predictor of crime.

In Dr. Perry's presentation, she described her research design in examining conflict-motivated crimes in families. Her design took into account two interactive processes: the specific residential environment of a family and the intra-familial forces that facilitate and/or constrain conflict and violence. Viewing the family as a system of conflict management and change, it was stated that the familial configuration must then influence that management process. She suggested that more knowledge is needed on how that configuration can ease or restrain violence, how sexual perceptions and expectations and the failure to fulfill these can lead to aggressive behavior, and how children are used in, or themselves create, a conflict environment.

Dr. Prescott used a film presentation to highlight his study of early deprivation. The film, showing animal and human studies of the effect of sensory deprivation, was presented in support of Dr. Prescott's neuro-theory of isolation-aggression which is based on the central role of the cerebellum in mediating sensory and emotional processes. In his view, immobility (deprivation of movement) is central to understanding the effects of sensory deprivation. Such deprivation in modern American culture, indicated by a lack of touching, carrying, and the withdrawal of parental affection in early childhood, has produced a society that is physically violent and self- and other-destructive. Dr. Prescott emphasized that the demonstration of affection and the ability to experience pleasure is paramount in understanding aggressive behavior: individuals who are so sensorily deprived cannot relate in positive pleasure modes and turn to violence. Often this violence is sexual in nature, as he pointed out that sex variables which control how much affection and pleasure we experience are most strongly tied to the expression of violence.

The presentors in Group C generated several recommendations for further work in each researcher's respective area of interest. A specific area for work was the study of the relationship between pulse rate and housing in the prison environment. In addition to areas previously cited focusing on family conflict, it was suggested that family dynamics within the home be looked at for the potential development of intervention strategies and coping mechanisms for responding to family violence. Once again, as in the previous two groups, the value of longitudinal studies was emphasized.

DISCUSSION SUMMARY

Several themes indicative of the common interests and concerns of the colloquium participants permeated the two-days' discussion periods. The subjects of discussion ranged from the problematic and alienating aspects of using genetics in understanding criminal behavior, to environmental effects on such behavior, to the consequences of sensory deprivation in an etiology of violent behavior.

The most frequently discussed topic was the use of genetics as a contributor to an understanding and explanation of criminal behavior. The ability to use genetics in gaining an understanding of and insight into criminal behavior, however, is hindered by the complexity and controversy surrounding this subject. It was emphasized repeatedly that genetics cannot be the explanation, that biological measures are not the primary variable, but rather can only be a partial contributor in a complex developmental process of a particular form of human behavior. As with other individual human traits, such as height, weight, and intelligence that both researchers and the lay public are willing to accept as being genetically and environmentally influenced, a genetically related factor affecting the development of criminal behavior (e.g., autonomic nervous system) can be influenced by environmental variables as well. Thus, the focus of genetics in understanding criminal behavior was stressed as being but a small part in the explanation; however, its greatest potential lies in looking at genetic interactions with non-physiological variables.

Specifically, part of the discussion focused on the influence of genetics on autonomic nervous system (ANS) recovery, the role of ANS recovery in the development of criminal behavior, and the treatment implications that derive from the knowledge that while genetically determined, ANS is also susceptible to environmental influences and modifications. There was consensus at the colloquium on this last notion that the environment can override or amplify genetic effects and that physiological markers could lead to the identification, tracking and eventual treatment (through environmental influences) of individuals with a predisposition to criminal behavior. This agreement seemed to alleviate the need for continuing to discuss the contributory supremacy of either environment or genetics.

Another focal topic of discussion was the role of sensory deprivation in affecting the development of criminal behavior. Attention centered on sensory deprivation (i.e., the lack of physical affection or parental bonding) as impeding the adequate stimulation and development of the pleasure system of the brain. Consequences of this were discussed in terms of drug and alcohol use as alternative methods (to physical affection) for releasing tension, the development of violent behavior as the expression of the reciprocal function of the pleasure system of the brain, and an impaired development of the normal processes of inhibiting impulses.

Intertwined with this discussion was one on the role of stimulation in understanding deviant behavior, starting with the observation that the need for stimulation is higher among criminals than noncriminals. The question of stimulation was addressed through a variety of approaches including: looking at the reaction to stimuli versus the level of need for stimulation; relating the experience of boredom to the need for and the level of stimulation as an impetus to deviant behavior; overstimulation of certain sensory systems (auditory, tactile and visual) occurring in some environments; and the effects of external stimuli (primarily in the form of the mass media and movies) as contributing to the etiology of criminal behavior.

Two other subject areas were repeatedly mentioned during the course of the discussion periods. One was the need to better understand the potential contribution of the sequence of life events (e.g., the temporal effects of prior behavior upon current behavior) as both a predictor and effector of the development of criminal behavior. Another recurrent theme was centered on research methodology; discussions touched on problems of obtaining an appropriate sample, or transferability of results, or measuring the criterion variable while discounting the effects of the situation in which the measure is occurring.

The final area of concern expressed in the discussion reflects the recurrent emphasis placed on the need for interdisciplinary research efforts if strides are to be made in understanding criminal behavior. The problems of undertaking such multi- and interdisciplinary studies were raised. Primary among the problems discussed was the difficulty of establishing a cooperative, interactive work environment that generates a mutual and collaborative analysis of data instead of an isolated, unidisciplinary analytical approach.

COLLOQUIUM RECOMMENDATIONS

Many of the recommendations that were generated during the workshop session of the second day parallel those made by the presentors of papers. Most salient of the recommendations were two themes already heard: the need to pursue interdisciplinary research and the need to engage in longitudinal studies. These themes were not necessarily tied to a specific research project, but were presented as overriding needs.

While many suggestions were mentioned as possible topic areas for research, few of the ideas were developed in detail. Some suggested areas of work were: the consequences of crime, studies of parent/child interactions or child/school interactions, continuation of the assessment of school and work performance and their relations to criminal behavior, and investigating the use of epidemiological techniques in social science research.

Some of the more specific suggestions relating to a research agenda on crime correlates and determinants of criminal behavior were:

- The development of a multidisciplinary center for prospective and longitudinal studies to foster the collection of data on family interaction, perinatal experiences, motor measures, biologic markers, cognitive functioning and maturational changes.

- A study to isolate the factors in a family that contribute to either the lack of control or positive control of violence. Several approaches were suggested: a comparison of the characteristics and structures between successful and non-successful families in high-risk neighborhoods; looking at various forms of stresses that may weaken the nurturing aspects of family life and precipitate violence; or looking at the role of punishment and pleasure in the home, neighborhood and school in family violence.

- To develop a taxonomy of criminal behavior by using such factors as the economic status of the offender, and looking at his or her interests, activities and the use of time.

- To gain knowledge of interaction, statistically defined, to allow the determination of how much variance is accounted for by any set of personal or social correlates.

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*Discussants

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CONVENING STATEMENT OF COLLOQUIUM CHAIRMAN

Marvin E. Wolfgang
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I am chairing this two-day session and it is a pleasure to have all of you here.

This conference on the Correlates of Crime and the Determinants of Criminal Behavior has been put together by Dr. Helen Erskine and we're grateful to her and to The MITRE Corporation for organizing it, and bringing us all together.

Without further ado, I'd like to have our introductory speakers make their remarks--Blair Ewing, Acting Director of the National Institute of Law Enforcement and Criminal Justice, and James Gregg, Acting Administrator of LEAA.

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.

I want to welcome you all here on behalf of the National Institute and repeat, just briefly, the thanks to Dr. Helen Erskine and The MITRE Corporation for putting this all together.

I have only a couple of remarks to make. I want to note for you and for me what this colloquium reflects about National Institute-sponsored research. It reflects some change in the focus of the Institute's research program. It reflects a decision on the part of the Institute and on the part of LEAA to focus on fundamental inquiry, to enhance that inquiry, and to give it greater prominence in the work that the Institute intends to sponsor.

As further reflection of this determination, there is a newly created center within the Institute called, by the very lengthy name, the "Center for the Study of the Correlates of Crime and the Determinants of Criminal Behavior." Dick Barnes, who is here today, is director of that center.

The subject matter of today's and tomorrow's sessions is one of the priorities for research at the National Institute for this current year and for the next number of years. We expect, of course, that this will be a long-term enterprise and that it will take a number of years and, indeed, may have to go on forever before we're able to, in fact, reach any conclusions on the subject. That expectation, in turn, reflects our confidence about the future of Federal support for criminal justice research in this and other areas.

This colloquium also reflects the fact that we have taken the advice of the National Academy of Sciences, which did a study of the National Institute and of its research program. Their advice was contained in a book which you may have seen called <u>Understanding Crime</u>. I suppose we have not taken every single piece of their advice, but what we have done is to make more fundamental inquiry - the kind we're talking about today and tomorrow - a priority; but, we've also established a list of priorities where we will concentrate our effort over the next several years. We have committed ourselves to research that aims at synthesizing findings and thereby advancing the state of the art.

I might point out that in one of the papers which is to be discussed here today Dick Blum observed that he hoped that we would not continue, perhaps, so vigorously along the line of what he referred to as "chrome-bright technology contracts and studies," and that we wouldn't also emphasize so heavily studies of the "management of the dinosaur," as he referred to it, in criminal justice. I think it is worth noting that we do have a mandate in the Act to continue to inquire into technology and equipment, and we will; but we have greatly reduced the scope and the dollar amount of those efforts in order to be able to do some things that we think have higher priority, including the kind of research which is reflected in many of your papers.

In addition, we, of course, intend--because we also have a mandate in the Act--to continue our support for efforts to improve the fairness, efficiency, and effectiveness of criminal justice operating agencies; but, again, this is an area on which we won't be placing quite as much emphasis as in the past, again in order to be able to have a better balanced research program.

So, this colloquium is a first step for us in this area. We are moving carefully. We are seeking your advice and counsel today, tomorrow, and in the future. We expect to learn from your discussion and to be aided by it in launching an exciting program in research in this area.

We are, therefore, looking forward with great eagerness to what will evolve during this conference, what you have to say to each other, as well as what you have to say to us. We hope it will be useful to you. We certainly expect it will be useful to us.

Thank you.

INTRODUCTORY REMARKS OF JAMES GREGG, ACTING ADMINISTRATOR
LAW ENFORCEMENT ASSISTANCE ADMINISTRATION

James M. H. Gregg
Law Enforcement Assistance Administration
Washington, D.C.

Blair has really put the meeting in context. I would just add that although this is a very small, informal colloquium, it has great significance for LEAA and the Institute. It has a symbolic significance for the agency in that it suggests, as Blair said, a new point of departure for us.

When you look back over the first ten years of the agency, you have to be struck by the emphasis that's been placed on the management and operations of criminal justice agencies within the criminal justice system. Some of the most successful programs that LEAA has mounted during this period bear such names as Career Criminal, Treatment Alternatives, and Integrated Criminal Apprehension. When you analyze these programs to see exactly what they mean, in most cases they mean bringing basic and rather simple management improvements to the operation of criminal justice agencies--something that is badly needed and a problem that hasn't been completely solved at this point.

We have made some progress in that area. I think, over the next decade or two, a great deal more progress will be made in bringing basic management principles to the operation of criminal justice agencies. In most cases, as we've seen those improvements occur, they have only served to illuminate the existence of a major factor constraining further progress. We really do not understand the basic client of the criminal justice system or understand the fundamentals of criminal behavior.

Practitioners in the field are recognizing that more and more. We're recognizing it. We feel the need increasingly to address these issues if we are to make further progress in protecting the public from crime and in dealing more effectively with the treatment and correction of criminals.

So, this is a very important beginning for us. We hope it will be the first of a series of meetings of this kind as we initiate another major area of research for the LEAA.

The term "criminal behavior" is possibly problematic. It is a juxtaposition of two terms that perhaps don't rest too easily together.

The one, "criminal", is basically a legal concept, while "behavior" is subject to scientific analysis. Conceivably, the combination of these terms and some of their related concepts have created some of the fog that surrounds this area of the discipline. I hope that through this colloquium and discussion, and the research that is being done now and that will be conducted in the future, we can begin to dispel some of that fog.

Practitioners in this field desperately feel the need to improve ways of separating and discriminating among the kinds of behaviors and the kinds of personalities that they have to deal with every day. Useful categories for treatment and correction don't seem to exist. The categories that have been developed often have been established for the convenience of criminal justice practitioners in carrying out their management responsibilities, rather than devised because of their relevance to the real conditions and behaviors of the people involved. I hope, therefore, that we can begin to further develop categories that can be meaningful for purposes of rehabilitation, treatment, and correction.

It seems to me that the little bit we seem to know about this subject suggests that most crimes are committed by people with perfectly normal personalities. I'm thinking now of the areas of vice, petty larceny, fraud, gambling, the numbers games, employee pilferage, welfare fraud, consumer fraud, young people shoplifting, and other business crimes. The great bulk of crimes, it would appear, is committed by people with quite normal personalities who have been socialized to some extent or another toward activity that our law declares to be criminal.

On the other hand, it would appear from the research that's been done that a substantial amount of the crime that the public is most concerned about--or, at least, fears most--may be committed by a category of offender that's been variously labelled psychopath, sociopath, and criminal personality. We do seem to be learning a little more about that type of personality and that individual. Of course, the papers that have been prepared for this colloquium suggest a number of other important categories. These categories help explain the great number or bulk of crimes, but may not account for certain crimes which, though few in number, are of great concern to the public.

We hope that what we discuss during these two days and the future research will help us develop a better system of classification, thus creating a better understanding of the size of the populations within these various specifications, the prevalence of criminals within these categories, the nature of the crimes that are committed by persons within these categories, the etiology of their criminal behaviors, the relative importance of social and biological factors in determining that behavior, diagnostic tools to determine which individuals fit into what category and, of course, ultimately and hopefully, therapies that will be useful in dealing with these various categories.

We're very excited about this meeting today. Our main desire, therefore, is to hear from you.

OPENING REMARKS OF CHAIRMAN MARVIN E. WOLFGANG

Thank you very much for your comments.

I'd like to give kudos to LEAA and the National Institute for their perception in calling forth a gathering such as this. I know that there are other such conferences and seminars being held. Last week, I was a participant in a conference on minority research in criminal justice. I think sessions such as the one at hand and last week's are a very appropriate way in which the Institute can obtain a sense of what may be viewed by the research community as important areas for further development. I think it is important that we, as researchers, recognize the significance of the Institute's coming to us for this kind of advice and consent.

I'm reminded of ten years ago when the National Commission on the Causes and Prevention of Violence was established, shortly after the assassination of Senator Kennedy. One of the first things that was done was to call a meeting in July--which was a very difficult time of year to get people together--of approximately 65-70 people from across the country who were involved mainly in various kinds of research--psychological, psychiatric, sociological, biological, to some extent--on violence and the correction of it.

Everyone who was invited, with one exception, was out of the country, yet they came to that rather hastily called meeting. It seemed symbolic of what later happened on that Commission that the research community was asked to have an inquiry immediately into the work on violence and aggression. Whatever you may think about the 13 volumes that were published, a strong effort was made to have research be an important part of the production of the finished report.

Another thing I am reminded of is that Sarnoff Mednick and I have been participants, for three years now, in the Interdisciplinary Symposium on Crime and Aggression. It's made up altogether of nine or ten people from different countries and different disciplines. We have met to discuss the state of the art on any topic from genetics to what Alfred Strong called the political science of sociology and to determine what variables are important in analyzing aggressivity and deviance. Our last meeting, which was in November in Holland, was used to try to set up various kinds of hypotheses and research projects that have an interdisciplinary character. It's impossible for me to come to this particular meeting without carrying the weight of those three years of conversations and correspondence.

I do hope that an interdisciplinary agenda will be the product of our two-day meeting. It appears to me that, more so than in the past, there is a considerable weight given, at least in the papers that have been prepared, to the bio-physiological, and psychological disciplines, and less to the social sciences.

That's a descriptive fact. It's not meant to be anything more than that, but I think we should not lose sight of the other kinds of perspectives and variables, beyond those the papers have presented, that have evolved in the topic under discussion here. The extent to which we enlarge the scope of our concern is a function of how we interact with that.

I hope that you've had an opportunity to read the papers* in advance. In case you didn't, our intention is to have each of the authors give a brief (10 or 15 minute) summary presentation of the major thrust of the paper.

You will find that there is a considerable amount of empirical research displayed in these papers. I would say that there is no explicit ideological orientation here. There's no attack on the power structure. There's little reference, if any, to what may be called the radical, critical, or new criminology. I'm not suggesting that there should be, but the absence of any kind of ideological rhetoric is clear in reading the papers.

There is much literature in criminology and in criminal justice that has never incurred this. At the University of Pennsylvania, we are just now sending off to the publisher, finally, our review of the criminologic or scientific literature in the United States from 1945 to 1972. We have reviewed between 4,000 and 5,000 articles and books that purport to have some scientific quality. Very much like what has been done in some of the physical sciences, we engaged in a science citation index analysis. Considerably over 50 percent of all that literature has appeared in the psychological, economic, sociological, criminological journals and law reviews, as well. Considerably over 50 percent has never been cited by anybody, not even self-citations. In one sense, this is appalling because it means that there may be some high quality work that has been buried, put on the shelf, and never referred to. On the other hand, if you use citation frequency as an index of quality, it shows that there has been low-quality work done in criminology.

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*For a complete presentation of the papers prepared for this colloquium, see "Colloquium on the Correlates of Crime and the Determinants of Criminal Behavior, Invited Papers."

There has been a considerable shift in orientation over those 28 years in the field of criminology. The shift has been primarily from case study, psychological and psychiatric analysis, to the broader macro-sociological or sociocultural or economic operations research systems analyses. I'm sure I'm not telling you anything you don't already know. What we have done is to document that shift. The changes that have occurred from an emphasis on what Parsons would call the economic system to the social system or culture system are abundantly clear if one does the kind of analysis that we've done.

I have begun to notice, however, that there's a shifting back again to the personality system. It's not necessarily in the style of the early approaches that date from Lombroso and Ferri and Healy in 1915, rather it's with a new kind of inter- or multi-disciplinary approach looking at the personality system as an average that may have some relationship in a legal context.

The same thing appears to be happening in the socialist countries, as well. Research in the socialist states, as far as I can judge by my acquaintance with some of the work that's been done at the All-Soviet Academy of Criminal Science in Moscow, seems to indicate that there is less work being done on the general social conditions--the family, community, and neighborhoods--and more emphasis being placed on the individual from a medical, clinical, psychiatric point of view.

There is much being said about the physiological factors that produce adverse and criminal conduct. Therefore, it's not entirely unexpected, by me, that quite a few of the papers that have been solicited for our meeting here reflect some of that change.

Our primary purpose, as I understand the call for this meeting, is to provide to the Institute, after we have gone through the distillation process, a research agenda of some sort. I hate to use some of the language that is most common in this part of the world, but we are to prioritize the items on our agenda to get some sense of the major concerns of those of us who are here; the agenda then, would be further guidance to the LEAA in the allocation of the resources that are available to them for research.

Without further comment then, let us start on what we will call Group A papers. The grouping process is along the lines of some coherence of intellectual affinity of the papers, albeit there is overlap.

Our first paper is summarized by Sarnoff Mednick. Sarnoff has shared with us a very fascinating work using data from Denmark that are, if not unique, most uncommon and not readily available in the United States. The usual problems of dealing with official statistics in criminal justice are inherent in the paper. There may be some problems, occasionally, with reference to terms like "criminal parents," but these are rather shorthand terms and refer to the fact that they are people who have had a criminal record in the official statistics.

YOU DON'T NEED A WEATHERMAN!

SARNOFF MEDNICK
University of Southern California
Los Angeles, California

My remarks, actually, will be less of a summary of the paper that you received than a logical extension with suggestions. I'm so pleased that there has been an encouragement of suggestions this morning because I feel a bit presumptuous in advising other people how I think research should be done.

I think there are some parallels between medical science's struggles in controlling disease and social science's struggles in controlling crime. The shared failures that we've observed in both fields come, in part, from a similarity of approach. Consider the problem of chronic diseases in the United States.

Before 1936, individuals with chronic incurable diseases were typically killed by the serious infectious diseases--primarily pneumonia--which carried them off in a fairly merciful and peaceful manner. In 1936, a group at the Rockefeller Foundation discovered sulfanilamide; sulfanilamide and penicillin and the other "wonder drugs" just about did away with pneumonia. The mortality rate for pneumonia in 1936 was 65 per 100,000. In 1940, it went down to 20 per 100,000 and by 1949 it was down to 10 per 100,000. And science had, once again, triumphed.

What medical science's triumph had done, in this case, was actually to increase the amount of chronic diseases and suffering because the treatment which controlled these lethal infectious diseases merely served to maintain chronic illnesses. You could maintain your chronic illness now at a very stable level; the patient didn't die, but it didn't cure anybody nor did it prevent anybody else from developing these incurable chronic diseases.

Developed societies are now facing the problem of a fantastic accumulation of suffering, chronically ill individuals maintained in hospitals and nursing homes. The cost to society is considerable and growing. Because we have so many people who are chronically and incurably ill, we are compelled to invest our research efforts in technological research directed toward improving our ability to keep these people alive. There is very little (or no) research done on the primary prevention of these illnesses.

Now consider criminology. The picture is not totally different. Mr. Gregg has just reported to us on some of the administrative management successes dealing with crime; but this really hasn't reduced the incidence of crime that much. We have lots of research on juries, lots of work on street lighting and patrolmen. Yet these efforts, I think, are patchworks.

We don't know how, yet, to institute primary prevention of crime and there's very little attempt being made to start such work. One approach to prevention may be societal manipulation. This is a logical approach from a socio-cultural-etiological point of view. This socio-cultural point of view was, of course, a reaction to the rather distasteful biological determinism of the 19th century; it's been the dominant force in criminology. It maintains that the criminal is a normal individual who's been socialized in some inconvenient way and, therefore, exhibits criminal behavior.

To change this individual one must change the way he has been socialized; we must change society's structures. This may be a very successful way of proceeding; but if there are individual characteristics--such as intelligence differences or the very publicized XYY chromosome anomaly--which perhaps increase any individual's chances of behaving anti-socially--then the societal change approach will not be totally successful.

Are there distinctive individual characteristics that at least some criminals have which perhaps could have some etiological role in their behavior? Let's examine some of the evidence. I'll briefly relate five facts which convince me at least that some criminals have such individual characteristics.

The first, I'll call it a fact--they may not be facts, but it's such a short word, let's use it. First we will talk about the twin studies that Marvin made reference to. These began in Germany in 1929 with Lange's publication in which he made some extravagant claims for the role of genetics in criminal behavior. He called criminal behavior destiny, an individual's destiny, as a matter of fact. As a consequence, I think there are probably no references to Lange in your studies.

1. Twin studies. The twin studies began in Germany with Lange in 1929. Before the modern studies, there were eight projects which found greater concordance for identical than fraternal twins. More recently, Karl O. Christiansen has completed a magnificent twin study in Denmark which includes 3,586 pairs of twins and presents evidence that there is considerably more concordance among identical twins than fraternal twins. This suggests--or at least causes us not to reject--the possibility that genetic factors might be involved in criminal behavior.

I might mention that we are trying to continue Karl Otto's research; we now have some 14,000 pairs of twins.

That's the first fact.

2. Adoption Studies. There's a simple fact that can be stated which summarizes the Danish adoption studies. If an individual is adopted at birth and he becomes criminal, the chances are excellent that his biological father was a criminal. This is based on a pilot study of 1,100 adoptions. We've now been able to examine the data on 8,000 adoptions. The results from the pilot study are being supported. In the near future, we will be able to look at genetic factors as they relate to specific types of crimes.

3. Autonomic Nervous System. Dr. Hare will summarize the data on autonomic nervous system involvement in anti-social behavior. I will describe two studies because of their special relevance to the research design which I will later discuss.

The 1946 British birth cohort consists of all of the people born in England in one week in March, 1946. At age 11, these individuals had their pulse measured in school as part of a school examination. When they were 20 years of age, Michael Wadsworth determined the delinquency the cohort had exhibited. Their pulse rate at age 11 was a very good predictor of their degree of delinquency. It was at least as good as a score expressing emotional deprivation in childhood.

In a small study conducted with Janice Loeb, we found skin conductance at age 15 to predict even mild anti-social behavior at age 25. In another part of this study we find that skin conductance behavior is a very good predictor of whether or not children will later be diagnosed psychopathic by an interviewing psychologist.

There seem to be nervous system differences between criminals and non-criminals, and between psychopaths and non-psychopaths. These differences do not seem to be the result of the exposure of these people to the criminal justice system.

4. Philadelphia Cohort. The fourth factor is a study by Marvin Wolfgang, et al, that you all know. It shows that a very small proportion of the males in Philadelphia account for a lot of the crime. This suggests to me that perhaps these highly recidivistic individuals have some personal characteristics that could be predictable.

5. Biosocial Interaction. Karl Christiansen was the first one to point out this interaction when he showed that the genetic contribution in his twin study was much greater in middle class individuals than it was in lower class individuals. His explanation of this finding was that lower class individuals seem to have economic and social reasons for becoming criminal. For middle class individuals, these motives are not as pressing. Therefore, in the middle class individual (genetic) factors are more important in determining criminal behavior.

In other research we've completed on the autonomic nervous system, we find that in families where there is a lot of criminality our physiological measures are poor predictors. In families where there is no registered criminality, our physiological measures are very good predictors. Christiansen relates these findings to Sellin's theory of culture conflicts. Where cultural and social forces suppress criminality, it's the deviant who becomes criminal.

I mentioned five conclusions that convince me that there are individual factors that contribute to the etiology of criminality. What are the consequences of this?

It suggests that we can, perhaps, predict from individual characteristics who will become a criminal. This prediction is improved by considering the interaction between the biological and the social variables. This suggests that if we wished to investigate the possibility of the primary prevention of crime, we might begin by launching a longitudinal study of young men and women--perhaps first-offenders, perhaps high school students, perhaps a birth cohort. We would begin with an intensive assessment and then follow these individuals to see which of them become criminal. Then we can ask which of the measures in our initial intensive assessment predict their later criminality. If the predictive factors are replicable, then this means that we are able to select from a general population those who will later be criminal. We can then concentrate our interventive research on those selected.

Thank you.

DISCUSSION

WOLFGANG: Thank you Sarnoff. I can't avoid saying that, although we have much maligned the scientific process in criminology, we are still caught in a similar problem of conceptualization because it is broadly indicative of predisposing factors, on the one hand - which imply genetic and biological factors - and precipitative variables from the environment on the other, so, we're really still in that same kind of conceptualization. There are, presumably, some predisposing factors. If we don't have a strong contribution from the precipitating environmental factors, then the predisposing ones will loom larger, as in the case of the middle class as some of you have referred to it.

Let us carry on. Robert Hare has a paper on psychopathy and crime. There are, I believe, additional definitions of the problems which are taken into account in the paper. A sociologist's reaction to most constructions of psychopathy in crime still raises questions about how many of these particular traits are required in order to get into the category labelled "sociopath" or "psychopath," and what are those agreed-upon traits that are required.

We talk about criminal psychopathy from a selection of prison inmates that may have a special bias in the sampling. What kinds of baseline data exist with respect to our knowledge about psychopathy? How many professors lack empathy and solid social relationships and so forth? These are all problems. I think that they have been well-addressed in the paper. I'll now turn you over to Bob Hare.

PSYCHOPATHY AND CRIME

Robert D. Hare
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I should mention at the outset that although I'm an academic, I managed to start out in a quite different way. In 1961 I worked for eight months in a miximum security institution in British Columbia. Of course I soon saw the light and decided that being an academic was much safer. Most of my early contacts in the prison were with inmates that I would later consider to be psychopaths. They made heavy use of the psychological facilities, visiting my office several times a week for all sorts of reasons, but mainly to get something for themselves.

While most of you are familiar with the concept of psychopathy, there is still a considerable amount of debate about whether or not a concept of this sort is useful in a criminological setting. There is also some discussion about whether psychopathy is a typological concept or a dimensional one. The problem is a difficult one, although my feeling is that there really is something qualitatively different about the psychopath. Marvin Wolfgang mentioned that we don't know how many of the characteristics or traits that define psychopathy must be present before we are entitled to label a person a psychopath. My answer is, all of them. We're discussing a particular package of characteristics here, not just impulsivity, lack of empathy, etc. Unless all the important defining characteristics occur together, and with a certain minimal degree of severity we wouldn't label a person a psychopath.

Much of the research that I will be discussing has been conducted with inmates in maximum and medium security institutions near Vancouver. Most of the inmates were white and from the lower socioeconomic levels.

Until a few years ago, the American Psychiatric Association (APA) category for the type of individual I'm discussing here was "sociopathic personality disturbance--antisocial reaction." This was later changed to "antisocial personality." I see that in the proposed revision of DSM-III the category is going to become "antisocial personality disorder." If you've seen the draft proposal you will have noticed that the APA <u>description</u> of the antisocial personality disorder is more or less consistent with the standard clinical description of the psychopath (or sociopath). What disturbs me about DSM-III is the proposed method for translating the description of the disorder into a method for diagnosis and for selecting subjects for research purposes. I think that the diagnostic criteria are far too liberal. I won't list these criteria here, since they are covered in my paper.

To give you some idea of just how liberal the proposed DSM-III criteria are, I'll briefly refer to a recent comparison made between these criteria and global clinical assessments. Incidentally, global assessments of the sort we make in our research are reasonably good. They can be highly reliable, provided that appropriate ground-rules are chosen and understood, and the raters well trained. We routinely obtain inter-rater correlation of over 0.8, and sometimes as high as 0.9. These global ratings of psychopathy (on a 7 point scale) also have a reasonable degree of validity. In any event,we've found that our global assessments of psychopathy are significantly correlated with the DSM-III criteria. For example, in one study we concluded that between 25 and 30 percent of an inmate sample were psychopaths, while by DSM-III procedures about 80 percent were diagnosed as antisocial personality disorder. This means that, according to DSM-III, the diagnosis of psychopathy is almost synonymous with criminality.

ROBINS: In the noncriminal population, it doesn't.

HARE: No. This analysis only applies to criminals and delinquents; I'm quite sure that many noncriminal psychopaths could be identified with the DSM-III criteria. One of the advantages of DSM-III is that the behavioral criteria used in diagnosis are pretty objective, although subjective elements creep in when it comes to actually applying the criteria. In any case, it's possible to manipulate the stringency of these criteria, and perhaps that's a strong point. We've found, for example, that increasing the stringency of the criteria results in a sharp drop in the percentage of inmates diagnosed as antisocial personality disorder. In the study referred to earlier, this more stringent procedure resulted in about 35% of an inmate sample being diagnosed antisocial personality, a figure that can be compared with the 80% figure obtained when using the regular DSM-III criteria. The smaller value seems more appropriate for the type of criminal population we were dealing with. I don't mean to be unduly critical of DSM-III; I'm simply saying that it has to be tightened up somewhat.

The next part of my paper has to do with the relationship between psychopathy and criminal behavior. Sam Guze recently stated that once an individual has a criminal history, psychiatric diagnoses are not predictive of the degree of later criminal behavior. I think that the reason for this conclusion is that Guze was using a very broad conception of sociopathy, with almost 80% of his group of felons being diagnosed as sociopaths. If much more restricted criteria are used (such as the ones we employ) a reasonably good relationship between psychopathy and future criminal behavior becomes apparent. I won't go into any detail here, but we've carried out follow-up research on inmates studied from 1964 to 1974. The sample consisted of about 200 inmates, half of whom were psychopaths. There were statistically significant group differences in the type and severity of criminal behavior, differences that occurred both prior to the time that we made our diagnosis and subsequent to the diagnosis. One conclusion drawn from the analysis is that the criminal behavior of psychopaths is pretty consistent over time. One of the variables that differentiated between groups was age of first conviction in adult court, with the psychopaths appearing earlier than the other criminals. Over 40% of the psychopaths, but only 19% of the nonpsychopaths, first appeared in adult court before the age of 17; some of the psychopaths were only 13 or 14 at first appearance. All of the psychopaths had a long history of antisocial behavior, starting well before age 15. During the follow-up period they committed more crimes than did the other criminals, particularly against people. One of the few things that the psychopaths were not involved in early in their careers was heavy heroin use. Later on, however, many began to use and traffic in heroin. Although the psychopaths had horrendous criminal histories, they were about twice as successful at obtaining parole as were the other criminals. Anyone who has dealt with these individuals, particularly the really charming, manipulative, verbal type, can understand how this could happen. They often join the "right" groups, e.g., alcoholics anonymous, go to church on Sunday, make regular appointments with the minister, psychologist, and psychiatrist, and write persuasive letters to the authorities. As a result, they seem to get paroled frequently, but of course they generally violate the conditions of parole as well.

Incidentally, one of the things discussed in my paper has to do with the so-called "burned out" phenomenon. As Robins had earlier reported, we found that the criminal activities of many psychopaths reached a peak around age 30, with the frequency and severity of criminal activity decreasing somewhat thereafter. I'm not really sure what this means in terms of the dynamics of psychopathy, although several hypotheses have been discussed in my paper.

Turning now to biological research on psychopathy, I'll run through some of the major findings very quickly. Detailed reviews are available elsewhere. First, many papers have been published on the electrocortical activity of psychopaths, with the general conclusion being that psychopaths exhibit abnormal amounts of slow-wave (theta) activity in the EEG record. Several hypotheses have been offered to account for this activity, including delayed cortical maturation, cortical dysfunction, and a proneness to become drowsy during routine clinical examinations. However, several recent careful reviews of the literature have pointed out that most of the published research suffers from methodological problems which make it difficult to accept generalizations about the presence of slow-wave activity in psychopaths. This doesn't mean that psychopathy is not associated with abnormal EEG activity, only that the association has not yet been unequivocally demonstrated. Similarly, an early report that the contingent negative variation (CNV) was reduced or absent in psychopaths has not been supported by more recent research.

The findings on the autonomic correlates of psychopathy have been reasonably consistent, particularly with respect to electrodermal activity. Psychopaths appear to be electrodermally hyporesponsive, particularly with intense stimulation; that is, they tend to give smaller skin conductance responses than do other criminals, with the difference increasing as the intensity of stimulation goes up. The rate at which the skin conductance response recovers has also been related to psychopathy and, more generally, to criminality. We've recently found that psychopaths exhibit slower recovery than do nonpsychopathic criminals, although only in the left hand and only when the stimulus which elicited the response was very intense. In a sense, then, we support Mednick's contention that psychopathy and criminality are related to slow electrodermal recovery. However, the physiological and psychological meaning of electrodermal recovery rate has recently been the subject of some controversy.

In general, cardiovascular variables have failed to differentiate between psychopathic and other inmates, with one interesting exception. The exception has to do with a particular pattern of physiological activity we first observed 8 or 9 years ago. It's quite a confusing pattern in a way, although I think that an interpretation is possible. Most of you know that in a standard conditioning paradigm psychopaths don't "perform" well when the dependent variable is an increase in palmar skin conductance. That is, in this and similar situations, they don't show the appropriate increase in electrodermal activity prior to the occurrence of an unpleasant or noxious stimulus. This is a very consistent finding, and it has led some investigators to describe psychopaths as poor "fear" conditioners and to relate their conditioning performance to an apparent difficulty in avoiding punishment. However, in several studies we have found that the psychopath's attenuated electrodermal activity during anticipation of an aversive stimulus may occur along with large increases in cardiovascular activity, specifically heart rate. There seems to be a particular pattern associated with psychopathy--poor electrodermal conditioning but good cardiovascular conditioning. A tentative, theoretical interpretation has been offered, but a great deal more thought and research are required.

DISCUSSION

WOLFGANG: When we go into the general discussion, I will be asking each of our presenters to suggest research for the future on the basis of what they have written.

Next is Ken Moyer who will summarize the "Physiological Determinants of Human Aggression." I have had the pleasure to be with Ken on a number of occasions, and it's a particular pleasure for sociologists because he has provided, with the clarity and succinctness of his writing, a vocabulary for sociologists to use that allows us to sound as if we know something about physiology.

PHYSIOLOGICAL DETERMINANTS OF HUMAN AGGRESSION

Kenneth Moyer
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I'm really not going to talk about criminology. I don't know anything about criminology. I'm going to talk about aggressive behavior.

Aggressive behavior is obviously a problem. I get hate mail from the sweetest people in the world, the chocolate industry. In the "Wall Street Journal" a couple of months ago, on the front page, I was quoted as having said that chocolate can turn you into a criminal. Obviously, I didn't say that, but it led to a considerable flow of mail.

I think, perhaps, this incident illustrates two things. Number one, it illustrates the relationship between frustration and aggression. Secondly, it reflects, I believe--both in the person who was initially talking to me and understood what I said and perhaps in his readers--a need for easy answers, especially easy answers from people who are physiologically oriented. It would be so easy, you see, if we could just pass a law against chocolate and bananas and solve the rising crime wave.

Those of us who read Doc Savage, as I did when I was younger, remember that he would send his criminals away to a farm on which they would have their brains altered and then be returned to society. Unfortunately, I can't recommend that, either, at the moment. So I don't have any answers and I guess I don't think you should look for them here, really.

I think I know some of the questions and that is what I shall address myself to. I'd like to present a general model of aggressive behavior. There is abundant evidence for each point I'm going to make. Obviously, in the 10 to 15 plus minutes that I'm going to have, I'm not going to have time to present all of the evidence.

The basic premise of this model suggests that there are, in the brains of animals and humans, innate specific neural mechanisms which, when fired in the presence of a relevant target, result in aggressive behavior toward that target. In the case of animals, the aggressive behavior is overt and immediately observable. In the case of human beings, the behavior itself may not be observable, but the individual has an internal feeling of hostility or feeling of irritability. As I say, there's a lot of evidence for this.

I will give just one example from Flynn's laboratory. Flynn implanted electrodes in the hypothalamus of cats. Flynn used cats which were non-predatory, which would not, under normal circumstances, attack rats. They would live with a rat for months. However, if he had an electrode implanted in the lateral hypothalamus of the cat and he stimulated that cat in the presence of the rat, it would turn and attack and kill the rat very precisely, very quickly, and very calmly in the typical predatory kill of the cat family--biting and breaking the cervical vertebrae of the rat's neck. It would, on the other hand, ignore the experimenter who was standing at the side.

On the other hand, if the electrode were in the medial hypothalamus and the cat was stimulated, it would ignore the rat in the cage and turn and attack the experimenter, and attack the experimenter not with the amorphous random attack of a decerebrate preparation, but it would attack with precision and as though it intended to do harm, and it would, in fact if the experimenter didn't get out of the way, do harm.

We have to be very careful about jumping from animal to human experiments, but I don't think that man has escaped from the neurological determinants of his aggressive behavior.

There are now a lot of people with electrodes in their heads in relevant places where stimulation results in feelings of irritability and aggresive behavior. One case by Gene King is illustrative. He had a very docile, calm, friendly patient in whom he had implanted an electrode in the amygdala. When she was stimulated, she immediately expressed feelings of hostility. She said such things as, "Take my blood pressure. Take it now. If you think you're going to hold me, you'd better get ten more men." Whereupon she stood up and attempted to strike the experimenter who, very wisely, turned down the current.

This is just one illustration of a number of these types of case studies. There are also a wide variety of brain pathologies in which the neural systems for hostility can be activated. These range from brain tumors in certain specific areas--obviously not all brain tumors--to the various episodic processes which Russ Monroe will talk about. If we have these specific neural systems for aggressive behavior, we should be able to go in and cut them out, and reduce the amount of aggressive behavior that we get--and, of course, we can.

We can take the wild cat, _Lynx rufus rufus_, and if you get hold of him and get him on the operating table and do a bilateral amygdalectomy, removing the part of the brain right in the middle of the temporal lobe, you turn him into a pussycat, and you turn him into a pussycat right away. As soon as he comes out of the anesthetic, he's going to have an amygdalectomy hangover for a little while, but he will never be spontaneously aggressive again.

Exactly the same thing has been done, now, with a wide variety of species, including a number of people--people who are pathologically aggressive, who are so aggressive that they can't control their behavior in any way. These are the kinds of people who have to be kept in a rubber room and who will attack any person or thing, and are completely destructive and must be kept in solitary under constant sedation or constant restraint for the extent of their lives.

A number of surgeons, now, have shown that it is possible to do a bilateral amygdalectomy, as well as a number of other operations. There are a variety of these operations now in which lesions can significantly reduce the pathological aggressivity of certain types of individuals that I've described. Heimburger reports cases in which he not only has been able to release these people from the back wards out into open wards, but two of his patients are making a reasonable adjustment out in society. There is a variety of reasons why I do not recommend this operation. There is a very small percentage of the population for whom this would be applicable.

In addition to the neural systems which one has for aggressive behavior, one also has neural systems which send inhibitory impulses to the first systems so that the activation of those systems results in a decrease in the tendency for the organism to behave aggressively at that time. There is a variety of experiments showing this. A number of areas can be lesioned which, presumably, result in lesioning of the inhibitory system, and then you get explosive aggression on the part of the subject.

It's also possible to implant electrodes in the inhibitory area, stimulate those electrodes, and reduce ongoing aggressive behavior. The classic case, which I mentioned in my paper, by Delgado is reasonably well-known. He took a boss monkey and implanted an electrode in an area of the brain called the caudate nucleus and hooked it up to a radio receiver. The activation of the radio transmitter would activate the receiver which would, in turn, activate a stimulation of the caudate nucleus.

Delgado would stand outside the cage with a little transmitter like the one you use to open your garage door, press the button, and stimulate the caudate nucleus of the monkey and its aggressive behavior toward the rest of the colony dropped immediately, and stayed down until Delgado stopped pressing the button, whereupon the boss monkey got things whipped into shape pretty fast. Delgado then put the small transmitter in the cage and one small monkey learned to stand by the button and whenever the boss monkey became aggressive, the little monkey reached over and pressed the button and calmed him down, and reduced the aggressive behavior in the colony in general.

Exactly the same types of systems are possible in humans. It has been demonstrated a number of times. You can stimulate an inhibitory area of the brain and suppress ongoing aggressive behavior on the part of the patient.

There is no reason in the world why you can't do this with radio control the same as with direct control. There are some problems with this. The radio has to be bolted to the skull. That means the bolts have to go through the scalp. This means an opening where possible infection and irritation can occur. There are also psychological problems. People reportedly tend to feel conspicuous with radios on their heads.

Even that, however, has been solved technically by the recent developments in microminiaturization in electronics. It is now possible to take the radio receiver, the power to operate it, and a radio transmitter which can send out brain waves from the area in which you have the electrode implanted--and put it in a unit the size and the shape of a half a dollar. It is now possible to implant an electrode in the inhibitory area of the brain, bring it to this very small radio receiver/transmitter which can be buried under the skin anyplace and, as soon as the hair grows back, the individual looks like anybody else. It's technologically feasible right now that the individual sitting next to you is under radio control. You wouldn't know it unless he parted his hair wrong.

Again, for a variety of reasons, I don't recommend this as a therapeutic device. It is highly experimental. There are all kinds of problems with it. We don't know what the side effects are. We do know that in some side effects there is a phenomenon known as "kindling" in which repeated stimulation of a portion of the brain results in the spontaneous activation of that portion of the brain, which recruits other neural systems, which may result in a total convulsion on the part of the subject. So there are some problems to be solved before that's a useful mechanism.

I guess one of the things we should do is ask: If we have these neural systems, both active and inhibitory, what are the mechanisms that turn these systems on and what are they that turn them off?

Aggressive behavior is a relatively small part of the total behavior of both animals and men, fortunately. I think the best way to think about this is in terms of the threshold. That is, it is obvious that provocation at time one is not the same as provocation at time two. This model would suggest that there is a threshold for the firing of these neural systems for aggressive behavior and there is a variety of things which contribute to the raising or lowering of the threshold.

I think it is clear that some of these are hereditary. Obviously, if we have such neural systems in the brain, the ease with which they can be fired should be inherited because the inheritance of changes in the neurology or the neural systems are exactly the same kinds of mechanisms as are related to the inheritance of the shapes of our noses. It is possible with animals to select from a very large group of animals both very aggressive individuals and very non-aggressive individuals, and within a relatively few generations breed highly aggressive and highly non-aggressive groups of subjects. We don't have comparable data on people for obvious reasons, but there are other data which certainly seem to indicate that this is the case.

There are a number of changes in blood chemistry which influence the threshold of these neural systems. There is considerable data to indicate that, in animals, it is relatively easy to reduce the specific kind of aggression that I have called "inter-male aggression" by any method which reduces the testosterone level in the blood stream. The most obvious method for doing this is castration. The original study was done by Elizabeth Beeman. She castrated fighting mice before puberty. When they reached puberty, she matched them and showed that they didn't fight at all, whereas comparable control animals fought the normal amount.

She then implanted testosterone under the skin of the experimental animals and they fought up to the levels of the control animals. Then she removed the pellets of testosterone by slitting the skin and taking them out and as soon as the testosterone level in the blood stream dropped back down, they showed no aggressive behavior.

There is also evidence from a variety of sources that for particular kinds of aggression--and I think it's important to note that this is specific to certain kinds of aggression--castration will effectively reduce the tendency for the individual to be a recidivist for that particular type of crime. There are problems with that. People tend to object to this operation.

We are within reach, however, of procedures which will provide for a chemical reduction in the testosterone level in the blood stream, which may help control sex-related aggressive behavior in humans.

There are a number of other variables. Hypoglycemia, that is a rapid loss of blood sugar, results--in some people, and only in some people--in a sort of free-ranging, impulsive tendency to become aggressive. You may have an extremely combative patient who will attack any number of people, but if you give him a glass of orange juice and he takes it, within a matter of minutes he will again become a civilized person and interact on a normal basis.

It is also true that chocolate can make one aggressive. There is a significant group of people--and I don't have any idea how many, that's one of the things we ought to find out--who have an allergic reaction to a whole variety of allergens, one of which is chocolate, another of which is bananas, and one of the most common is milk. This allergic reaction, instead of resulting in the production of hives or an asthmatic type of response, results in the activation of the neural systems for hostility so that the individual becomes extremely combative and aggressive--also impulsive, which may relate to other aspects of criminology. It is clear, then, that if one removes the allergen from that person's environment, one gets a reduction in this kind of behavior from that individual.

Let me make one more point and I'll cut off the last couple of points I was going to make. One of the most important points to make in all of this is that learning is a significant and potent variable in the determination of aggressive behavior. Regardless of these physiological components, learning is a highly significant variable.

This should not surprise us. The fact is that we can take an animal and teach him to eat until he is obese, or we can teach him to starve to death in the presence of food simply by manipulating the contingencies of reinforcement.

All of the biological determinants of aggression that I know of are subject to the same kinds of learning processes. I don't mean to talk specifically about reinforcement, but I would include, of course, role modeling, cultural determinants, all of the other variables which contribute significantly to making us human beings and making us integrated behaving organisms. I think that is a point which people who think about the physiology of aggressive behavior frequently miss.

There are certain changes we can make in the physiology of the organism as it relates to aggressive behavior, but much of the control of aggression is going to come down to changing the organism's tendencies to behave in given ways by manipulating the contingencies of reinforcement and the other variables that control learning.

DISCUSSION

WOLFGANG: Thank you for your always stimulating comments. I know we will have all kinds of interesting reactions to them.

Let us move to Dr. Monroe's presentation, as it has been referred to several times by preceding persons, on "Episodic Dyscontrol."

EPISODIC DYSCONTROL IN CRIMINALS

Dr. Russell Monroe
University of Maryland School of Medicine
Baltimore, Maryland

What I would like to present to you today is a two-dimensional classification of a group of criminals. The materials that were circulated were two general chapters from a book that will be published in August by Lexington Press, <u>Brain Dysfunction in Recidivist Aggressors</u>. My colleagues and I completed this study. One of my co-colleagues is Dave Barcik, across the table here. I hope he wasn't invited to this meeting to keep me honest, but he might have been.

The theory behind this was based on the earlier work I reported in <u>Episodic Behavior Disorders</u>. We classified the patients on a dyscontrol scale into high and low groups and then on EEG abnormalities, using theta waves, into a high and low group. I don't have time to go into the rationale for this kind of classification except to say that other data had suggested that high theta might differentiate an epileptoid dyscontrol--a group with, perhaps, limbic system dysfunctions--and that there were undoubtedly a number of dyscontrol patients who had little in the way of faulty equipment, but, who, in terms of faulty learning, were impulsive.

So we went into the study with a concept of one group as an "epileptoid dyscontrol"--from empirical data, we made those kinds of predicitons--and that there would be, at the opposite pole, a "hysteroid dyscontrol" group. We assumed that there would be a group that showed high theta activity, but not dyscontrol behavior. We didn't have any idea what that group would be. It turned out to be a very interesting group, and we subsequently labeled it "inadequate psychopaths". Then we had group four, the "pure psychopaths", and that in a normal population or in a general population screening, I suppose, would have been the normal group, but in our population at Patuxent Institute it was what we called the pure psychopath, with apologies to Dr. Hare.

The only thing I have time to say about the group at Patuxent is that they were dealt from the bottom of the deck in terms of heredity, social factors, economic factors, biological factors, and medical illness. They really got a bad deal and probably the one common characteristic of that group is that repeatedly in the past when they were returned to the street, they immediately committed criminal acts and were re-institutionalized.

This slide, (Figure 1), presents what's called the Monroe Scale. Actually, it was designed by Plutchik on the basis of my monograph so he called it the Monroe Scale, which I thought was very nice of him. These are 18 factors reflecting dyscontrol behavior that were self-rated on a scale--zero, one, two, and three. You will see it is not a measure of overt violence, although there are a few items there suggesting overt violence. Of the 18, only five of these items are related to overt violence.

On the next slide, (Figure 2), modified from Plutchik, you see the kind of scorings that he got. The means for the temporal lobe epileptic are close to 25. For the non-temporal lobe epileptic, they were also high. Violent individuals still are high as are male prisoners and female prisioners. High, in our definition, was 20 or greater on dyscontrol scale.

This slide, (Figure 3), is a scattergram. You will see, in group one--that's our epileptoid group--there were 28. In group two--that's our hysteroid--27. In group three, 12. And in group four, I think it is 28. This is the way they plotted out on the ratings of a scale of zero to 40 on the Monroe Scale and zero to eight on EEG theta ratings.

This slide is a summary chart, (Figure 4), on intergroup differences in the four classifications. I think they are particularly interesting. Group one, our epileptoid group, was one of particular interest to us. As you see, there was a suspicion of epilepsy or an epileptoid mechanism, as determined by a neurologist and a psychiatrist and it was based on entirely different data bases. The neurologist was making it on neurologic history and a neurologic examination. The psychiatrist was making it on my concept of epileptoid dyscontrol.

A neurologic scale for soft neurologic signs was high in this group. Also, excessive motor activity was noted in psychometric tests and in group therapists ratings.

Unrealistic, bizarre, or unusual thinking was also found in this epileptoid group.

Sexual aggression, both in the past history and the present setting itself, was prominent as was hostility rated by any one of a number of observers. Poor academic performance (although on intellectual measures they were no different than the other groups) was characteristic of this epileptoid group.

1. I have acted on a whim or impulse.

2. I have had sudden changes in my moods.

3. I have had the experience of feeling confused over a familiar place.

4. I do not feel totally responsible for what I do.

5. I have lost control of myself even though I did not want to.

6. I have been surprised by my actions.

*7. I have lost control of myself and hurt other people.

8. My speech has been slurred.

9. I have had "blackouts."

10. I have become wild and uncontrollable after one or two drinks.

*11. I have become so angry that I smashed things.

*12. I have frightened other people with my temper.

13. I have "come to" without knowing where I was or how I got there.

14. I have had indescribable frightening feelings.

15. I have been so tense I would like to scream.

16. I have had the impulse to kill myself.

*17. I have been angry enough to kill somebody.

*18. I have physically attacked and hurt another person.

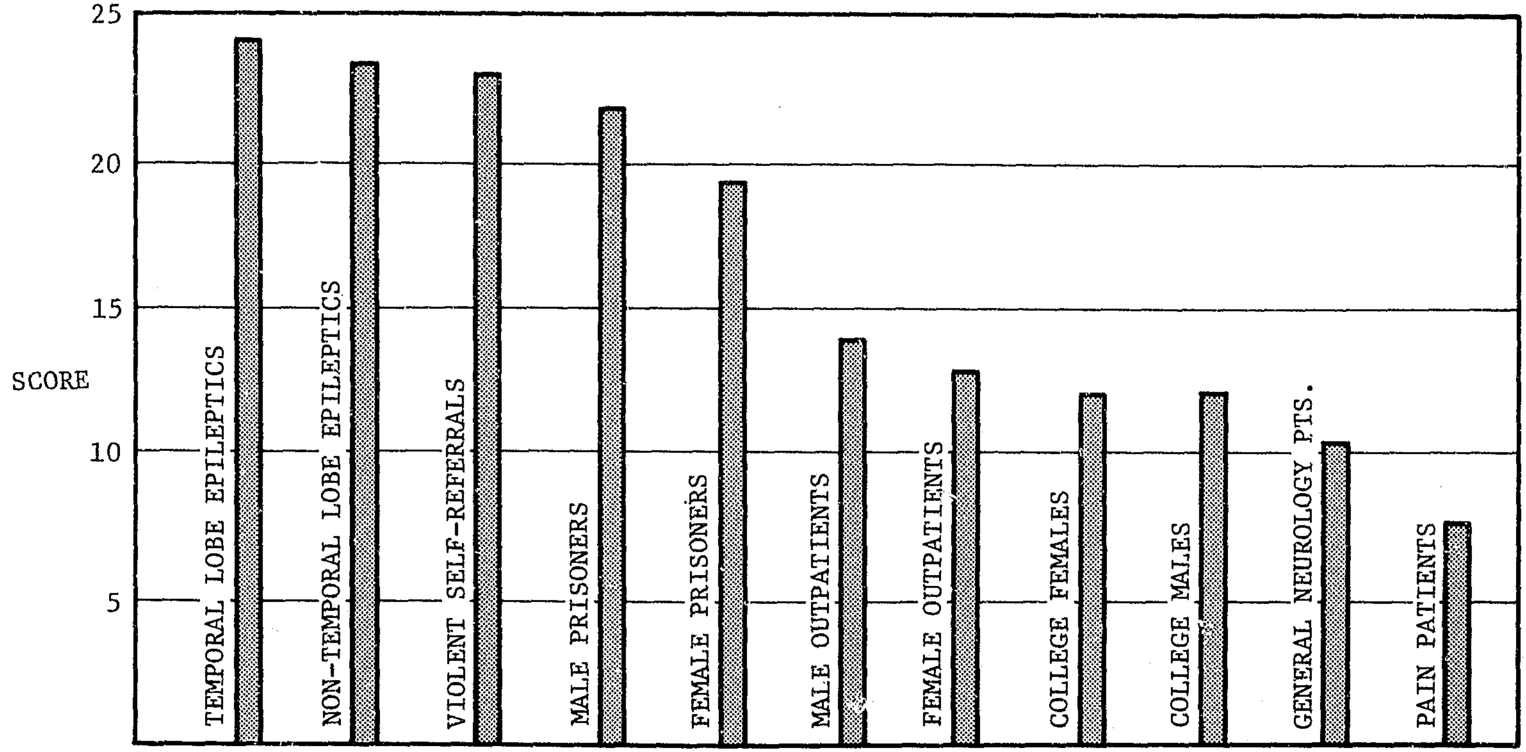
Self-rating: never (0), rarely (1), sometimes (2), often (3).

Source: Appendix A from "Neurologic Findings in Recidivist Aggressors," by Russell R. Monroe, et al. in Psychopathology and Brain Dysfunction, edited by C. Shagass, S. Gershon, and A. J. Friedhoff, (c) 1977 by Raven Press, New York.

*Overt violence sub-scale.

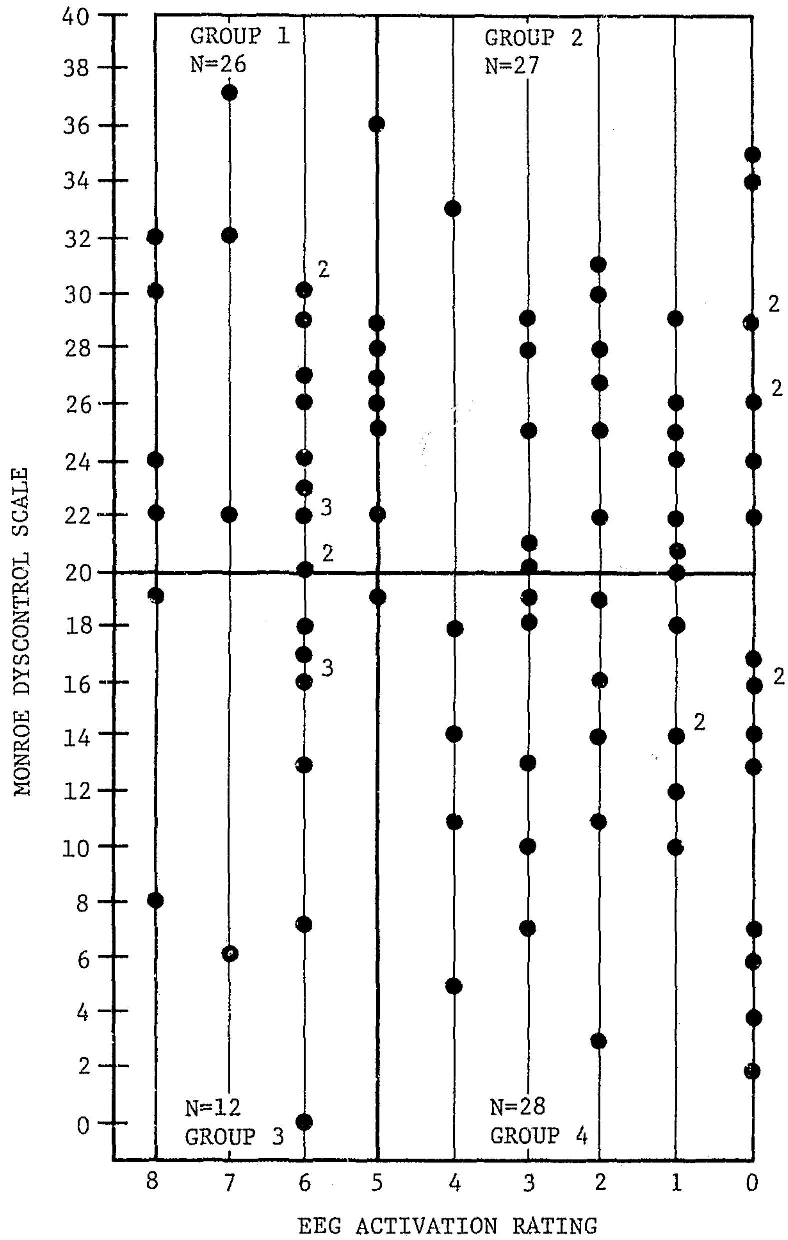
FIGURE 1

MONROE DYSCONTROL SCALE



Source: Figure 3 from "Research Strategies for the Study of Human Violence," by Robert Plutchik, Carolos Climent, and Frank Ervin in Issues in Brain/Behavior Control edited by W. Lynn Smith and Arthur Kling. (c) 1976 Spectrum Publications, Inc. New York, p. 84.

FIGURE 2
MONROE DYSCONTROL SCALE



Source: Figure 8-1 from Brain Dysfunction in Aggressive Criminals by Russell R. Monroe, et al. (c) 1978 by D. C. Heath and Company, Lexington, Massachusetts.

FIGURE 3
SCATTERGRAM OF THE FOUR GROUP SYSTEM

	GROUP 1	GROUP 2	GROUP 3	GROUP 4
Suspicion of epilepsy	+			
Neurologic dysfunction	+			
Excessive motor activity	+			
Deviant thinking	+			
Sexual aggression	+			
Poor academic performance	+			
Passive-aggressive		+		
Amnesia		+		
Less overt guilt		+		
Socially inept			+	
Irresponsible			+	
Poor judgment			+	
Aimless			+	
Poor interpersonal relations			+	
Alcohol abuse			+	
Better abstract thinking				+

Source: Table 12-1 from Brain Dysfunction in Aggressive Criminals by Russel R. Monroe, et al. (c) 1978 by D. C. Heath and Company, Lexington, Massachusetts.

FIGURE 4

SUMMARY OF 4 GROUP CHARACTERISTICS

The interesting thing about those groups of symptoms was that they were very similar to what is now being called the "adult minimal brain dysfunction" syndrome. We had a meeting just a few weeks ago at Scottsdale reviewing this and it seems to me - although we did not collect the data because we did not expect this - that in childhood this group would have probably been called the hyperactive or minimal brain dysfunction child.

The other group that was extremely deviant from within our four-group analysis was group three, the ones we call "inadequate psychopaths". Remember, these people did not show dyscontrol symptoms but did have abnormal theta activity and they were by far the sickest, at least in terms of any social measurements. They were the ones that were socially irresponsible. They showed poor judgment. Their behavior was aimless. They had very poor inter-personal relationships. And they were more likely to abuse drugs and alcohol.

Group four, interestingly, did not come up with any particular differentiating characteristics except that their abstract thinking and reasoning was better, and they did seem to make a better adjustment to prison than the other group.

Group two, the hysteroid group, came up with a few correlations which were along the lines of our predictions, namely that we considered this a more neurotic group. There was less overt guilt in this group. Interestingly, this was the group that claimed amnesia for their aggressive acts or their episodic dyscontrol - again what we predicted, and we predicted that the epileptoid group would not have complete amnesia. In the epileptoid group we predicted some clouding of sensorium, some vague difficulties in remembering, but they would remember, at least in a vague way, what went on. That was true in our study. The pure amnesias were in our "hysteroid" group.

This slide shows the neurologic scale, (Fig.5), which dramatically differentiated the epileptoid group from all other groups. This was based on two sets of data - one historical and the other, a neurological examination. The historical data were much more potent than any cross-sectional data. I think this is an important point. We tend to, in our rating scales, emphasize cross-sectional data when a good evaluation of the historical data is much more powerful.

The neurologic exam is on the next slide, (Fig. 6). These are the kinds of factors that went into our scale which differentiated this epileptoid group. Again, many of those findings are not surprising, except I want to point out that gross coordination was better in this group, rather than poorer.

PART I

A. HISTORICAL DATA

1. BIRTH DATA - Includes age of mother at time of birth, number of other children, birth difficulties, forceps delivery, bleeding or other complications in pregnancy, multiple births, prematurity, resuscitation problems, abnormal apgar signs, or combinations of these factors.

2. HEAD INJURY - Includes range of symptoms from trauma to facial soft tissues to repeated closed head injuries with periods of unconsciousness.

3. EPILEPSY SUSPECT - Includes range of symptoms from dizziness, lightheadedness, blurred vision, deja vu or jamais vu, forgetfulness, distortion in size, space, time, or shape, absentmindedness, dropping objects, episodic enuresis, frequent falls, to a definite history of tonic-clonic convulsions.

4. OTHER CNS "INSULT" - Includes evidence of frontal lobe symptoms such as poor judgment, recent memory impairment, various infections with delirium, or drug abuse to a point of unconsciousness.

FIGURE 5

THE NEUROLOGIC SCALE

PART II

B. EXAMINATION

1. CONGENITAL STIGMATA - Small head, small ears, pectus excavatum, extra toes or fingers, large birthmarks, amblyopic eyes, strabismus, and odd behavior or hyperactivity during exam.

2. HYPERACOUSIS - Distractibility, intolerance to high pitch, cacophony.

3. PHOTOPHOBIA - Intolerance to bright fluorescent light including a history of wearing dark glasses, excessive pupillary reaction to bright light

4. APRAXIA - Fine motor dexterity.

5. MOTOR STRENGTH - Evaluated in terms of the extremities, with particular emphasis on difference between opposite extremities. Arms held overhead against resistance, external rotation against resistance, flexion-extension of elbows and wrists, and grasp. Flexion-extension of knees, dorsiflexion of feet and toes.

*6. COORDINATION - Finger to thumb coordination with emphasis on mirror movements, dysdiadochokinesia, rapid alternating tongue movement, and foot tapping.

*7. SENSATION - Pain (pin-prick), vibration, proprioception.

*These variable scores were subtracted from algebraic sum of 9 other variables as they were found less disturbed in our epileptoid subjects

FIGURE 6

THE NEUROLOGIC SCALE

Other people have noted this - that in their brain dysfunction group (hyperactive group) gross coordination is better, whereas fine coordination may be poor. Also, they were less likely to have any proprioception or other sensorial defects than the rest of the population.

I think this classification is important because it does have prognostic, diagnostic, and therapeutic implications. Our study did include a drug study on this group and we have some interesting results that time does not permit for discussion now.

Our impression is that the epileptoid, episodic group may have a fairly good prognosis, like the hyperactive kids with the abnormal EEGs who are likely to have even better prognosis than those without abnormal EEGs. I think this multi-dimensional kind of analysis of the data offers a good research strategy in looking at the criminal.

DISCUSSION

WOLFGANG: I would like to ask each of our speakers to address the group and now tell us, in very clear remarks, where their work might lead us in terms of needed future research. Then, after that, we will open the meeting for reaction and comments.

Sarnoff?

MEDNICK: Briefly, I think we should aim at prevention. The way to begin to work toward prevention is to find ways of selecting those individuals who are at a very high risk of becoming serious criminals. That is not a very large group of the population. Then you ought to subject them to a variety of research projects to test and develop methods of intervention.

WOLFGANG: Bob?

HARE: The concept of psychopathy comes up so often in criminological research that I think people should pay much more attention to it to make sure we are all talking about the same thing. What's happening now is that the concept is misused a great deal, and all this really does is introduce a lot of problems into the system. If you don't like the use of typologies, then look at the specific behaviors that people suggest make up the different concepts. That may end up telling us exactly the same sort of thing.

Sarnoff's right. Longitudinal research is crucial, but it requires a great deal of investment of time and money. I think some of us are willing to put the time in. It's whether or not we can get the money.

Obviously, we need a far greater amount of interdisciplinary research. I'm not talking only about psychopathy, but crime in general. We have to study psycho-physiological concepts, biochemical concepts--particularly biochemical concepts. Research in biochemistry and psychopathy has been sadly neglected. We need more interaction with sociologists, psychiatrists, criminologists and so on.

An important area of research that is emerging has to do with left/right hemisphere differences and their relationship to crime in general and to psychopathy in particular. I think that more sophisticated research needs to be done in this area.

WOLFGANG: Ken?

MOYER: I guess my first comments would be on more work on basic research, broad, continued basic research to solve the problem. I don't know if it will concur with LEAA or not, but I think that's a general policy--that we clearly need more of that.

On a very broad basis--I didn't get a chance to talk about it much in the lecture nor in my paper--I think we need to have a better understanding of the relationship between positive and negative affective systems and the way in which they relate to the individual's interactions with his environment. My reading of the physiological data suggests that the activation of positive affective systems tends to neurologically inhibit the negative affective systems, which include, among other things, extreme aggressive behavior. We don't know very much about that, frankly, at a basic neurological level. We need to know a great deal more because this might ultimately give us some real hints as to how to go about changing the environment, as well as changing the neurological systems themselves to prevent not only aggressive behavior, but impulsive behavior and perhaps a variety of other behaviors.

Proceeding from the more general to the more specific, it seems to me that we need some concentrated research on the physiology of sex-related aggressive behavior. There are people in the room who know much more about this than I do, but I gather that we would only be influencing a relatively small segment of the population--that is, the people who commit the extremely violent sex crimes. On the other hand, this is one of the worst types of crime that can be dealt with, and it is the kind of crime about which people have the greatest amount of concern, partly because of the media presenting it to us continuously, day after day. I think that we're reasonably close to a physiological control of that kind of behavior, and I think that that is clearly within the lifetime of people in this room. I think it will be quite positive and quite clear, but we don't know enough about it yet so we clearly need work in that area.

Two more physiological systems--and I come at this from a physiological viewpoint--are the problems of hypoglycemia, specifically, and the problem of aggression as an allergy reaction. It's perfectly clear that a lot of people have hypoglycemia. We don't really know how many, although there are figures. I'm not sure how reliable they are. It is clear that some people with hypoglycemia behave in this impulsive, aggressive manner. We can't yet identify who those might be.

We don't know whether we're dealing with a very large population or not. And we don't really know the physiological variables of which this is a function.

Finally, with the allergic response--we're talking about very difficult areas--we need to know something more about how many people who have allergies do exhibit this kind of behavior, and we need methods of identifying them. Apparently, it is really not all that difficult. This is a look at some specific points that I think we should work on.

One further, more general, point is the continued work and the more intensive work on the development of anti-hostility pharmaceutical substances which have, as one component of their action, clearcut anti-hostility action. We need more development in this. We need to know more about them. Some of them, for example, give a paradoxical reaction. Librium, for some people, reduces their tendency to hostility, but, in others, there is a paradoxical reaction where they break up the office furniture. We need to know more about that, and we also need to continue the development of anti-hostility drugs because they would be very useful in helping people who behave aggressively that clearly do not want to behave aggressively.

I was invited to a conference a couple of years ago in which the major question of the conference was: Should we put lithium in the water supply? Lithium is an anti-hostility agent for certain kinds of aggressive behavior. I don't know if I need to take the time to point out that I do not agree with that idea or the use of any type of drug, lithium or any other anti-hostility drug, for that. But it is a useful drug for certain purposes for certain people. We need to know more about it and we need to know more about a lot of other agents.

WOLFGANG: Russ?

MONROE: I'm not a lumper, I'm a splitter. I think this is characteristic of research in psychopathology in general. We are looking at sub-groups of schizophrenia, of affective disorder, and I think--in terms of criminology or aggression--similar subgroups.

There are so many possibilities - you have a theory to start with and then look at possible sub-groups. Ken, you were referring to a response to drugs. Why do some aggressions respond to Librium? and some don't? I think we already have some data that can differentiate differences of drug responses.

Why are some of the hyperkinetic kids probably dompamine-deficient kids and some aren't? I think we can get the behavioral criteria for this and we can begin setting up some kind of sub-grouping which would be pertinent in terms of specific drug therapy.

Because we've been interested in the EEG, we've gotten very high correlations with our drug-activated theta waves and baseline hyperventilation doesn't indicate dyscontrol behavior. But if other factors are also present these factors lead to impulsive behavior.

WOLFGANG: Very good.

I hear loudly and clearly reference to longitudinal studies and preventive predictions. I'd like, now, to open the discussion, keeping in mind that while we may all have a general consensus about certain things, I expect to hear some differences, too.

There are many legal and ethical issues that are associated with some of the suggestions that have been made. I think that that is so obvious that I won't belabor it. The issues that were raised long ago with respect to Eleanor Glueck's work on prediction produced, as you know, a considerable reaction about labelling. We are still faced with many of those same problems--perhaps even more so today because of the greater sensitivity to the issues of invasion of privacy.

PRESCOTT: I'd like to comment on several things, especially biological measures. I think one of the real problems, certainly in the public mind, is that when we talk about biological measures it means genetics. I think that's the most serious error that exists in the psychobiological sciences today. Wilson's sociobiology has really forced upon both professional and public minds that our biology is determined primarily by genotypes, and that simply is not true. Many of these psychobiological variables are more under the influence of environmental factors than can be shown to be linked to certain specific genotypical characteristics.

I would like to know if you have any data that can show a specific linkage between genetic characteristics and biologic measures? There's a whole host of developmental data that shows how the environment can alter the structural and functional development of the brain--e.g., morphology, electrophysiology and neurocentral transmittal substances. There are profound changes and the literature has well established the effects of the environment upon the developing brain.

I would like to make a plea to try to better present that point of view to the public and to show how the social/cultural environment can, in fact, be translated into a system that can be reflected in brain processes. I have a bias, that unless our social variables and cultural variables can, in fact, be translated into effects upon brain processes, then they are not very good variables. The brain is the organ of behavior and we have to understand brain process to understand behavior.

Two other points. I'm very pleased that Dr. Moyer raised the issue of a reciprocal relationship between the pleasure system of the brain and the violence system of the brain. I probably feel more strongly than he does. In fact, I'm convinced that this is the basic way the brain functions, so that when you activate the pleasure system of the brain, you reciprocally inhibit the neural system mediating violence. I think there is sufficient data to support that point of view.

This now gets us to the real thorny issue of the role of pleasure in our society. I feel that if the reciprocal relationship between pleasure and violence is valid, then obviously physical pleasure and affection becomes a major variable of importance in studying the origins of violence. I'll talk about some of that this afternoon, but I would like to end on the point of the suggestion that Dr. Moyer raised about pharmaceutical agents to help control violent aggression.

The inaneness of our society is illustrated with respect to the specific issue. We have probably the most effective pharmaceutical agent available to control violent aggression--marijuana--and yet we have, as a national policy, the elimination of marijuana from our society. At the same time, we have a national policy that aids and abets the consumption of alcohol which is demonstrably known to be linked to the expression of violence and aggression.

We have national policies that work against each other in terms of controlling one of the major problems in our society--violence. I think we ought to address ourselves to that issue. We have to reverse the drug policy posture of our society by supporting the use of marijuana and decreasing our support of the use of alcohol. I think that, in itself, could go a long way. There are social/behavioral strategies that could reduce alcohol consumption by over 50 percent in our country in a very short time, e.g., taxation. They would force the production of 20 proof distilled spirits that would sell for $5.00 per bottle. Eighty proof would sell for $20.00 per bottle. We should start examining facets of our society and what we permit to be expressed.

The sexually violent film "Clockwork Orange," had no problem being shown in our neighborhood theaters. However, the really pleasurable and nonviolent films, like "Deep Throat," is banned. There can be documented a variety of social phenomena which reflects the conditions of our society that support the expression of violence and inhibit the expression of pleasure, e.g., massage parlors.

WOLFGANG: What Dr. Prescott said early on about the language that's used in biology does not necessarily translate, but it is important. Its being differentially understood is especially important for LEAA if they are going to fund any kind of research in this area because those types of research efforts can very quickly and easily be misinterpreted.

Last week I mentioned this to some researchers on minority issues attending a meeting for the Minority Research and Criminal Justice workshop. There were only about two or three whites in the room.

On the initial agenda sent out in advance, there were physiological factors which considered about 18 variables. When I mentioned that in one of the workshops, I was immediately put down in the sense that the reaction was extraordinarily negative. One of the members said he would not be identified in any way with a statement dealing with physiological factors that would come out of the meeting. It was for the very reason that Dr. Prescott mentioned: there is an immediate tendency to look upon it as a genocidal policy.

Unless or until that misinterpretation is corrected, there are dangers--political dangers--involved in the support of such research. I find it rather appalling to have that kind of reaction within a research community. That etiological stance immediately suppresses the curiosity of the scientist.

ROBINS: I just wanted to push this one step farther. I agree that biology doesn't necessarily mean genotypes. I think you can also argue that you don't necessarily have to see biology as the <u>primary</u> variable. That is, if people are scared, their palms sweat. The important question is: Can you predict that people with sweaty palms will feel scared? Or if you know somebody feels scared, can you predict that his palms will be sweaty?

The assumption that psychophysiological differences <u>explain</u> differences in behavior is a problem. We need to look very carefully at whether we are looking at something "basic" when we look at physiological research, or whether we're just asking, "Are you scared?" in a different way, and perhaps in no better a way than by asking outright.

HARBURG: I would like to respond to some of the ideas I hear in terms of what I call a continuum from something polemic to something called scientific research, which, in terms of the attack by counter-cultures in the 1960's, no longer holds an absolute position, and it, too, can be constrained by public policies and public moralities.

I think we are here--we self-define ourselves by being here--as scientists. In which case, we get into a framework from a pure science point of view in which we can put electrodes into anything unconstrained by anyone. On the other hand, we are citizens and we have to understand that there's an ongoing social system and culture which we're in, in which there are political and social rights that we believe in, at least in our society.

I don't think that just because there seem to be apparent contradictions of the logical extension of arguments that we should waste time with those kinds of things. If we are talking as scientists--and I think many statements that have been made to us today were from that position--we are also talking as citizens in terms of advocating practical applications of knowledge in which, if the whole society wanted to, it could participate. I think it was great--from a viewpoint of scientific imagination--that it was suggested that we all have SOMA pills and take care of our deviant sexual aggression and everything else we want to take care of. We could flood society with SOMA pills, marijuana, alcohol, eating too much--anything you want. We all have drugs of our choice. We all have a responsibility to talk first as scientists but then we must pursue, somewhat, the social implications of our scientific work into the kinds of political and social responses that we might get if we were to pursue that study.

There's this awful dialogue and dialectic that administrators giving out money have to do in terms of balancing these kinds of forces. I would hope that we, here, don't get involved in those kinds of sensitivities to the exclusion of scientific pursuits--genetic or whatever it is. We all realize we as scientists are all constrained from defying other institutional areas.

Whatever wants to come out of the conference, I think it has to be both something scientific and some practical implications for law enforcement agencies.

MEDNICK: I'm a little sensitive to this sort of discussion. So I've though about it a little bit. I don't think I can be convinced at this point--without a lot of new data--that there are no genetic factors relating to anti-social behavior. It would be very hard to convince me of that. It was very hard to convince me that there were genetic factors.

PRESCOTT: I didn't say that there were no genetic factors relating to anti-social behavior. I don't want to be misunderstood on that point. I thought you might be responding to my emphasis upon the role of the environment in shaping our biology and our behavior.

MEDNICK: Given that--and I think there are several people who also believe this now--what does it mean?

First of all, you have to ask: How much variance does it explain? As I tried to explain before, it doesn't seem to explain that much variance. It explains a lot more if you consider the social factors that are interacting with the genetic factors.

What does it mean? It means that there are some sorts of predisposing factors that increase the probability of a person becoming criminal, and that these predisposing factors are biological.

WOLFGANG: I want to recognize David Barcik.

BARCIK: Let's say I'm on the parole board. I'm not, but let's say I'm on the parole board reviewing cases in the State of Maryland and I want to use whatever data I can build on. It doesn't really matter to me if it's physiological, sociological, developmental, or genetic factor. I have a specific question to answer. That question is: What kind of person is this that I'm considering putting back on the streets of Maryland?

Right now, we are using psychiatric information, psychological data, and past historical data. What I'm saying here today is that there is a very substantial possibility that in the collection of the kinds of physiological data presented here that maybe we can begin to cut some of the variables in terms of our predictions to use the data in a practical way so we can complete the process of getting the inmate back on the streets who is the most likely to succeed on the streets.

That's one. Two is that if we allow this physiological approach to develop in the next five years, if we can begin to tag and identify certain groups of criminals by their physiological profile, we have just about automatically set up a basis to treat people who are in the system and we don't have that now.

The treatment effects are zero, basically, with what we are doing now. If we can somehow function under the potential criticism of society and continue this development from the genetic all the way through to more potential data, very sophisticated computer analysis data, we will have, potentially, a way to look at the people in our institutions very differently. Therefore, we can give them treatment procedures and, therefore, we can give them parole, let's say.

I would hope that, in the future, anybody--LEAA or anybody else--would very seriously look at those things. What if we could establish a physiological fingerprint?

WOLFGANG: My reactions to what you have just said cause me to ask this group to keep in mind the growing movement towards what is called the "just deserts" model. The shifting of the sands of thought about retribution, treatment, prediction, and the move from the medical model to the retribution model.

I'm not advocating that here, necessarily, but to shift it is certainly there. It's in Senate Bill number 1437. It's in state statutes, it has been passed in 15 states. The notion of having a physiological fingerprint with respect to predicting future danger signs is not at all a part of that new philosophy in the criminal justice system. Somehow or other, we have to keep that in mind as we examine some of these bio-social variables with respect to prevention. It seems to me that they are more consistent with prevention and treatment, and not as a basis for determining the degree of sanction.

I'm sorry to have used the Chair to express my own thoughts.

BUCHSBAUM: I was going to speak about prevention, but first I wanted to comment on being scared in defense of why we use biological measures. That is where culture comes into contact with it. If you ask somebody whether they are scared, they may or may not want to admit it. They may think about being scared in a different way. They may not want to say they are scared to an attractive woman, so all sorts of cultural barriers enter into it.

The reason why we have something like a palm-skin response to use as an index of that is that presumably that's a relatively specific indicator of something going on inside the central nervous system, which would be the same across various kinds of cultures and social experiences. That's the advantage of using that over asking the person if they are scared.

ROBINS: Again, I think we're making the same assumptions, that one is actually better than the other. It may be that there are some people who are scared and their being scared does inhibit their aggressive behavior, but whom you would not measure in that way. It may be that you need to use them both.

HARE: You made the distinction yourself. You were saying, why not simply ask the person if he is scared.

ROBINS: Yes, but I was raising the question of what's basic. That is, if the skin response is there, does that show he is a certain kind of person?

HARE: A whole literature has been developed in the last 10 or 15 years involving physiological activity and behavior; the two are viewed as a package. I'm a little concerned about some of the discussion here because it seems as if we're moving back 200 years and talking as if we have a separate mind and a separate body. We want to look at the whole man. Perhaps we should be talking about psycho-social-physiology. One approach is not better than the other. The skin conductance response may not be a better indicant of fear than is a self-report; but it can be used along with the self-report, along with behavior, and so on.

ROBINS: Right.

HARBURG: I think your plea for all the inter-disciplinary research has been the cry in the wilderness for the last 30 years. I think it's a strategic idea in terms of research; however, I think that we now know why multi-disciplinary research does not happen. It's for a variety of reasons.

HARE: But it is happening.

HARBURG: But I just want to say that it is a very, very difficult, very complex organization. It's very difficult in terms of the administrative funding and all sorts of things. We agree, yes there should be more multi-disciplinary research. I would appeal to everyone who agrees with that and say: How can we do so? How can we ably administrate it to move along those lines? I don't hear any answer. In fact, all the trends are militating against it.

HARE: A good example of an interdisciplinary approach to important problems is in the area of biofeedback. It started out with people being concerned with measuring heart rate and skin conductance and giving the individual some feedback on what was happening. But now we are beginning to talk about the influence of procedural effects, psychological and social variables, psychosomatic concepts, etc. It seems to me that the same sort of multidisciplinary approach is required in criminological research.

HARBURG: If I could just finish with the other half of my point. That is the fact that we are trying, here, to study, we'll say, criminals or socio-paths--some diagnostic division. Now, there are two ways of defining that subject population. The legal establishment defines them in certain ways. If you are convicted for petty larceny and imprisoned, you are defined as a criminal and you could then do your biology studies.

There may be great similarties between a person who reacts biologically and a person who is actually convicted and incarcerated, and a person who is merely a philanderer--who cheats on his income tax--and who never comes under the legal system. Some of the personality characteristics and, conceivably, some of the biology might be in common.

The second issue is that the criminal population may be extremely heterogeneous with respect to biological causes. There may be 20 kinds, 50 kinds, 1,000 kinds of biological insults or deficiencies that can lead to a variety of criminal behavior. Therefore, if we start with a biological measure--some autonomic skin resistance, some biochemical measure--and we measure populations of criminals and compare them with populations of normals we may never come out with any biological contribution that is valuable at all.

If, on the other hand, we start with our best guess of particular kinds of mechanisms, let's say, chromosome counts and follow the biology of it and look among criminal populations for the chromosome that is analogous and study that through a specific biological entity, we may come up with a result a lot faster than in counting the number of chromosome abnormalities in the entire prison population and comparing it with a large population of normals.

BARCIK: In a prison when we run a treatment program, we rely on external behavior as well as institutional adjustment and psychological testing. Quite often, it's not enough. We don't know where we are in the treatment process.

Isn't it conceivable that if you go beyond psychiatric interviews that there are some physiological handles on which we can hang the hat of progress in treatment within institutions? Wouldn't that be handy for us to know that, maybe, we have identified a group of psychopaths, and find that, in the process of whatever treatment we set up, we can actually use the same measure that were used to diagnose them to establish that we've gotten somewhere and can predict that they're ready for the street again.

HARE: These are all empirical questions. Unless we investigate, what we are doing is accepting the hypothesis without actually testing it.

BARCIK: Right.

MONROE: I don't know whether this is changing the subject or not, but I would like to ask Dr. Wolfgang a question. I'm always citing your work concerning the six percent of your cohort that committed 50 percent of the offenses. I have an explanation for it, but I've never heard your explanation or whether you have any comments on that small number of individuals that are recidivists.

WOLFGANG: I don't have a good explanation. What I can add to your information is that, on the follow-up of a sample up to age 30, that six percent of chronic offenders has now grown to 14 percent with five or more official offenses.

The other thing I would add is that at age 26 we interviewed as many as we could find. It was a long interview, and among other things, we had a self-report schedule. We asked them if they had committed any of many criminal acts, ranging from murder down to petty offenses, before age 18 and 18 and after; the usual methodological problems about validity of self-reports existed, plus the fact that we didn't have the focused time frames that the victimization or survey has utilized. Therefore, it was rather wide open. We did, however, find a relatively high correlation between the official records and the unofficial acts that they admitted having committed but for which they were never arrested--particularly with the chronic offenders. The officially-recorded chronic offenders also admitted committing more and more serious offenses which gives us a little bit stronger confidence in the official record as an index of criminology.

Now, as for an explanation of the six percent up to age 18 were responsible for 53 percent of the offenses, and for around two-thirds of the violent offenses, I am not prepared, yet, to offer one, but I certainly would like to hear yours.

MONROE: Well, I take other people's data and make it fit mine, like everybody else. I was assuming that the six percent was somehow related to the ones that had been found with serious neurologic findings, the high genetic predisposition and perinatal complications, early childhood involvement of the central nervous system, etc. In other words, they are the ones that had multiple etiologic factors.

WOLFGANG: That is a working hypothesis. I would like nothing more than to be able to capture that six percent--now 14 percent--and subject them to a battery of examinations that would be psychological, physiological and endocrinological.

My own working hypothesis about that six percent is very much like yours. I would also guess where they would be in your typology, but I don't know. This is where the inter-disciplinary, multi-disciplinary research is really necessary. As a sociologist, I really cry out to my colleagues in psychology, biochemistry and endocrinology for their assistance and help. I think that's where the linkage is between these prescriptive, empirical facts and the individual personality factors, which needs to be examined.

HARBURG: Is there any ecological location, the places in the city where these people come from?

WOLFGANG: Yes, we have them by census tracts.

HARBURG: Do they cluster?

WOLFGANG: They cluster and are in the general demographic disadvantaged areas.

CLAYTON: I like the idea of a prospective study that's long range and that ties into a general population. It strikes me if we could use something like yours, the 6 or 14 percent cohort, and retrospectively get life history data on drug abuse and whatever else, then take your psycho-physiological, biological measurements from a range of people, not just the 14 percent, but a sample of those others, that you might really have information that would be extremely valuable. It might not be as tight as we would like, but we could space it out over a life history approach to developmental approaches. I think we could really be doing a test on these things.

MOYER: I want to respond that biological treatment certainly seems to be one of the things needed. Criminologists may not agree, but it seems clear to me that we need some extensive studies on taxonomies so that we can identify the kind of individual who is likely to respond to a given treatment. It isn't going to help at all to give anti-allergenic agents to petty larcens. It isn't going to help them, and the same thing applies to a whole variety of other possible potential physiological treatments. There are large percentages of the criminal population that they are not going to help at all, and one wouldn't expect them to. Until we can do this reasonably, I think we need to have some facts that at least give us some indication of the probability that a given criminal could be useful in society.

BARCIK: I wasn't talking about physiological treatment as much as I was about psychological treatment, although both, of course, are extremely important. I just find that, when I'm in the middle of running a treatment program, I don't know where I am with people. Why do some of the people respond and some don't? Maybe it's because they have a different profile. If I could know that, I could be much more effective as a treater within the prison system.

HARE: We are a long way from that. People are always asking if there is a psychophysiological marker for psychopathy. I wish there was, but I fear it will be a long time before we have a really good one.

MOYER: Perhaps the diagnosis cured them.

CLAYTON: What bothers me about that is something that we spoke of before. We talked about a pulse rate at age 11 being a good predictor of delinquency later on. It strikes me that those are important measurements to take. As a sociologist, I would want to telescope in on some of the precipitating conditions and the environmental conditions, specifically thinking of aggression.

We often look at just the person who is the aggressor and don't look at the person who is the victim. It may be that there is something that is indescribable there - based on what we know now about the person who is the victim in child abuse. It may be that the child who is abused is responsible for precipitating the abuse in some unknown number of cases.

HARBURG: I think it's inherent to what we were talking about before--the multi-disciplinary approach. For most biologically oriented researchers, a sample of three rats, six dogs, or 12 patients walking down the hallway, or somebody as an outpatient is considered sufficient to observe the event of interest. But, when I suggest to them, well, why don't you get some kind of sample that speaks to the environment from which these people come, I get a blank stare.

What I would like to see is more research in which you encourage meaningful sampling--and you can put whatever meaning you want in there. I don't care whether it's a representation a la Leslie Kish or whatever, but that the sample is drawn from the high-risk density places in the urban areas that you are talking about, and you get the people who do it, who don't do it, and so forth.

We're going to speak to that later, but I think there has to be what I'm going to call "better sampling."

HARE: One relevant sample could come from the area of child abuse, a topic which was brought up earlier.

HARBURG: Yes, but the physiologists and the biochemists and the sociologists have to work with a sample of a population that they can all learn from and then can cross their variables from.

PRESCOTT: I thought I understood that the climate in the Department of Justice was not to accept a research strategy that would give us unique brain signatures to identify uniquely cataloged individuals. Is that right?

WOLFGANG: No, that's not stated correctly. The only remark that I meant to make was not in terms of the direction or suppression of any kind of research, but rather a suggestion that we keep in mind that the general orientation today within criminal justice--both in legislation and administration--is moving towards a retributive model. Retribution is not viewed as negatively as it was in the 1940s.

GREGG: I think that's a very good point. I think the reason, or one very important reason, that that has happened is because of the frustation of people who have virtually given up on doing anything more meaningful. If that hope can be revived, possibly their thoughts will change once again.

EWING: I would just add that as far as the signals we get about the kind of research we sponsor, we're more limited than we were in the past. Indeed, the reverse is true. The Administration is eager to support more fundamental kinds of inquiry.

FIRST DAY AFTERNOON SESSION

WOLFGANG: I think you will all agree that we were blessed by having four people this morning who were able to provide a very neat summary of their papers and then add some comments. I'm sure that we will be able to do as well this afternoon. We have two sessions, really, of three papers each.

Let us begin right away. The two remaining groups are Group B and Group C. You can see that in Group B there is some generalized clustering as we move from the drug studies to post-high school experiences to alcohol and crime in veterans.

Let's start right away, without any specific introduction, with Richard Blum.

TOWARD A DEVELOPMENTAL APPROACH IN CRIMINOLOGY: CLUES FROM DRUG STUDIES

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The approach was simply to concentrate, first, on the description of relationships between drug use and criminality, and then focus on the work which we have done over the last 10 or 15 years. That has little to do with the relationship between drugs and criminality, but has tried to evolve concepts of the developmental process which, I believe, is worthy of our attention--the development process whereby people come to express themselves by drug use and criminal acts.

Illustrative correlations are as follows: (see author's paper) "Users of illicit drugs are more likely than non-users to be engaged in non-drug crimes. Known offenders are more likely to have histories of extreme drug use, either illicit or sanctioned alcohol, than are their non-offender peers.

"Heavy alcohol users are more likely to have offense records than are moderate drinkers. Drug use itself, with alcohol foremost, is perceived as a precipitating factor for crime and police contact by users."

You will see the paper describes some of the family work we have done. Those family studies led to our interest in long-term etiological factors in that population where there is a correlation between drug use and criminality. For example, from our interpretation of the data:

"... one infers three themes present in the high drug use youngsters contrasted with long drug use ones--when their families are matched socioeconomically and when all are intact. One factor seems to be a 'trouble' variable beginning in infancy and including early health problems and parental uncertainty.

"It includes craving as measured by over-eating, mild conduct disorder beginning early in school, and an apparent early lack of self-confidence. A second theme reflects a philosophy of life which is self-centered and self-indulgent but which by no means implies impairment in ability to work or achieve.

"A third theme reflects learning from parents--both direct conduct and ideas, as for example, drug use and self-centered values.

"As children learn from parents, so have the grandparents been influential on the child's parents. The positive nature of parent-grandparent ties and the sense of affectionate, respectful family continuity appears to differentiate families with low drug use youngsters from others."

I move now to another summary, this from the school studies we've been doing for the last eight years. These are longitudinal beginning in the second grade.

"... movement into new drug use was associated with poorer school and peer adjustment; 6th and 10th graders showing exaggerated drug use changes had more personal, school and family problems."

Now, for an integrated summary, I refer you to my paper, for the purpose of our later discussion. I will not recite it here.

I do call your attention to our work with a marvelous population for getting a concentrated analysis, a purified sample of the intense relationship between drugs, crime and drug dealers.

From these varied studies, we have concluded that it is useful to consider the development sequences and epochs starting at least with the grandparents' generation, moving to the parents' generation--both in terms of their values and their child-rearing style, their own conduct with reference to the use of drugs and alcohol, and their own criminality, and then moving to the birth of the child where we find, quite interestingly, a consistent, although low order correlation, with birth difficulties, per se, among those kids who end up in our extreme drug use groups. Next, one inquires about the relationship between mother and child. There is observed a "trouble" variable, one which we can as yet define in no other way--a child that the mother is very early more concerned about when compared with siblings that become our extreme drug user in the family.

It's a child who, early on, seems to have eating disorders. We find this curious distribution of the notion of craving, of appetite, moving through childhood and into adulthood in both family and our normal population studies.

As school begins, we find the later extreme youngsters do have more troubles early on. These troubles predict involvement in classroom delinquency, outside-of-school delinquency, identification, naturally, by the teacher as troubled kids, and decreasingly adequate school performance.

In California the opportunity to observe illicit drug use, itself, begins in the fourth and fifth grade. Perhaps it is a rush to conform, and in California conformity is to become like one's older peers who are heavily involved in drug use. Thus, the span of observation for childhood events leading to extreme drug use--or delinquency--is short.

In adolescence, one sees the emergence of patterns of attitudes--many of which are not at all related to criminality, but which are related to kinds of drug use. These correlative clusters are political and religious involvements, intellectual postures, and so forth, including perceived and actual parent-child interactions.

Thus, one must be interested, not only in value conflicts and child rearing styles, but family pathology.

It is very clear, for example, that the children who become very involved in extreme drug use and who are--on the basis of our descriptive approach--much more at risk of delinquency are those who come from families which psychiatrists and family counselors would call "very troubled families." These troubled families are not different socioeconomically in our studies from their control match families.

I suggest that if we continue development we will find, for each period of development, a certain set of variables which will distinguish youngsters into several classes. One class will be those who later emerge as heavily involved in drug use. In that group there will be a sub-set for whom drug use is intimately associated with delinquency. Among them there will be a sub-set evidencing extreme drug/crime dealers.

One of the important features in the study of development processes and epochs will be the identification of critical transition points, the identification of variables associated with development periods, the description of the length of these etiologically consequential development periods, and distinguishing the transformation of genotypical variables which are expressed as phenotypes.

From an epidemiological standpoint, it's critically important that one not make assumptions based on assumed external classifications. One does not want to assume that drug users are one class for the purposes of developmental etiological analysis; that heroin users are one class. One does not assume, for instance, any more than one wishes that criminals are but one category.

The research prospect then is of longitudinal and developmental work with stage or epoch identification and both antecedent and correlated variable identification for distinguishing paths or careers or syndromes with quite specific outcome criteria.

I think we may be optimistic about this type of research and it can be done economically in short cross-sectional series or short-time span periods. For example, one need not observe a cohort over 30 years; one can observe a cohort two or three years at a time.

I'm not as optimistic with regard to practicality. What we learn in this may not be at all immediately applicable to crime control.

Nevertheless, I would strongly argue that such basic research is, itself, exciting and that we need not promise practical results, even though we may have practical purposes.

DELINQUENT BEHAVIOR LINKED TO EDUCATIONAL ATTAINMENT
AND POST-HIGH SCHOOL EXPERIENCES

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When Ms. Erskine asked me to participate in this conference on the determinants of crime and correlates of criminal behavior--something like that--I wasn't quite sure why I should be involved in it since I hadn't been a student of anything like that, so far as I knew.

She said she was interested in something that I had done about drop-outs and delinquency. I have been very interested in reading the papers and listening this morning, and deciding that maybe there is some relationship with my own work.

The research I reported on in the paper started over a decade ago and did have very much to do with the causes and consequences of dropping out of high school. It expanded in a number of directions after that.

One of our initial concerns was with a wide range of behavioral and psychological outcomes, and the behaviors included some measures of delinquency. We later added measures of drug use.

Those of you who read the paper closely may be aware of some of the problems with our retrospective data and some time intervals in delinquency, and things like that. In this summary, I'll gloss over all of that and just tell you what I think we found--although I feel cautious about some of those conclusions.

One of the ones that I feel less cautious about, perhaps, is that an awful lot of the differences that one finds between groups who have attained different amounts of education seem to be differences that were there before they ever got the different amounts of education.

Now, that sounds like an awfully obvious thing to say; yet it is surprising the extent to which people will look at cross-sectional data and reach a different conclusion. Their reasoning goes something like this: (a) drop-outs have much higher rates of crime, much higher rates of delinquency; therefore (b) if we could just keep those kids in high school for the last couple of years, we would save the Nation billions of dollars in the costs of youthful crime.

Now, that argument is in a Congressional Committee Report of a few years ago. The author and I had an interesting exchange of letters about that sort of thing, and some other things, because the assertion was really made that there are differences in delinquency rates between drop-outs and high school graduates, and it was clearly asserted that dropping out was the cause.

Well, those of you who looked at the paper recognize that our measures of delinquent behavior--which were first collected at the start of tenth grade, but were retrospective throughout junior high school--show very large differences between those who later became high school drop-outs and those who went on to varying other levels of education. Indeed, the more education somebody went after, the lower, on the average, was his level of delinquency back in junior high school.

It seems fairly clear that the differences in delinquency and, for that matter, probably the differences in cigarette smoking and alcohol consumption that we found related to different levels of educational attainment, were back there during high school and prior to high school, if anything.

I think much the same thing can be said for some smaller differences that we found related to levels of occupational attainment--status of the job attained five years after high school. There are some differences there in delinquency and other things. They tend to be smaller than the educationally-related ones, and they tend to be, I think, explainable in terms of the educational differences.

Once again, these are things that seem to have been there before people sorted themselves into these different levels of attainment.

Now, it's perhaps gratuitous to point out that you don't pin down those things without longitudinal research. I think that in a lot of areas one doesn't need longitudinal research; but where you have a strong relationship that's being subject to these kinds of policy implications, it's often useful if you can get prior measures before people sort themselves into different environments.

Well, if we didn't find consequences of dropping-out in terms of higher rates of delinquency and a whole bunch of other things that we looked at--lower self-esteem and so forth--if we didn't find those kinds of consequences, we did find what seemed to be some effects of unemployment.

The numbers of cases aren't terribly large. The proportions in this aren't terribly large. Therefore, the correlation statistics tend to be small.

But we did find that levels of aggression, marijuana consumption, and use of illicit drugs were well above average for the unemployed. Fully twice as much inter-personal aggression and illicit use of such drugs as amphetamines, barbiturates, and hallucinogens was found among the unemployed as the employed, and those differences weren't there--at least as strongly--back when they were in high shcool.

So it's not quite iron-tight. The longitudinal interval is rather large after high school with a four-year span in our study from one year after high school to five years after high school; thus it's certainly possible that somebody got to be a heavy drug user or more delinquent or more aggressive during that period and then got unemployed, but I think it's a little less likely than the alternative interpretation.

At least, with this one limited longitudinal study, the possibility remains quite open that unemployment has contributed to some of these kinds of problems, unlike the business of dropping out of high school.

The positive side that we mentioned was that those who became fathers by age 23 apparently improved along some dimensions, although they started out worse than others; also, those who were married were less likely to show the big increases in marijuana use than those who were single.

So there are some interesting hints here and in other areas that some environments and experiences during late adolescence and early adulthood can have an impact on some of these outcomes.

But I suppose the most compelling finding, predictably, about what I was asked to talk about initially--dropping out and delinquency--is that the delinquency seems to come first.

Now, I don't conclude from that that one should not worry about educational problems or something like that. I don't really know for sure, but I have a very strong hunch that our findings imply that remedial educational efforts should start much earlier because these individuals who dropped out had, for example, twice as much likelihood of having failed a grade prior to high school. They had much poorer self-reported grades. They had much less positive attitudes about school. A whole host of things contribute to dropping-out and also correlate with delinquency. Therefore, it would be a mistake to say that educational experience doesn't have anything to do with delinquency, but it's very likely something that happens a great deal earlier than high school.

I would try to speculate a little bit, now, about possible application implications and maybe some research implications, as well--the ones I have already talked about.

If one's going to deal with educational issues as they impact on crime and delinquency, I suspect one ought to start a great deal earlier than we did. I think maybe the other side of that is that we probably ought to back away from this anti-drop-out campaign that's been going on for the last decade or two.

Sometimes people ask whether I would recommend that a kid drop out of school, and I certainly wouldn't do that. Right now, the dice are loaded against somebody who does. The way things are organized currently you do increase the chance of being unemployed, and being unemployed creates a lot of problems in addition to the ones that we have mentioned here.

On the other hand, I think that we could do something to readjust the way those dice are loaded. I think one of the bad side effects of the anti-drop-out campaign is that we really make it more difficult for somebody to get a job.

When we were doing some early pilot studies that led to this Youth in Transition study, we were talking with one assistant principal in the Ann Arbor/Detroit area who complained bitterly that the auto companies were hiring his kids out of high school--hiring them away from school--and the kids found the jobs so attractive that they didn't go back to school. He told me that he had tried very hard to persuade those auto people not to hire anybody without a high school diploma, because everybody knows that high school dropouts have a tough time getting jobs.

That was an extreme case. But a lot of this business is really very troublesome. I think it means that we have to work on a better set of credentials for people than number of years of education, number of diplomas--and that certainly doesn't apply just to a high school diploma, although that's a particularly critical one.

It's the case right now that if an employer wants to know something about a whole bunch of job applicants and he only has a limited number of jobs, it's true that knowing whether somebody is a high school drop-out or not provides some extra information. My research, and a lot of other research, shows that the odds are higher that the dropout has had a history of delinquency and a few other things; so it's not like there's no information there. And that's part of the trouble with these credentials: they do tell a prospective employer something.

If we were going to think in terms of applied research in this area, we had better think of some better credentials to substitute Right now, just knowing how many degrees or how many years of education someone has piled up does have some predictive value. In the absence of anything better, I think an employer's likely to use that.

I think that is an area that we might work on.

An obvious research implication--which I have pointed to before--is that, for some things, longitudinal designs are just invaluable. I think it's particularly true for those of us who are interested in sorting out the impact of social environments or experiences (I have trouble sorting out environments from experiences, so I lump them together).

For those of us particularly concerned about trying to get at the impact of those things, we have to have longitudinal designs and then, I think, we still have to be very cautious about what we can say about them because, in this area, people just self-select these kinds of experiences. The more they self-select, the more troublesome it is for the researcher.

One of our favorite illustrations is this: imagine you have two high school students who are perfectly identical in all respects, except one of them drops out of high school and the other one goes to college.

Well, it's nonsense. They aren't identical in all respects, or even very many respects, and you have this problem of sorting out what came first.

I don't think a longitudinal design solves all that. There are all sorts of hairy problems of really getting it sorted out there. But in some cases--and a few of them, I think, are illustrated in this paper--it does become fairly clear that some of the sorts of things that get correlated with different amounts of education or other kinds of social experiences were there beforehand, and are either a contributing cause or are jointly caused by some other things much earlier.

DISCUSSION

WOLFGANG: It's always gratifying when independent researchers, not having any relationship with each other, come to the same conclusions.

I didn't notice a reference--I probably missed it in your paper--to Delbert Elliott and his study.

BACHMANN: It wasn't in there, but I know the study.

WOLFGANG: He found essentially the same thing.

There was a study that was not published, but which was a dissertation at Penn some years ago, which used our birth cohort and examined the same issue about dropping out. To make a very long story short, delinquency was diminished when they dropped out of school. It was the school environment that was really a propelling and precipitating factor in their having official delinquency. Their unofficial self-reports, too, dropped when they left high school.

BACHMAN: I guess I should respond.

I would have loved to have found a similar kind of thing because it's such a jazzy thing to say. We didn't.

We found that the contrast in delinquency between dropouts and others stayed about the same, but that's as far as we could push it.

ALCOHOL AND CRIME IN VETERANS

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I would like to start by saying that there are a few problems with alcohol. It's not like bananas. The difference between alcohol and bananas is that there is no general public disapproval of eating bananas. There's a lot of public disapproval of drinking large amounts of alcohol.

For that reason, you get into the problem of "Is it the alcohol?" or "Is it the kinds of people who drink alcohol?" It is a problem that, of course, Dr. Moyer doesn't have. We assume that nice people eat bananas.

The other problem with alcohol is that when you look at its relation to crime, you have to decide whether you're talking about the effects of intoxication or the effects of being an alcoholic, and those are two quite different things.

A normal person who doesn't drink very often might do things if he were intoxicated that he wouldn't do at other times. These might be illegal behaviors and he might get arrested for them.

On the other hand, people who are chronic alcoholics may be acting more normally when they have had something to drink than they would if they were totally deprived, and in discomfort.

On the other hand, being an alcoholic puts them at an economic disadvantage in terms of not being able to hold a job, and leads to interpersonal problems. They may be required to support children they are not living with, but can't hold a job to do it. This economic pressure might lead to arrests that are quite independent of their current state of intoxication.

There is no difficulty at all in showing that there is a strong correlation between alcohol and crime. It was never shown better than in Marvin Wolfgang's study. The question is: What do we make of it?

One of the problems in trying to understand cause is that you need longitudinal information about drinkers within a general population. We have already heard that if you use loose criteria for psychopathy among prisoners, everybody in prison is a psychopath.

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The same thing would be true if you use loose criteria for alcoholism among prisoners. You would hardly find anybody in prison who hadn't had quite an extensive drinking history. So that makes it hard to know to what extent the alcohol contributes to the course of their crime.

We were very lucky in that we were doing a study of returning Vietnam veterans. That was lucky in two ways:

In the first place, they were young males, the most vulnerable group for both arrest and heavy drinking. Men drink most in their early twenties.

In the second place, we had identified them through Army records at the time they were still on active duty, ready to leave Vietnam. And we interviewed them twice--once after they had been back eight to twelve months, and again when they had been back three years.

The advantage of this as compared with doing a household survey is that we had a list of names and we had to account for everybody. If you try to get a general population by doing a household survey, you are likely to miss most of the criminals and most of the alcoholics because they aren't at home. They are in jail, they are in hospitals, or they are just out on the street.

Therefore, we had two advantages--a roster so that we could do a prospective study, rather than just picking them up in an area survey, and a high-risk age and sex group.

At the same time, we interviewed a matching sample of non-veterans. They turned out not to be as good a match to the veterans as we had hoped. Veterans are a kind of middle-of-the-road group, particularly Army enlisted men. They were not the more upper status groups you might find among officers, in the Navy or in the Air Force. On the other hand, they had to have some good qualities to get into service: they were not too stupid, too criminal, or too sick.

Since we matched our non-veterans to the veterans for age and education and eligibility to serve at the time the veterans entered service, we missed the worst non-veterans--those excluded because they had dropped out of school and were too criminal for the Army--but we didn't miss the best ones, so that our non-veterans were a bit more middle class and less deviant than the veterans.

If you look at arrests among veterans, they seem high enough to be of concern. Twenty-three percent had had an arrest in the two years prior to their second interview--that is, in their second and third years back from Vietnam.

However, if you look at the kinds of crimes they were arrested for, they were really very trivial. Most of it was "drunk on the street" or "possession of drugs".

Only four percent were arrested for property crimes and only two percent for violence.

Veterans did not differ even from those more virtuous non-veterans in their rate of violence, despite the fact that they had been sent to war and taught to use guns.

In fact, we found only one whose violence has resulted in a possible charge of murder, out of approximately 1,000 men.

Their "crime" is mostly small stuff and they usually don't go to jail. I think this is important to remember when you're comparing the results we get with what Dr. Hare finds, because he has a highly selected group of criminals. Only about a third of our offenders ever went to jail at all; of those who were sent to jail, most were just there overnight or for a few days. Felons in a prison are a very highly selected group even among those who have some sort of arrest record.

We also found, as we had expected from their age and sex, that heavy drinking was extremely common among veterans. The ones that we thought most likely to be at least potential alcoholics, if not already alcoholics, were those who claimed to be drinking at least seven drinks a day every day for some period of time.

Fifteen percent of the sample reported a period like that in the last two years.

Our strategy, then, was to look at the relationship between heavy drinking and being arrested, and then to try to find out whether that relationship was maintained when we considered their pre-service deviance, which we had evaluated by retrospective interviewing, and their polydrug use.

We found, indeed, that there was a strong correlation between daily heavy drinking and being arrested. Forty-two percent of the daily heavy drinkers had been arrested in the last two years, compared to 15 percent of those who had never been heavy drinkers as much as six months.

But we also found, as we had anticipated, that daily heavy drinkers were more deviant long before they went into service, confirming what Dr. Bachman says--they begin very early with truancy and arrests and other kinds of problems as young people, and they also were very heavy drug users.

I would like to emphasize that point in the light of Dr. Prescott's remarks. Unfortunately, it's not a case of marijuana <u>or</u> alcohol. It's generally a case of neither or both. The more marijuana they used, the more they drank. I wish it weren't that way, but unfortunately it is.

We decided to use, as our criterion for heavy drug use, using four or more different types of drugs, because we found that there was no such thing as a heroin specialist.

Indeed, the heroin users averaged about ten other drugs. By choosing those using four or more different illicit drugs, we were getting the veterans who were most involved with serious drugs.

We found that for veterans with all three characteristics--that is, highly deviant before they went into service, heavy daily drinkers now, and using four or more drugs in the last two years--the arrest rate went up dramatically.

Almost all of that group--79 percent--had been arrested in the last two years and more than half of them had at least two arrests.

This contrasted remarkably with people who had none of those three characteristics. Twelve percent had been arrested in the last two years, and only two percent had been arrested twice.

If veterans had a history of deviance and a history of drugs in addition to daily drinking, they had a much higher arrest rate than those with daily drinking alone, 32% of whom had an arrest and 21% of whom had two or more arrests.

But, interestingly enough, having <u>none</u> of those three characteristics was no better than just not having been a heavy drinker for as much as six months. That is, we found that an <u>absence</u> of heavy drinking is a marvelous predictor of <u>not</u> having an arrest record--but heavy drinking alone did not lead to arrest.

In fact, of those who had had daily heavy drinking but who had not been deviant as a kid and who were not using a lot of illicit drugs, only 15% had had any arrest in the last two years, virtually the same rate as those who were not heavy drinkers.

The old saying, <u>in vino veritas</u>, seems to be true. If you drink and have a predisposition to deviance, you are a lot more likely to express it. If you don't have that predisposition, you can drink a lot and you're still not going to get arrested.

I made an effort to see how much alcohol was adding as an independent variable and it does, indeed, make a significant difference, but a very small one. We accounted for about two percent of the variance in arrest rate by alcohol, independent of early deviance and other drugs.

So, in general, alcoholism may be a very convenient way to identify people who are at high-risk of arrest, but it is, very often, not the major cause that they are in trouble. It is a part of it. It may be a good indicator, but it doesn't add much on its own.

I would just like to add that I am excited about Dr. Bachman's paper, because it seems to support so much of what we found in our prospective study of young black men born and brought up in St. Louis. We collected their school records, as well as their self-reports.

We found truancy and being held back were excellent predictors of high school drop-out, which in turn was an excellent predictor of all sorts of later problems. For almost every child who ever became seriously truant or who was held back in school--and these were kids with normal IQs, not kids who were held back because they were unable to do the work--there were very, very early signs. Children almost never were first held back or first grossly absent from school after second grade. Children with these early signs were more likely than average both to drink heavily and to have arrests later.

DISCUSSION

WOLFGANG: Thank you, Lee.

Despite the fact that some suggestions have already been made, I would like to ask the three presenters if they would like to say anything more specific about their research and the researchable items that were initially derived from their work.

BLUM: I think I did my duty in the paper itself setting forth four or five things that I though should be done.

One thing, however, that does strike me constantly--one part of my personality does research; another part actually works for a police department. And I've never found any transfer.

What I do is be a social scientist and criminologist, and what I'm asked to do is be part-cop or whatever a criminologist is in the police department.

In the course of events, as we all do, I actually sometimes meet offenders, which is sometimes stimulating, as opposed to reading computer output.

I had a suggestion for Lee about where to find the burglars that weren't at home. They will be at your house if you wait long enough. There you have a sampling method which is unbeatable. You don't have to leave your room.

Just keep the lights turned off.

Again, as a westerner, I keep a .38. We have no trouble. The Ethics Committee might have a problem, but we've never failed to view them as anything but an aid to cooperation.

The thing that strikes me is how little we talk about what fun it is to be an offender. At least the guys I know who are really at it busily love it, and I know a lot of them--and there are dealers I know. They wouldn't want to do anything else. For the guys I know who are dealers in something else, it's the most exciting--I don't want to say "meaningful" because that implies sociological theology.

I wish I had a handle on the development of the experience of fun and the kinds of things that bring fun. I'm sure it's related to psychopathy. I'm sure it's related to the autonomic nervous system. I'm sure it's related to a lot of other things.

It's surely related to the use of drugs and in the relationship to crime. I would be very grateful if somebody tells us why things that are fun for those folks are not so much fun for us, or maybe we're not so different. We're just afraid to try.

WOLFGANG: Do you have a response to that, Jim?

PRESCOTT: Yes, I view the use of marijuana and alcohol together e.g., as a structural breakdown of the normative relationship between pain and pleasure as sensory experiences. Alcohol dulls pain and marijuana facilitates pleasure--both experiences are needed and both by-pass the somatosensory system.

The normative systems of the brain--pain/pleasure systems--are reciprocally related. Under poly-drug use of this kind--ingesting alcohol and marijuana, a violence-producing drug and then a violence-quieting drug--it effects or reflects a breakdown of that normative mechanism--and I view masochism the same way.

Now, in my studies of normal subjects, I found two clear factors--the marijuana factor with marijuana preference to alcohol. Now, I haven't reported these here, but in these data there is clear evidence that links the preference of alcohol--or I should say that links alcohol induced aggression--with reports of maternal deprivation in males and paternal in females.

One of the predictions of my theory is that those who become violent under alcohol are those people who have a developmental history of deprivation of physical affection and pleasure which reflects impairment of the normative inhibiting processes of the pleasure system of the brain.

It also relates to sexuality. We also find that when alcohol is preferred to sex, it is also associated with alcohol-induced aggression and deprivation of affection.

Of course, the sexuality variable is simply a continuation of the same system. What I have simply been doing is monitoring or tracking the pleasure experiences from infancy, through childhood, and adolescence and adulthood.

When you do that, the data fall out very nicely.

I might add, again, that the research strategy that I have followed both here and in the psychometric studies is based upon comparative animal studies and the brain model of reciprocal relationships between pleasure and violence.

With that kind of theoretical frame of reference, the data are very consistent. It's very nice.

WOLFGANG: I wanted to ask you, also, if you had any response to what I think was Richard's not entirely facetious comment about fun and crime.

PRESCOTT: Exactly.

The reason I think we use alcohol and drugs of various kinds is that we are trying to find a means to reduce bodily tension, bodily pain, which is normally reduced by physical affection/pleasure through the central nervous system. In other words, the systems of touch and movement are the basic sensory processes to the central neural system associated with bodily relaxation, pleasure, and contentment.

Now, in our culture, there is, unfortunately, a systematic deprivation of these kinds of sensory experiences. Therefore, we are forced into alternative modes of behavior to get the reduction of physiological tension, body tension, that we all need.

It is much easier to reach out for a bottle than it is for another body, and I'll show you in the film this afternoon how deprivation of touch and affection results in an aversion of touch.

Again, many of these questionnaire items are based upon the behavior of animals, and you have this paradoxical effect. You have deprivation of affection leading to a high need for touching and yet an aversion to touching, and that aversion to touching sets up a natural barrier to the very kinds of therapies that are needed to correct the problem, which means you have to reconstruct the emotional sense to experience affection and pleasure and not pain.

Let me share with you one other aspect of this phenomenon. We know that animal subjected to conditions of sensory deprivation will engage in chronic stimulus-seeking behaviors to obtain sensory stimulation to the sensory modality in which they were deprived.

If you rear an animal in the dark, for example, studies have shown that these animals will press for light reward a thousand times an hour compared to an opportunity to press another lever for food when deprived of food for 24 hours. The effects of sensory deprivation become potent in setting up chronic preservative behaviors, e.g., the rocking isolation-reared monkeys. They rock because they are deprived of movement. It tells, in my view, a very succinct story, and that's the major theme that I'll be talking about this afternoon--the effects of sensory deprivation during early development upon later behavior.

I don't know whether that answers your question or not, but I think it accounts for a lot of thrill-seeking behaviors that we see in delinquents. I don't want to take too much more time.

Let me mention one more item which is anecdotal.

Someone commented, after a lecture I had given on some of this material, that they now understand why all those delinquents behave the way they do. I said, "What do you mean?"

They said, "Well, we went out to the County Fair for a day and they damn near tore the bus apart, and they were quiet as church mice when we drove home that night to take them back to the institution. I could never figure out why."

Of course, what they did was to go on merry-go-rounds, ferris wheels, and a whole variety of playground equipment that produced vestibular or motion stimulation. I will show you in the film how potent motion stimulation can be in inhibiting hyperactivity.

So, it's perfectly understandable to me, the stimulus-seeking behaviors of psychopaths and criminals.

WOLFGANG: I'm reminded of an article in Harper's Magazine several years ago by Arthur Miller. It was on delinquency and boredom. The whole thrust of the obviously well-written article was that most delinquents are bored with the conforming world and its roles, and that is the chief reason they become delinquent.

I've considered this question of boredom and it doesn't satisfy me because everybody doesn't react in the same way to being bored. It may be differential, but still everybody gets a little bored. Not all of them rush out and rape their baby sisters.

HARE: Yes, but some people do do something about the boredom.

MEDNICK: Then you have to ask why. That's the interesting question.

HARE: We're talking about delinquency as sort of a homogeneous thing, again, and that's a mistake.

MADNICK: I agree.

HARE: I don't think that all criminals have fun. I do think that psychopaths may have a lot of fun. Life can be really good for them, at least in the short run.

ROBINS: Have you ever given them a depression scale? They're not very happy.

HARE: Except for situational depression, the ones I know seem pretty happy. As a matter of fact, that is one of the problems in trying to get them to change their behavior. They can't understand why anyone would expect them to change. They get caught up periodically, but the rewards outweigh the losses.

BARCIK: I think you're making--or somebody's making somewhere along in here an equivalent statement between being happy and being stimulated, and I don't see that as one we want.

The criminals I have dealt with have been, indeed, interested getting stimulated, but they haven't necessarily had fun.

WOLFGANG: I think they are going to have trouble, operationally.

BARCIK: In my experience, there is definitely a need for stimulation, but boy it's a long step between that and fun. It really is.

MONROE: Related to that is a concept that comes, for a large part, through the psychoanalytic literature but from other areas, too, of the stimulus barrier--of the aggressive criminal not being stimulated by what you and I are. It doesn't get through to them.

In other words, the excessive amount of stimulus that's necessary, perhaps, to get to him as opposed to the more normal people like you and I.

At least, anecdotally and clinically, this seems to make sense.

PRESCOTT: I would like to support your observation very strongly because I think that's clearly what's happening in the animal studies in which there is stereotypical rocking behavior. I think that the stimulus input due to rocking simply is not getting through to the critical brain structures. It's being inhibited somewhere along the pathway. I don't want to get too technical, but I think there is some evidence to show that the cerebellum's output from the cortex has massive inhibitory affects upon those pathways so that the new sensory input is prevented from getting through.

Again, I'll show you on the film how we overcame that in children who engaged in hyperactive sterotypical circling behaviors, which is a chronic form of stimulus-seeking behavior. My interpretation is that kids behave this way because they need the sensory input. That's why they behave the way they do. So the solution was to give them more of it, but at very high levels; and it eliminated their stereotypical circling behaviors.

HARE: One of the models of psychopathy, of course, is that there is some sort of inhibition of sensory input. They need more and more intense stimulation to reach some sort of optimum level.

PRESCOTT: Exactly.

BLUM: One could postulate, as we did--and never had a chance to test--that very interesting bunch of guys that are herion addicts who are characterized, ordinarily, as psychopaths. They are sure as hell busy being criminals.

Heroin may have some special function for these folks in terms of their immediate responsivity to it on initial administration as opposed to the dysphoria which most normals report.

It's conceivable, pharmacologically, that heroin has a special capacity to produce a differentiated experience or, if you want to speak in terms of sensory barriers, to make a mark on those folks which is otherwise not made by their experience and which is not a mark which the rest of us, presumably, need.

Most of us, taking morphine, don't like it at all. Taking heroin, we like it less.

I think this is a fascinating set of studies to work on, to construct reaction profiles on acute drug administration, various classifications of it.

PRESCOTT: Have you looked at the sexual life of the drug abusers? Studies have shown a reciprocal relationship, i.e., heroin and the other drugs are a clear substitute for sexual functioning.

I think that you'll find this in the alcoholics and other drug users, that there is impaired ability to experience pleasure.

BLUM: I would have to be sure that the impaired pleasure experience in sex is not equated with an expectation of decreased activity. You can have a lot of activity without getting gratification in terms of how hippies spend their days--or did spend their days.

There's an awful lot of plain and fancy sex, including for money. One doesn't make the assumption of pleasure in it.

PRESCOTT: Exactly, but I think that's an important distinction. It's not the sexual activity, per se, but how much pleasure is actually occurring from that.

BARCIK: This need for stimulation or establishing a threshold for stimulation is another part of this automatic biological type fingerprint that needs to be looked at very seriously in addition to skin resistance and the other things.

I read something very nice about this need for stimulation recently. Throughout that whole series of things, there is just a constant need for stimulation.

How can we measure that? Are there physiological ways that we can measure it?

MOYER: We can measure reactivity. I was trying to formulate in my own mind how to operationalize the need for stimulus. That's not easy. It isn't at all assured that we can because different people have different levels of reactivity, or need--or if there's any correlation between reactivity and need. It seems that this hypothesis hinges on the differential need for stimulus.

MEDNICK: I don't think that that indicates that the desired stimulus will satisfy the need.

HARE: Actually, there is a big literature on this. Most of you are aware that Eysenck's model of introversion-extroversion relies heavily upon the concept of need for stimulation. There has also been some research with delinquents and psychopaths on the rewarding effects of various forms of stimulation. Years ago, we carried out a small study that has never been published, but which bears upon this issue. We had found that electric shock and pictures of nude females were not very effective unconditioned stimuli for psychopathic inmates. It could be argued that the pictures were not effective because the subjects did not find them rewarding enough. So we devised a simple operant conditioning paradigm in which the faster a button was pressed the longer the glimpse of the picture was. Most of the inmates, including the psychopaths, pressed the button at a furious rate. So the pictures were a rewarding form of stimulation for the psychopaths, and they worked hard to receive it. What it did for them psychophysiologically, I don't know. However, they didn't display very much physiological arousal in anticipation of seeing the pictures.

WOLFGANG: Pornography conditioning and its effect. They found that the level of attention to nude photographs can't be defined.

PRESCOTT: Marvin, I think your comment is to the point. That was for normals.

WOLFGANG: Yes, that was for normals.

PRESCOTT: And I think we will find that for the stimulus-seeking personality, they don't habituate. The demand for input is much greater. They don't habituate like normal people.

That's another way, I think, of discriminating.

BARCIK: That is another. We have to put up the signposts where we are when we work with these people. That's a way of looking at all of this stuff as the threshold and the habituation process changes over the course of the treatment.

We would then have some evidence to base our statement on that, yes, this guy has been rehabilitated or is out of danger, that he will now habituate.

PRESCOTT: Bob, don't you have some data on habituation?

HARE: Yes. With some variables, like skin conductance responses, there is little difference between groups in the rate of habituation. However, cardiovascular responses seem to habituate relatively slowly in psychopaths, for reasons that I've discussed elsewhere. Monte Buschbaum has a neat way of looking at the central processes involved in stimulation, but not much research of this sort has been done with psychopaths yet.

MONROE: The other anecdotal information that I have is that prisoners in Maryland, when they are injured--cuts, scalp lacerations, hand lacerations, broken arms, this sort of thing--come into our hospital for treatment. The doctors are impressed by the fact that they do not seem to need anesthesia. They can sew up the worst damn laceration and they never bother to get novocaine or things of that kind. They just sit there.

I talked with some of the prisoners who seem very anxious about all kinds of things, but not about pain.

BARCIK: I've seen them in the prison hospitals.

MONROE: It fascinates me.

BARCIK: You or I would be out. All of us would be unconscious.

PRESCOTT: I can't help but comment again.

Again, the sensory deprivation studies show impaired pain perceptions. In Melzak and Scott's Studies of sensory deprivation in dogs there was no observable pain when dogs would seek intense levels of tactile stimulation. For example, the dogs would push their noses into lighted cigarettes and cigars with no observable pain response.

This phenomenon also exists in the isolate monkeys. Isolation rearing results in self-biting and mutilation with no observable pain perception. You will see it in the film. Experimental studies by Lichstein and Suchett show that isolation-reared monkeys will hang onto a shock water tube at a level that would cauterize the lips. Normal monkeys will avoid this high level of sensory stimulation. Paradoxically, the isolates are hyperactive to low levels of electrical current which normal monkeys tolerate.

There is a wide variety of systematic data that tell a holistic story. Behaviors of humans parallel, in many striking ways, what has been documented experimentally in isolation-reared monkeys.

WOLFGANG: I must return to Lombroso because he said, in one of his five volumes which very few people read in Italian, that his criminals and his born-criminal types were virtually immune to pain and that they always healed much more rapidly. He did have some control groups in this. It is very interesting.

Jerald, I wanted to ask you the same question. Do you have additional research data on this? I could expand for a moment on this business of looking at some of the early factors involved in educational success or failure, because I think that is an area worth looking at. When you talk about going back to earlier times, examining the school situation and earlier period, do you also have in mind the sequence of events as a forecaster?

BACHMAN: Not really, no. It simply comes from isolated bits of data, the fact that there is this history of great failure, much of it in the first six grades, and poor classroom grades by junior high school is what they are reporting on, which I assume is part of the pattern, and a whole bunch of reports of negative school attitudes.

There was one other thing that didn't get into this paper and it's an intriguing finding. I found it very interesting in some of our first analyses when we only had cross-sectional data.

One of the things that related to self-reported delinquency was reports of parental punitiveness. The first time around, I didn't pay a whole lot of attention to that because these were two different kinds of questionnaire measures/indexes that were correlated with each other and both of them had a kind of reporting-bad-about-self character, and I didn't get very excited about it.

I got more interested in it when I discovered in the next round of analyses, where we had dropping-out data, that parental punitiveness was about the only family background item that predicted the dropping-out and that was really quite striking because now we had tied it to an objective measure.

So we are left with this thing that does correlate with dropping out of high school, and which correlates with delinquent behavior. It's an intriguing kind of problem because I don't really know whether it's the case that when parents are punitive it makes kids delinquent and do badly in school, or that when kids are delinquent and do badly in school their parents tend to get on their case.

WOLFGANG: That's why I asked the question about sequences.

BACHMAN: I really don't know. It's simply by way of saying that there is a lot more research that can be done and the suggestion coming from our work is that they better start a lot earlier than we did to get at these kinds of things.

I guess the other thing I meant to observe in my remarks before is that certainly the sorts of things that we found are perfectly compatible with what a lot of others have been saying about physiological factors. Very much consistent with that is the notion that there may be physiological predispositions to different levels of delinquency, and that would fit very nicely with the fact that we don't seem to be finding a lot of other things that are impacting.

CLAYTON: One of the things that they haven't looked at enough is the Pygmalion effect. We need to separate out the effect of labelling early on from the effect of other sorts of variables.

One of things that I think Dick left out of his discussion is historical versus cohort effects, especially if we are going to start talking about drug use. As you move toward the macro-level, you need to start figuring out what's going to be the effect on delinquency and drug use, and the interaction of the two phenomena when we have a small proportion of the population in the high-risk ages and a much larger proportion of the population in the adult groups.

One third comment, with regard to what Prescott is saying. We need to look at delinquency and criminal careers, drug careers, sexual careers, and all of these things, and figure out ways of looking at the temporal effects of prior behavior on subsequent behavior.

BACHMAN: I would like to comment on the Pygmalion thing; without worrying about the validity of that particular research study, the principle is a kind of a compelling one.

In a way, I guess, some of my concern about the anti-drop-out campaign would suggest a kind of labelling, but another kind of labelling that occurs much earlier is when a kid flunks a grade in school.

It was one of the things that struck us when we looked at that particular predictor of dropping out, later on. It is a very public kind of thing. Everybody knows when a kid is no longer moving along with his particular age cohort. It's a very public and, in most cases, humiliating kind of thing in a big area that a kid is involved in during those years. It is very traumatic.

CLAYTON: To get to the prospective longitudinal study that Sarnoff was talking about this morning. Kids are graded today the way they used to be graded. They not only give them a score for English and reading and writing but they also have, "is considerate of others," and so on.

If you could start looking at sets of siblings, for instance, going through the same teacher, and get older siblings, younger siblings, same sex, opposite sex, to get at the Pygmalion effect sort of thing, and could get health measures in the first grade as well, you could begin to get some meaningful resource variables dealing with early self-concept types of variables.

You could tie that into studying the home environment--parent/child interaction, corporal punishment, or whatever is there.

I think that's the way to get an answer to some of these questions, at least from the sociological point of view.

BLUM: In this case, you are saying you get the ratings of kids' teachers. Do you expect the elder to have an effect which is, then, different upon the juniors as they go through school?

CLAYTON: Oh, yes. Let's say I'm the oldest kid and I'm a troublemaker, for whatever reason. I go through the first grade of this neighborhood school. My younger brother comes along. He may be a real nice kid, but he's going to get some of the spin-off from the reputation.

I don't know how to assess all of this, but we need to pull out what's truly attributable to the kid, what's attributable to the situation, and what's attributable to just him getting a bad reputation.

Maybe they were all present, but there might have been one that was considerably more important that the others.

ROBINS: I think a lot of things came out in the first part of our conversation that point us in certain ways in this area.

I think what we are all going to agree on is that one needs to do longitudinal studies looking at both biological and social variables, and that it would be a good idea to identify high-risk populations and include them in.

On the other hand, you don't want your sample entirely made up of high-risk persons because then everybody will be alike. I think it's a matter of feeling your way to find strategies and picking good places to do special studies.

Certainly one opportunity is to study children of arrested individuals. There have been few studies like that.

One thing we need is to develop good measures of home environmental factors. We are planning to do this retrospectively for alcoholics and controls. We will interview alcoholics about their childhood, and interview a sibling who is not affected, to see if we can find variables which are agreed upon as well by siblings, one of whom is affected and one of whom is not affected, as by siblings neither of whom is affected.

We need to do this because one worries that measures made retrospectively may be rationalizations and justifications for what happened afterwards. But if affected siblings agree with their unaffected siblings, as well as do siblings neither of whom has this motivation to change the past, then perhaps we have identified variables that can be reliably used retrospectively to describe early childhood abnormalities.

So far, we really don't have very many measures of home environment, although we all agree home environment is terribly important.

MEDNICK: Concerning that I think you should worry about the fact that maybe the siblings were treated differently and had different experiences. They may be giving you very reliable reports, but they don't agree.

ROBINS: I think you have to ask not only whether they agree about the family as a whole, but also if they agree about how a particular child was treated.

MEDNICK: The other thing I worry about is reports on parents beating their children.

BACHMAN: It's mostly not beating. It's other forms of punishment.

MEDNICK: It would be nice to know what true relationship the punishment had to the behavior of the child. Did it come immediately after the child was bad?

HARBURG: Whenever we use a word that is a concept like "stimulation" or "pleasure", part of the meaning, at any rate, is related to the other terms of our research. The careful selection of comparison groups that do or do not engage, or engage in varying degrees, to whatever you're talking about is just terribly important for the interpretations that you are going to make at the end of the study.

I think that a lot more attention should be given to the research design, that is in terms of comparisons--who is being compared to whom, and how much do we need to pull out of those comparison groups to have meaning for the research that's going to be flowing through LEAA?

MONROE: First I wanted to ask Lee a question. Did I hear you right, that you thought by second grade you thought these could be identified?

ROBINS: Yes.

MONROE: That's what I thought I heard you say. One of the ways I might get some help is from our colleagues who are really into the attentional syndromes, the hyperactivity, the minimal brain dysfunctions. They have developed some fairly sophisticated scales, at least they have some standards of looking at these children, not only in their first contact with school, but they even have preschool scales now for measuring this behavior. These kids are at a very high risk for criminality; I think that, maybe, borrowing some of their scales in some of our studies might be a very useful thing to do.

WOLFGANG: Is it too late by the time people are adults--25-30--to get proper measurements of any sort that would be related to their behavior? For example, I have been talking with some colleagues who have been suggesting that our original birth cohort is too old to be meaningful in terms of responses to various kinds of biophysiological or endocrinological measurements that would be helpful as a set of hypotheses for predicting any future behavior. Is that correct? Or is there something that still can be done with a group that gets to that age?

HARE: We have been talking about longitudinal studies and, certainly, they are extremely valuable. But one of the problems with them is that we often do not know, when the study is started, which variables to include. Ten years later we may find out that new variables have been discovered or defined and which could be predictive of something important. But by that time it is too late to do anything about it. So for that reason, cross-sectional research done at various stages of the lifespan is important. If the question was,"Is it too late to obtain meaningful biological correlates of criminality when the subjects are adults?", then my own research would indicate that it is not.

WOLFGANG: Is it too late with an adult group to make predictions on a young population?

HARE: You mean to take the data from the adult studies and then, from those, develop hypotheses? I would say no.

MOYER: The answer to that is we don't know. Certainly, that's the kind of data that ought to be collected so we can find out.

HARE: Research with adults can also tell us which variables are likely to be of importance. We can then ask whether a longitudinal study using these variables would tell us something about the development of criminal behavior.

HARBURG: I think there's an analogy here to hypertension. Normal tension, borderline pressure and hypertension each carry different mechanisms. So in the age development of organisms as they proceed towards high blood pressure you're searching for something in a certain sense which may not be age-related. It's related to the specific mechanisms that you're interested in.

If you had some neural path that you could measure that may be tipped off genetically later in life--maybe some enzyme system--that would then be a major kind of thing that you could forecast. It depends on what you're looking for.

MEDNICK: There hasn't been very much research on the reliability (over time) of the measurements. We know that certain indices that we have have very good reliability over time and some don't.

Your question is, if you measure these people in their 30s and they have certain kinds of reactiveness, does that have any implications for knowing where we were at 12? The only way to find out is to do it.

WOLFGANG: Also, 30 is critical in many ways, apparently. What do we know from the literature and research about the relationship between this burning out of criminal/deviant behavior in the 30s and the biophysiological circumstances?

PRESCOTT: There are some, not very good data, showing that criminals with excessive slow-wave activity had the best prognosis, and the slow-wave activity changed to more normal activity later on and the criminal behavior changed at the same time, but the research isn't very good.

MONROE: Our data show that on our generalized theta there is a definite correlation with age. Of course, 90 percent of the six-year-olds have it, you know, normally when they are going to sleep, and about 2 percent of the 20-year-olds have it. Quite often, that 2 percent are in trouble of one kind or another. It's almost non-existent after 30.

BARCIK: Our indications are that it changes where we pick them up, too, i.e., between 20 and 30.

MONROE: Those data, right now, are fairly controversial. Hill claimed aggressiveness will correlate with theta activity in the anterior temporal region, but the most recent article finds this in the posterior area and in a much older age group. It's confusing at the moment.

PRESCOTT: One electrophysiological finding that may be helpful here is the report of Heath of spike discharges in cerebellar and limbic structures of isolation-reared monkeys. Salzburg has developed a computer signal analysis technique that can depict the presence of these depth spikes from ordinary EEG scalp recordings. It is possible that deep spike discharge pathology may now be safely and routinely identified in certain subsets of violent individuals. It should also be mentioned that Coleman reported significantly lowered platelet serotonin level in isolate-reared monkeys when compared to controls. These measures need to be included in studies of human violence.

PSYCHOPHYSIOLOGICAL RESPONSES TO CROWDING IN PRISONS

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First of all, I am pleased to have the opportunity to be here at a time when it appears that some new priorities are being set and, perhaps even more important than the priorities themselves, the methodologies to be utilized are being discussed in order to determine which approach would be optimally beneficial over a, hopefully, relatively short period of time.

My orientation is really quite different than others here, in that my work really deals with the physiological and behavioral effects of the prison environment on its inhabitants. The data that I'm going to briefly describe come from two sources.

One is a cross-sectional study of three institutions and the second is a four-year longitudinal study currently under way, which has almost been completed. I will discuss the data that appear to have the most relevance to some of the issues that have been talked about today. I will speak with a special emphasis on the differences in physiological values--i.e., pulse rate or blood pressure--because those are variables that some investigators might put into an overall physiological profile early-on. I think that there may be some problems in attempting to do that but I'll talk about that a little later.

Our initial work, that is, the cross-sectional work, began as an effort to replicate in man the crowding and blood pressure relationships found in mammals. As we started that study, we really expanded into a more encompassing study of the effects of the prison environment in general.

First, I will relate some of the cross-sectional data and then move into the longitudinal data. I'm going to skip some of the demographic characteristics of the persons studied because they are all spelled out pretty clearly in the paper which has been distributed.

In the cross-sectional study, there were three correctional institutions, each of which has multiple modes of housing for their inmates--a single cell being one, double occupancy cell being another, and the dormitory as the third option. Our hypothesis was that in a more crowded situation, as defined by type of housing arrangement, we would find higher blood pressure levels, and indeed we did.

In the first institution we found systolic blood pressure differences between housing modes of 20 millimeters of mercury, and a diastolic difference of 12. In the second institution, we found a systolic difference ranging from 10 and 20 millimeters and the diastolic differences in this institution ranged between 8 and 14. In Institution C we again found this gradient, showing a difference between housing modes, of 12 to 16 points systolic, approximately 8 diastolic.

In the two institutions in which we took pulse rate data--that is, in Institutions B and C--we found that the pulse rate was higher in those people confined to a dormitory than in those occupying a single cell. This is cross-sectional data. We had no real control over where men were. Therefore, we couldn't really say a great deal but report these associations and, of course, state how they supported our initial hypothesis. We believed that we ought to investigate this relationship in more detail.

We found a few other interesting relationships. One of these associations relates to duration of confinement of an inmate and blood pressure level. We found that inmates who had recently come into the institutions tended to have higher blood pressures. These cross-sectional data plotted along a time axis attempt to give us a longitudinal perspective. Between 15 and 30 days, blood pressure levels had dropped and, after that point, had gone up again. We speculated that this might mean, initially, a reaction to a unique or novel situation for the inmate, something different for him and thus associated with high anxiety. At about two weeks, there may have been some kind of a habituation to the impact of the environment, the inmates finding that perhaps the environment wasn't quite as critical as they thought it was. After two weeks, however, something systematic seemed to happen associated with the elevation of blood pressure again, which we attributed to the phenomenon of crowding.

We also examined the association between attitudes and blood pressure levels. We found that inmates who described the guards as very harsh or very easy-going had blood pressures ten millimeters or so higher than those who viewed the guards in some intermediate way. Most certainly, you can think about the ones who said they are very harsh as highly anxious and those who think that the guards may pose a certain serious problem to them, but the others--those who say they are very easy-going or pleasant might fit well into the construct of suppressed hostility or aggression and its relationship to blood pressure. When we looked at the association between institutional confinement and the perception of guards' attitudes, we found that the longer an inmate was confined, the less apt he was to view the guards positively.

When the relationship between the type and size of the community that the inmates live in prior to coming into the institution and blood pressure level was examined, we found a negative correlation between size of community and blood pressure level. This suggests that some prior experience or the fact that they had been in somewhat more of a crowded situation before had a mediating effect.

The longitudinal data were gathered to help us more fully understand what had happened cross-sectionally. We had found these very consistent findings in three institutions and believed that a longitudinal study was the only way to determine whether or not these associations really followed the proper sequence.

We had the opportunity to go into a correctional institution, take men into the study as they were brought into the institution, interview them, take certain physiological assessments, follow them during their period of confinement--the first interview being two weeks later and monthly thereafter--and even follow them as they were released into the community for a period of 18 to 24 months.

I am going to skip the methodology because, again, it's spelled out in the paper, but tell you what we found. The longitudinal data confirm, to a great extent, the cross-sectional data.

When we looked at the personality variables we found that men who described themselves as anxious, bothered by nervousness, fidgety, terrified, or tense nearly all the time had higher blood pressures than those who did not describe themselves in that manner.

When perception of the environment was examined, we found higher mean systolic and diastolic blood pressures in the group of men who viewed the institution as very dangerous as opposed to those who viewed it as quite safe. Another interesting trend appeared. Inmates who viewed the prison environment, itself, as favorable had higher blood pressures than those who were more critical. For example, men who described their cell as very comfortable and pleasant, the guards as good natured, cooperative, and understanding, had higher blood pressures than those men who viewed the environment and the guards in an opposite manner. This set of responses, like the suppression of hostility, fits well into the general construct of repression of aggression and blood pressure. These data are also consistent with the earlier cross-sectional findings.

The initial impact of the prison environment on psychological well-being seems to be clearly documented by comparing the distribution of inmate responses to each of 11 anxiety items with age/sex specific data gathered by the National Health Interview Survey. For instance, 66 percent of the inmates had trouble getting to or staying asleep, while only 18 percent of the NHIS subjects felt that way. Fifty-two percent of the inmates couldn't take care of things because they couldn't get going, contrasted with 17 percent of the National sample.

Thirteen percent of the National sample were also bothered by headaches, but 48 percent of the inmate population reported this complaint. Over one-third--in fact, 35 percent--of the inmates were bothered by their heart beating hard while only three percent of the comparison group was similarly affected. One-quarter of the inmates felt that they were going to have a nervous breakdown, contrasted with seven percent of the National sample.

The magnitude of these differences between the inmates and the NHIS sample suggests that the prison environment, per se, plays a contributing role in the high prevalence of anxiety symptoms among inmates.

When we examined the relationship of blood pressure and personality among different age groups, we found that there was an interaction between age, personality, and mean systolic and diastolic blood pressures. This was true particularly for items of anxiety and suppressed hostility, and was greatest among men 30 years old and greater, versus those younger.

Next we examined the relationship of blood pressure over time by different housing units. These data come from the first four sets of interviews. Men who were in cells basically maintained a relatively stable systolic/diastolic blood pressure value over time. For inmates who were dorm residents, mean blood pressure levels went up after the two week interview, over the period of the next two interviews. The men who were housed in work-release settings also showed this upward trend, in fact, more significantly than the dormitory setting. Men who moved from cells to dorms and inmates who were placed on work-release during that period showed very marked elevations in their blood pressure.

In general, the data suggest that blood pressure decreased slightly from the time of intake to the two-week interview, and then rose again as time progressed. This trend appeared to be the strongest among dormitory residents, inmates for whom the increase in blood pressure came at the time that they were transferred from cell to dorm, and men who were placed into work-release.

An important exception to this pattern, however, is the item of privacy. When men were placed in dorms at one month, 81 percent of them reported that they felt a lack of privacy, contrasted with only 34 percent at the prior interview. A similar proportion--79 percent of those still in dorms at one month--also felt there was no privacy.

A different picture, again, arose when inmates' perceptions of the guards were analyzed. In this regard, perceptions grew more negative over time. For instance, only five percent of the inmates initially felt that the guards were bad-natured, compared with 11 percent one month later and 15 percent two months later. Of special interest was the finding that dorm residents reported the greatest increase in proportion of perceiving the guards negatively. Thus, we can see that the increases in blood pressure from these different interview periods among inmates who were concurrently being assigned to dormitories were correlated with increases in feelings of lack of privacy and in negative perceptions of the prison guards. The striking increase in blood pressure for work-release men, however, was not associated with subjective reports of anxiety or negative perceptions of the environment.

That's all the data I'd like to present at this point. What I would like to address now is what relevance the data may have in terms of some of the issues that have been discussed by others today.

One of the things that concerned me--and maybe it's because I'm not quite familiar with some of the physiological and biochemical assessments discussed as others here are--is that our data show quite clearly that blood pressure and pulse rate tend to change over time and, in fact, they are more or less of a state variable, an index of reactivity, versus a trait variable that can be used as a predictor of criminality over time. Therefore, I would be opposed to using blood pressures as part of a biochemical/physiological profile to be utilized in attempting to predict criminal behavior.

One of the other areas that interested me while I was listening to some of the papers today dealt with methodological issues. They were the comments on prospective and retrospective data. Retrospective data collection in a prison setting is an extremely difficult and, in our experience, not a very reproducible or valid method. We find longitudinal data have a real advantage. However, it's extremely expensive. A longitudinal study, to be mounted for the purpose of evaluating the success of intervention now--when we are not really sure of an effective intervention strategy--would be unwise.

It was stated in the first presentation today that we are prolonging illness. We've discovered drugs like penicillin and, therefore, people no longer suffer from acute infectious disease but have chronic illnesses. Now, of course, there's a great move in NHLBI for primary prevention as well as control of high blood pressure. We know that a few pills a day will knock it down in most cases and that this will subsequently save the lives of these individuals for some time, or prolong their lives.

However, it does not appear that we know, right now--for the purposes of an intervention study--the variables that are good predictors of criminal behavior, nor do we know the effective intervention strategies.

I certainly do think that a high-risk group can be identified. A high-risk group may be the children of a person who was convicted of a crime, or a high-risk group may be a group that exhibited, for the first time, some kind of criminal behavior during adolescence.

I'm very much in favor of longitudinal studies, but I believe that, right now, there still is a great need for some basic cross-sectional and even retrospective, although, admittedly very difficult to conduct, studies to be done.

URBAN FAMILIES AND ASSAULT: A FRAMEWORK FOR RESEARCH FOCUSED ON BLACK FAMILIES

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This morning, one of the areas that was mentioned as needing further research was acquiring some basic data about aspects of crime. I think it is from that perspective that we developed this paper, particularly in the area of family violence.

I would like to describe some of the involvement we had before getting to the final paper to indicate why we directed our attention to this area. We looked at the literature and we noticed that there were a lot of data on crime, but usually the data were descriptive or the data were on crime rates or the data were dealing with variables. We were also particularly interested in a lot of the work that was going on in Detroit, Michigan, with the study of what was called "conflict-motivated crimes." These are crimes against persons, usually crimes among families and people who know each other.

From the perspective of the law enforcement department, the same "conflict-motivated" crime or the same violence was also very, very important in terms of police mortality. A serious proportion of police were being killed in their involvement with family violence. With the work that Ernie (Harburg) has done in Detroit on community and certian high-and-low stress areas, we were particularly concerned about how crime in its own areas was manifested. Thus, all of the literature and the various research projects that were going on led us to focus on looking at family crime.

Our aim in developing the paper was to present a design and a conceptual approach for examining crimes in the family. This morning, as I listened to the different participants, I was struck by the notion that, as we were working on the paper, my thought was that we were trying to be systematic in trying to attack a problem from a comprehensive perspective. After hearing the various papers this morning, I'm not so sure I would use the word "comprehensive." I would now say from a more comprehensive perspective than what we found in the literature.

Our design was to look at assaultive behaviors in families taking into account the interactive processes between two major forces: the family within the context of the specific environment in which it resides, and assaultive behavior from the perspective of facilitating forces--those things that precipitated crime that we hear so much about in the literature and those forces within the family or within the community that serve to constrain crime or assaultive behavior. Given that framework, which you will see in our paper, we developed a research design whereby the interactive processes of family dynamics and environmental forces could be examined.

I would like to just mention some of the rationales for this design. We wanted to look at families that engaged in assaultive behavior and those families that did not engage in assaultive behavior in both the high- and low-stress areas. Just for information, our high-stress areas dealt with those ecological factors in communities that create stress. They have been defined as, in urban areas, communities with high crime, high marital disorders, and generally disorganized or unstable communities. We looked at families who engaged in assaultive behaviors in both kinds of communities--those that are producing stress and those that are not producing stress--so that we could make comparisons between families that engaged in assaultive behavior.

One of the rationales is that there are a lot of families in high-stress areas that are not engaging in assaultive behavior, just like there are families in low-stress areas that are engaging in assaultive behavior. Why? What are some of the forces that we might look at? Another rationale is it would allow us to make comparisons between families in high- and low-stress areas. That is, we could look at those families that engage in assaultive behavior in high-stress areas and those that engage in assaultive behavior in low-stress areas. The model also allows us to make such comparisons against their race.

Given this model, our survey of the literature indicated that there are certain kinds of sampling processes that might be more salient than others. What we did was to identify those that we think are important and that require further examination.

The first one has to do with a first classification of family processes which have to do with family structure--what we call family structure. Under that group, we identified three different areas that may have some impact on our understanding of the forces constraining behavior in families.

We make the assumption that families are systems of conflict management and change. Based on that assumption, we see the composition--that is, the number of people in the family--having some bearing on the kinds of changes that might surface. We also know that the concept of the nuclear family is diminishing. We are seeing a whole range of what are now considered more legitimate types of family structure. If we have these various types of structures, then we can assume that the major change in the conflict within them will differ. We can also assume that, in certain situations, the conflict may lead to certain kinds of assaultive behavior.

In our paper we note that there are also certain positive uses of conflict which may impede assaultive behavior in families. We don't know enough about those forces. So, looking at family structures in a dimension seemed important to us. Also, in terms of particular family members, it was noted earlier this morning in some of the research coming out of Chicago that, in particular situations, the presence of a grandparent in the home may serve as a mediating force in terms of conflict. Some of the other writings suggest, also, that the presence of a grandmother can serve as a force to escalate conflict between adults over the children. We have not examined this process. We do not know too much about this.

A second area that's important had to do with sexual perceptions and expectations. This is an area that varies with the economic picture.

Dr. Bachman mentioned, I think, in his paper the role of unemployment in certain aggressive behaviors. Consistently in the research we found that unemployment was a variable that had to be taken into account when looking at relationships between spouses. Within our study on Black families, this becomes a critical factor because men, in particular, who are unemployed or when employment is unstable cannot quite perform those roles in the family which they are expected to perform. So, certain kinds of conflicts may emerge which may have a bearing on assaultive behavior or--as some of the literature suggests--that become alternatives of controlling the family--that is, the use of other resources, physical force being one, when you don't have economic resources. So we felt that this is an area in which little is known and where we need information.

A third area in terms of the family has to do with children. Recently, there was a study in Detroit on problems in marital dissolution. One of the biggest factors precipitating conflict was the children. Previous information on family conflict and children has primarily been focused on this. We don't really have any information on the dynamics that go on between parents when they are in conflict and how they use the children, or how the children begin to create an assaultive act between parents.

Along that same line, we talked about child abuse earlier this morning and we inferred that sometimes children may provoke violence in their family; but there is another situation where it may be the victim child. We know that, in many families, there are children who, for just irrational reasons, aren't liked by the parents, one or both. If one parent doesn't like the child and the other parent supports the child, there is a conflict situation which can escalate to assaultive behavior. Unfortunately for the child that both parents don't like, he becomes the victim and this has some ramifications for how the siblings treat this child. These are areas that we thought needed more exploration.

Another factor has to do with family management of their environment. We've gotten into coping, and we've included in there internal coping. That is, how a person deals with his own anger--how he expresses it or doesn't express it, whether he drinks or uses drugs or a whole range of mechanisms that may be at his disposal in terms of handling conflict. That's internal.

The other has to do with what we view as the external coping mechanisms--how does the family, as a unit, handle stress in the environment around them. The study of McQueen suggests that in very high-stress areas there are families that tend to manage their resources and handle their stress better than other families. We were concerned about what are the dynamics going on in those families that are managing in those high-stress areas as opposed to those who are not.

We have approached the assaultive behavior in the family in what seems to be the isolation of certain variables, looking at the interaction of the family variables with environmental variables. We just want to note that we are aware that all the things that we have identified are variables that are interacting with each other, and that you can't really isolate them because when you start talking about structure you start getting into sex role expectations or when you talk about children you get into coping mechanisms. So for the sake of just more simplicity, we isolated them out where we could and stress that one would have to look at the interaction approach.

Let me close with a couple of comments in terms of why we see this type of research to be important. Increasingly, the area of family violence is getting attention because so many deaths as well as physical assaults are occurring as a result of such violence and law enforcement officers are trying to develop intervention strategies to combat some of the problems. But it's very hard to develop intervention strategies when you don't have baseline information, when you don't understand the dynamics of what's involved. You're very hard-pressed to develop programs for prevention, for control, or for management if you don't understand some of the processes.

It is our opinion that if we look at a family and the dynamics going on within that family within the context of the environment that it lives in, some of those dynamics may be better understood. We have generated a series of hypotheses, and I'll wait until we finish to comment on them.

DISCUSSION

WOLFGANG: Thank you very much. I think your paper, as much as or more than any other, gives us some research items which are available for consideration. We appreciate that. I know on several pages you have set some very specific objectives.

I'm reminded of the fact that the Police Foundation studies of domestic quarrels recently showed that in looking at households where homicides occurred, in 85 percent of those households there had been at least two prior domestic quarrel calls that the police responded to. I believe it was in around 50 percent of these that there had been five such calls. That kind of information in itself is useful, but it still doesn't answer the questions that were raised about the dynamics that go along with the problem.

I'm reminded, also, of the James H. Boswell work that produced the very interesting proposition that as the number of persons in the family--of course, this can be applied to any social group--increases arithmetically, the number of interrelationships increases geometrically. We've spent a long time talking about the build-up of those interrelationships and how they interract.

We are now ready for our last presentation and we eagerly look forward to the film on Early Deprivation and Criminality. I think it is fair to say, if I were to summarize it in one line, that the prison environment facilitates the affectional deprivation and dysfunction of persons.

EARLY DEPRIVATION AND CRIMINALITY

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I have this film, which has some unique footage which really doesn't exist anywhere else in the world. I think I can say that safely. I just wanted to share that with you.

Given the limitations of time--the film is about 20 minutes in length--that's not going to give me time to discuss the formal paper. But, I felt that we would all benefit more by the film than by the paper--the paper you can read, but you won t be able to see the film at another time. So I've taken the liberty to impose upon you the film and hope you might agree with me that it will be a worthwhile experience.

The film is really a collage of different kinds of phenomena. Basically, my interest in the violence problem stemmed from the Harlow studies. I was struck by one of the comments of Harry Harlow when he said that his studies of maternal/social deprivation--that is a term coined by men, which is a bum rap on women--and when we are finished with the film I think you will agree with me because men can provide the same kind of physical affection that women can, although women do play a very special role as the primary mediator of infant physical affection.

Anyway, he made the statement that maternal/social deprivation does not involve sensory deprivation. Well, to someone who has an orientation to sensory neurobiology and sensory psychophysiology, I was mystified by that statement. Obviously, the only basis on which we can communicate with other people in the world is through our senses, so they had to be involved. The question that immediately came to mind was: Which senses are most important in trying to account for this emotional-social-behavioral deficit associated with separation from mother and peers?

So, I examined the experimental literature and sorted out the various social isolation of loving experiments in terms of which sensory modalities were most involved. It turned out that a rearing condition in which an animal is reared in a cage by itself in a colony room where it could have social communicative relationships based upon visual, auditory, and olfactory contact but not tactile contact--that these animals developed all the classical behavioral disturbances that had been described by the Harlows and their colleagues.

So it immediately became clear that visual and auditory sensory processes, at least, were not very important in the "material-social" deprivation syndrome. Of course, I focused in on the tactile system, which was nothing new because Harry Harlow has talked extensively about tactile deprivation, contact comfort, and so forth. But Harry Harlow never dug below the skin. So I became interested in the central neural system's mediating touch. I was fortunate to see a film that was an experiment by Drs. Mason and Berkson in which they reared infant monkeys by themselves on a swinging mother surrogate. Motion was the only rearing difference between the two groups of monkeys. Control monkeys were reared on an identical but stationary surrogate mother.

The results were profoundly dramatic. The infant monkeys raised on a swinging artificial mother did not develop isolation-reared syndromes. The monkeys that were raised on the stationary surrogate--identical except that it did not move--developed all the classical isolation-reared symptoms.

Immediately, it became apparant that the movement was a critical element in the isolation-reared syndrome. From the viewpoint of sensory neurobiology, that means the vestibular system, and the cerebellum which is a major recipient of vestibular impulses. These initiated the development of a neurobiological theory of isolation-reared aggression which was based on the central role of the cerebellum in mediating and regulating sensory emotional processes. I won't go into the details of the theory, but it led to some studies which will be shown in the film.

That is a brief overview of my beginning involvement in this problem area. It led to a number of other research activities. One was a cross-cultural study in which I examined cultures from the Human Relations Area Files for those cultures that were coded high and low with respect to infant touching, holding, and carrying. Carrying--movement--was the critical variable. Cultures which left their kids alone--very little carrying, touching and picking up--were extremely violent and I was able to correctly classify the violence in 73% of the 49 cultures based on that one variable alone.

Premarital coitus was included as a second variable because that represents a developmental continuum of physical affection and pleasure. Utilizing acceptance or rejection of premarital coitus as a second variable for classification, I was able to correctly classify violence in 48 out of 49 criminal cultures.

Michael Harner, an anthropologist specializing in the study of the Jivaro, pointed out to me that the Jivaro culture was misclassified in the original coding and ought to belong in the deprivation of infant affection category. This change produced a 100% correct classification of physical violence (high and low) in these 49 cultures distributed throughout the world.

I then developed a questionnaire because I wanted to assess these same kinds of variables and relationships on modern cultures, such as ours. All I can say is that the psychometric data are very, very consistent with the expectations derived from the somato sensory deprivation theory of physical violence. If we have time, we can discuss these data later. I would like to now show the film because it dramatizes the effects of social (somato sensory) isolation in a way that is not possible with the written word.

(FILM PRESENTATION)

In one of the lectures I gave, a woman raised her hand and said, "Now I understand my boyfriend's behavior." Everyone said, "Well, what is it?"

It so happened that her boyfriend was a lawyer and she could not really get close to him--touch him, hold him, have any affectional closeness--except on Saturday evening after he came back from glider flying. Then he was like a cuddly little bear.

It was just a dramatic documentation. Here's this fellow who is uptight about being touched, couldn't get close, goes out flying a glider, gets all this vestibular (motion) stimulation, and comes back wanting to be touched and held and so forth.

I also get a number of anecdotal comments about juvenile delinquents. It is a kind of serendipitous confirmation.

Let me cite one of Zubeck's studies because it is so clearly relevant. As you know, Zubeck and others developed a long history of sensory deprivation studies on human subjects, but in this particular study what he did was to immobilize the heads of volunteer subjects - and these happened to be teachers, faculty members. The heads were placed in a holding mechanism, a vise, so that they couldn't move their heads, and I'll tell you what happened. Only eight of the 40 subjects were able to endure immobilization for the prescribed 24 hours. Vision and hearing were not interfered with; the social interaction was maintained. Head movements were prevented.

These investigators reported about 85 percent found immobilization stressful and that 75 percent stated that they would not repeat the experience in a week's time. The restriction of activity alone in the study resulted in showing more "intellectual inefficiency, bizarre thoughts, exaggerated emotional reactions, time distortions, changes in body image, unusual bodily sensations, and various physical discomforts." Specifically, subjects felt "that some part of their body was disconnected or did not belong to the rest of their body, that they were melting or merging into their surroundings, and that at times they felt like a different person." Reports included, "whole body floating or revolving in space, arms or legs rising, whole body feels as heavy as a ton of bricks, and various distortions of body properties such as one limb being much shorter than another."

Psychosomatic complaints were also frequent, including periodic aches and pains, numbness, dizziness, physical discomfort, chills, perspiration, weakness, strong desire to scratch parts of the body, and difficulty in sleeping. These physical symptoms, together with the emotional changes reported earlier, were responsible for almost all of the early terminations of the experiment prior to the prescribed period.

Now, there are several other comments I would like to make. There is really quite an extensive literature in documenting the role of the cerebellum in regulating autonomic functions--regulation of heartrate, blood flow, blood pressure, gastrointestinal activity--and the major regulatory systems. Yet, in the modern textbooks on functional neurophysiology the cerebellum is still a motor regulatory system.

There is also a study of Don Reis' which I think is very relevant. He stimulated the rostal fastigial nucleus--one of the deep cerebellar nuclei in cats. With very low levels of stimulation, he induced grooming behavior in the cats--licking and so forth. When he increased the level of stimulation, he got gnawing and self-mutilation, self-biting. He increased the level of stimulation higher and it precipitated predatory behavior.

So here was a beautiful demonstration that just initiating or changing the levels of electrical stimulation to one of the deep nuclei of the cerebellum could produce a change of behavior from a positive affectional emotional state all the way through to self-destructive biting and vicious attacks upon other animals. This range of behaviors resulted from the same neural site of stimulation, only differences in levels of stimulation provided dramatic differences in behavior.

There is a variety of other studies that have been reviewed elsewhere that implicate the system, vestibular-cerebellum, and I mention these data as a frame of reference and background for the psychometric studies. The sensory processes, pain and pleasure, and a variety of lifestyles in relationship to these life experiences throughout development account for, I believe, a wide variety of our pathological behaviors.

In the formal paper, the psychometric data is highly consistent. There are some surprises. I would like to mention a few.

Table 7a is a comparison between male prisoners and normal males. Individuals who agreed or disagreed to both questions: "My mother did not hug and kiss me a lot," "My mother is awfully indifferent toward me,"--constituted the two groups and were tested for significant differences on all other items in the questionnaire.

There are highly significant relationships with material deprivation in the prisoner population: e.g., "I do not get enough touching," "I feel like killing myself," "Others don't care about me," "Drugs are more satisfying than sex," "Sexual pleasure helps build a bad moral character," "Alcohol is more satisfying than sex," "Prostitution should be punished by society," "My parents have frequent arguments," "I feel in sex that I'm being taken advantage of," and "I do not get much pleasure from sexual activities."

One of my arguments is that deprivation of emotional, physical affection from one's parent during infancy and childhood produces an impairment in one's ability to experience pleasure, as an adult.

I might point out that recent studies by Austin Riesen and Bill Greenough have shown morphological changes in the sensory cortex, motor cortex and in the cerebellum due to isolation--that's in primate brains. Prior to this, we have had no morphological data on alterations of the brain due to social isolation. It has taken 12 years for the suggestion that "material-social" deprivation leads to brain dysfunction to become believable and to result in empirical studies of this concept.

We need to emphasize the neural anatomical substrate for the range of emotional behaviors and abnormalities that we see consequent to social isolation. The American culture, in my view, is a profoundly sensorally deprived culture due to the lack of touching and caring.

Cross-cultural studies report some cultures carrying their infants as much as 15-16 hours a day. Contrast this with our culture where our infants probably get less than an hour of carrying a day. This, in my view, is profound stimulus sensory deprivation and undoubtedly affects the structural and functional organization of our brain. Culture influences brain development!

TABLE 7a

MY MOTHER DID NOT HUG AND KISS ME A LOT
MY MOTHER IS OFTEN INDIFFERENT TOWARD ME

MALE PRISONERS (N = 75)			NORMAL MALES (N = 456)	
X^2	P	PHI	PHI	CORRELATES
22.41	.0000	.59*	.19	I do not get enough touching.
17.14	.0000	.51	NT	I sometimes feel like killing myself.
16.95	.0000	.52	.45	My father does not really care about me.
12.07	.0005	.44*	.06	Drugs are more satisfying than sex.
11.54	.0007	.44*	.00	Sexual pleasures help build a bad moral character.
10.85	.001	.43*	.15	Alcohol is more satisfying than sex.
10.48	.001	.42*	.02	Prostitution should be punished by society.
9.65	.002	.40*	.13	My parents have many unfriendly arguments.
8.85	.003	.39*	.01	I take drugs more often than I experience orgasm.
8.70	.003	.39*	.08	I often feel I am sexually taken advantage of.
8.30	.004	.39*	.06	I usually do not get much pleasure from my sexual activity.

*PHI-coefficient significantly greater than normal males.

NT = Not Tested.

That's why I think our society and societies like us are physically violent; self-destructive; other-destructive; exploitive and alienating. It is the consequence of the failures of nurturance.

Again, Table 7b we find the linkages to violence, particularly sexual violence against women, being associated with deprivation of maternal affection. I might point out that this is not intended to criticize women as mothers because my argument is that the women in our culture are impaired in their ability to give affection because they never learned how to from their fathers. How can women in our culture, whose fathers were not physically affectionate to them, learn to give physical affection to men? The questionnaire data and other data support this point of view.

The men in our culture are not physically affectionate to their daughters for a variety of reasons: taboo, fear, etc. But, again, it comes back, I think, to the role of the father. There are other data here that help support the primary role of the father in terms of regulating the emotional and affectional tone in the family. See Table 9 for the data that support this statement. Table 10 in the manuscript lists all those question items that significantly discriminate normal male college students from the male prisoners. The most significant discriminant is "capital punishment should be permitted by society" with 66% of college males and 20% of prisoners agreeing with that statement. This is followed by "alcohol-induced aggression reported by 11% of the normals and 48% of the prisoners. An inspection of Table 10 indicates for the prison sample that there is significantly greater parental indifference, punishment and lack of affection; interspousal violence and lack of affection; family incest (father-daughter; mother-son; brother-sister); mistrust of men and women; sexual violence-prejudice-discrimination; rejection of oral-genital sex; and drug/alcohol use which is also "more satisfying than sex" when compared to normal college student males.

Table 11 presents a similar analysis for female prisoners compared to college female students. The most significant discriminator is preference for homosexual or lesbian sex relationships (26% of prisoners and 1% college students agree with this statement). The next most significant discriminators are parental indifference; alcohol is more satisfying than sex; and sex discrimination concerning equality of women in expressing their sexuality. Similar to the discriminants obtained for males, family incest (father/daughter; mother/son; brother/sister); distrust of women and men; sexual violence and prejudice; drug/alcohol use which is found more satisfying than sex and rejection of oral-genital sex were salient and significant discriminators between female prisoners and female college students.

TABLE 7b

MY MOTHER DID NOT HUG AND KISS ME A LOT
MY MOTHER IS OFTEN INDIFFERENT TOWARD ME

MALE PRISONERS (N = 75) X^2	P	PHI	NORMAL MALES (N = 456) PHI	CORRELATES
7.10	.008	.38*	.05	I get hostile and aggressive when I smoke marijuana.
6.99	.008	.34	.28	My father did not hug and kiss me a lot.
6.19	.01	.34*	.02	I enjoy sex films where the sex partner is physically beaten or hurt.
5.91	.02	.32	NT	I personally know a family where the father had sex with his daughter.
5.58	.02	.34	NT	Rape scenes in the movies gives me ideas about raping someone.
5.32	.02	.30*	.08	I often get "uptight" about being touched.
5.30	.02	.32*	.03	I sometimes feel like raping someone.
4.64	.03	.30*	.16	I sometimes feel unhappy, sad or depressed.
4.57	.03	.29	NT	I sometimes feel like killing someone else.
4.21	.04	.28*	.09	I would rather drink alcohol than smoke marijuana.
3.87	.05	.27	NT	I remember when I used to "head-bang" or rock back and forth.

*PHI-coefficient significantly greater than normal males.
NT = Not tested.

TABLE 9

PARENTAL DEPRIVATION CORRELATIONS

	N	FATHER EFFECT UPON MOTHER	MOTHER EFFECT UPON FATHER	P
Normal Males	375	.46	.28	.0001
Normal Females	760	.57	.46	.0001
P <		.02	.001	
Male Prisoners	75	.72	.34	.001
Female Prisoners	25	.51	.43	NS
P <		.05	NS	

TABLE 1C

SIGNIFICANT MALE DISCRIMINATORS:
NORMALS (146) vs. PRISONERS (117)

			% AGREE		
X^2	PHI	P	N	P	QUESTION
52.64	.46	.0000	66	20	Capital punishment should be permitted by society. (32)
41.96	.41	.0000	11	48	I get hostile and aggressive when I drink alcohol. (15)
41.25	.41	.0000	11	47	I have been or need to be treated for venereal disease. (48)
40.97	.41	.0000	10	45	Abortion should be punished by society. (31)
40.36	.40	.0000	01	28	I have been accused of raping someone before. (59)
35.55	.38	.0000	07	38	My father does not really care about me. (5)
35.01	.38	.0000	13	47	I remember when my father physically hit my mother. (91)
34.74	.38	.0000	00	23	I personally know a family where the mother had sex with her son. (78)
30.00	.35	.0000	16	48	I do not enjoy oral-genital sex. (49)
27.10	.33	.0000	86	56	I am proud of my country. (95)
27.05	.34	.0000	19	51	I do not trust men very much. (84)
26.62	.33	.0000	01	22	Drugs are more satisfying than sex. (18)
26.08	.33	.0000	08	34	I personally know a family where the father had sex with his daughter. (77)
24.59	.32	.0000	63	31	Laws should not be passed to eliminate rape scenes in our movies. (93)
23.86	.31	.0000	01	20	My mother does not really care about me. (4)
21.08	.29	.0000	72	43	I can tolerate pain very well. (11)
20.49	.29	.0000	10	34	I personally know a family where a brother and sister had sex together. (79)
19.56	.28	.0000	37	66	I smoke marijuana quite often. (13)
19.27	.28	.0000	11	34	The government should have more control of the people. (42)
16.94	.26	.0000	21	46	I use and experiment with drugs quite often. (12)
16.69	.26	.0000	21	46	I do not trust women very much. (85)
14.68	.25	.0000	46	71	I have rarely seen my parents hug and kiss each other. (1)

TABLE 10
(Continued)
SIGNIFICANT MALE DISCRIMINATORS:
NORMALS (146) vs. PRISONERS (117)

			% AGREE		
X^2	PHI	P	N	P	QUESTION
14.27	.24	.0000	28	52	I often have had sex when I didn't want it. (69)
13.83	.24	.0002	56	32	I would rather drink alcohol than smoke marijuana. (16)
12.24	.23	.0005	01	11	Alchol is more satisfying than sex. (17)
12.08	.23	.0005	10	28	I do not enjoy sex films where the sex partners give each other pleasure. (27)
11.91	.22	.0006	46	68	I have several scars on my body. (61)
11.21	.22	.0008	06	21	I often feel I am sexually taken advantage of. (54)
11.03	.21	.0009	11	28	White men should not have sex with black women. (65)
10.84	.21	.001	67	46	As a child I rarely, if ever, masturbated. (74)
10.35	.21	.001	63	82	I frequently pray to God for help with my problems. (55)
10.34	.21	.001	02	13	Marijuana is more satisfying than sex. (45)
10.28	.21	.001	50	30	Married persons having sex affairs with their lovers is wrong. (25)
10.14	.21	.001	05	18	I prefer homosexual or lesbian sex relationships. (62)
9.44	.21	.002	01	11	I get hostile and aggressive when I smoke marijuana. (53)
9.40	.20	.002	22	40	Some women deserve to be raped. (63)
9.11	.20	.003	03	15	Society should interfere with private sexual behavior between adults. (30)
8.49	.19	.004	12	26	I sometimes feel like killing someone else. (58)
7.18	.18	.007	10	23	We would be better off if blacks and whites lived in their own neighborhoods and went to their own schools. (68)
6.87	.17	.009	29	46	My mother did not hug and kiss me a lot. (2)
6.84	.17	.009	25	41	I tend to be extreme in my political points of view. (41)
6.62	.17	.01	76	60	I would like to be held and hugged without having to have sex. (71)
5.97	.16	.01	50	66	Some women enjoy being raped. (86)
5.91	.16	.02	25	40	I remember when my mother physically punished me a lot. (35)

TABLE 10
(Concluded)
SIGNIFICANT MALE DISCRIMINATORS
NORMALS (146) vs. PRISONERS (117)

X^2	PHI	P	% AGREE N	% AGREE P	QUESTION
5.76	.16	.02	04	13	Sexual pleasures help build a weak moral character. (38)
5.43	.15	.02	27	14	I sometimes feel like raping someone. (50)
4.94	.15	.03	30	44	Nudity within the family has a harmful influence upon children. (9)
4.83	.15	.03	23	37	My parents have many unfriendly arguments. (6)
4.26	.14	.04	18	30	Women should not have the same sexual freedoms as men. (70)
4.24	.13	.04	43	56	I drink alcoholic beverages quite often. (14)
4.22	.14	.04	26	39	Religion and not science will ultimately solve our problems. (94)
4.21	.14	.04	08	17	I usually enjoy rape scenes in the movies. (51)
4.15	.14	.04	16	28	I remember when I used to "head-bang" or rock back and forth. (73)

TABLE 11

SIGNIFICANT FEMALE DISCRIMINATORS:
NORMALS (277) vs. PRISONERS (41)

			% AGREE		
X^2	PHI	P	N	P	QUESTION
46.41	.41	.0000	01	26	I prefer homosexual or lesbian sex relationships. (62)
41.93	.38	.0000	07	44	My father does not really care about me. (5)
39.45	.37	.0000	05	37	My mother does not really care about me. (4)
37.91	.37	.0000	02	26	Alcohol is more satisfying than sex. (17)
36.87	.19	.0000	07	42	Women should not have the same sexual freedoms as men. (70)
32.14	.33	.0000	87	49	I am proud of my country. (95)
29.38	.34	.0000	01	18	Drugs are more satisfying than sex. (18)
24.74	.30	.0000	04	26	Abortion should be punished by society. (31)
23.57	.29	.0000	08	37	I personally know a family where the father had sex with his daughter. (77)
20.63	.27	.0000	04	24	I have been or need to be treated for venereal disease. (48)
19.98	.27	.0000	13	44	I use and experiment with drugs quite often. (12)
19.26	.26	.0000	15	46	I tend to be extreme in my political points of view. (41)
18.81	.26	.0000	14	44	Some women deserve to be raped. (63)
18.54	.25	.0000	24	59	I do not trust women very much. (85)
18.04	.26	.0000	04	23	Sexual pleasures help build a weak moral character. (38)
17.03	.25	.0000	11	37	The government should have more control of the people. (42)
16.67	.25	.0000	05	24	Society should interfere with private sexual behavior between adults. (30)
15.85	.24	.0001	25	58	I do not enjoy oral-genital sex. (49)
15.24	.25	.0001	01	13	I personally know a family where the mother had sex with her son. (78)
14.50	.23	.0001	10	34	I personally know a family where a brother and sister had sex together. (79)
14.13	.23	.0002	92	71	I frequently feel unhappy, sad or depressed. (56)
14.03	.22	.0002	20	49	Some men deserve to be raped. (64)
13.17	.22	.0003	04	21	Physical punishment and pain help build a good moral character. (37)
12.93	.23	.0003	02	15	I enjoy sex films where the sex partner is physically beaten or hurt. (26)

TABLE 11
(Continued)
SIGNIFICANT FEMALE DISCRIMINATORS:
NORMALS (277) vs. PRISONERS (41)

X^2	PHI	P	% AGREE N	% AGREE P	QUESTION
11.88	.21	.0006	06	24	I get hostile and aggressive when I drink alcohol. (15)
11.59	.23	.0007	01	10	I have been accused of raping someone before. (59)
11.50	.20	.0007	36	65	Brothers and sisters who agree to have sex together should be severely punished. (82)
10.99	.20	.0C09	08	27	Hard physical punishment is good for children who disobey a lot. (21)
10.65	.19	.001	59	29	Married persons having sex affairs with their lovers is wrong. (25)
10.34	.19	.001	72	45	As a child I rarely, if ever, masturbated. (74)
9.83	.19	.002	18	41	I remember when my mother physically punished me a lot. (35)
8.64	.18	.003	60	32	I would rather drink alcohol than smoke marijuana. (16)
7.05	.16	.008	30	53	I do not trust men very much. (84)
6.96	.16	.008	37	60	Mothers and sons who agree to have sex together should be severely punished. (81)
6.86	.16	.009	07	20	We would be better off if blacks and whites lived in their own neighborhoods and went to their own schools. (68)
6.54	.16	.01	21	42	I often get "uptight" about being touched. (8)
6.09	.15	.01	15	32	I remember when my father physically hit my mother. (91)
5.85	.15	.02	11	26	I do not enjoy sex films where the sex partners give each other pleasure. (27)
5.22	.14	.02	52	32	I usually experience orgasm about once a week or less than once a week. (47)
4.96	.14	.03	22	40	I remember when my father physically punished me a lot. (34)
4.87	.14	.03	11	26	I like to bite, scratch or hit my sex partner when having sex. (90)
4.76	.13	.03	42	62	Some women enjoy being raped. (86)
4.70	.13	.03	37	57	Fathers and daughters who agree to have sex together should be severely punished. (80)
4.30	.13	.04	47	67	Some men enjoy being raped. (87)

TABLE 11
(Concluded)
SIGNIFICANT FEMALE DISCRIMINATORS:
NORMALS (277) vs. PRISONERS (41)

			% AGREE		
X^2	PHI	P	N	P	QUESTION
4.25	.12	.04	30	49	I have several scars on my body. (61)
4.16	.14	.04	04	13	Rape scenes in the movies give me ideas about raping someone. (83)
4.14	.13	.04	15	29	I have been "knocked-out" (unconscious) at least once in my life. (60)
4.12	.13	.04	18	33	Nudity within the family has a harmful influence upon children. (9)
3.91	.12	.05	60	43	As a teenager I rarely, if ever, masturbated. (75)
3.70	.12	.05	34	51	I often have had sex when I didn't want it. (69)

These data clearly define a constellation of variables in the affectional domain, specifically parent/child affection and punishment which apparently influence the quality and equality of male/female sexual relationships. Dysfunction in both of these affectional domains is highly linked with the expression of violence and alcohol/drug use.

That's why, I think, you will find so much violence involving sexuality. These men, these individuals who are deprived, do not know how to relate in a positive affectionate mode. The only way they know how is through violence. That's why you have sexual violence rather than sexual pleasure.

Dave Barcik, when I discussed some of these findings with him said, "You know, these conclusions of yours perfectly confirm the findings that we obtain with our projective tests--the enormous problems in Black male/female relationships," and it just comes through in a variety of ways.

DISCUSSION

WOLFGANG: Thank you very much. Are there some questions to pose to any of the last three presentors?

MOYER: Jim, I found this very interesting, and I found it very provocative. The films are really impressive. I think I got lost somewhere, though, between the cerebellar stimulation and the general positive need for stimulation during the very early childhood.

It seems to me that what you're convincing me of in the films is that you don't need touching, you don't need positive affection, you don't need really good interactive stimulation at all. All you need is movement.

PRESCOTT: I think certainly that is true for the monkey studies. Now, the monkeys who were reared on artificial movements are not entirely normal. No laboratory-reared animal is. There are some deficits, but they are not serious.

Secondly, I would argue that compared to the human level, in which the emotional repertoire is far more complex, it obviously has to be imbedded in a meaningful human relationship.

But I think what these studies demonstrate is the overriding importance of this kind of sensory input. They identify a major neural system of pleasure and affection which induces violence. Therefore, what happens is that the neuronal systems to the brain--we are in complete agreement on the systems concept here--are incompletely developed. The branches and neurons in these systems are significantly reduced in the isolated animals.

So what you have is a neuronal system that is impaired. It's incapable of exercising an inhibitory influence on the other neural systems that create violence, but that's not true with the animal that's been given movement. That animal received the necessary sensory stimulation to insure the normal anatomical growth.

MOYER: It has nothing to do with some kinesthetic sense?

PRESCOTT: That is true in this case. I guess my argument is that the vestibular stimulation is most important during the early post-natal period, but the tactile system is obviously involved in the issues of emotion and pleasure and they become more important later in development.

MOYER: What size sample are we dealing with in this?

PRESCOTT: Bill Mason had seven animals each week. But, again, I would argue that the vestibular system and the tactile system are very much involved, but the other argument, also, is that the cerebellum, as I conceive of it, is the master regulatory and integrating system for all sensory input. If you foul up the master regulatory, you are really going to have profound alterations in a variety of other behaviors. That's why, I think, we see so much happening as a consequence of this particular form of deprivation.

HARE: One of the things I noticed in the film was that you had one animal swinging around while the other was moving. In each case, the vestibular system was being stimulated, but with the stationary surrogate mother the animal is doing the moving. In the other case (a swinging surrogate mother) it's something else that's moving the animal.

What's the difference in terms of the vestibular stimulation?

PRESCOTT: What you were seeing was an endpoint, a developmental endpoint, at ten months of age. In other words, if you take a look at what happens to the other monkeys that are not on the moving surrogate mother, they are just huddled up there and they don't move at all. They don't rock. It's only at about three months of age that the baby starts rocking, but by then the damage is done.

HARE: Does it make any difference, do you think, whether this stimulation is self-induced or whether it's produced by something else swinging the monkey around?

PRESCOTT: I think the primary importance is that there is movement. It's secondary as to whether it's self-induced. The point is that, at the very early periods of development--right after birth - the animal is incapable of really providing the stimulation. If you take a look in the wild, the infant monkey is attached to the body of the mother and rarely lets go, and the mother is bombing all over the trees.

If you take a look at the magnitude of the vestibular input in a feral, in a wild-reared animal, it's enormous when you compare it to a laboratory-reared animal. It's just a major change.

I don't think we've been attuned to the enormous variations in our sensory environment that we impose on ourselves. Certainly, that's true when you compare them.

HARE: So that a newborn child bouncing around in a Volkswagen van touring Europe with its parents would be better off than a child staying at home in his crib.

PRESCOTT: That's one aspect. Obviously, you have to look at it in the context of positive affectional relationships because if you really are a nurturing and affectionate person, you are going to be picking up, cuddling, and carrying your kid.

If you're not affectionate and nurturing, then you're going to let the kid shift for itself and be in the crib for a long time. In fact, I think this is where Benjamin Spock gave some very, very bad advice when he said it was all right for babies to cry themselves to sleep at night. If you take a look at what happens, these infants then stop crying and they are emotionally withdrawn, and you've done major damage.

HARBURG: When you say "affection," is that different from the neurophysiological pleasure system? Is that a different thing?

PRESCOTT: Physical affection, to me, is the psychological label that describes the pleasure that is associated with neurological activation of the pleasure system.

In other words, when we say we are physically affectionate towards somebody, that means that we get a lot of pleasure out of touching, holding, massage, and so forth, okay?

HARBURG: Is there anything between the back and forth of the two organisms that would have to do with affection and not with what's inside each system?

PRESCOTT: I'm not sure I understand what you are asking.

HARBURG: I mean the interaction of the two, moving, connecting pleasure systems. I just want, again, to clarify what we are saying.

PRESCOTT: Physical affection is a term that describes a far more complex set of human relationships--a critical element which is essentially stimulation of pleasure.

The point I really want to make is that the vestibular system is a major pleasure system of the brain, and we haven't recognized that before. That's particularly important to newborns in their development, and then the tactile system comes in as the dominant pleasure system.

As I said, I was surprised by that story of the woman and her pilot-boyfriend. I really would not have suspected the vestibular stimulation in an adult to have such a profound impact upon him.

MONROE: There is so much that you have presented that I don't know where to start, but I will mold your data to suit my purposes.

In the explanation of my data that I presented this morning regarding correlations with theta waves, we chose to use theta waves because they seemed to correlate with depth-electrode studies, with seizural activity or spiking activity in the limbic region. Also, when I looked at Heath's most recent data placing a radio-stimulation pacemaker into the cerebellum, 9 or 11 cases were extremely aggressive and destructive, and they were the ones that showed good results.

HARBURG: I'd like to get back to the dormitory effect. The only rat study that I ever did had to do with the relationship between early gentling and isolation. The gentle ones left the cage, and the ones who were isolated did not leave the cage.

Now, the open field tests seem pertinent here. I was wondering, in terms of the dormitory physical setting, was it an open, large room?

D'ATRI: It was. They were all open, good lighting, a lot more square feet per individual than single cell or double cell occupancy, and yet, because of the lay-out, their ability to control their own interactions was severely restricted because they just couldn't do what they wanted to do. All the other inmates in the dormitory engaged in a wide array of activities, producing what I would call stimulus overload, and not necessarily stimulus deprivation.

HARBURG: They were being watched by the guards?

D'ATRI: Yes, they were watched by the guards. An example of the type of use conflicts in the dormitory is: There is a poker game going on next to an inmate when he is trying to sleep, a television set is on, and a radio blaring.

PRESCOTT: Over-stimulation. Over-stimulation in the visual and auditory senses. The prison environment really has profound deprivation of physical affection, except when there's rape and sexual violence, and that's where you get tactile stimulation.

D'ATRI: There was also a high level of arousal and fear that they may be attacked.

CLAYTON: How are they assigned to the various housing types? It strikes me that this is a good type of situation in which to do research that has a clinical trial approach so that you can assign people to different locations on a random basis, controlling for some variables.

D'ATRI: In the cross-sectional study, we had no control at all of what was going on. We went in and took them the way they were. In the longitudinal study, we got agreement from the institution to let the men be assigned to the first available space. Yet we still found that, no matter what, men were first put into a single cell for a short period of time, after which, as space opened up in work-release or a dorm, they were reassigned and it usually was a dorm first and then work-release. We were able to establish a baseline on everybody in their single cell condition, and then got to see what happened after that. In this way, we were able to see everybody in a single cell situation for at least two weeks.

CLAYTON: You can also, almost, design a Latin Square study where you have different starting points and different orbits. It would be very interesting. You could really get a feel for what kinds of effects there are.

D'ATRI: We could do that. We had people from single cell to dormitory, back into a single cell, so that data analysis will be complex and indeed, very interesting.

BLUM: We face a real problem right now based on your proposal, and you may have solved it. We are involved in research in the jails with regard to developing classification procedures which are obligatory, given an increasing number of Federal Court rulings about the obligations to minimize violence. There isn't the possibility, I think - at least in our setting - of making any kind of random assignment since there is, already, primitive knowledge to the effect that violence is predictable on the basis of a prior violence record in the same jail. Rape is predictable on the basis of a prior rape record, complicated by some knowledge to the effect that the risk of violence increases if you put large Blacks with small Whites, et cetera.

While this is but a "seat of the pants" classification procedure, it raises some important and continuing ethical issues in the kinds of research we might want to do with respect, not simply to the prediction of violence by blood pressure or anything else, but with regard to the applications that we might wish to, then, use to learn. Do you have a solution to this which can be exercised so that people are not, in fact, at risk even when one is studying.

HARBURG: There's another point of method here that relates to Russell's paper. He noted that when you are in a strong restraint environment, you might have less dyscontrol so that the environment in which the biological data are obtained, therefore, has to be considered with respect to constraint.

The other thing is that, in terms of environment, it will be interesting to see whether you have different drug responses--let's say in terms of control of blood pressure--at the different stages of social constraints.

BLUM: You're bound to get a different drug response as long as you have people who are interacting or receiving different drugs or the same level of drugs but with different baseline behavior.

I wanted to ask you if you would consider it worthwhile to broaden the next set of measures. I was reading a book this last year on illness in prison--Smith and Jones, or somebody, in Tennessee.

D'ATRI: Paulus and Cox have done similar work.

BLUM: It's the illness rates, which were so much higher for gastrointestinal disorders, for example, along with homicide rates. I wonder if you would try to differentiate your illness distributions on the basis of what you call crowding.

D'ATRI: We're collecting those data, now.

MOYER: Did I understand you to say that your aggregated prisoners had more square footage than the others?

D'ATRI: They do, absolutely.

MOYER: Then you define "crowding" in an unusual way.

D'ATRI: I don't define crowding by density alone. I think Dan Stokols has a good model where he talks about crowding as a complex psychological construct, not a density model.

I like to look at it in terms of it limiting a person's options and his ability to govern his own interactions. This is why I think there are going to be some problems in setting square foot standards in prisons, although I know guidelines have to be drawn up for minimal spatial requirements. People are talking about it being 60 or 70 square feet per inmate. In fact, this is true in the institution in which we have been working. We really found that the number of people per living unit is more important than the square foot per person.

MOYER: It's interesting. You went from the crowding definition of the animal studies, which is density, to a non-density kind of definition.

D'ATRI: That's right. The crowding in the animal studies--the interesting study that we picked up on was one by Henry Stevens Axelrod that was done around the early 1970's--which showed that the combination of crowding and intense psychosocial stimulation, or competition for females was one that really spiked blood pressure. We began to see that even these studies were not dealing with density alone and that there are a whole lot of other issues involved. That's when we started looking into the whole crowding phenomenon as a multidimensional issue.

MEDNICK: You had some differences in the cross-sectional study between the single cell and the dormitory. I was wondering how much this might be a function of differential placement of certain kinds of offenders in single cells for longer periods of time.

D'ATRI: Again, in the cross-sectional data, we didn't assign the men. However, there were no systematic age/race differences between groups. In fact, when we did an analysis which took all the demographic characteristics and previous institutional histories into consideration, our results held. The factor that accounted for the vast majority of the difference in blood pressure is the housing mode at that time, and not some of these other factors.

Now, in this case, where we were examining blood pressure levels as the outcome variable, what we ended up doing in the subsequent analysis of the data was to perform an age and height/weight adjustment of all bood pressure values, to make sure when we were talking about different groups of people, that we had already adjusted for all the factors which influence blood pressure. Again, these results came out very striking and significant.

There is a problem in dormitories. It is more likely that somebody assigned to a dormitory had been in the institution for a longer period of time and, therefore, if the relationship held, that is finding blood pressure going up over time, we would find a difference by housing mode. But we've sorted that out in terms of the initial analysis and the multiple regression analysis which considered duration of confinement as an independent variable. Finally, we controlled for duration by really following people over time in our longitudinal study.

HARBURG: I would like to brainstorm about these high-stress areas which are, in fact, more dense than most. There is enough knowledge, at this point, to know that particular families have exhibited assaultive behavior over time that has at least merited police attention.

This goes on over a period of time and it clusters within the high-density, over-crowded areas. This may be a factor in rising blood pressure. Again, out of our study, the blood pressure of the black males in high-stress areas was the highest--higher than blood pressure of black males who don't live in those areas.

In terms of the import or practical application of these things, even while the development of the basic knowledge is uneven, the application can go on and you can pull it out if you are good at engineering.

Now, I think--and this is difficult, but we should think about it--what are we, given this kind of knowledge, supposed to do about these family conflicts? They can track. They know how many families are out there. They have to intervene when they are called.

There seems to be a whole area of research to engineer intervention and inhibit this kind of behavior through social pressures. This gets us out of the neural substrates, but it gets you into social networks, if you want to use the same kind of terminology.

CLAYTON: I think, along that line, there is something that perhaps ought to be antecedent. We need epidemiological techniques that would give us a feeling for certain types of phenomena, like assault in families. We just don't know the extent of child abuse or assault in families. We don't have a good feel for it. It may turn out to be higher than we think it is. Or it may be that the media make it seem much more prevalent than it really is. Some sort of focus on hidden prevalence might be a good place for the Institute to begin.

MOYER: It seems to me that Dr. Perry had developed a very nice series of hypotheses that are stated in such a way that they are eminently testable and could be developed and carried out.

HARBURG: I think that there may be a repeatable phenomenon there in terms of assaultive behavior among males--it's probably same-sex assault--and this gets back into one of your types of aggression, Dr. Moyer.

BLUM: I have a minor point. You said something I thought was very important, and I wanted to bring it out--a kind of a constructive use of conflict. This does not, of course, imply that people can raise hell as one way of defining lots of human interactions.

In our data we were comparing families who had kids who were at very high risk. One of the things we rate them on was humor as a way of conflict release.

Then you were saying: What do cops do? What cops often do, very wisely, is to humor people along, to use humor. To use this as a teachable technique makes sense.

PERRY: We also looked at this concept of humor in terms of individuals coping and how they repress, maybe, feelings of anguish, particularly those in these high-stress areas. They laugh it off so that they don't explode.

HARBURG: There was an interesting study, reported in the American Journal of Psychiatry, which has a lot of design problems. The point was that adults are so interested in the effects of TV violence on children. I guess this was the first report about TV and adults. The author came from UCLA so he took the denizens in the hills around him--up-the-hill class professional families--and got them to cooperate, and then randomly distributed what he called "dosages of TV programs" such that "The Waltons" and the family ones were non-violent, and then "Starsky and Hutch" and all those--the violent ones. In effect, the family tension went up and down, depending on the programming that the husband was seeing over a period of time.

I would suggest that this is a terribly important kind of research which we should look into a lot more, regardless of methodological difficulties of design. I think it is something that we have to look at--the effects of media, and TV, in terms of promoting, rehabilitating, and constraining assaultive behavior.

D'ATRI: What occurred to me is that when I have gone into a number of correctional institutions--at least in New England--many of the men have TV sets in their rooms, which could be causing them serious problems adjusting to the prison setting.

BLUM: Just a question. One of the violence indicators in our jails is the extent to which they break the TV set. Is there a relationship between the violence and the breakage, and the format of the programs which have been on that day?

WOLFGANG: I don't know how to relate this particular comment to the prison environment. I am supervising a dissertation on prison homicides and one interesting factor, among others, is that the probability of a Black male being killed in prison is five times less than it is on the outside. The probability of a White male being killed in a prison is three times greater than on the outside. As I said, I don't know what to do with that information.

BLUM: In Tennessee, it's just the reverse.

WOLFGANG: These are national data.

BLUM: For Blacks over Whites, it was higher in prison, versus parole, versus normal configurations.

HARBURG: I would like to get back to this TV thing, again, in terms of the correspondence between inputs that are socio-cultural and things that happen neuro-physiologically. We can't give everybody drugs all the time to settle the problem. You have to find some kind of correspondence between drug effects and socially induced effects, such as happens through hypnosis. Then try to use the social mechanism to achieve results rather than the drugs.

A friend of mine did a study--and I think several others have shown this, too--that showed that the majority of people believe that the things they see on TV are real. In terms of the police department and police work, when they ask the police, "What do you think about these police programs?" there's a very high incredulity or disbelief that these programs represented the "reality" that they were involved in.

When you ask the viewers, 78 percent of them said, "That's real." Now, what we are doing is creating these kinds of--what do you want to say--quasi-reality systems which people respond to in terms of their images. One of these kinds of quasi-realities that get into assaultive behavior are the movies. If you accept movies from Hollywood as reality, people get their images from these films and they sort of model their behavior on them.

I think there is an awful lot of work that we have to do in terms of understanding the effects of mass media, journalism, TV, and all the rest on assaultive behavior. When you say that to a journalist, they instantly have ten dozen arguments why you shouldn't intervene in their business and so forth, but I do think that it's important.

WOLFGANG: Ladies and gentlemen, I think the time has come to close.

(At 5:45 p.m. the colloquium was recessed to reconvene at nine o'clock a.m., March 31, 1978)

SECOND DAY MORNING DISCUSSIONS*

WOLFGANG: The planned agenda has us spelled for an open discussion until 11 o'clock, at which time we move into informal workshops. I've been asked by Sarnoff if he could have about ten minutes or so to make a presentation. He apparently has had some discussions with some of you about the implications of that part of his work which relates to genetics and biological/social factors. I am certainly happy to have Sarnoff make his statement which may provide a springboard for the period of our open discussion.

I would like, however, to make sure that at some time--and perhaps we can wait until this afternoon--we discuss the administrative, organizational, structural, and institutional problems associated with attempts at multi- or inter-disciplinary research, and if there is not some way in which we might make suggestions for encouraging or providing a consensus for a reward system for people to do such research connected with crime, and the correlates and determinants of crime. I suggest this because two things stand out as catch phrases of what we have been discussing thus far: longitudinal studies and multi- or inter-disciplinary activities.

With that I'll turn it over to Sarnoff.

MEDNICK: Some people were saying to me that there were social difficulties in this genetic approach. I noticed Marvin, who is a good friend, trying to make the position seem softer by more or less saying, "Lombroso wasn't such a bad guy after all." I thought, rather than try to brighten up Lombroso, I should make a positive statement about how I thought genetic factors might interact with social factors to produce criminal behavior.

I think a good beginning for this is the notion that it is not so surprising that there are criminals. The funny thing is that there are people who aren't. When you see children, as they grow up, fighting and kicking and stealing and biting and behaving like little savages, it's clear that the function of the family, the function of society, is to civilize these little beasts. They have to <u>learn</u> to be civilized. There are anti-social impulses which children have that society has to teach

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*Participants in the second day of the colloquium were the same as for day one, with the exception of Robert Hare. Dr. Hare was unable to attend the second day.

them to inhibit. The question is: How do they learn to inhibit these impulses? We ask: How do they learn not to be criminals? If you understand how people learn <u>not</u> to be criminals, maybe you'll understand what failures in learning have occurred.

Learned inhibition of response is essentially a learned avoidance response. We learn not to do something. We learn not to punch the kid who is smaller than we are. We learn not to steal the strawberries from the fruit stand,and so forth. Some of us do, anyway.

The method of that learning is well-described and I think that the details are well-known. Let's consider a child (A) who is going to learn to inhibit an aggressive impulse early in life. He sees child B. For some reason or other, he wants to strike child B, and he does.

His mother punishes him for doing this. If this happens repeatedly over some period of time, at some point when he raises his arm to strike child B all of these trials of punishment that his mother has given him will result in a fear response. If the fear response is great enough, he'll inhibit the aggressive response.

Now, what happens at that point, when he inhibits the aggressive response,is critical. Review: child A contemplates an aggressive reaction. Because he's been punished previously, he suffers fear when he anticipates physical reaction. He inhibits the aggressive response. What happens to his fear at this point?

As soon as he inhibits the aggressive response, his fear begins to dissipate. His fear begins to be reduced because the stimulus that elicited the fear is now gone.

Fear reduction is probably the very best reinforcement that we have discovered that occurs naturally. To do better, you have to make holes in the head. When child A inhibits the aggressive response and his fear begins to dissipate, that dissipation of fear is a reinforcement for his inhibition of the aggressive actions. The amount of reinforcement that he receives is critical for his learning of that inhibition.

What, then, shall we say? In order to learn to inhibit an aggressive response, you need to have, first, some censoring agent to punish the anti-social response. Second, the individual has to develop an adequate fear response. Third, he has to have the ability to learn that fear response in anticipation of aggression. The fourth factor is he has to have dissipation of the fear in order to reinforce this inhibition of the anti-social response.

I think it is point four that I am most interested in: namely, that the person has to have dissipation of the fear as a reinforcement for the inhibition. I think that people differ (in their nervous systems) in terms of how quickly they show this kind of recovery from fear.

If people have a nervous system that allows them to recover very quickly, they are going to learn very easily to inhibit anti-social responses because whenever they do inhibit they will get a large reinforcement. Individuals who have very slow recovery from a fear response are not going to enjoy as much reinforcement--and so they are going to have a great deal of trouble learning to inhibit anti-social responses.

What I've said so far is that we have an interaction between the family behaviors--training for individual anti-social responses--and the physiological variables. Where does genetics come in? I think genetics helps to determine the rate at which the autonomic nervous system recovers. In our own studies in Copenhagen, we found that of all the autonomic variables, the one which is very heavily genetically determined is the recovery response.

I'm suggesting that the genetic factor is one of the determinants that help decide how easily an individual will become civilized.

WOLFGANG: Why do you mention genetics at all, if the differential responses of the autonomic nervous system in that region may be due to early infant conditioning?

MEDNICK: No. What I said was in order for a child to become socialized, you have to have certain autonomic nervous system characteristics which are in part genetically determined, and certain family-rearing characteristics which are environmentally determined.

WOLFGANG: I understand; whether or not I feel comfortable with the conclusion is something else.

What do we know about differential autonomic nervous systems holding constant with social class, absence of father, race, and cross-culturally? Probably the most consistent finding in this field that I know is the very slow autonomic recovery of criminals and psychopaths. I don't know of any more consistent finding.

I put forth this notion about five years ago. I think that a lot of people who have studied the autonomic nervous system of criminals and put their findings on a magnetic tape have gone back and restudied it to see what the recovery rate looked like. I think there must have been about eight to ten of these studies done in different countries of the world and under very different circumstances. Every single one has found slow autonomic recovery in persons in prison and, within prison, slower for those who are more psychopathic. That doesn't agree with formulas about genetic variables associated with it.

ERSKINE: I wonder if our trouble is not that you're not saying this is a perfect correlation. You're only saying it is partial. I wonder if this isn't getting in people's way. At most, you found 36 percent criminals when the biological father and adoptive father were both criminal.

MEDNICK: Yes.

ERSKINE: It's just a slight trend in that direction.

MEDNICK: Yes. I'm glad you said that. You can see that the whole question is how do societies deal with a child who transgresses? What do the peers do? What does the family do? Then there is a recovery variable which helps determine how easily an individual learns to inhibit that anti-social behavior.

BARCIK: Let's say that my genes say that I have a slow recovery. What am I going to do?

MEDNICK: Your genes also say what height you should be, but if you don't eat right you will not attain it.

BARCIK: I'm 25 years old and I'm an active psychopath. I have a slow recovery, but that recovery rate is due to genetic things.

MEDNICK: Probably it can be influenced by other factors--by feedback training, by drugs, etc. I would feel more optimistic about being able to change some biological variables than I would about being able to change the way the mother raised the child.

BARCIK: I would too. What I'm trying to do is get a tab on why people might want to resist talking about genetic influences.

WOLFGANG: You forget that Sarnoff's research program goes for 75 years.

ERSKINE: What we need, probably, is a good study. It makes sense to me that the autonomic reaction thing probably arises more out of sensory deprivation. The infant is simply ignored. He does not develop, feeling-wise, and you get this kind of reaction through gross early neglect.

BARCIK: You know, that need for stimulation is also part of it. When you talk about slow autonomic recovery, couldn't that be a part of that need for stimulation?

WOLFGANG: I have a list of people who have asked me about that.

MOYER: I have a couple of small points. It seems to me that we are talking about this autonomic recovery and autonomic system as though it were a determining characteristic. I think the autonomic nervous system reflects only what's going on inside the brain, and that fear is in the brain. It's not in the autonomic nervous system.

Secondly, it seems to me that with any of these variables--autonomic recovery and these other things--there is a continuum and they run from very fast recovery to very slow recovery. There isn't any point at which you say, this individual is so unrecoverable that he can't learn. What it means is that, from the top of the continuum to the bottom of the continuum, it requires progressively greater amounts of training so all of them, presumably--except at the very bottom of the barrel--are going to be trainable and are going to be subject to the socializing influences, and it's going to take them longer. If Sarnoff's hypothesis about avoidance is a valid one--and that is a testable hypothesis--then it simply means that some people are going to require more than others. This seems to me to be something that anybody can handle.

PERRY: Would it also mean that there is an equal chance of having criminals and non-criminals in the continuum?

MOYER: No, I doubt that there would be an equal chance. If, in some perfect world, you can hold all the other things constant, then these people will learn to avoid criminal situations faster than those people, but, on the other hand, if you have some perfect world in which you could give to each individual the optimal training that he requires, if that were the real world, then you would expect an equal amount to fall down the line.

PERRY: If we are looking at a criminal population and we talk about response recovery as fast and slow and we have no group to compare them with in terms of the distribution of fast and slow recovery, we get into a problem. I question whether genetics is going to get at the issue involved. It is rather human that we apply a label because of this characteristic which is found in that group when we don't have the basis for looking at its existence in another sub-group.

WOLFGANG: Well, if there's a continuum to autonomic nervous system response, there's surely a continuum to criminality. People may be inhibited in some situations and not in other situations.

HARBURG: That's important, and the places where all these studies measure that appear similar to each other more as laboratory places than places where people are accustomed to being, out in a world where they usually are. Therefore, there is a sort of a dyscontrol here, a strain, in the kinds of circumstances in which Russ measures those things. They probably all look like the same laboratory situation to the subject. You bring a human being from another world into this world and then you measure while he is in there, and you make generalizations about what you find. Just the very place where you take your measurement, just the life situation where you take your measurement is a constraint, and I'm not even bringing in the other forces in the situation.

Measuring physiological variables is just as difficult as measuring any other kind of variables. There is a lot of variability to these things. If you are looking for interaction effects--which is what I think you are saying--then you're going to have to show interaction effects before your case gets stronger. That's where the whole thing rests. And you're measuring only one side of it.

BLUM: There were a couple of issues that really had very little to do with research. I just hope, in terms of our recommendations for policy or LEAA research priorities, that we would all, regardless of whatever minor problems we would have in methodology or concerns about interaction, not adopt a stance which might publicly be inferred from some of our doubts here. That is, we should not voice opposition to genetic concepts as such, because we are an environmentalist, optimistic about society or as I heard some people wanting to be optimistic about what one can do for criminals or anybody.

I trust we are in agreement that the genetic area is a very powerful, important scientific one in which research should not be deterred simply by virtue of the very powerful environmental forces which are part of the scientific community or even our desire for progress. Are we in agreement on that, at least?

WOLFGANG: I think that's well-stated. I would like to pause for a moment and ask if there is any opposition to that statement.

PRESCOTT: Yes. I would like to register a small dissent.

Maybe I should begin by picking up what you have recognized, that the social physical environment does have a profound influence on our biology and physiological functions. In fact, I think the data suggest that the amount of variance in these variables is more attributable to these than non-genetic factors than have been demonstrated. That always poses a problem when you attempt to put a genetic label on physiological variables, particularly when we know that such variables are strongly influenced by environmental factors. So I think we are in very muddy waters when we try to provide genetic labels.

I think there is another aspect of this which, from my point of view, really reduces the importance of the genetic approach at the human level. I think that as you look at evolution the overriding lesson is that the brain becomes increasingly plastic during phylogenesis. This would mean that it's more and more adaptable, more responsive to environmental changes; whereas, in lower mammals, the brain is more under the influence of what we might call genetic programming. Insect behavior is an example of highly genetically-programmed behavior.

Certainly, I think the general principle here is that through evolution our brain becomes increasingly more adaptive and less and less under the genetic kind of blueprint for specific kinds of behaviors. And I think the lesson here, then, is simply that in higher primates--particularly man--the genetic component becomes less important. This doesn't mean that genetics is not important. It is. I just think it's less important. It gives us, I think, the general blueprint upon which the environment can stamp a whole variety of kinds of effects, which is not as strong for lower mammals.

Now, I don't know whether you believe that perspective, but I am really impressed with the facts of the primate brain, particularly the human brain. It is for this reason that I'm a skeptic of genetics. The other reason is measurement. How do you measure genetic influences of a specific character that would be useful? We have to, eventually, translate measurements of genetic variables back to the individual, and I don't think our science is such to permit this kind of level of specificity.

WOLFGANG: What you're saying is different from what Richard has said. You're making an adversary statement for the importance of environment over biology, and that you may do. What is unknown is not necessarily--given our present measuring instruments--unknowable. What Richard is asking is whether we agree that we not suggest any kind of moratorium on or suppression of research into the area that you may call genetics.

PRESCOTT: When stated that way, I would agree. But I think the issue is one of priorities and not one of moratoriums. I thought he was directing himself to priorities. If we were to come out giving a priority to genetic research in this area, I would strongly object. I obviously would not agree to a moratorium statement either. But I guess I misunderstood his intent.

BLUM: Just in response to that, it would be my own hope that we avoid like the plague any giving of priorities. Who, here, is smart enough to anticipate ten years down the line what we should have done, particularly when so many other branches and kinds of work are not represented here? We might say what we think is most fun or what we are most hopeful for, but to endorse priorities strikes me as a frightful act.

PRESCOTT: May I respond to that? I feel very strongly about that. I'll only take one minute.

I think this has been the tragedy in federal science administration and the role of scientists as advisory to the federal agencies. They have been consistently taking the point of view of opposing advocate positions; of setting for oneself a responsibility to establish priorities. Because of this, what happens? Congress comes in and sets priorities. I've seen it happen time and time again.

The NIH went through this exercise. What are really the important issues in your discipline? You are the experts. Tell us what really ought to be supported. What are the priorities? They absolutely refused to address themselves to the issues. They claimed that they could not predict what line of research would have a pay-off.

So what happens? When you vacate that kind of position of responsibility, then somebody else will take it over for you. That's exactly what's been happening.

This happens to be an issue that I feel very strongly about because I think the scientific community has not been responsible in having the guts to say, okay, the best knowledge that we have is this. This is what we think are the priorities for these reasons. Because if you don't do it then somebody else is going to do it for you.

ERSKINE: There is one thing I think we keep forgetting. One does inherit one's sex. Obviously, it does depend on your chromosomes and hormones, the endocrine system--and this is what anger and all the rest of this hangs on, and aggression. I hope I don't sound too much like Women's Lib', but the male has more propensity for violence than the female. So I think that genetics does come in it this way and we can't deny that.

Or am I wrong?

PRESCOTT: Well, also, it's a learning, social role here.

MOYER: Most people agree that a significant component of intelligence is inherited, and yet there's enough information presented that intelligence is one of the predictors of criminality. Nobody gets upset about intelligence factors being inherited.

ROBINS: There's a lovely analogy I read somewhere which said that to argue about whether genetics or environment determines behavior is like arguing about whether length or width determines area.

I think that the difference here is not really a difference at all. I think that when Sarnoff says genetics does not imply untreatability, that is saying that behavior is subject to environmental input. This is exactly what Jim was saying. I don't think there's any disagreement about that. What is treatment but environmental input? It does seem to me that the kinds of statements that Sarnoff makes are very exciting and tantalizing and it means an awful lot of basic research about developmental patterns, about at which point in time these things become permanent enough to predict from one age to the next, and to what degree they are consonant between parents and children.

The parallel study I think of is the study by Chess and Thomas in which they tried to identify individual differences in newborns, and they couldn't predict much about behavior at one year or two years or three years. One would expect that what you see in a newborn is about as genetic as anything could possibly be. They haven't had any social interventions at all in the first three days in the nursery. That doesn't mean that there aren't genetic factors involved. Of course, there are, but the question is: At what time do they become predictable? At what time can we measure them so that we can select children who are at high risk?

It seems to me that we need to treat psychological variables with the same care that we have expended in developing growth curves. I think that if we start with height as our analogy, we do much better, politically, than if we take intelligence as our analogy.

HARBURG: We simply do not have the methods, yet, for getting at precise, unbiased estimates of genetic effects in human populations. I don't think we have them.

My second point is that there is a critical age where certain enzyme activities emerge. We hardly know anything about that in our studies, but when you cite the studies there are these kinds of effects. Certainly, there are children such as those with phenylketonuria where there is a known genetic mechanism and there's a known genetic enzyme and you can intervene at a particular critical age and you can change the course of development. That's certainly the kind of model, I think, that genetic requirement is talking about. So it becomes far, far more difficult to get measures of behavioral things and enzyme systems interacting.

PRESCOTT: I think there are two responses to that. One, nobody questions the fact that you can use the environment to override genetic effects.

HARBURG: When you get to a critical age.

PRESCOTT: Right.

But the other point here is that I think it is not an appropriate analogy to borrow the specificity of genotypes and metabolic disorders, which is a very primitive elementary function, and compare it to the issues of complex social behavior.

HARBURG: That's what I'm saying.

PRESCOTT: Okay, but I think this is a mistake. There are no genes for complex social behavior as there are for these other kinds of primitive physiological functions.

ROBINS: I don't believe that's true.

PRESCOTT: Not at the higher mammalian level. At the insect level, yes.

ROBINS: I have two children who sucked a thumb and two who did not--it wasn't a thumb, actually. The two children who sucked were the first and the last, so they didn't watch each other. It started while they were still in the hospital, and both of them chose the middle finger of the left hand, an extremely unusual choice. I think the chances of that being coincidental or environmentally-influenced are remote.

MEDNICK: I would make a plea that we drop discussion of genetics. It seems to me that it's been demonstrated that there is a genetic factor. The only thing that this means is that it's not a hopeless thing to examine biological variables related to crime. Let us begin to discuss some biological-environmental interactions. We've seen them. We can measure them.

WOLFGANG: As a sociologist I have great affinity for the work that Sarnoff has done and for, as I mentioned yesterday, reaching out to my colleagues in neuro-psychiatry, experimental psychology and physiology for assistance in learning and understanding more about criminal behavior.

The one caution that I wish for you to keep in mind is that, as we all know, there is hardly any validity to a clear-cut dichotomy of criminal and non-criminal, delinquent and non-delinquent and I needn't draw upon self-research studies to strengthen that statement. Moreover, people weave in and out of behavior that is labelled as criminal. People grow in criminogenic environments and may engage in criminal behavior that's both official and unofficial and then desist after a period of time. I don't mean burn out at 30. They may have a long record of engaging in assaultive behavior and then, at some age--18, 19, 20--stop and are no longer seen in the official records. Either they become more efficient in escaping capture or they just stop. And then there are those who are late starters and those who are early starters. There's a whole complexity of interaction, plus the fact that, at the same time, society is going through re-definitions of what is criminal behavior. Therefore, there are difficulties in measuring the physiological responses. There are surely equal difficulties in measuring, over periods of time, the quantitative degree of, rather than the qualitative static characteristic of, criminality.

MONROE: I liked Sarnoff's model and I use it in teaching my students, particularly in showing the interactions between genetic and environmental family factors. I think it's a very simplistic model. That's why I like it and that's why I use it with my students, because, in one hour, I can present it and discuss it.

I'm not sure, however, that, whatever you call it, inhibitory conditioning or avoidance conditioning is necessarily a complete model for explaining socialization in this world. I have a lot of doubts about it, particularly what goes into what those of us with the psychoanalytic orientation would call conscience mechanisms. I mean, the child learns to fear clenching his fist and raising his hand when his mother is around to punish him, but why does he ultimately learn to inhibit it when mother isn't around, or why isn't our socialization always dependent on having a policeman at our elbow? I'm protesting, a little bit, the simplicity of the model.

MOYER: Sarnoff's model need not explain everything in order to be a valuable model. It's still useful. I don't guess anybody--perhaps Sarnoff does, but probably nobody else--believes that it explains everything.

MEDNICK: To tell you the truth, I can't really think it explains anything. But it is simplistic, very easy to test and serves to organize my thinking in the field.

BARCIK: One thing that's been going back and forth inside my brain is that in this model the two precepts are the slow reactivity and the small reinforcement that develops as a result of it. In terms of clinical experience, in terms of much of what Jim Prescott said, the need for stimulation seems to almost be on the opposite side of that where, instead of a drive reduction model reinforcing some behavior, a need for an increase in stimulation seems to be there as a kind of reinforcement. I have talked with many, many inmates who have found that they needed to create activity because they couldn't function under conditions of school because it just wasn't exciting enough. They even change their crime lifestyle as they develop because it's not exciting enough. They seek extra stimulation. So if you look at what you're proposing with fear as a stimulation, a pattern which is reduced in intensity with reinforcement, you will see an increase in some stimuli patterns.

The question I have in my mind is: Can we use the information that a slow autonomic recovery would indicate that, for these people, there is this need for the increase in stimulation and intensity as a reinforcer?

MOYER: I don't think these two things are necessarily incompatible. It's quite possible, according to this, to have a slow dissipation of fear and yet to still have a need for various kinds of stimulation under quite different systems.

BARCIK: The slow dissipation--I'm not sure that that doesn't set up the need for stimulation. What I'm trying to say is that if we find people--and maybe we will find people--who have this slow autonomic recovery and in addition to that, this need for stimulation ...

MOYER: But they need not be correlated. They could be quite independent.

BARCIK: They could be independent, they could be correlated, but that pattern, in itself, may be a diagnostic aid. Again, if it's a diagnostic aid, maybe we can create some other kinds of things to change that pattern.

When you actually get down to it, treatment becomes an experimental manipulation. We have something that comes in--they combine slow reactivity and the need for stimulation--and we do something, and then we test them again, and that pattern does not change.

MOYER: Let me respond to that because I think it's a consistent point that is being made.

I don't see any reason to believe--if we have an individual with a slow autonomic recovery rate and we bring him in and, through biofeedback, we change the recovery rate--that we are going to change his behavior because the behavior is caused by what's going on in the brain and it's quite possible to independently manipulate various parts of the brain without affecting the central processes.

It, again, relates to this sort of idea that's been proposed that the autonomic activity is, in some way, a cause of the behavior. I don't think that that's a valid construct. I think it's important that we be aware of that because I keep hearing this general idea.

MEDNICK: You can change the peripheral measure without affecting the learning process.

MOYER: That's right. There may be ways, ultimately, of making these central changes. We may ultimately be able to do this by manipulating the neural transmitters or through the use of various pharmaceuticals. We can't at the moment, but certainly that's within the realm of possibility.

BARCIK: That's what I'm after, you see. Right now, we can measure the peripheral responses and it's very easy. You don't have to invade, you don't have to do anything except connect an electrode, but it's very possible that when we begin to go back from the periphery, back towards the central member, we can try to formulate some research to look at some computer analyses of central mechanisms. If we find a person who doesn't change from his external peripheral responses, that may be one thing. Maybe the treatment--whatever it is, whatever the manipulation we produce--didn't have an effect and it won't have an effect until we begin to measure those mechanisms.

MEDNICK: Wouldn't it be easier if we found such a child--just to say, "Oh, here's a child who has slow recovery. I guess we better tell the parent to train him a little more sternly"?

Doesn't that seem like a much easier way of handling it? Sure, his older brother learned very quickly to be a decent citizen, but he's going to have more trouble doing that and the parents better not treat him the same way.

MOYER: Better give him more positive input.

GREGG: I would like to ask a question that would help me. It relates to the last statement you made as to whether it's more negative reinforcement or more positive reinforcement that's needed to change that individual's behavior. Studies have been done indicating that, perhaps, psychopaths have greater difficulty with avoidance learning. Is this an absolute deficiency, or is it relative to the degree of negative reinforcement provided? For example, if you double the strength or force of the negative reinforcement given the psychopath in avoidance learning experiments, does he then learn as well as the normal subject, or does it seem to be an absolute deficiency not related to the strength of the negative reinforcement?

MEDNICK: There have been four studies on this, maybe five. They haven't systematically varied the size of the shock.

GREGG: It seems that that would be an interesting thing to try.

MEDNICK: One more thing. What someone has done, however, is administer ANS-stimulant drugs, and they have then found that they were getting avoidance responses normally. But when they tried to repeat that, they couldn't. However, it is not an area that I think should be abandoned.

PRESCOTT: I would like to add to that because I think we're really getting to the heart of the issue here, and it really involves Sarnoff's assumption that fear reduction is the best reinforcement. I think we need to question that assumption.

From my point of view, it's the positive reinforcements that are, in fact, the most useful. Let me give a broader perspective and then we'll come back to some specificity.

In many cultures, there is no punishment of children so there's no fear and there's no avoidance which you, again, can manipulate to bring about inhibition of the undesirable behavior. For example, there are cultures in which, if an infant cries, it is immediately picked up and put to the breast. That's usually very, very effective in eliminating crying--no punishment. If that's not effective, some of these cultures will start manipulating the genitals. Very pleasurable. That usually does the trick. Now, try to translate those kinds of operations into our culture and you can see what kind of problems you have.

You really offered data to indicate that the negative reinforcement approach--pain and punishment--is, in fact, ineffective in the criminals. That's where they are defective, but not for the positive reinforcements. What this suggests--and I think it's consistent with the findings--is that isolation-rearing produces impaired pain perception. It is not really surprising that pain is not an effective reinforcer, given that kind of impaired pain perception and we ought to start focusing in on the use of positive reinforcement systems in changing social behaviors. I think that has major implications for treatment.

MEDNICK: Jim, you have children, right?

PRESCOTT: Yes.

MEDNICK: When your children start walking out onto the freeway, onto the streets, what do you do? Do you hug them when they come back?

PRESCOTT: No. That's a classic example.

MEDNICK: What do you do when the children reach up for the flame on the stove? Do you reward them when they don't do that?

PRESCOTT: No.

MEDNICK: Okay. What I'm trying to say is that there is a certain kind of conditioning, there is a certain kind of learning, that is best done by positive reinforcement. There are certain things which are very much better learned by negative reinforcement.

PRESCOTT: No. You see, Sarnoff, I think that's the mistake. As adults we create environments which require and demand negative reinforcement, and that's destructive. That's when you are destructive to the socialization process.

As I pointed out, I have two children--a 13-year-old and a 6-year-old. I've never struck the younger one. I gave a spanking to my older one once and regretted it. I have not had to use physical punishment whatsoever, and I think they're fairly socialized. They are affectionate kids and usually happy.

I'll tell you one thing we did do. We never let them cry themselves to sleep at night. We were always right there with a lot of emotional support.

But, anyway, I really think we're coming to conclusions on something here.

D'ATRI: What do you do about the freeway?

PRESCOTT: My answer is, well, the adults should never have created a structured environment in the first place which is dangerous for children, which they cannot comprehend and deal with. It's like putting wall sockets in the bottom of the baseboard so that kids, when they crawl around, can stick their fingers in. Those are design problems adults have imposed upon children which create a discipline problem, or I should say a social control problem. But then we go back to punishment to solve the stupidities of our own selves in creating environments which are impossible for children to deal with, and I think we have to examine what we do as adults to produce structured environments that are really detrimental and which produce the problems in the first place. That's why they have taken the gas controls off the ranges. Remember they used to be in the front? Now they are moved in the back because they found that kids could go up and they would turn on the gas ranges. It's a design problem.

MOYER: In regard to socialization and the use of positive and negative reinforcement, you don't have to wait for a child to steal something in order to teach him not to steal. We inculcate our cultural variables in a lot of ways which can be positive. They need not be negative. You need not use negative reinforcement in order to inculcate culture values. Now, I don't mean to imply that one should never use negative reinforcement. All I'm saying is that it doesn't necessarily follow that all socialization must be a function of avoidance training.

In fact, I think this is probably not the most effective way of socializing the child because there are a lot of other components to negative reinforcement which many people--including Skinner in some detail--have spelled out as being complicating factors producing states in the organism that you really want to avoid--the anger, the resentment.

MEDNICK: There's a trade-off. Anybody who says that all socialization is by negative reinforcement is wrong. I hope I didn't say that. Obviously, most socialization is by modeling, by positive reinforcement. There are certain inhibitory responses that we have to learn.

BARCIK: There's one that you need to watch out for in positive reinforcements. I had some flashes as you were talking about this positive reward and what effect you might have in introducing a schedule of 100 percent positive reinforcement in prison, for example.

I think the way to summarize what I expect might happen in this would be to recall the anecdote about the Butler box when it was first developed. Butler decided he was going to wait until the monkeys stopped pressing the lever in order to open the side to look out at nothing. When they found them the next morning, as the rest of the people came into the lab, the monkeys were still pressing away, they were still opening it, and Butler was there going crazy waiting for the monkeys to stop pressing the bar for this positive reinforcement, or however you want to describe it.

The thing that I have difficulty with when you present that is that, let's say that maybe he's right, maybe there is some defect in there--whatever it is--with the negative reinforcement. Maybe we can't just look at it as a defect in learning avoidance learning, but maybe as a part of that same pattern is an inability for separation of the positive reinforcement side. In other words, the monkeys kept going and would go on apparently indefinitely responding and responding and responding and responding.

What kinds of strain would that have in a family where there is a particular child who needs this kind of stimulation for whatever reasons. I can just see the family flat out on their backs trying to continue to provide this positive reinforcement. The child may end up not really benefitting from it, and the family, in the meantime, is exhausted.

PRESCOTT: I can give one clinical case. There was a woman who called me about two years ago about her three-year-old child. She remembered the film "Rock-a-By-Baby" and she said, "My kid's impossible. It's dependent. It complains. I can't leave the room without him. He used to be very peaceful, very content."

My first question was, "Did you leave the child for any extended period of time?" "Yes." They went on a trip to Europe and left the child with somebody else. I said, "There's your problem."

She was working with a psychiatrist. The psychiatrist was interpreting the situation as, "It's your will against his and you've got to win." I said, "Forget that. What that child needs is reassurance of the emotional trust that you had before. My advice is that you don't ever leave the kid anymore until you're over that. Pick up the kid and carry it. Put it on your back and just stay with it."

She asked, "How long do I have to do that?"

I said, "I don't know, but if you want to overcome the kind of dependence that has developed, that, I think, is the solution."

She carried her child continuously for three days--she didn't get any houswork done and other things--but then the kid reverted back to his previous state--he was happy, he could play by himself, no dependency, no crying, and so forth.

I think that treatment success follows directly from a variety of literature findings. I'm not concerned about the families being overwhelmed by the burdens of children who require stimulation, because I think the lesson is, the more stimulation they require and demand is in direct proportion to the amound of deprivation they have experienced; and the more deprivation the more demands you're going to get. It's a classic stimulus-seeking kind of behavior. We see that in many animal studies.

MOYER: I don't think that necessarily follows. There are a lot of other variables in that, such as the innate need for stimulation. I think this differs from child to child. The hyperactive child is an example. I think it would be difficult to make the case that all hyperactive children were deprived. I think this is a neurological problem and I think that's the type of child that you were referring to.

BARCIK: That's exactly the type of child.

MOYER: In that kind of case, you're not going to satisfy that child with continuous attention, or at least it's very difficult.

BARCIK: However, I think that this is what we need to look at. When we talk about the slow autonomic reversal--and this is what I'm trying to propose to you--maybe we should look at the other side of that, too, with the same people who have been demonstrated to have this deficiency. Maybe we ought to look at what they do under positive circumstances.

PRESCOTT: If I might make just one point of clarification.

I was not implying that environmental sensory deprivation is the sole cause of these problems. Theonatal anoxia, for example, damages the sensory tracts with an end-effect of sensory deprivation. There is a variety of multiple causes that can reduce sensory input to the brain. The end result is somewhat predictable: variants of stimulus-seeking behaviors, e.g., hyperactivity.

DUNN: I just want to add one comment to the discussion and it perhaps reflects something that was alluded to in the beginning. That was the critical criminology effect.

We've been talking about positive and negative rewards in various psychological senses, but solving those particular problems with basic research doesn't completely do away with the kinds of problems that Dr. Barcik mentioned, or the kinds of issues that he raises in using those solutions, because when you attempt to move from that investigatory level to any sort of applied level, you have a fundamental normative evaluation of what is positive and what is negative that may not have any degree of correspondence, necessarily, to the way in which those particular kinds of concepts are used in a psychological sense.

So that, to me, is an issue that although I don't know where it fits, I simply wanted to raise in regard to all the topics and areas of concern. Nonetheless, these fundamental normative questions about application are ones that we hear more and more cries for attention to and for simultaneous consideration.

WOLFGANG: Are we about finished with our general discussion?

HARBURG: I would just say, in terms of priority and moratorium, that I don't think anybody would say there should be a moratorium. I think that the inquiry about priorities is a tough job in terms of the scarcity of funds and all the other issues, politically.

In view of the vast ignorance in the scientific community because of methods and all the difficulties of getting at this issue, what seems to happen is by default--not by lack of responsibility, but by ignorance. We can't help the administrators put priorities on something that we're still, at the best, ignorant about; and then, political realities come into setting priorities about which there may be some handles on so that this "issue" comes down to the line regardless of what we do.

PRESCOTT: I don't think we're that ignorant, and maybe I'm wrong. I would really like to know from this group whether we feel that we cannot set some priorities - set some issues as being more important than others.

MOYER: I think we can make good guesses from our own research. Each of us knows where we ought to go next in our own area, and what you try to do is make the best guess. You are frequently wrong, but you try to make the best guess of what ought to be done next. It doesn't strike me as an impossible task.

HARBURG: I'm not saying that we all don't know what the next study that we'd like to do is, but that does not help the administrators in terms of setting priorities as to what amounts of money can be given out.

MOYER: Doesn't each of us take a global view and think, at least at some time, in terms of what things ought to be done, what areas we ought to focus on?

HARBURG: I think it's possible to do so, yes; but again, in terms of this particular matter, let's say you're going to give money to intervening in family disputes and that kind of thing, and then there's another area here in which we're talking about autonomic systems and social class. Well, there have to be priorities and some very important decisions about money. The criteria for discussion, it seems to me, we are unable to give to the administrators. Inevitably, for some of us in a discussion like this, we have to say, "Well, I have to put that down at the bottom."

PRESCOTT: I would like to see us go through an effort to see whether we could come up with some kinds of priorities that address themselves, really, to the objectives and the mission mandates of this agency. It seems to me that, from my own personal point of view, prevention should certainly be very high in our consideration as we establish these priorities. What is really going to make the most significant dent in reduction of crime and violence and what kind of knowledge is going to get us to that point the quickest?

I think, in that context, we can structure some priorities that can be helpful, but I am always very, very upset when the scientific community simply abrogates their responsibility. We have information that Congress and the politicians do not have. That's why the federal agencies bring in scientific conferences like this--to aid and assist, and then they cop out every time. I've seen it for 15-17 years now in this business, and my frustration is very high. So if you can bear with me in my frustration I would like to see us make a serious effort. Our priorities might not be the right ones, but at least we have a beginning and then we can work from there.

ROSENBERG: I'd like to suggest that we think of prevention and treatment as separate domains in which to set priorities and, within the prevention area, that we try to distinguish primary from secondary prevention. That is, do we know enough to make recommendations about primary prevention? I'm not so sure we do. I think for secondary we do. That would be my suggestion in terms of prioritizing how we proceed--that we make these separate entities.

CLAYTON: One of the things I've been responsible for doing this year is trying to determine research priorities in drugs and crime. It's a hard thing to do. You can do it starting at the top by identifying the five major issues or categories about which we should start asking questions. Or, you can start at the bottom with a detailed review of the literature, and see what factors out. Then you can say, what is it we know, what is it we don't know and what is it that we can reasonably expect to achieve. Perhaps we should identify the simple questions that should be asked.

I think Sarnoff and Jim both hit on a very interesting question. You can start with criminals at one end of a continuum and you can start on the other end with children in the beginning stages of life, perhaps focusing on children in families that live in stress areas, and children who were birthed with a Lamaze Technique or who were born by use of a Le Boyer method, and you can follow these kids up.

You can talk about positive reinforcement, negative reinforcement--there are a lot of ways of approaching this issue and I think we can probably come up with them. It doesn't take much. You can always come up with the ideas if you want. The real irresponsibility is not coming up with the ideas.

BLUM: Since we are, in fact, about to begin another type of work here and since I don't want to drive your frustration tolerance right over the edge, is it acceptable to all of us, as we approach the next stage, that we accept some responsibility for accepting areas of work which might be done? I personally would find it overly optimistic to say that this is what should be done by someone else in terms of their administrative priorities.

As you said, there are our best guesses in areas of our interest, things we believe are sensible; but carried to the next step--to say that, therefore, they should govern either administrative policy-making or, wildly, the kinds of judgements which even a peer review group might make about very different order or missions, strikes me as ruling out lots of things that we shouldn't rule out. Is that acceptable, that we limit ours to a modest statement of our guesses, and not try to sell these too hard?

PRESCOTT: It wasn't my intention to sell goods of any kind, but rather to provide some structure and some best guesses, best estimates. I really think that it has to be embedded within some policy frame of reference and I think that both Gregg and Ewing could, maybe, say something to us as to where they see their Institute going and provide a frame of reference for us to respond to. I think that would be helpful.

WOLFGANG: I was going to ask Mr. Ewing if he has any guidelines for us as we move into this.

EWING: I'm not sure I have more to say at this juncture about the policy frame of reference than what I said and Jim said yesterday as we started. Maybe the most useful thing would be to repeat that briefly.

From our point of view, we have a mandate in our legislation at the agency to pursue, as the Congress put it, "the causes of crime". That's pretty broad. That lets us do pretty much as we choose within the limits of Senator Proxmire's concerns and others on the Hill. Truly, we have done very little in that area until the last two or three years, when we began to explore some of the variables that appear to be most closely related to crime, including, and in particular, unemployment. But what we have not done is to go the next step and explore some of the physiological, psychological, sociological and cultural variables which work on the individual in a way that, in terms of research we've done so far, we really have no or very little experience with.

So we've really, as Jim said, done a great deal of research of a kind that explores operational agency concerns, the kind that raises questions of efficiency and effectiveness. What we've learned from that is that things can be done that are useful. More interestingly, what we've learned is that there is a wide range of those studies that point us or turn us in the direction of looking at these more fundamental determinants of criminal behavior and correlates of crime.

In response to Marvin's earlier comment, I don't feel in the least uncomfortable with the directions that this discussion has taken. On the contrary, I think that it begins to get at some of our central concerns. For example, the agency has--in terms of its action dollars--put a lot of money into career criminal programs, programs to select out for special priority prosecution those whose careers exhibit habitual offense characteristics. That is both a reasonable and popular approach. But, in the end, we also recognize that we don't understand very much about these people; consequently, and in perhaps what may be the usual way of a federal agency, we began the actions first and the research later.

But, at least we have begun the research and so we have major efforts going on which are exploring a whole range of questions relating to habitual offenders. What the research is telling us, again, is that there is a whole range of physiological and social variables that are absolutely essential and that we don't have a very good grasp on.

So I have a feeling that, at least for the moment, we have a fairly broad license, as well as a fairly broad mandate--one that I think will allow us to explore a wide variety of questions.

I think it's important for people to recognize that our mandate probably stops short of the most basic kind of research, by which I mean research which is not in some way related to our mission at the agency. But that relationship, I think, can be certainly extended or said to include almost any of the kinds of research which we have talked about in the last day and a half. Therefore, our problem will not be one of attempting to determine whether or not what is likely to be suggested--and I think the outlines of that are, if not clear, at least already before us--is appropriate, proper, and within our mandate, but rather which things we can afford to do and in what order we ought to do them.

GREGG: I would join with Blair in saying that as you deliberate on this question of priorities, don't feel too constrained by our particular limitations and missions. We can really serve two functions: we can and should be supporting research directly; also--and I think this is fairly significant--we can play an advocacy role within the federal establishment for areas of research that seem to be important. For example, the Office of Education and LEAA have some very mutual interests in several areas of research that we have been talking about in the last day and a half, and we--both agencies--could play an advocacy role, perhaps with respect to the Institutes of Health and NIMH and the National Science Foundation, for supporting work that seems important.

So I would hope, as you think about priorities, that you not be constrained, but indicate what you think is most important. Then we'll deal with the outcomes in any way we can.

Jim Prescott mentioned several criteria that might be used in determining priorities and I thought they were good ones. I would add one more, though I am sure there are many others that you could use. I think the idea of getting inter-disciplinary participation in this work is important. That's one of the attractions, for me, to the kind of longitudinal studies that some of you seem to be talking about. I can't imagine how major studies of that sort could be designed and conducted without very substantial inter-disciplinary consultation.

WOLFGANG: Those are very helpful comments. I feel less restrained and restricted than I did before.

MONROE: Could I just ask a question?

One thing I noticed in this program announcement, for instance, was that it seemed to preclude juvenile research. This is the kind of question I wonder about. It seems to me what we have been talking about today and yesterday focused a lot on that.

EWING: This is a complicated issue. The Congress has, in its wisdom, passed two pieces of legislation under which LEAA operates. Really, there are more than that, but two pieces for your purpose.

One is the Juvenile Justice and Delinquency Prevention Act, which established a second research arm within LEAA--The National Institute of Juvenile Justice and Delinquency Prevention. The law also says that 19.15 percent of the appropriation that LEAA receives must be spent on programs and projects and activities that have to do with juvenile justice and delinquency prevention.

What that all amounts to is that, on the one hand, we try to stay out of the bailiwick that is carved out as the exclusive domain for juvenile justice and delinquency prevention, but, on the other hand, we're obliged to do some work in that area. It occasionally confuses us and them as to how we work this out.

This particular program is one in which, typically speaking, activities that are exclusively limited to juvenile justice and delinquency prevention types of work are not funded by us, and they are instead forwarded to this other office. On the other hand, I take it that longitudinal studies, which are aimed at issues that have to do with "the causes of crime" are, under our legislation, certainly legitimate for us to pursue.

I don't know whether that answers your question exactly, but it's a bureaucratic and legal problem which we do our best to overcome by massive human efforts to coordinate things.

MONROE: The other thing I wondered is whether or not, as a practical matter, we're not going to have to address ourselves to the new regulations on informed consent within the prison in research studies. It seems to me that this is very serious.

EWING: We will have to do that. We have consulted with our general counsel about whether HEW's regulations govern Justice Department research. The general counsel was unsure.

MONROE: At the local level, HEW regulations will govern our research because they will have to go through our research committees and I am sure that they will be guided by HEW.

EWING: Yes, they will govern it there. Our general counsel went on to say that if they didn't, we ought to have our own which would be consistent. So I think the answer is that however that comes out we will have to address that issue ultimately.

WOLFGANG: We'll break and go into our separate workshops. We have been designated as groups A, B, and C. Group A consists of Mednick, Moyer, Hare, and Monroe. Group B is Blum, Bachman, and Robins. Group C includes D'Atri, Perry, and Prescott. At 2:30 we will reconvene.

I think that Nathan Rosenberg's suggestion about treating prevention and treatment as separate domains might be considered, and also the suggestion that Richard Blum made that we might put forward suggestions and best guesses of our own interests. Then let us see what happens so far as any allocation or classifications of the factors that were exciting. Social scientists, in general, have some degree of honesty about not wanting to impose a priority allocation heavily on other people, and we'll try to overcome a little bit of that collectively.

It would be desirable if someone from each group were able to come back when we reconvene in the afternoon and give us an oral report. I'm not inclined to step up to a board and make all kinds of sales and pitches, but I'd like to see if each group can't come back with some written statement to be presented at the last session.

(At this time, the colloquium recessed to the workshops to reconvene at 2:30 p.m.)

SECOND DAY AFTERNOON SESSION

WOLFGANG: First in this session I'm going to ask that a statement be made by Blair Ewing to respond to some concern about the extent to which this particular group and any recommendations that may come out of it represent the totality of recommendations to be considered by the Institute.

Then I will ask each group to present its recommendations. After that, I would like us to be sure to consider briefly the issue I raised earlier about the institutional structure to encourage inter-disciplinary research.

EWING: We will be developing a draft research agenda in this general area and we want to share that with you, and would invite, as well, any comments you might have on it. When the final agenda is ready, we'll share that with you too.

Now, as we prepare that draft agenda, I think we would be remiss if we didn't consult a large number of people in the process of composing it. This group is one that we thought would have some very useful and distinctive contributions to make to that. But there are a number of people, from other disciplines and other areas, whom we also want to consult. So, for those of you who have raised--with me or with others--some concern about the agenda, that is a brief explanation as to how it will be prepared and developed.

WOLFGANG: All right. Let us move right away into Group C. Jim?

PRESCOTT: I'm going to call on my colleagues to provide information. To begin I'll just go through some of the notes I have and highlight some of the issues, and then have the other three members of the group complement that.

Actually, in the first hour we spent time on issues that really were not related at all to the subject matter at hand, but they were obviously of concern. White-collar crime was one of the issues that took a portion of our time, and the consequences that might follow crime in dealing with problems of stresses and neighborhoods and families, and so forth. We were concerned with victimization and victim research--why some individuals do not report crimes and do not utilize what is available. This led to a discussion of the relationship between families and the police, and how they work together or don't work together in dealing with the problems of violence.

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We dealt with the issues of alternatives of prison and punishment, these being the positive/negative reinforcement systems. We discussed the emphasis being placed on developing alternatives in dealing with the problems of criminals. The roles of leisure and pleasure activities are a part of the alternatives in communities that could deal with this.

Toward the end, we concentrated basically on the family and the various factors and processes within the family that contribute to the lack of control of violence. I think one of the strongest things that came out is that we needed to study the successful families in the high-risk neighborhoods and what the characteristics and processes have been in families that have successfully coped with the violence and the structures in which other families were not successful in coping. That was one of the points that we ended up on. Again, we were discussing other kinds of basic issues--the consequences of unwanted pregnancies; the consequences of foster care; single-parent families; these kinds of variables in the development process.

Emphasis was placed on economic factors. Managing budgets was mentioned as one aspect of examining the interaction between the economics of family life as it affected the affectional relationships that exist in the family and, generally, how to understand how a variety of stresses interfere with the nurturing aspects of family relationships, which take away a major source of control of the functional aspects of violence.

One item that I mentioned specifically was breast feeding, which is really a source of great nurturance and is critically involved in affectional bonds, and, again, we find that stress of various kinds will interrupt and interfere with that process. That's just one small item of a variety of things we talked about in our discussion.

Toward the end, we discussed the roles of punishment and pleasure--or reward responses, if you will--not only in the home, the neighborhood and the school with respect to the problem of structured family violence, but also in relation to the concept of psycho-ecology, i.e., of nurturance, of self-esteem, and of confidence, and all of the factors that contribute to that.

This, really, has not been a very adequate representation of what we discussed on the state of the neighborhood and what goes into structuring, for example, stable neighborhoods and how they affect the families, and this should become an important part of consideration of research. I really would like to call on the others to make their comments because I know I may have missed something. We spent a lot of time talking about the role of the police, for example, in the family processes.

PERRY: The only thing I would want to add is where we ended up. We agreed that should there be an inter-disciplinary study over time, we might look at family unity. We talked about things which were brought out by crowding, and we looked at variables such as crowding. Do we use the pleasure and affection that Jim talked about, his concept? Do we use the biological aspects of families and facts coming from school records, family histories and medical care? If we use the family as a unit, it provides us with multiple perspectives to address.

HARBURG: I want to throw in something coming out of left field. I believe the methods we use intimately affect our interpretative conclusions. I'd like to see more studies where data that are being collected by so-called researchers get in immediately to police agencies, and that this kind of information be utilized, instead of waiting five years for the report to come out. I can imagine a lot more interaction between the researcher selecting things to look at and the people who are going to use the information being built into the studies. That's a difficult concept, but it can be done and I think we ought to look more at that because I just am too impatient at this point to wait five years for studies to come out when we can start applying knowledge while we are doing studies. I would like to see more attention to that.

WOLFGANG: You would like appropriations for dissemination of information.

HARBURG: It's not just dissemination. It's data collection and utilization at the same time. When you talk about utilization that means you have already done some research, and then the next sequential step is to disseminate that research. I'm talking about a simultaneous kind of thing. For example in terms of a hospital organization, or the police and family situations, the information system could be used for multiple purposes--for management, for evaluation, and even for research at the same time. That's very difficult to pull off, but if you do it right you will get it all out.

WOLFGANG: Certain kinds of things will come out, and others, I think, will have to wait.

HARBURG: Right. I'm just saying that I don't think we move in that direction now. I think it's kind of rare.

D'ATRI: The idea of collecting data and utilizing them really gets into a larger issue, and that's collecting representative data or baseline data so we know the distribution of certain characteristics and certain problems. Those are the only kind of data that will really help us make any intelligent decisions. A good comparison, again, is a program now funded by NHLBI* which attempts to mount a high blood pressure control program, but before those involved do anything, a probability sample is being drawn to assess certain characteristics in the community. Admittedly, these are expensive samples, but the data will be invaluable not only in assessing the quality of the program, but also in helping design the program to be implemented in a three or five year period. I would like to really emphasize that the data to be collected shouldn't be just spotty data here and there, where you talk about one little community or part of a community, but rather data that could be used on a more general level.

We also discussed different research methodology. Of course, everybody is very much in favor of the longitudinal approach, the cohort study. A number of comments were made--for the most part by me--dealing with the utility of other data that had been collected in the past, data that may be on file someplace. It may be that psychological records collected in a systematic fashion 20 years ago on a cohort exist which, then, can be utilized in a prospective study in retrospect. That would enable us to do relatively "inexpensive" research to assess the importance of these characteristics, and good cross-sectional studies that we could later build on as a prospective study. So, in terms of the methodology I believe that we ought to investigate some of the epidemiological techniques that are currently being used for chronic diseases and for psychiatric disorders that might be utilized so that we are able to maximize whatever funds are available.

WOLFGANG: Relative to that, I think I heard Ernest say that we should also encourage multiple methodologies.

D'ATRI: Yes, and I fully agree.

WOLFGANG: Some qualitative as well as quantitative.

PRESCOTT: I would like to make two more comments.

*National Heart, Lung, Blood Institute.

There was an expression of trying to understand better what it is in the family or the community that sanctions the value system of violence as this appears in so many different ways. In child abuse, we find examples of how a child can learn a value system of violence, and we ought to better understand how a child acquires such a value system from those experiences.

Then, in that context, mention was made of premature infants being a particular group of infants that are subject to abuse and neglect as a part of that. Then related to that, of course, was, again, a variety of structures upon the nurturant aspects within the family. Specifically, what is the quality of sexual functioning between the husband and wife, in the affectional sense? Disruption in this area of relationships clearly has all kinds of consequences and implications for violence. Therefore, one has to look at the general role of nurturance and affectional relationships in the family as a whole. Again, Marvin, your data mention this point.

WOLFGANG: Very good. Any other comments from Group C? Russ, Group A?

MONROE: What we did was to reemphasize what has been emphasized over and over again--the importance of longitudinal data, of longitudinal studies, of prospective studies, and we looked at this in some concrete ways. Specifically, we discussed the publication that's coming out shortly, listing, I believe it is, seventy prospective studies in Europe. Sarnoff has collected these studies and described them in detail, including the kind of data collected. We wondered whether maybe we couldn't get LEAA to aid in the dissemination of this information and perhaps even help in terms of indexing it because of its importance.

The next step would be to collect the same kind of data for the United States and Canada including the types of data that have been collected, the willingness of the investigators to cooperate in terms of sharing data. This would yield the advantage that you mentioned--the cheap prospective/retrospective studies that one could obtain.

The next step, of course, would be a center for prospective studies or longitudinal studies. This is a great idea and one that would require a lot of multi-disciplinary participation, and which certainly would, of necessity, transcend any study of criminology and be a study of all kinds of psychopathologies. We quickly reviewed the kind of data that you would want to collect in a study like this, which would be very expensive, very large, and probably have all kinds of political and scientific hurdles to surmount, but probably would be worthwhile in the long run. Of course, the kind of data we were talking about were hereditary data, family studies--not only for criminology, but for hyperactive behavior, for psychoses, for some of the neurological disorders--prenatal data, family interaction data--particularly in regard to early behavior that Jim mentioned in terms of his attachment behavior, and then the kind of family interaction data that Lorraine reported.

We would want to collect cognitive function data in terms of the attentional syndromes. You want to have motor measures involving hyperactivity, rather extensive screening in the neurologic field involving soft neurologic signs and histories, or evidence for neurologic stigmata or histories of neurologic insult. You would get into the whole problem of impulsivity, low self-image. In essence, we only mention these because any center that was doing this sort of thing would be working out their own programmatic analysis or collection of data, but it emphasizes the point that no one particular federal agency seems sufficient to handle this sort of thing.

I see a great problem politically and bureaucratically, in trying to organize such a study, but I think its important because none of the prospective studies that were started 10 or 15 years ago could possibly have collected the kind of data that we can collect now, in terms of the much more sophisticated biochemical and other measures of behavior that would not have been collected some years ago.

We mentioned a number of miscellaneous items related to these kinds of prospective studies. Of course, it is important to get the maturational changes in whatever measure you're using, whether it's biochemical, psycho-physiologic or electroencephalographic. We have the problem of matching groups, we can match them for sex, age, IQ, education and genetic factors too, which may, in some way, help us tease out the environmental influences that I think are important, but not clear.

Sarnoff mentioned some new multi-variant analysis programs--and I'll let him discuss those--that are appropriate for these kinds of longitudinal studies. We mentioned, of course, the importance of the biologic markers, but we felt that it was highly unlikely we would get this until we had a different taxonomy of criminal behavior than we have now. Probably any biological correlations would have a biologic marker and would depend on a much clearer phenomologic analysis of the anti-social act or actions.

I think that some of the other things that came up, and Ken mentioned this earlier today, is the area of sex-related aggressions. I hadn't thought about it before, but it occurred to me, too, that we have available to us techniques that we didn't have two years ago in terms of being able to get hormonal profiles. One of our biggest mistakes in previous hormonal research was to look at one particular hormonal system, and now you can run whole profiles as easily as you could make one analysis two or three years ago. This might be a very important area to look at and might have a relatively quick pay-off.

I think I'll stop at that point and let the rest of our group make any additions to what I've omitted.

MOYER: I guess the only other specific item that I would add to the recommendations is aggressive behavior as an allergic reaction and the continued development of anti-hostility drugs in these cases.

MEDNICK: There is a method which is especially appropriate for longitudinal studies in Sweden. We've been using it quite a bit ourselves.

WOLFGANG: We've been using it, too.

D'ATRI: Does this data analysis take into consideration repeated measurements over time?

WOLFGANG: Yes.

D'ATRI: That's really quite a statistical problem and has been over the years--how do you handle this?

CLAYTON: It's called covariance structure analysis.

MEDNICK: I have another point that I would make and that's with respect to this control of genetic variables in research. I think that a very good design for exploring environmental causes of criminal behavior is the use of twins. If you have a pair of identical twins, where one has become a serious recidivistic criminal and the other has not, then the differences between them have to be due to environmental factors. Their genes are identical. Exploration into the lives they have led and the differences between them, I think, could be a critical method of detecting environmental effects in this area.

WOLFGANG: Relative to that, I meant to mention yesterday that it seemed to me that we are almost calling for another William Healy/ Augusta Bronner comparison of siblings in the same group culture where one was delinquent and the other wasn't. That study was done in 1936 and it's never been repeated, as far as I know. This was not using twins, except as they happened to appear. I always found that a fascinating study, but if brought up to date with our new research methods and techniques, it could certainly tell us a lot.

There is one other thing that I wanted to mention relative to the center that Russ mentioned. As you must know, there is a mammoth study that NIH sponsored some years ago dealing with cerebral palsy. Philadelphia may be the only place to have done this: for 9 years for each birth in Pennsylvania Hospital in Philadelphia--around a little less than 10,000 births--a dossier was established on the mother, the birth, the follow-up each year and a psycho-physiological analysis. There are tremendous amounts of data. Apparently, that collaborative study just collapsed. We have put all those data on tape. Some of it was put together by the private non-profit corporation called The Center for the Continuous Study of Man. Those are tremendous data, but unexplored.

Are there any other comments on Group A?

D'ATRI: I just have one question. I guess it's getting back to the possibility of doing a study, maybe, in Finland. I don't know anything at all about the crime rate in Finland and that's why I'm asking the question.

When you do a prospective study, a cohort study, you identify a group that is free from all disease or free from whatever you're going to study and you watch for the appearance of that outcome over time and associate it with factors that you have assessed over time. Unless there is a good prevalence of whatever you're looking at, the studies are very, very expensive and won't yield much. If the outcome prevalence is 15 to 20 percent, then the predictors may be quite good. But, in Finland, if there is a very, very low crime rate, then Finland may be the cheapest place to do this kind of study, but you won't be able to determine any of the correlates.

I think that's an important consideration in your determination of where you're going to conduct a study and whether or not you're going to do a study of all births in an area or do a weighted sampling of areas that may be at higher risk for the development of certain episodes of crime. So I think there are a lot of considerations that we have to go into before a real decision can be made.

MEDNICK: I think, to answer that, Finland was second to the United States in violent crimes, but I think the general point is well-taken. If you are looking for example, at sexual crimes and you know that they are one percent of the population, it's a very small group. If you only have a population of 5,000 in a one-year cohort, then I don't know what you'd get in the way of sexual criminals. You do have an appropriate population size, but if you're looking at petty thievery, there are so many.

This brings out another point. That is that, when feasible, in studying a population like this you can look at more than just petty thievery or more than just sexual deviance. You can look at any kind of illness at all that is reasonable in comparison with the size of the cohort.

D'ATRI: Yet the way you really determine the sample size is by first predicting the differential prevalence rates between whatever groups you think you're going to be seeing and working the equation backwards. If you expect one percent overall, you may expect it to be twofold in one group versus the other group and these figures determine your sample size.

MEDNICK: I've been doing some research in schizophrenia. The probability there is about one percent. In order to increase that probability, as you are suggesting, we took children who had schizophrenic mothers where the probability, depending on the seriousness of the illness, would be between 10 and 15 percent. There are ways of increasing the yield, but I would say you are better off taking a larger population, at least in principle, because whenever you study some selected group you have trouble in interpretation.

D'ATRI: Well, you do unless you construct properly weighted samples. The general population is nice because then you can get a true distribution of the values you are looking for. In other types of studies you have to statistically manipulate those values to get them to approximate the more general level.

WOLFGANG: These are internal problems dealing with your analysis; however, having baseline data is better.

We will go to Group B.

BLUM: I don't think it's surprising that our emphasis was environment. Our procedure was simply to generate a research proposition without dealing with the difficulties of methodology. So, we have elicited 40 or 50 ideas that we felt, were worthy of our attention during the period that we were there.

I would begin, however, with a cautionary note, which was for the use of planning the next session. It was felt that it would be helpful if some of the specifics or particularities of sub-group characteristics of crime itself were emphasized so that we would then--and in the future--be careful in assuring the similarity of types of crimes that is not assumed when we discuss etiological characteristics or differential studies.

The criteria, then, that we would employ would be a narrow one with the notion that taxonomy is important. It follows from that, of course, that when one is discussing research priorities, these would be linked to types of crime, per se.

One practical proposal for criminal taxonomy which might generate administrative priorities was to continue with the victimization studies that are being done by LEAA. We should be sure that, in the course of those, one measures simultaneously the recidivism concern and estimates of risk, as has been done, and combines that, perhaps, at the same time, with self-reports of criminality and see if one might not come up with a scale of priorities based either on prevalence or degree of worry. This was part of a larger concern that we had with the definitional matters.

With regard to kinds of studies which were proposed, it's easy for us to suggest these in three categories - one was prevention, one was treatment, one was basic. I found it harder and harder as we went along to be sure what the applications of any one of these were and, finally, I gave up, although there were very clearly some experimental studies and treatment intervention evaluations that came along.

Environmental factors, such as housing, families, and neighborhoods, were frequently mentioned and the roles that they played. More often, I think, our suggestions were for development studies with particular attention to the parent/child interaction and to family work, evolving characteristics of the child and his family, school and other environment. However, I think partly because of our learning in the session and the inspirations offered by our colleagues, we found ourselves insisting that such measures of psycho-physiological functions would be included in our other endeavors where measures might be achievable and applicable.

Another type or set of studies which emerged from our group might be characterized as epidemiological and graphic. Perhaps the fourth set - this is a very rough classification - were those that focused on special methodological problems. Illustrative, for example, would simply be the adequacy of public or frequently non-public records and the problems which we face, not just practically but in law, with respect to access to these records for confidential histories.

In the course of our discussion, we had some special references, I think, to the considerable importance of school experience. Three of us have all done work in school settings. It's sheer coincidence that that emerged as something that we thought ought to go on. Unemployment experience was certainly marked as a later possible criminogenic variable, or at least an influential variable--and, again, the emphasis on early interaction patterns. There was also mention made--I think an important one--that the recognition of the problem child is easily accomplished. What to do with the problem child, what to do with those correlates, is by no means so easily known.

There was another set of foci. Several of us have been involved in drug studies. Much reference is made to the associations concerned with alcohol and criminal behavior and some sets of specific proposals were made with regard to that.

There was also a set of proposals made which could best be described as studies of the economics of the offender as opposed to the economics of the crime. This has a way of getting at the activities and interests of the offender, his money budget. Then a broader suggestion was made--which would be very interesting indeed--that we really learn how different classifications of criminals spend their time. What things might be derived from knowing how they spend their time are unknown as yet, but I'm sure we would find interest in this research.

There was a special concern expressed, and several studies suggested, with reference to the two kinds of opportunities. One was the opportunity to engage in violence which is offered by the presence of guns. There was a practical goal here which had to do with further arguments for gun control--studies of the density of gun ownership with reference to other characteristics of violence and so forth, carrying on, really, from some of the earlier work in violence commission with guns.

The other kind of opportunity in which we were interested was environmental controls or environmental structuring. What about opportunities for theft, opportunities for other kinds of criminal expression that are built into our environment? Are there crime control programs which derive from examination of these environments?

I am reminded, as an aside, of the simple reduction in burglary rates which was a chief result when one of our police departments insisted upon participating in building plans--the approval of lighting, parking arrangements, and so forth--in the city. It did seem to work.

As noted, some specific experiments in intervention were proposed. Let me read two or three. Knowing the relationship between school achievement and misbehavior, we still don't know which comes first. Longitudinal studies are in order. Important is combining research on criminals involved in delinquency with other work which is continuing and expanding on the learning deficiencies. An interesting application of this would be continuing research in head-start or head-start-like programs where, in addition to intervention outcome measures, it would be well to have measures of delinquency and measures of truancy.

We were sharing some data and it seems clear, for example, that school attendance is a remarkably constant and useful measure or marker for other kinds of school behavioral problems. Why not start using that consistently to try to persuade others who are doing research on school success to employ these measures as well?

Given our knowledge of many personal and social correlates of children's problem-behavior, later delinquency, and later adult crime, we have a considerable problem, still, in describing how much the variance is accounted for given any set of correlates with one or a group of studies. It's the problem of weighting, at a given time, the role of various inputs. Although many earlier studies have not engaged in the difficult problem of studying the interaction between such variables, it is very clear that knowledge of interaction, statistically defined, is certainly essential.

The problem, then, is also an interpretive and theoretical one of going beyond knowledge of correlation and not being satisfied with the actual levels of contributions to variance which are currently set forth.

I promised not to identify one of our members who said, in view of the literature, we suggest that the best we could ever do is .50 with a variance of about 25 percent, and suggested that there would still be work for us in our lifetimes if 75 percent remains a subject of uncertainty and we were given the opportunity for discovery.

Suggested methods, of course, were to use short longitudinal studies across blocks of time, cross-sectional studies related to these longitudinal blocks and so forth.

It was conceived as optimists always must, that if one, in fact, had been able to examine interaction and achieve weights at a given point in the developmental epoch, one might then propose an intervention strategy which, upon testing, would tell us whether or not we had had success. Indeed, retrospectively, that's what we're setting.

Another example noted, in the course of evaluating intervention programs, was that one should not wait for adjudicated crimes to occur but find intermediate events and their correlates, e.g., intelligence, achievement, attendance, peer judgment, teacher evaluation--as measures which will allow early delinquency clusters or factors--or paths--to be charted. Here, of course, we note psycho-physiological learning. If one does have an effect on delinquency, if one does know the correlaries, would it not be nice, then, to follow the earlier indicators to see the course of evolution of these early indicators? Wouldn't it even be nicer, if one watched some of the known correlates, let's say, of early delinquency disappearing, and one followed, then, to see if the later delinquency was also diminished?

It's a twofold optimism. It suggests that we don't have to wait quite as long as the 10 year/20 year evolution of criminality and it suggests it will then give us a little more certainty about some predictor variables we've been using, putting them in a longer framework of time span studies.

I'll give you two more of those. One is perceived parental difficulty in applying discipline. This struck us as consequential. It is assumed, indeed it is known, that siblings behave differently with regard to being problematic, learning difficulties, and delinquency variables. There is no difficulty in proposing--as has often been done--measurement of these differences. But there should be more attention to the parental response and the adequacies of parent response to these within-family and between-family differential discipline problems. Measurements of characteristics, perhaps, in a family and with individual parents who are capable both of setting forth alternate strategies of discipline or capable of knowing the alternate strengths of energy reservoirs which are required to cope with kids who really are quite different could be looked at. Setting this forth in more abstract forms, we know that some relationships present chronic and others sporadic disciplinary problems, and that it is important to distinguish between these developmentally. One speaks of periods or one speaks of times, but certainly how parents respond to these differentially characterized youngsters is a consequential thing, and to watch that in relationship to the emergence of other variables as they come into it is important. It was noted that the detection of the problem child-qua-psychopath or, in some cases, possibly later so defined--is rather easy very early in the school years. I think all of us did agree that that did seem to be the case.

If one has a reliable constellation of traits and characteristics which allow measurement and agreement on classification, then can one, over time, decide the transformations in this problem-behavior which represent essentially a continuing difficulty, or can we identify discontinuities in behavior?

Can we, in fact, chart the episodic versus the chronic functionings? Can we, in the course of that, identify precipitators that make episodic cases more frequent or induce prior episodic cases to be chronic cases?

The question, then, is: Are assumed underlying personality variables present from birth, continuing throughout early life, and observable? What classifications of youngsters and variables would be required to be created in order to account for the end result of a series of delinquents or delinquent behaviors?

You now have a sample of our deliberations. I will leave, now, the non-bizarre commentary to my colleagues.

ROBINS: I would just like to expand a little bit. I thought that was an interesting way of looking at psycho-physiological variables. We were talking about them in the group as predictors of failure, that is, predictors of delinquency. There are a lot of families that cope very well with difficult children and we thought that if psychophysiological variables were a good predictor of delinquency, then one ought to be able to link them to parental reports--link the kind of information that Sarnoff was talking about to parental reports that this is a particularly difficult child to control, even if the child is controlled successfully. Then you don't have to wait through adolescence to see what eventually happens. The parent is probably a very good evaluator of whether a child is difficult to control.

BACHMAN: I think Dick did a fine job of summarizing. I would just mention, very quickly, that we do have the interaction of teachers and parents about children who have this slow recovery.

WOLFGANG: I would ask you if, for 5 or 10 minutes, we could just address the issue of inter-disciplinary research--specifically, as I mentioned earlier, the related administrative, organizational and institutional problems.

Obviously, we're not going to exhaust this issue or resolve it. Since, however, the term has come up so often and suggestions have been made--particularly of longitudinal as well as cross-sectional studies--I think that we ought to place in the record any comments we have about ways of encouraging that kind of research.

MEDNICK: I would just comment that it's very hard, very hard. I'm working now with a man who is a geneticist. We're working on an inter-disciplinary project trying to understand the consequences of having an extra Y chromosome. We have a lot of psychophysiological data. We have alot of neuro-physiological data. We have a lot of social data, intellectual data, cognitive data. It requires that he and I sit down together and he really teaches me. And, I have to sit down and teach him about psychophysiology and the few things that I know about criminology.

You can't simply have a person who is a biochemist and a person who is a sociologist deciding that they are going to do inter-disciplinary research. It just means that the sociologist will analyze the social class variables and the biochemist will analyze testosterone, and then they will publish in their own little journals, and that's the end of that. It requires mutual instruction to make use of these things. I'm beginning to learn something.

MOYER: I think this implies that you have to set up some ongoing procedure whereby you have continuing interaction among your co-investigators. A continuing seminar on the problem, I think, is the essential factor incorporating all disciplines.

ROBINS: I think we must also support training programs so that people from different disciplines actually spend some time actively learning from each other. I think Sarnoff's point is terribly important. You have to know both sides of the street.

D'ATRI: I'd just like to support the idea of a training program in a career development sense. I think it's essential.

DUNN: Coming from an academic program where that was standard operating procedure, I have since developed a training program under NIMH for just that kind of thing. It is on a very, very small basis where one post-doctoral fellow was brought together with someone from, for example, anthropology, economics, and so forth, to form a research team. This is the kind of thing that I think we are talking about encouraging. I think we should develop a whole expertise along that line.

BLUM: I'd like to make a suggestion of a very different order. Having had experience in this kind of thing--both successful and grotesquely failed--one resorts to clinical commentary at this time.

I think the management environment is the real problem. We talk about ourselves as suffering human beings or enduring human beings, but let's assume that inter-disciplinary work is difficult because we're strangers to one another. One has to learn, and people in academic and professional lives are sometimes competitive.

I think there are lots of reasons not to work equally together in the normal environments which we've created and I think it is important, then, to create affectionate management whereby people are given the opportunity to work together pleasurably. The important thing is to really enjoy being with each other and pleased with the chance that the work gives the focus of that pleasure.

This imposes an immense obligation on whomever is managing the program, and usually the manager of a program in a university or in government is, himself, a sufferer since the structure he works in is almost always painful to him.

The consequence, then, of having to be a good father while being jumped on by a bad foreman is difficult. How can one take the strain and be sure that kindliness reigns so that learning can occur?

I don't think we're that different from six-year-olds in our sensitivities and in the need we have for a nourishing environment in which to learn. Yet whenever we talk about these damn places or jobs, we never mention the things that are probably most important to us--what each day is like. It's an old prescription. You might find it in a Bible or two. But I don't think we should forget it if we want interdisciplinary efforts to work.

HARBURG: I'd like to back that up. It has been my own experience, too. But along with that, however--even if we have these kinds of people who want to work together--there are some kinds of barriers. I've known of groups that could sit around and talk about concepts and really get on well. Then you begin to set the design up and you say, "Well, we're going to use this way of observing," and all of a sudden the group breaks up because one wants to do it their way, one wants to do this, one wants to do that, and so forth.

So, technically, there are techniques that people, in terms of professional training, have made and then they create whole institutions around that. And they, themselves, are not able to get into another technique without seriously disrupting the design of the project.

Love is not enough.

WOLFGANG: Are there any other comments on this? I think we ought to hold a separate colloquium on this issue.

EWING: I would just like to acknowledge, because I forgot to earlier, that one of the sources of assistance we had, in putting this together was the Center for the Study of Crime and Delinquency. Saleem Shah couldn't be here, but I wanted to acknowledge his help in setting up this meeting.

GREGG: I'd like to just thank everyone--the Chairman for leading us through this day and those who put this group together.

It's been very stimulating.

WOLFGANG: I would like to thank LEAA and the Institute and all the people who have helped in this colloquium. I would like to thank you all for what I consider to be a very stimulating and gratifying experience.

END