National Institute on Drug Abuse

Research

16

MONOGRAPH SERIES

The Epidemiology of Heroin and Other Narcotics

U.S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
Public Health Service Alcohol, Drug Abuse, and Mental Health Administration

The Epidemiology of Heroin and Other Narcotics

Editor:

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NIDA Research Monograph 16

November 1977

DEPARTMENT-OF HEALTH, EDUCATION, AND WELFARE
Public Health Service
Alcohol, Drug Abuse, and Mental Health Administration

National Institute on Drug Abuse
Division of Research
5600 Fishers Lane
Rockville, Maryland 20657

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, D.C. 20402 (Paper Cover)
Stock No. 017-024-00690-4

The NIDA Research Monograph series is prepared by the Division of Research of the National Institute on Drug Abuse. Its primary objective is to provide critical reviews of research problem areas and techniques, the content of state-of-the-art conferences, integrative research reviews and significant original research. Its dual publication emphasis is rapid and targeted dissemination to the scientific and professional community.

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The Epidemiology of Heroin and Other Narcotics

ACKNOWLEDGMENT

The Division of Research expresses its thanks to the staff of the Stanford Research Institute for its implementation of the Task Force sessions and for its assistance in preparing resulting papers for monograph publication. These activities were conducted under NIDA contract 271-75-1140, work order 07.

Library of Congress catalog number 77-88154.

DHEW publication number (ADM)78-559
Printed 1977

NIDA Research Monographs are indexed in the *Index Medicus.* They are selectively included in the coverage of *BioSciences Information Service, chemical Abstracts, Psychological Abstracts,* and *Psychopharmacology Abstracts.*

FOREWORD

This monograph reports the work of the Task Force on the Epidemiology of Heroin and Other Narcotics.

Through this volume we at the National Institute on Drug Abuse are expressing our continued commitment of the understanding of heroin epidemiology. Indeed, since the Task Force first convened nearly 2 years ago, a number of new initiatives have been undertaken by the Institute staff. Among a host of others, particularly promising are the research and development of measures of hidden prevalence of heroin; beginning studies of epidemiology among minorities, and development of a method for estimating heroin use prevalence through summary statistical indicators in 34 Standard Metropolitan Statistical Areas (SMSAS).

This document represents a quantum advance in the Institute's heroin epidemiology efforts and reinforces the Task Force's prediction that we will experience another decade of significant breakthroughs in this vital area.

Robert L. DuPont, M.D.
Director
National Institute on Drug Abuse

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Executive Summary

In response to its mission to advance knowledge and understanding of the drug abuse phenomenon, the NationaI Institute on Drug Abuse (NIDA) sponsored a task force meeting on the epidemiology of heroin and other narcotics.

The charge to the task force members was to examine the state of the art of measurement of heroin-narcotic use and to recommend improved research technologies. Invited contributors were asked to assist in meeting four specific purposes: (a) to discuss the state of the art of heroin epidemiology; (b) to identify the gaps in knowledge; (c) to suggest to the NIDA how such gaps might be addressed through research; and (d) to identify any apparent policy implications.

The Conference Planning Group established a four-part taxonomy to be addressed by contributed papers within the established Terms of Reference: the spectrum of use and its diversity (Robins); the methodologies for measurement of use (Hunt and others); the contributions of treated prevalence to epidemiology (Sells); and consequences of use (Lukoff). Each of the major papers received critique and commentary; the discussion is summarized in this volume; and a synthesis of the session was attempted (Rittenhouse and Cisin).

Spectrum of Use

Major Paper

Robins' paper on "Estimating Addiction Rates and Locating Target Populations" identifies the problems of definition, of measurement, and of interpretation that attend the attempt to estimate the number of heroin users. Robins suggests that a single definition has limited application, and the solution may be to develop an array of alternative definitions fo reflect the dispersion of experience. There are,

for example, registered clients with methadone-maintenance programs legally addicted to a narcotic; persons who use heroin on weekends only; users who interrupt their use from time to time.

Robins has attempted, from this diverse population of users, to predict which groups among them are likely to become addicts and, therefore, should be special targets for intervention.

By decomposing the addiction process into a set of stages, it is possible to detect (a) early predictors of addiction, (b) the predictors of the transitions, (c) the proportion making the transitions from one state to the next, and (d) the time intervals that generally elapse between entering one stage and moving to the next.

Critique

In his critique, Room questions this strategy. He suggests an alternative in which the dependent variable — addiction — is decomposed and the predictors of its various elements examined separately. Room feels that this disaggregating is desirable because the epidemiologist has responsibility for exploring not only the predictors and correlates of drug use, but also the nature of the dependent variable itself.

Estimates of User Populations for Heroin and Other Narcotics: Available Methodologies and Their Limitations

Major Paper

Hunt reviews heroin prevalence estimates derived from selected survey, treatment, and law enforcement sources. Hunt then applies "correctives" to selected estimates; for the years 1968 to 1974, he multiplies Greenwood's recapture estimates by a factor of 6 and general population survey estimates by 4, and thereby arrives at a "ball park" number. His resulting estimate for active heroin users is of considerably higher magnitude than has been found by other methodologies, Though admitting he has presented a "rough" estimate, Hunt believes that the prevalence figure is in the several millions rather than the generally accepted hundreds of thousands.

Critique

Gould finds that Hunt's prevalence estimates are striking because (a) they are larger by several factors than estimates made using other methods and (b) they reflect a 300% increase in heroin use from 1968 to 1974 and, therefore, are at odds with other trend figures reporting decreasing use. Gould also questions the source of the national survey data used by Hunt for the period 1968-1974 because national surveys were not conducted during most of these years. He states that it seems unreasonable to devote further effort to developing derived indicators of incidence or prevalence of heroin use because

> the assumptions on which these indicators rest are so numerous and untestable that the validity of their conclusions must always remain in doubt unless they can be corroborated with actual population-wide studies. With population studies, however, derived indicators become superfluous.

Gould concludes that Hunt's "figures are probably too inaccurate to be used for purposes of planning future drug policy."

O'Donnell comments:

> Hunt's estimate is based on two independent procedures that produce roughly identical results. But the first procedure is based on dubious data, the Greenwood estimates, and on two multipliers, each of which rests on inadequate samples, The-second procedure is based on survey data, but the multiplier is based on only one set of data, on a discrepancy that is not established to exist and which, if it does exist, would suggest a smaller multiplier.

If a multiplier is applied to these data, O'Donnell urges that it be in the form of ranges; he finds values of 1.5 and 3.0 reasonable as upper and lower limits. The resultant population ranges for heroin use (815,625 to 13,050,000) are wider than those produced by surveys even for rare events.

Commentary — Surveys

In separate papers, Johnston and O'Donnell both content that the survey method - despite its limitations — is one of the better available alternatives for the systematic collection of data. Although Hunt was interested in surveys as a source of the *number* of heroin users and changes in the number over time, Johnston values surveys as providing indices that reflect relative levels and changes in narcotic use in the nation. To develop survey data with sufficient numbers of users of heroin to yield stable estimates, however, will require either larger samples than usual, or samples in which high-risk subgroups are overrepresented.

Commentary — Econometric Methodologies

Goldman asserts that public concern with the consumption of heroin and other narcotics is primarily due to its presumed association with criminal activity. It is generally believed that addicts almost invariably commit crimes to support their habit. Goldman asserts: "We have yet to adequately test or even formulate the hypotheses that would let us judge" that assumption. Studies report between 41% to 66% of various populations had legal sources for funds; i.e., they were employed immediately before their arrest, incarceration, or treatment. Goldman suggests that further investigations are needed to determine more precisely the relationship of drug use and crime. In the absence of such information, Federal attempts to reduce demand through supply-reduction programs cannot have predictable outcomes.

Silverman asserts that the relationship between the number of heroin users and public policy toward heroin is complicated for a variety of reasons, the most relevant of which are: (a) many factors in addition to the number of users influence the social cost of heroin use, and (b) alternative policies would affect the number of heroin users and the social cost associated with use in ways difficult to predict. He reports a study in Detroit in which an econometric model predicting complex relationships among change in price of heroin, level of income-producing crime, and elasticity of demand was tested. In Detroit, increases in heroin price were associated with increases of income-producing crime in poor neighborhoods. Demand for heroin, however, was found to be comparatively inelastic, thus limiting the utility of price as an indicator of heroin prevalence. Though of unknown relevance to heroin use, demand, and addiction, continued empirical work may provide the basis for measuring the effect of alternative treatment; education, and law enforcement policies.

Treated Prevalence

Major Paper

Sells reviews data from a treated subset from the Drug Abuse Reporting Program (DARP). He emphasizes the limitations of generalizing prevalence estimates from treatment samples for estimating addiction rates in the general population, but they can enrich understanding of the phenomenon. Sells addresses questions in three areas as follows:

- Differential patient characteristics and types of social environments are associated with variations in drug use patterns that discriminate narcotic addiction from polydrug and nonopiate use and that provide insight concerning transition between stages of drug use.
- Transition between stages — initial use, continued use, and dependence/addiction — and patterns of drug use. Many drug users experiment and discontinue without becoming heavy users or addicts.
- The effectiveness of treatment as an exit or transitional path and the implications for Federal policy. Sells states:
  - — During the time that patients remain in treatment programs, the use of opioid and nonopioid drugs have been effectively controlled in almost all types of patients, and there is reduction in drug associated crimes. Methadone maintenance has demonstrated the greatest effectiveness in these respects. Despite variations in effectiveness among programs, the overall results suggest that treatment is a viable, and probably less costly, alternative to such forms of social control as incarceration.
  - — Rehabilitation efforts are as important as reduction of drug use and crime control.
  - — Many treatment programs have inadequate or no provision for employment counseling, training, placement and similar activities.
  - — The social context is significant in initial and continued drug use and should be included in the assessment of treatment outcomes.

Critique

In his comment on Sells' paper, Smart raises questions about the epidemiological inferences that can be derived from treatment data alone. He indicates that not much is known about the total (identified and unidentified) population of heroin users or addicts. He characterizes a treatment population as a special subgroup of the total population that probably is less employed, less socially stable, and has fewer resources than untreated drug users.

Given such biases, it is important to know Sells' methodology in assessing differences among addict groups; how many variables were examined, and which have unique explanatory power. He adds that the Texas Christian University (TCU) models of the development of drug using behavior, and models generally, lack the explanatory power that longitudinal studies provide.

In terms of the efficacy of treatment, Smart identifies a number of validity problems: the lack of a control group of untreated patients, the difficulty of assigning clients to alternative treatments; and the lack of specificity in the analysis to determine the

most important elements in treatment. Smart concludes that although the TCU study is a large and valuable source of information, contributions from treatment data to general epidemiology are quite restricted.

Consequences of Use: Heroin and Other Narcotics

Major Paper

Lukoff reviews selected current epidemiological findings; suggests the impact of high levels of heroin use on a community; discusses life cycles, stages, and role typologies associated with narcotic use; and critiques the institutional matrix of methadone treatment programs.

Lukoff states that although heroin use in the general population may be rare and its effects trivial, concentrated use characteristic of ghetto and similar communities has serious consequences for affected areas. Lukoff states that the generally more talented members are "removed from the creative work of the community." Both human and material resources are diverted from community development to social control mechanisms, drug treatment programs, and efforts to deal with the attendant crime and social disorganization associated with areas of high drug use.

In his discussion of heroin related life cycles, stages, and role typologies, Lukoff describes several methodological approaches that have been used to study role typologies relative to involvement with heroin and other narcotics and notes their static quality. He proposes canonical analyses (a multiple-regression technique) as a method to accommodate the dynamic events in users' lives.

Lukoff points to another consequence of heroin use — the organizational and institutional one; i.e., the growth of a complex and expensive private and public industry to deal with narcotics. Methadone maintenance treatment, as a modality, has been more responsive, he believes, to political than empirical imperatives.

Critique

McGlothlin emphasizes the significance of the coercive environment in which addicts live in our country. He asserts that it is not clear how much of the problem of high minority drug use is due to heroin use per se and how much to the impact of social policies.

He also contests Lukoffs view of methadone maintenance. Preliminary findings from his own study suggest reduced daily use, reduced criminal behavior, and somewhat higher employment associated with methadone maintenance.

Winick supports Lukoff's contention that there are wide variations in levels of opiate use in different subareas or communities. He suggests these variations need to be viewed as a series of separate social problems shaped by local situational forces.

Winick states that the Dole—Nyswander methadone maintenance treatment model criticized by Lukoff is no longer used even by its creators. However, it is true that an unanticipated consequence of this treatment program has been the availability of street or illegal methadone: this availability clearly has significant community impact on some areas of New York City.

Winick concludes by asking that a major effort be launched to order and publish the massive literature on the epidemiology of chronic opiate use that has accumulated over the past ten years. Such an effort would be a tremendous boon to the field in its furtherance of understanding of opiate use.

Reflections

Rittenhouse and Cisin state that the papers in this volume correctly reflect the state of the art of the epidemiology of heroin use; that is, they are an admixture of fact and opinion, of diverse viewpoints and approaches. The contributors' scientific inquiry is not limited to the count or number; they seek to understand developmental processes of drug use and its antecedents and consequent correlates.

Winick's "quantum leap" in knowledge of opiate use is reflected in the Stanford papers.

The salient features of the session were its strong call for definition, its effective critique of available methodologies, and its creative identification of new research issues. The session suggested this axiom: *Measurement, which is the starting point of epidemiologic research, requires exquisite clarity of definition, differentiation, and precision.*

Essentially, three estimating techniques are being applied to heroin epidemiology: (a) special and general population sample surveys,(b) recapture, and(c) treated prevalence extrapolated to total prevalence. Each of these three theoretically sound techniques has practical problems of implementation. The task force session represents an important step forward in acknowledging difficulties across techniques and proposing solutions to the problem of estimating such rare events as heroin use.

The papers reveal the complexity of the heroin phenomenon. The term "heroin user" applies to experiences both reversible and inconstant. Increasing evidence has shown that self-reported mild use without social and health problems is not uncommon. There is movement in and out of use and there is considerable variability in patterns of use. The social context has strong influence on the extent and patterns of use. In nonclinical populations, spontaneous remission is not unusual; in clinical populations, relapse after treatment to addictive use is common. These contrasts in heroin experience between general and clinical subpopulations are so diverse as to suggest that they may be experiencing quite different phenomena.

Future research efforts to study drug use behaviors in various environments should include longitudinal studies. A series of well-designed cross-sectional studies to study subpopulations, each at different and identifiable stages of development, would shorten the time lag between study implementation and findings.

In summary, it may be that the many constructive contributions of the task force members reflected in this volume may lead to another quantum leap in our knowledge of opiate use in the next ten years.

Overview

INTRODUCTION

BRIEF TERMS OF REFERENCE

SELECTED THEMES OF THE DISCUSSION

Introduction

JOAN DUNNE RITTENHOUSE, Ph.D.
Task Force Chair

The National Institute on Drug Abuse is deeply committed to its responsibility of reporting to the Congress, the Executive, and the people concerning the nature and extent of drug abuse. In the relatively few years of its existence, the Institute has made impressive progress in expanding through its grants and contracts research program our common pool of knowledge about the drug abuse phenomenon.

Under the assault of evidence from Institute funded research, long held beliefs and myths on the extent of drug abuse have given way. During the late sixties and early seventies, surveys of the general and special populations have provided new understandings of drug use and experience. During this same period, monitors of drug related behaviors in the identified populations — often called captive or casualty-populations — were expanded and made more systematic. This Task Force session represents part of the Institute's continuing sponsorship of these efforts, with particular emphasis on what is at once the most elusive and most visible drug group: heroin and the narcotics.

The original purpose of the Task Force, conceptualized as examining the epidemiology of heroin use, was broadened, chiefly in response to currently available empirical findings, to include investigating the epidemiology of other narcotics. It is now accepted by most scientists in the field that heroin addicts also use many drugs that are not physiological substitutes for heroin. This suggests that it is not possible to understand the phenomenon of heroin use without understanding the context in which it is most often embedded — namely, involvement with all kinds of drugs. Surveys and other studies show that people using one drug are at higher risk of using other drugs or chemical substances. Of these, one of the most salient is alcohol, which most investigators now include conceptually in studies of the epidemiology of drug use.

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Given this interrelationship, the reasons for abstracting heroin and other narcotics from the larger drug matrix may not be immediately obvious. Because we do not suppose that the epidemiology of heroin and other narcotics encompasses the epidemiology of all drug use, we have purposively abstracted this limited aspect. It is precisely in this limited arena where we find our measurement capabilities least powerful. As the Task Force Papers demonstrate, the available methodologies falter in the face of the statistically rare events of heroin and narcotic experiences; they fail even more dramatically in that very rare phenomenon of addiction. The Task Force members have been identified as scientists most capable of examining, as it were under a microscope, the delimited area of heroin-narcotic use to analyze the state of the art of measurement and to recommend improved research technologies.

Specifically, contributors were asked to (a) discuss the state of the art of heroin epidemiology; (b) identify the gaps in knowledge; (c) suggest to the NIDA how such gaps might be addressed through research; and (d) identify any apparent policy implications.

Originally, it was our intent to include in the body of this report some record of the valuable and stimulating discussion provoked by the papers. Technical and other difficulties made this impossible. We hope that the inclusion of some limited number of verbatim quotes will suggest the flavor of the interaction. For an exploration of the Great Debates in methodology and inference, the reader should turn to the papers. themselves.

Thanks are due to many for making this Task Force session possible. Most particularly, we are much in the debt of the Planning Group, members of which are identified elsewhere in this report. Among their many formative contributions, the Group succeeded in generating, through its Chair, the Brief Terms of Reference, included in this report. We direct the reader's attention to this paper, which was distributed to contributors as part of their commission. We believe it made possible the conceptual organization and definitional consistency across most papers, resulting in a wholeness often not achieved in such undertakings. General quality, relevance, and empirical and interpretive judgments remain the responsibility of the individual authors, to whom questions and comments should be addressed.

Planning Group
August 1975
July 1976

CHAIR: Dr. Lee Robins, Washington University

Dr. Ira Cisin, George Washington University
Dr. Gail Crawford, University of Chicago
Mr. Leon Hunt, Alexandria, Virginia
Dr. Lloyd Johnston, University of Michigan
Dr. Irving Lukoff, Columbia University
Dr. William McGlothlin, University of California at Los Angeles
Mr. Robin Room, University of California at Berkeley
Dr. Saul Sells, Texas Christian University

STAFF:

Dr. Joan Dunne Rittenhouse, National Institute on Drug Abuse
Ms: Mae Stephen, Stanford Research Institute

Brief Terms of Reference

LEE N. ROBINS, Ph.D.
Chair, Conference Planning Committee

The goal of the meeting on epidemiology of heroin and other narcotics is to provide the NIDA with several things relevant to the problem area: (a) a survey of what experienced people consider to be the state of current knowledge, (b) a list of gaps in our knowledge, (c) suggestions about how such gaps might be filled, and finally (d) advice as to the policy potential of presently available knowledge and that likely to be garnered in the near future.

Much of the information requested by those charged with policy formulation about drugs is epidemiological in nature. Legislators, for example, often want to know how many heroin addicts there are. They want a number that will guide them in deciding, how much of the tax dollar should be directed at reducing or preventing addiction, and that will go down when programs are effective and remain stationary or rise when they are not.

From the science side, epidemiologists know that providing "the" number of heroin addicts in the United States is fraught with problems. A heroin addict gets into a methadone program and so is legally addicted to a different narcotic. Should he be in the count? What if he shifts to illegal methadone? For how long should an addict continue to be counted after he quits daily use? Need he quit entirely, or should he also be excluded if he drops to weekend use? Should men be counted just because they administer heroin daily even if the quality of the drug they are getting is so poor that they have no withdrawal symptoms when they stop? The generic nature of many of these questions causes significant difficulties. The lack of distinction in the popular press between users and addicts, and between users of heroin and users of other narcotics, is well known to epidemiologists. The general public universally assumes that there is virtually no migration out of addict status, at least not without heroic treatment efforts. Yet codeine users much less often become

addicted, and we have increasing evidence that many heroin users never become addicted, and that many of those who do become addicted recover without medical intervention.

Because of these and other definitional ambiguities and because of preconceptions about the addict career, any number of epidemiologists might suggest as "the" figure representing addicts in the United States is at risk of misinterpretation. But this danger does not relieve the NIDA of an obligation to provide such numbers. Without them, budgeting is without basis and program evaluation cannot exist.

For NIDA's internal needs, a single figure representing the number of narcotic addicts is not at all sufficient. NIDA is interested in having the estimates necessary to make plans applicable to populations with very different histories of drug involvement. When planning drug use prevention programs, it is important to know, at the minimum: (a) risk of any narcotics use in the general population, and (b) greatest risk of narcotic use by demographic subgroup. But for purposes of planning intervention programs, predictors of any use are of little or no value. Decisions have to be made about which groups of users who happen to come to public attention are likely either to initiate others into narcotics or to have problems of their own with their narcotic use. In planning aftercare services, predictors neither of use nor of progression to addiction are useful. What is needed for that group are demographic and personality correlates of relapse among addicts.

NIDA, in short, needs estimates of the likelihood of transitions from one stage to the next, covering all the steps between never having tried a narcotic to relapsing to readdiction. NIDA also needs predictors of each transition to greater or less involvement with narcotics for the population that has reached the last prior stage at which policy options can be exercised. In addition, it would be very useful to NIDA to have estimates of the length of time normally elapsing between entering one stage and moving to the next. Such information would inform the government about how soon to expect to see the effects of a new policy, whether it is intended to prevent new users' progression to addiction or to prevent relapse among treated addicts. Finally, NIDA needs to know to what extent the transitions about which we have knowledge involve heroin as compared to other narcotic drugs.

As epidemiologists, we know that such concrete goals are difficult to achieve. There is no agreement, for instance, on how to define narcotic addicts. Are they daily users? Are they people who say they need the drug? Are they those with positive urines? Are they those who show a specific set of involuntary physiologic responses when the drug is withdrawn? We do not know whether there is sufficient overlap, between "addicts" defined in these varying ways to allow us to combine data from studies in which addict samples were chosen by differing criteria.

Further, to produce any prevalence figures for use or addiction, we must use some measure of "a given moment in time." Is it 1 day, 1 month, 1 year? And how much error do we introduce by combining studies done at different historic moments, when we know that narcotics supplies have fluctuated?

Because it is expensive to ascertain population rates of rare events like heroin use and addiction through surveys, there have been and will continue to be attempts to extrapolate to the general population on the basis of indicators available from records accumulated by drug treatment programs, by emergency rooms, and on death certificates. To know how to weight these indicators to get the best possible estimates for

the general population, NIDA needs to know what those rare general surveys that have been done tell us about the chances that people at each stage of involvement with narcotics will appear in these records and how often (to allow taking the probability of duplications within a given source into account), and the extent to which appearance in one record is correlated with appearance in another (to allow taking duplications across indicators into account).

In your reviews of the literature on the epidemiology of heroin and other narcotics, we would hope that you will keep these needs in mind. Wherever you can, please specify:

1. How the population is defined in terms of
   a. what narcotics are covered
   b. how addiction is defined
   c. the demographic characteristics, including location as well as the usual age, sex, race, socioeconomic status
   d. the dates between which data were collected.

2. To what levels of prior drug involvement the results apply (e.g., everyone, those who volunteered for treatment, those who have used narcotics at least once, those who have used regularly, those addicted).

3. The length of the interval and the variability of the interval between entering into one level of drug use and transition into the next (e.g., between first use and addiction, between release from treatment and relapse, between addiction and spontaneous remission).

4. The reported appearance of surveyed users in various public records and, where available, the number of times and the patterns of appearance.

We all know that most studies will not provide all the information you need to describe their findings along all these dimensions. Just be as complete as the data allow and specify what is missing. At least then, we can provide NIDA with a clear idea of what is known and what still needs to be done. If we all proceed in this way, the volume our conference papers produce will have a common framework and a rare coherence. We think it will be useful to epidemiologists and policymakers alike.

Many thanks for agreeing to undertake this necessary and important assessment of our current knowledge about heroin addiction. We look forward to a most rewarding and exciting conference and a landmark publication as its product.

Selected Themes of the Discussion

JOAN DUNNE RITTENHOUSE, Ph.D.

Models of Epidemiology

The concept of epidemiology as it applies to drug abuse varies widely from one special part of the health field to another-even from one investigator to another. A fairly traditional notion shared by many is that the appropriate model for the epidemiology of drug use is that of the acute communicable disease; actually the strategies of the chronic disease model might indeed be more appropriate to the phenomena of drug abuse.

Strategies derived from chronic models imply that the proper business of epidemiology is understanding that the etiology of drug use is of more social significance than fine estimates, and that a taxonomy of consequences of drug use is of more value than unduplicated counts of users. What does it mean, for example, to be on heroin? What does it mean for the family of a heroin user? What does it mean for them when he or she is at home? When his or her whereabouts are unknown? What are the systematic factors affecting prevalence? How does desirability and "image" of a particular substance affect use at a particular point in time? An appreciation of the heterogeneity of the user population(s) is just now being developed from emerging findings of current work; such an understanding may have more permanent long term policy implications than any prevalence figure, regardless how refined.

Acute models have, according to some critics, distorted the field by a preoccupation with counting cases, resulting in a working premise that numbers are a sufficient basis for understanding the problem. There is growing agreement among involved scientists that models built on such premises are seriously deficient. Unwitting support for their persistence comes from the public and from the policymakers who both represent and lead public perceptions. Scientific response in the direction of

improving estimates, or providing "better numbers," reinforces such priorities and drains attention, energy, and research resources from activities that would provide broader understanding and add to new knowledge.

Considerable discussion among the participants focused on scientific responsibility in this matter. On the one hand, scientists were urged to reflect on the purposes to which their data will be put, on the true rationale for epidemiological activities in drug abuse, and on the alternative policy outcomes of any implications of the findings. Investigators might often ask themselves, "Heroin epidemiology for what?" with considerable profit.

On the other hand, participants felt that scientists should appreciate the limits of impact of any numbers that they produce. There are many areas of social significance on which data are regularly collected-unemployment and school performance, among others-where impact in any direction is difficult to detect. The quest for a better count or a better measure is a scientific responsibility independent of the utilization of such measures. An important part of the scinetific activity is the successive refinement of the question.

It was not suggested that efforts to refine estimates should be abandoned or that attempts be launched to extinguish questions requiring enumeration, but that such mandated counts be undertaken in a richer matrix of inquiry. In fact, it was agreed that counting efforts would, and should, continue for several reasons. First, in current social science it is almost impossible to propose achieving an understanding of any problem-along such dimensions as etiology, affected populations, and consequences-without reference to some index of extent of problem. Secondly, while it may be possible to express the desired data as some ratio, such as that between treated prevalence and untreated prevalence, it is difficult to understand how to establish that ratio without two reliable numbers.

One participant restated the consensus on the place of numbers in the understanding of the problem as follows:

> I don't think any of us here are seriously proposing that it is not useful to know the magnitude of various things we are dealing with. I think the issues are much more around what issue, what things, is it that we want to know about, and don't we want to know a great deal more diversity about things than have been rearrested of us? And don't we want to indicate the inexactness of what we are doing more in terms of talking about ranges rather than arriving at a single figure? In fact, in terms of exactness, what we need is only orders of magnitude. And I think that the fundamental problem coming out in this discussion is that, in a sense, we care too much about policy rather than we care too little. If you don't care about the policy then you can manufacture a number on any day of the week. I get a sense that a lot of people in this room feel that there is a great gulf there between what the politicians are accurately reflecting as the understanding of the American people about what is going on with respect to heroin, and the kind of secret knowledge that there is in this room. We may be on the brink of a situation in which some of the news will get out. But that until the news gets out, and I'm really serious about that, until the news gets out, there is a real feeling among the people who have this arcane knowledge that the wrong questions are being asked of them, for very good reasons that date back to the understandings, the governing images, that the American people have about opiates.
>
> Alternative conceptualizations may be fruitful. The notion of prevalence, for example, may be disaggregated into subsets. In the old epidemiological terminology, "treated prevalence" referred to the kinds of data with which DARP, DAPRU and related systems deal. Beyond such treatment generated data, there are a number of other definitions of "treated." We have deaths, which is a kind of treated prevalence, we have voluntary entry into treatment, which is a kind of treated prevalence, we have involuntary entry into treatment-you go get treated or go to jail-that's another kind of prevalence.
>
> Untreated prevalence is also a construct of distinct subsets. Indeed it is in this body of studies in very recent years that we have begun to learn about the phenomenon in the general population. One insight from this experience is the conclusion that generalizations across clinical-nonclinical populations are rarely defensible; we seem to have established two rather distinct

phenomena. We have something called the lifetime experience, the "ever use" concept, which some of us at least have found quite useful in terms of defining that portion of the population that has had some experience with various kinds of drugs. These are untreated. There are current users who are untreated but whose habits are as big as those who are in treatment. There are some current users who technically will never need treatment, and current users who are at a lower level than the threshold for treatment. It is possible that with respect to heroin, the only untreated prevalence figure of significant meaning is the number of users who shoot up regularly. There appear to be some benefits to defining the problem classically, i.e., as a relationship between treated prevalence and untreated prevalence. Primarily such an approach would inform empirical investigations so that better estimates would be developed over the long term. Specifically, such studies would attack the question, "What is the relationship between the treated prevalence and the larger but less known phenomenon of the untreated prevalence?"

And, in the emphasis on prevalence, it is important that incidence not be neglected. Some monitor on who is entering the list of users enhances the predictive implications of epidemiological findings.

The Methodologies

Survey Methodologies

Surveys on drug taking behavior are currently in process on a community, regional, and national level. As research experience in this kind of measurement has accrued, questions have been raised about the value of a national survey. It is by now accepted that variability in drug related behavior from one community to another is very great. In national studies these variations are masked or smoothed out. To preserve the variability in a national study, an enormous number of cases is required. But it is clear that to construct a national policy national data are required. Can there be a unitary national policy when within the national sample there are diverse populations?

Clarity of purpose is essential for a national survey, given not only the diversity problem, but also the fact that a national survey is an enormously expensive undertaking. Overly restricting the goal to one question, for example, the number of heroin users, would be wasteful. The resulting reality is that national survey design emerges from a number of compromises between requirements of the competing questions.

When the best possible estimate of the number of heroin users in this country is the goal, then the investigator would probably stratify according to his best guess as to where most of them are. However, if there is interest in, for instance, the characteristics of heroin users in places where there isn't a lot of heroin use, who are, so to speak, fish out of water, then it may be undesirable to undersample in low use areas. If you want to ask about use of other drugs which are not distributed geographically in the same way as heroin, then again you may not want to stratify only according to geographical patterns of heroin use. In the case of a survey a group of us here were involved in, there was a long series of discussions precisely around the question, "On what basis should we stratify?" and really what it came down to was a series of compromises to make the survey speak to the various issues it addressed. There is some limit to the number of purposes which can be imposed on the same study.

This puts the burden for the final framing of the survey research question in the hands of the principal investigator, whether this be the project officer in contract research or the academic scientist in a grant funded effort. While in an ultimate fashion the policymakers frame the questions, scientific expertise is required to conceptualize and to define the problem and to decide what aspects of it (a) can be answered, given the available methodology, (b) should be explored, given the nature of the phenomenon, (c) should have priority, given proximal implications for social policy. With regard to the third point, it is possible that in the phenomenon of heroin use, motivational questions should take precedence over estimate questions.

The limitation to the investigator's role is that it is neither wise nor justifiable to refuse response to questions which policymakers give high priority. If the capability for estimates exists, estimates will be provided, admittedly with the requisite qualifiers, such as are now becoming well accepted with respect to heroin use.

> (1) We know heroin use is a relatively rare phenomenon in this country. (2) We know that only a small fraction of people who ever use become addicted or regular users. (3) We know that intermittent use is extensive, that people chip for long periods of time and never get into trouble. (4) We know that those people who become addicted are not permanently so, at least not all of them, perhaps a significant fraction. (5) We know that the risk of use is highly situational. All these, among other empirically based assertions, limit the impact of any estimate of "users;" in fact, the category is exposed as a collectivity of a number of subgroups and the number is shown to be an artifact.

The question is whether all estimates based on national surveys are artifacts or whether they have a defensible value. Some would argue that they be abandoned in favor of local or other special studies. It is possible, for instance, to identify areas where prevalence of opiate use is quite high. If survey methodology can provide prevalence estimates for such special areas where heroin use and other phenomena of interest "clump" together, should not the methodology be applied to these circumscribed areas?

Cautions need to be observed in interpreting surveys in such areas; an intimate knowledge of the community and its characteristics is required. Since one of the reasons for studying such an area is its deviance from expected national norms, the limitations of its meaning from a national point of view has to be made clear. The following description of a community survey (Lukoff & Brook, 1974) provided by the principal investigator at the session serves as a case study in interpreting generalizability of observed race differences in "drug" use.

> There were special characteristics of the whites in our sample, who were living in a predominantly black community. We found that almost none of them were child-bearing families of any kind; they were mostly young couples moving into the brownstones, pushing the blacks out because of the cheap prices they were getting there. They were very different demographically, socially, and every other way both from the black sample and from the whites generally and precisely in important characteristics for drug use like age groups: they were young, adult professionals who smoke pot and take pills. I don't think this sample tells us very much about anything but a peculiar group.
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> If you look at the breakdown of the particular drug patterns, you'll also notice that the whites are heavily invested, as in most other studies, in the pills-amphetamines, barbiturates, and psychedelics-much more so than the blacks, but this comes out in other studies . . . . You could argue, I think plausibly, that the blacks in the area are something like blacks in other urban ghetto communities in a lot of characteristics, although we won't generalize any numbers on that basis, but you certainly wouldn't want to use the whites in our sample as a vehicle for talking about black/white trends.

A complementary research program of national and specialized surveys is needed. The generalized population study will give a better perspective as to where a specialized study fits. Reliance on an amalgam of specialized studies provides no overall national perspective, no framework of more generalunderstanding against which community studies can be interpreted.

> There are in fact things that national surveys can do, and there are things that community surveys can do, and there are things other methods can do. They aren't substitutable for one another and shouldn't be so regarded. In particular, a lot of stress in this discussion has been put on local variations. We've been tending to interpret that local variation chorographically: Phoenix is different from New Orleans. Now what is the response if someone asks, "'Where do you go from there; you've got all these differences from one town to another, what kind of sense do you make of it?" Perhaps one would try to look at it nomographically: to look at the characteristics of the different places or of the people in them and analyze in relation

to those characteristics. If there is a national sample, then such an analysis can be done without a new collection of data or mere speculation about what the chorographic differences might mean, because that nomographic capability is built right into a nationwide survey. It may be that there are characteristics in common between middle-class suburbanites and their drug use, for instance, that existed across the country, or maybe are confined to one half of the country versus the other half of the country, and that these regularities are worth finding out.

A lot of drug policies, for instance customs policies, are set on a national level rather than at a local level, and a national sample is an appropriate tool for speaking to national policy issues. On the other hand, a lot of things relevant to heroin use go on at the local level, and there is certainly a point also in doing community studies which are sensitive to community factors, or state studies to ask questions of interest to the state. So these methodologies and these sampling practices should not be set up as being in opposition to one another. They have to be seen as complementary.

If people want to do a national survey by setting up a lot of regional surveys and adding them together, then that's perfectly acceptable, as long as you can ensure that there is going to be comparability in the methods and in the questionnaire across the samples. But that is very difficult to maintain. For example, with the O'Donnell study, there was an initial idea that one investigator would do the Western half of the country and the other would do the Eastern half of the country. But that was immediately abandoned because it was seen that very quickly a divergence in methods, divergence in questions, would develop. With these divergences, there wouldn't be any way to add the data together into a national survey. If you are proposing to go by the route of adding regional or local surveys, then, unless you have an ironclad hierarchy of control from above (which does away with the advantages of doing a local survey) nothing will "add up."

Nonsurvey Methodologies

Questions about the variability of the phenomenon of heroin use and addiction from one part of the country to another and about the robustness of the survey methodology in the measurement of rare events suggested that alternative methodologies be considered. The latter question is considered by a number of commentators to be most serious. It has been asserted that the estimator has a Poisson distribution. The absence of theory connecting cluster sampling error with Poisson distributed random variables raises fundamental questions about validity of prevalence estimates for a rare event such as heroin use. In a large national cluster sample, is the requisite statistical precision possible? Since nonresponse and nonsampling error further distort reported rates, some discussants proposed reliance on alternative measurement technologies to estimate heroin use.

The difficulty of identifying alternatives with not only less, but measurable, error, variance, and biases was considered. The addition of two or three populations and the subtraction of the intersection was proposed. It is possible, for example, to take treatment data from discreet populations-which have a tremendous variability-and, by adding data together from enough discontinuous, separate facilities, to develop a smooth, unimodal distribution. It is clear that such a distribution may become the basis of an erroneous interpretation; it may obscure underlying realities and produce a specious uniformity.

Nonsurvey methodologies, based on measures presumably related to heroin, seem plagued with at least as much variability as surveys. In drug related deaths ascribed to heroin, great fluctuation was observed across two locations: 18% in New York and 47% in Los Angeles. No theory provides an interpretive guideline for such results; in survey-produced data, fluctuations from one locality to another are less extreme, and theory is available to guide the interpretations.

Many of these alternative measures, often called indicators, have a local or maybe regional significance. Combining diverse local measures into a "national" indicator

may not be justifiable, since the operational procedures used are often not standardized. Beyond this, the relationship between the measured indicator and the presumed underlying phenomenon, heroin use, may differ greatly from one place of measurement to another.

An example of the latter case seemed to be the indicator STRIDE (the DEA developed price/purity ratio), which behaved in the predicted way in only one of three regions. Under such conditions, interpretation is difficult, and the notion that there is a unitary or national indicator called STRIDE is difficult to demonstrate.

The utility of hepatitis as an indicator of incidence was discussed. Current medical understanding is that there is permanent immunity following one episode of hepatitis Type B (i.e., serum). This belief, together with certain interview data from patients, has formed the basis of the interpretation that a reported case number occurs within first year of parenteral heroin use. If those assumptions are valid, an increase in reported cases of Type B hepatitis should be reflecting some increase in new cases (incidence) of parenteral heroin use.

> Above a certain threshold, the Center for Disease Control (CDC) considers hepatitis cases potentially drug cases. The CDC has maintained a consistent surveillance for all the communicable diseases since 1953. Reviewing the trends in the hepatitis data over time, we believe we have a view of trends in incidence of heroin use over this same period. Based on this belief, we see a large increase in heroin incidence through 1972 and then a sudden decrease of about 20%. The last few years have been fairly constant; actually, reporting of this serum hepatitis type has not shown any upturn since 1972. This suggests that the incidence of parenteral heroin use has been constant; the "epidemic" or rapid increase of new cases is over.
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> I think it is important that the participants know the difficulties in reaching this or any conclusion from hepatitis data. Briefly, there are diagnostic problems, and there are new questions about the assumptions of permanent immunity. The technology of diagnosis has been improving steadily. Older tests which were the basis of discriminating Types A and B were somewhat insensitive and unreliable, so we have to be conservative about conclusions on long term trends. New studies in California and elsewhere suggest there is another variation (non-A, non-B) which has never before been identified. We don't know if this finding is the result of better tests or a changing virus. Reports based on self report rather than diagnostic tests are likely to be conservative because an unknown proportion of cases are subclinical; such cases are picked up only accidentally if blood work for other difficulties identifies hepatitis B antibody in their serum. And of course. the inference that Type B hepatitis cases reflected new heroin cases was based on independent knowledge of drug availability. Serum hepatitis is associated with route of administration (parenteral) and not with a specific substance.

Another indicator that has been of interest and utility for some time has been the application of the capture-recapture strategy to law enforcement data on drug violations. Applying the tagging method developed for fishery and wildlife purposes to the identification of addicts, Dr. Greenwood has provided a series of estimates of the prevalence of heroin experience. The probability of being tagged (i.e., arrested) the first time is not relevant. The significant question is: what proportion of arrested heroin-involved persons have been previously tagged or arrested?

In the past, criticisms have been raised about the definitional consistency in the system; at various times, the "fish" have been referred to as *addicts,* abusers, and users. In response to the recent epidemiological findings on the spectrum of use, Greenwood has refined his operational definition by applying a fraction to the raw count. The fraction represents that portion of all users who are daily users, the nearest possible approximation to *addicts*. For 1975, Greenwood's totals were drawn from 100% of three cities' rap sheet reports for a given period of time. Applying the corrective fraction (.77), to the results and using the appropriate confidence intervals,

Dr. Greenwood estimated 546,000 daily opiate users, with the true measure ranging between 500,000 and 580,000.

Two scientific difficulties with the procedure were identified.

1. It is unlikely that the necessary assumption-that first tagging does not affect mortality-can be met. The probability of second arrest is affected by first arrest; it is not a random event. Some assumed that chances of a second arrest are greater; it is also possible to infer that first arrest is a learning experience that decreases the likelihood of a second arrest.

2. The law enforcement data base has many problems. There is no national standard for the collection of arrest data that is uniformly enforced. It is operationally, at least, a quasi-voluntary system. Under such conditions it is not possible to come to a conclusion about the quality of the prevalence estimates.

The Research and Policy Implications of Program Goals

Most experts in evaluation assert the importance of clear outcome goals as a condition for researchability. In attempting to apply evaluation procedures to drug treatment programs, researchers often find that this condition is not met. The practice of aggregating across discrete programs, in order to build the clinical sample and for other reasons, often forces the adoption of criteria that have the characteristics of global process rather than behavioral outcome measures. In studies comparing the efficacy of methadone maintenance treatment with "drug-free" and similar therapies, the conclusion is often drawn in favor of methadone maintenance.

The most important consideration here is the criterion of improvement employed. If the criterion requires a demonstration of significant improvement in such areas as family relations, in the majority of cases there is no demonstration of much improvement. Using remaining in treatment as a criterion results in a clear judgment of the efficacy of methadone maintenance. Given this result, many have questioned the 'wisdom of the new FDA rule requiring detoxification (i.e., withdrawal) from the maintenance dose. The defensibility of such a conclusion is contingent on the appropriateness of the criterion. Methadone maintenance and drug-free therapies have quite different goals. In methadone maintenance, indefinite maintenance can be considered an appropriate goal where termination of treatment is in itself undesirable. In drug-free therapies, attaining a drug-free state is the goal-detoxification during treatment is the interim goal and continued post treatment abstinence is the ultimate goal. These different goals reflect differences in the models used to define the drug abuse problem.

> The methadone program, irrespective of what the people in it thought they were doing, was essentially an end run around decriminalizing heroin. in a situation where it was inconceivable to people that overt decriminalization, given American values and American beliefs, could actually happen. Methadone maintenance is, of course, a very limited kind of decriminalization. How you have to interpret the enormous success of methadone programs programmatically was that it was the only conceivable modality that would deal with the issue of numbers. It is just about the first time in world history that someone seriously proposed to obliterate a condition involving human behavior by treating it out of existence. And to deal seriously with that issue of numbers, the numbers of heroin addicts that were assumed to be out there, you had to have something that was cheap, and it was also a way essentially of decriminalizing.

There are some experienced researchers in the field who have studied programs meeting the criterion of outcome goal specificity, and have been disappointed in the results.

On the other hand, some believe that treatment, variability really doesn't matter an awful lot. This seems to be the case in alcoholism research. I don't know of a good study that's been done on heroin addiction. But the quantity of treatment research that I'm aware of indicates to me that the person being treated brings his probability of recovery with him. There's very little affected by what goes on in treatment. In other words, the variability in outcome is attributable more to subject variability at intake or baseline than it is to any variation in treatment or goals.

On a broader social level, it is clear that goals as related to costs and policy trade-offs have not had sufficient study. It is possible, for example, that reaching the assumed goal-in this case getting people drug free-may be cost ineffective.

By way of illustration, there's a study of the economic costs of smoking in England where they did an estimate of what would happen if there was a 20% and a 40% drop in the smoking rate in England. In a welfare state where you have to support people on pensions and so forth, the government has a vested interest in everyone dying at 65, and in England the costs just about balanced out. If enough people gave up smoking, then what the country gained in production, if you could in fact put them to work, was lost by the added welfare costs for extra people after age 65.

There is also a very important distinction to be made between the costs at the individual level and the costs at the social level. Lastly, epidemiological study of the consequences of opiate use needs to pay attention to the variation in those consequences under the different social policies. We have to examine carefully, when we are talking about the issue of what kind of social policy people want to have with heroin, what are the ways in which the costs in fact alter under different kinds of social policy. I think that anyone who is seriously going to take the Szaszian position does have to take into account the kind of point that Dr. Lukoff brought out. Anyone, for instance, who wants a free availability of alcohol without a strong tax structure and a system of controls has to take 18th century England seriously. And in 18th century London what were undoubtedly liberal, not bluenose, people, the Fielding brothers—the author of *Tom Jones* was not a bluenose—who were magistrates in London, fought for long periods of time to establish controls over the sale of alcohol because they saw it as totally destroying the fabric of the society. So that I think that there are various half-way points between the situation we have now and the policy of providing a bucket of heroin at City Hall. Those half-way points have to be discussed by the polity as a whole, by the citizenry as a whole, in terms both of moral issues and in terms of the costs and benefits. To that discussion scientists can contribute data; we can choose to study certain questions and by that choice perhaps express our politics. But fundamentally the decision isn't going to be up to us.

The Spectrum of Use: Heroin and Other Narcotics

Estimating Addiction Rates and Locating Target Populations

How Decomposition into Stages Helps

LEE N. ROBINS, Ph.D.

Over the last six years the government has invested large sums of money in the study of illicit drugs, in efforts to treat those addicted to them, and in the prevention of addiction by educational and law enforcement efforts. It is not surprising that legislators who have written bills and appropriated the money for these purposes should want to know what kind of return they are getting on their investment. At the present time, we simply do not have the two critical figures that would give them this information: what the level of addiction is now, and what the level of addiction would have been without the government's investments in money and effort. The difference between those two figures would provide a measure of program effectiveness, Clearly it should be easier to estimate the current level than to guess what it might have been without government activities, but even estimating the current level of addiction presents serious problems.

It is not important that an estimate of the current'addiction level be precisely correct so long as that estimate fluctuates in phase with the correct number or can be made to do so by some mathematical transformation. For instance, we might appropriately use a figure based on the number of people in treatment if we could assume that that number was some reasonably stable proportion of all addicts. Unfortunately we know it is not stable. Opening new treatment facilities raises that proportion, as does a sudden drying up of the illicit narcotics supply. If it were a stable proportion, and if we knew the lag between becoming addicted and entering treatment, the number of first admissions to treatment could serve as an adequate if delayed measure with which to evaluate the public effort to control heroin addiction, even though it underestimates the number of addicts in the community.

The purpose of this report is to bring together people with experience in the field of narcotics epidemiology to consider to what extent we now are able to provide such

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an index of heroin addiction so that the effects of historical change and government effort can be tracked over time, to list the gaps in our knowledge that keep us from having a satisfactory index, to make suggestions about how such gaps may be filled, and finally, to advise the government as to ways in which knowledge currently available and reasonably likely to be garnered in the near future might be used in planning policy and predicting the outcome of changes in policy or changes in the supply of narcotics.

As epidemiologists, we are acutely aware that providing "the" number of heroin addicts in the United States is fraught with problems. These problems concern the definition of heroin addiction, its measurement, and its interpretation. HOW should we define a heroin addict? If a heroin addict gets into a methadone program and is thereafter legally addicted to a different narcotic, should that individual still be counted as a heroin addict? For how long after an addict quits daily use should he continue to be counted as an addict? Need he abstain from narcotics entirely, or should he also be excluded if he drops to weekend use? What if he substitutes some other illicit narcotic such as codeine for heroin? Is he still a heroin addict, or does he become a codeine addict? Should men and women be counted as addicts if they administer heroin daily, even if the quality of the drug they are getting is so poor that they have no withdrawal symptoms when they abstain? Perhaps there is no one satisfactory definition, but instead we may need a set of alternative definitions to meet various purposes.

When we decide to measure rates of addiction, we can use surveys of general populations or we can collect data from agencies dealing with addicts. There are difficulties inherent in getting an accurate estimate of heroin addiction from general population surveys for three reasons: (a) Heroin addiction is so rare that our best estimate at one time could double or halve in the next survey without our being able to tell whether we were observing a real change or simply sampling error. (b) Because heroin addiction is discreditable and requires illegal activities, it will be hidden by those interviewed unless they are certain of the interviewer's tolerance and his or her willingness and ability to guard the confidentiality of the data. (c) Extrapolating from survey results to population figures requires that each member of the population have a known chance of entering the sample. We know that addicts have a somewhat reduced chance of entering a general population sample compared with the rest of the population because they have a decreased life expectancy, and, if alive, are especially likely to be "underground," transient, unidentified with any household unit, in hospitals or jails, or out on the street hustling when the surveyor knocks at the door. The difficulty is that we do not know how much such factors reduce the representation of addicts, and so cannot assign an appropriate weight to the number of addicts falling into our sample.

If general surveys obtain flawed estimates of the number of addicts in the population, estimates based on other sources have equally severe problems. We mentioned that the number of addicts obtained by surveying treatment resources will rise as more treatment slots become available. Counts from various treatment programs cannot be easily combined, since treatment programs differ in the degree of contact required before a record is established, as well as in the length of the interval after a last missed appointment or after an enquiry without further contact before a case is dropped from active files. Dropouts from one treatment program may enroll in another before being removed from the rolls of the first program, or may reenter the first program under a new identification number and so be counted twice. Further,

since treatment records often fall to specify the drug for which a patient is in treatment, much less whether he or she meets any specified criteria for addiction, it is often impossible to distinguish heroin addicts from other patients in a program.

In any case, even if we could agree on a definition and a way of counting addicts, we would be wary of publishing our resulting figure for fear that it might be misinterpreted. Because the general public and legislators as well often fail to distinguish between users and addicts and between users of heroin and users of other illicit drugs, they would not see a figure representing addicts as being only the tip of the drug-abuse iceberg that it actually is. Since it is commonly believed that there is virtually no migration out of addict status, at least not without heroic treatment efforts, they would likely assume the number given them could only grow in the future unless immediate action were taken. Those of us involved in research in narcotics have been learning how wrong these common assumptions are. We have increasing evidence that many heroin users never become addicted at all, and that many of those who do become addicted recover rather quickly without any medical treatment. But the fact that misinterpretations are difficult to avoid does not relieve us of an obligation to provide some number or sets of numbers that do represent levels of addiction at the present time. Without them, budgeting is without basis and program evaluation cannot exist.

While trying to provide a single figure that could represent the current number of addicts in this country is a difficult enough problem, that number alone is not at all sufficient for the government's needs. Changes in that number can help the government decide whether its past input has had an effect on the total number of addicts, but in planning future programs, policymakers need to know which *parts* of the population contribute most addicts so that these subpopulations can be special targets for intervention. When planning how to distribute educational programs aimed at preventing all experimentation with heroin, they need to know what the risk of any use will be in various subpopulations defined by age, sex, and geography. At the other end of the spectrum of narcotic involvement, in planning where to invest in aftercare services for treated addicts, they need to know the demographic and personality correlates associated with high relapse rates among addicts.

In short, the information needed by program planners concerning the use of heroin and other narcotics depends on the purpose for the program they are planning and the characteristics that can reasonably be used to choose subpopulations to be exposed to that program. (To be useful, population characteristics must not only be predictors of who needs and will benefit from the program, but must be easy to discern. Thus, region, referral. source, and age variables correlated with need and outcome are-more likely to be useful than personality traits or family history, even if the latter are more highly correlated with need and outcome, since it is not clear how to determine where to locate programs to reach people with the relevant personality traits and histories.) If program planners knew the likelihood of taking every step between first trying a narcotic through addiction, remission, and relapse to readdiction, and if in addition they knew which subpopulations defined by prior drug program or drug enforcement agency contacts and demographic characteristics were at highest risk of taking each of these steps, they would indeed be in possession of the kinds of epidemiological information most helpful in planning and evaluating a great variety of programs.

In our study of Vietnam veterans, we faced the difficulties one would expect in trying to identify those subgroups of veterans whose risk of addiction after Vietnam was sufficiently high so that intervention might have a reasonable payoff. Even though our Vietnam veterans were a relatively high risk sample-being young males heavily exposed to heroin in Vietnam-only 2.8% were addicts at any time in the first three years after their return. When we tried to identify subgroups in whom addiction was common enough to justify intervention, we found some correlations but no truly high risk groups. We then found that by looking at heroin addiction as the end point in a series of transitions from one level of involvement with drugs to the next, we solved some of the difficulties in identifying the high risk subgroups in a population with a low addiction rate overall. We also found that the subgroups that appeared to be at relatively high risk in the population as a whole were not necessarily at high risk at all stages in the progression from use of heroin to addiction. The goal was to find predictors of heroin addiction, whether or not these predictors were of a type useful in program planning. Despite the fact that the choice of predictor variables might not be those most appropriate to policy planners, we decided to present some of these findings to illustrate how decomposing the process of heroin addiction into a set of successive stages can help to develop epidemiological findings. The method may be useful in planning governmental action, even if our particular findings are not, because it overcomes the problem of the typically weak correlations we find between social correlates and events as rare as heroin addiction.

The Study

Between October and December of 1974, we completed the second of two interviews with 57 1 Vietnam veterans who had entered the Army in 1969 or later as enlisted men, and who had returned from Vietnam to the United States in September of 1971. These 571 interviews represented a 91% completion rate for both interviews among the sample selected (96% received the first interview and 94% of those interviewed the first time were reinterviewed two years later). The sample was selected in two parts: about half were a general sample of all men returning in that month who had been inducted from 25 states, and the remainder were a sample of men who left the same month and had enlisted from the same states and who, in addition, were detected as drug positive at departure from Vietnam.

We selected the second sample because, having anticipated problems with the rarity of serious narcotic involvement after return, we wanted to oversample the high risk group. We assumed those involved with narcotics just before departure from Vietnam would be especially likely to use after return. To adjust for our oversampling of the high risk group, we have weighted those detected as drug positive appropriately, so that our results pertain to all first-term Army enlisted men who left Vietnam for the United States in September 1971. We have, however, provided unweighted Ns throughout, so that it will be clear on how many interviews each calculation is actually based.

A number of efforts were made to verify the accuracy of interview information. Urine samples were obtained at the end of the interview to verify the reports of no current drug use, and military records were obtained to verify denials of detection as a drug user in Vietnam. Both checks on validity confirmed that there was little denial of actual involvement with heroin in interview.

Addiction to heroin was defined as an interview report of use of heroin plus meeting three or four of the following four criteria for addiction: use of narcotics more than once a week, feeling dependent on narcotics, having at least two of the four classic symptoms of withdrawal (chills, stomach cramps, insomnia, muscle pain) when narcotics were discontinued for a day or more, and these withdrawal symptoms lasting for more than two days. Since the questions about dependence and withdrawal were asked for all narcotics taken together, we could not be certain among those who used other narcotics frequently, as well as heroin, that the addiction was to heroin per se. However, since we found no men who met the criteria for addiction but reported using other narcotics only, and since almost all men who used more than one type of narcotic said that heroin was their *main* drug, it is very probable that it was heroin that almost always explained the addiction.

Results

Forty-five men met the criteria for heroin addiction at some time in the first three years after their return from Vietnam. Seven came from the simple random sample of 284 veterans, and 38 additional cases came from the additional 287 men detected as drug positive at departure. The oversampling of the high risk group did indeed turn out to be valuable in providing a reasonable number of post-Vietnam addicts. If our 57 1 interviews had all come from a simple random sample of Army enlisted men, the yield would have been only 15 or 16 addicts in the three years after return, and our problem in finding predictors of addiction would have been even more severe.

As mentioned previously, the 45 men addicted at any time after return represented less than 3% of the general sample after weighting. Most of the post-Vietnam addiction occurred in the first ten months, i.e., before the first interview, with 1.5% addicted in that time period. An additional 1.3% became addicted at some time over the next two years.

Our goal was to find characteristics of the men before entering service that could predict who would be a heroin addict thereafter. The preservice predictors we looked at were drug experience before service, various types of deviant behavior other than drug use before service, demographic characteristics, and home backgrounds. Our expectation that these might be the important causal variables for veterans' addiction grew out of descriptions of heroin addicts arrested or in treatment in this country. As the review of epidemiological studies to be presented at this conference will show, heroin addicts in treatment are disproportionately young, minority group, inner city males, often from disrupted homes, with a personal history of poor job records, school dropout, and arrests, and who began their drug careers with marihuana or glue or progressed through a variety of other drugs before becoming involved with heroin.

We developed four preservice scales. Our Demographic scale included age, race, and whether reared in the core city of a metropolitan area; our Parent Behavior scale included whether the home was broken by divorce, separation, or nonmarriage, and parents' arrests, drug use, and drinking problems; the Deviance scale items consisted of preservice arrests, school dropout and expulsion, truancy, fighting, and early drunkenness; and the preservice Drug Use scale treated the use of marihuana alone as a low level of drug involvement, and the use of amphetamines, barbiturates, or narcotics as evidence for more serious involvement. Scale scores were obtained by summing scores for each element included in the scale. In a previous paper we showed that this resulted in reasonably well-behaved scales; that is, scale elements were more

highly correlated with at least one other item within their scale than with any element in other scales, and all correlations between scales were low and positive (Robins, Davis, & Wish, 1975).

When we tested the predictive power of these four scales describing the preservice history against heroin addiction in the post-Vietnam period, we found no strong relationships. The best of our four preservice scales was the Drug Use scale, but it was inefficient. Veterans at the top of the scale, those who had had experience with hard drugs before entering service, were addicted to heroin after Vietnam in 8% of cases (Table 1). If one were to use experience with hard drugs before service as a criterion for eligibility for a preventive program, one would be selecting one-quarter of all returning veterans to obtain a group of whom only 8% were in danger of becoming addicts. This would be a very large program for a rather small potential payoff, although it would reach 73% of all prospective addicts.

To learn how well we could do using all four predictor scales, we entered them into Multiple Classification Analysis (MCA). We found that all four of the scales together explained 6% of the variance in post-Vietnam addiction, with the Drug Use scale alone explaining slightly over 2% of the variance when the contributions of the other scales were taken into account. Clearly, with so little variance explained by all four scales together, we could not improve our predictive capability a great deal by using combinations of these predictors.

Having failed to find adequate predictors of heroin addiction after Vietnam in the population as a whole, we considered how we might decompose the process of addiction into a set of necessary stages in the progression toward addiction. We could then try to predict entry into each of these stages in succession, rather than trying to predict post-Vietnam addiction immediately. There was one obvious necessary prior stage to be considered: it was not possible for someone to become an addict after Vietnam without using heroin after Vietnam. We also found another stage in the process that while not absolutely necessary, came close to it. All but one of our 45 addicts had also used heroin in Vietnam. Heroin was so widely available in Vietnam that anybody who wanted to use it at that point in his life easily could. This temporarily exhausted the potential market for users. Indeed, no man used any type of narcotics within the first ten months after return who had not also used narcotics in Vietnam. In the following two years, there was some narcotic use by men without narcotics experience in Vietnam, but it was extremely rare. Only four new heroin users were found, and only one of these became addicted to heroin in the latter part of the follow-up period. If we ignore these very few new heroin users after Vietnam, we can look at heroin addiction as an end point in a progression through three stages: use of heroin in Vietnam, continuation of use after return among men who used heroin in Vietnam, and addiction to heroin after return among men who continued their use after returning to the United States.

Once we decomposed the process in this way, we found that each transition from one stage to the next occurred in a sufficiently large proportion of the population at risk to make the search for predictors practical (Table 2). Of all the men who-went to Vietnam, 35% used heroin while there. Of those who used heroin in Vietnam, 28% continued that use after return to the United States. Of those men who continued their use after return to the United States, 28% became addicted to heroin.

As Figure 1 shows, when the four preservice scales are taken together (left-hand group of graphs), more variance is explained at each stage in the progression to post-Vietnam

TABLE 1

Preservice Predictors of Post-Vietnam Heroin Addiction

	N	Weighted percentage of population with these scale scores	Weighted percentage addicted after Vietnam	Percentage variance explained by predictors Individually	Percentage variance[a] explained by MCA analysis
Total sample	571		3		
Preservice scale scores				4	2
Drug Use					
0	277	60%	0		
1	87	13	4		
2	207	27	8		
Parent Behavior				2	1
0	355	69	1		
1	154	22	7		
2	62	9	5		
Deviance				1	[b]
0	102	24	0		
1	194	39	3		
2	168	25	2		
3	107	12	7		
Demography				1	[b]
0	58	15	3		
1	241	54	2		
2	185	26	4		
3	87	12	7		
All four scales					6

[a] β^2 Individual scales, R^2 for all four together
[b] <.5%.

TABLE 2

The Decomposition of Post-Vietnam Heroin Addiction into Three Stages

Stages	Weighted proportion of the total sample at risk of this step	Number at risk	Weighted proportion taing that step
1. Using heroin in Vietnam	100	571	35
2. Continuing use after Vietnam	35	376	20
3. Post-Vietnam users becoming addicted	10	144	26

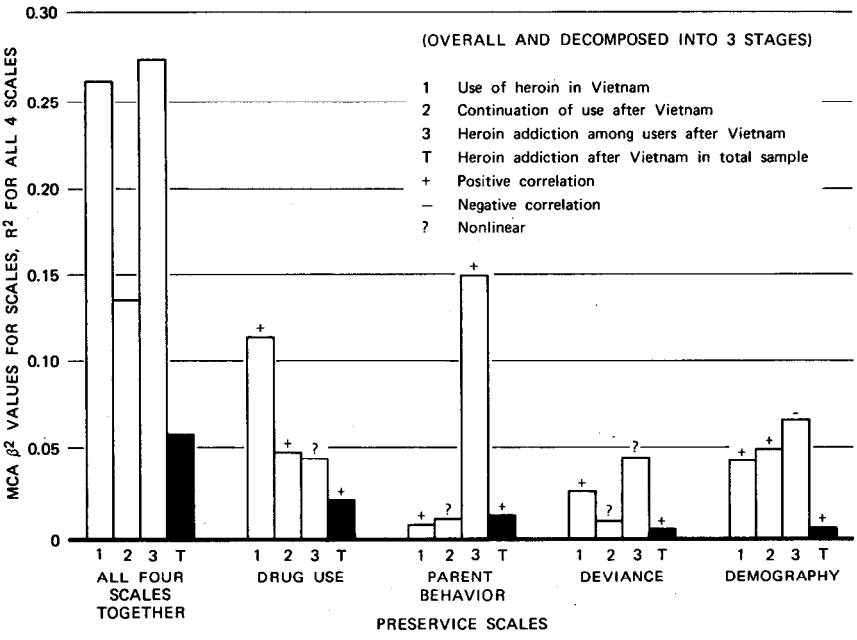

Figure 1. The explanatory power of preservice predictors of heroin addiction after Vietnam

addiction than in post-Vietnam addiction taken as a whole (the black column). In predicting addiction among heroin users after Vietnam, for instance, the four scales combined explain 27% of the variance. As Figure 1 shows, the step most difficult to predict was whether men who used heroin in Vietnam would continue their use after return. Our predictor scales accounted for more than 25% of the variance both in deciding to use heroin in Vietnam and users' becoming addicted afterwards, but for only 13% of the variance in explaining continuing heroin use after return from Vietnam.

The remaining histograms in Figure 1 show the impact of each scale separately in explaining the three steps in the progression to post-Vietnam heroin addiction as compared with post-Vietnam heroin addiction taken as a whole. It is of interest that the most powerful predictor of addiction *among heroin users after Vietnam*

(Stage 3) was not the drug scale, which had been the best predictor overall.[1] Instead, it was the parent scale. While 30% of the post-Vietnam heroin users who had used a hard drug before service became addicted, 59% became addicted if their parents had two or three of our three indicators of problem behavior. The drug scale is the best predictor of post-Vietnam addiction overall (the black columns) mainly because of its impressive effect on the use of heroin in Vietnam (Stage 1). The drug scale alone accounted for about 12% of the variance in who used heroin in Vietnam, but less than 5% of the variance in which post-Vietnam heroin users became addicted. Indeed, its relationship to addiction among post-Vietnam users was not even linear. It was the middle group, those who had used only marihuana before service, who were at highest risk of addiction after Vietnam if they used heroin.

The most striking finding of all was that the demography scale, which had weakly predicted post-Vietnam addiction in the population as a whole, was *negatively* related to addiction among post-Vietnam heroin users. The highest liability to addiction found in any category of these predictive scales was found for heroin users who were *atypical* by demographic criteria. Although it was black, inner-city, young soldiers who had been likely to use heroin in Vietnam and to continue it afterwards, it was the older, white heroin user from outside central cities who was at the greatest risk of becoming addicted. The fact that demographic determinants acted in one direction when predicting use, and the opposite direction when predicting addiction among users, accounts for the fact that their overall effect on post-Vietnam addiction in the total population was minimal. This seems not to be an accidental finding, because each individual element of the demographic scale behaved in the same way: being black, from inner cities, and young all favored heroin use in Vietnam, while being white, not from the inner city, and older all favored liability to addiction if heroin were used after Vietnam (Table 3).

These unexpected results, with the reversal of direction for demography and the appearance of the parent problem scale as an important predictor of addiction among users when it had been so weak in the population as a whole, made us question whether the findings might be the result of an inappropriate application of MCA. The use of the MCA has two important limitations. First, it assumes an additive model. If interactions between the predictors are present, its summing of effects is inappropriate. Second, it is risky to use with rare events (Fienberg, 1975). Thus the MCA might have given us false information about factors in heroin abuse in the total sample, where the outcome was less than 3%. Low outcome rates could not, however, have explained the fact that these predictors acted differently when we were predicting use of heroin in Vietnam from when we were predicting addiction after Vietnam among heroin users, since neither of these were rare events in the subpopulations still at risk.

We checked for interactions by performing a stepwise logit analysis on the same data, using methods suggested by Goodman (1971). This method also turned out to have severe limitations when the outcome variable was a rare event. The criterion suggested by Goodman for a satisfactory explanatory model is that the variance remaining unexplained is reduced below statistical significance. When an event is rare, the total variance to be explained can be below statistical significance, and thus the criterion is not applicable. The statistical significance of the unexplained variance also depends on the number of degrees of freedom in the predictors. In an attempt to get meaningful results, we tried dichotomizing our predictor scales to reduce the degrees of freedom. Even dichotomization of the predictors did not provide a statistically significant level of unexplained variance taking post-Vietnam addiction as the

TABLE 3

Demographic Characteristics and Transitions Between Stages in the Heroin Addiction Process

	Post-Vietnam addiction		States in the addiction process: Heroin use in Vietnam		Continuation after return		Addiction if continued	
	N	%	N	%	N	%	N	%
Central city								
No	445	3	445	33	275	28	96	29
Yes	126	3	126	48	101	27	48	24
Black								
No	449	3	449	33	271	24	87	33
Yes	122	4	122	54	105	49	57	14
Young at return								
No – 23+	87	2	67	16	37	30	20	46
21, 22	329	2	329	36	209	21	69	24
Yes – <21	155	6	155	55	130	44	55	27

dependent variable either in the sample as a whole or among post-Vietnam heroin users. Addiction in the sample as a whole was too rare a phenomenon for this method, and addiction among users after Vietnam was based on too small a sample at risk. However, in the two analyses that we *could* do by logit analysis after dichotomizing our scales, predicting heroin use in Vietnam and after, we did find that there were no significant interactions among our predictors, and that the models selected by logit analysis picked the scales found best by MCA analysis in the same order. Thus to the extent we could validate the MCA method, it appears to have produced correct results.

While we had not predicted that the demographic variables would have opposite effects on use in Vietnam and addiction after Vietnam among users, this discovery does seem to make sense. The chief contribution of demographic variables to addiction is probably that they facilitate exposure to opportunities and pressures to use narcotics. The young inner-city black uses narcotics for a variety of reasons-because his friends urge him to, for relaxation and companionship, to satisfy his curiosity, as well as to achieve euphoria or narcotize pain. Individuals who live in environments in which none of their peers use narcotics are not being urged by friends to use them for social purposes. Indeed, if they use narcotics at all, they are both violating local norms and expending great effort to maintain their supply, since narcotics in their area are scarce. Presumably, older whites who live outside of the central city and still use narcotics must be driven to this use by an unusual compulsion to achieve euphoria or by serious subjective problems. We infer that use against demographic odds implies a greater need, a need so great that it overrides considerations of disciplined moderation that might prevent addiction.

Similar findings have been reported for drinking behavior. In teetotaling environments such as the southern bible belt, alcoholism rates overall are low, but among *drinkers* they are high (Cahalan & Room, 1974; Globetti, 1967). Presumably, in the absence of a strong personal need for alcohol, the normal individual in a teetotaling environment adapts to local custom and refrains from drinking altogether, leaving as those who drink as individuals whose needs are so great as to override local customs. In the United States, except for the urban black ghetto, virtually the entire country is a "bible belt" as far as heroin goes, where the norm is abstention and the supply is low. In Vietnam, in contrast, narcotics were available to all, even more available than they had been in the urban ghetto. Under these circumstances of plenty, demographic variables favoring use were positively, not negatively, related to addiction liability (Table 4).

TABLE 4
Demographic Factors and Addiction among Heroin Users in Vietnam and After

Demography scale scores	Heroin users			
	In Vietnam		After Vietnam	
	N	% Addicted	N	% Addicted
0	20	35	9	77
1	131	53	35	30
2	144	57	59	23
3	81	60	41	22

Future Research Strategies

Decomposing the addiction process into a set of stages makes early predictors of addiction easier to detect and shows at what stage in the addiction process they function. It also shows at what stage in the process the proportion of addicts is large enough to give a-stable estimate as to the addiction rate with-a reasonable-sized sample. A sample of 500 from a population with a 3% addiction rate would need a reduction of about 50% in the rate of addiction before that change could be distinguished from sampling error. That same sample size from a population with a 20% addiction rate allows distinguishing a reduction of only 17% as a true drop in rates. Thus, if we are to do repeated measurements to estimate changes in rates of addiction, there is considerable economy in decomposing the addiction process in order to locate the stage where addiction rates are high in a subpopulation that still contains all or virtually all those in the total population who will be at risk of addiction within the time frame of the study. One can remove those not at risk by setting age limits, or requiring a history of some illicit drug use, or, if the time frame is narrow enough, requiring narcotic experience. We would, of course, need to do larger studies occasionally to verify the fact that the parts of the population excluded continue to be almost entirely not at risk.

Not only would it be useful to identify the proportion making the transition at each stage of the addiction process and the predictors of those transitions, it would also be useful to estimate the length of time that usually elapses between entering one stage and moving to the next. Without information about the lepgth of that interval, the government does not know how soon a survey of the population at risk could be expected to show the effects of a new policy, whether that be a policy intended to prevent new users' progression to addiction or to prevent relapse among treated addicts. It is also possible that a new policy may fail in totally preventing the beginning of addiction or its relapse, but may still prolong the period of mild use or abstention. We might miss discovering this valuable result if we did not know enough about the time intervals that normally elapse between one stage and the next to choose the proper interval after which to do the evaluation. Studies that wait too long to evaluate the outcome will show no reduction in final addiction rates.

So far we have talked about heroin addiction as an entity in itself that can be predicted by a set of precursors and analyzed in terms of its natural history. We have not yet considered the fact that the heroin addiction process interacts with use of other types of drugs. Every other narcotic can be used to stem withdrawal symptoms when heroin is scarce, and is so used. Heroin addicts also use many drugs that are not physiological substitutes for heroin. In the long run, we need to see the natural history of heroin addiction as it is embedded in involvement with all kinds of drugs, including alcohol. Once we have such natural history data about the polydrug activities of heroin addicts, we will be in a much better position to evaluate government programs. An excellent program is one that not only reduces the number of heroin addicts, but in doing so does not increase their involvement with other drugs that may be as harmful or perhaps even more harmful.

Having begun by saying that it is very difficult indeed to get even a single figure representing the current number of heroin addicts in the population, I have gone ahead and argued that that number alone is really hardly enough. I have suggested broad goals for the kinds of information that we would like to have, goals that will be difficult to achieve. Part of the difficulty derives from the fact that the very nature

of illicit drug use complicates the research problems of identifying and counting people at various stages of involvement with narcotics, and relating those counts to addicts' underlying characteristics and use of other drugs. These difficulties will always be with us. But a large part of the difficulty in using past research for the estimates we need lies in the failure of researchers to do three things: to agree on how heroin use and addiction were to be measured, to report precisely how they *did* measure it, and to describe fully the populations sampled along such relevant dimensions as the year of the survey, age distribution, and location of the sample during the years of maximum risk of beginning drug use.

The authors of this report who are evaluating studies in the epidemiology of heroin addiction that have already been done are all themselves investigators whose current and future studies may someday present future summarizers and policymakers with problems of comparability and usefulness of data, just as we are now limited by what has been done previously. Those who prepared papers for this conference have tried to reconcile results from studies that have insufficiently described the criteria they have used, have insufficiently described the samples they have reached, and have sometimes failed to tell us what time period is covered by "current addiction." If that exercise in searching the available literature for the state of current knowledge does nothing else, it should make us all resolve to be as clear as possible in the future in describing how each of our studies is done, so that even if we cannot all be persuaded to use the same criteria, at least we would know when the same criteria have been used, so that studies can be compared when comparison is justified. May I commend to you the recent publication "Operational Definitions in Sociobehavioral Drug Use Research" (Elinson & Nurco, 1975), which represents an attempt to find a common set of definitions and to set up standards of precision for describing populations studied, and where and when they were studied.

The advice contained in that document was aimed at survey researchers. Even greater problems arise when we try to compare information gained from various record-based studies. Lack of comparability between record-based studies derives not only from the researchers' idiosyncratic preferences, which we can hope to talk them out of exercising, but from disparities among the record-keeping processes of various agencies. This volume will be especially valuable if it has an influence on behalf of the minimal record-keeping standards that are crucial if treatment programs and law enforcement programs are to produce data that will be useful in estimating the prevalence of addiction and transitions out of addiction status.

Because it is expensive to learn population rates of rare events like heroin use and addiction through population surveys, there have been and will continue to be attempts to estimate general population figures by summing cases known to treatment programs, emergency rooms, coroners, and police. There are two difficulties in extrapolating from these sums: there are duplications of an unknown proportion because the same people appear more than once and there are omissions of an unknown proportion because some addicts do not show up at all. We could get a good idea of the amount and direction of the correction necessary if in those rare and expensive general population surveys we *do* carry out, we were to ask those who have been involved with drugs the frequency with which they have appeared in various public records as drug users and how close in time these appearances were. The accumulation of survey data about which records they have appeared in, how often they have appeared in each record source (so that the probability of duplications in

records from a single source over a given interval of time can be estimated), and the variety of record sources in which their names appeared (to allow taking into account duplications across sources) could be used to calculate a factor by which we should correct record indicators to get the best possible estimate for the frequency of heroin addiction in the general population.

In preparing papers for this conference, the contributors were asked to review the literature on the epidemiology of heroin addiction and other narcotics, keeping in mind the kinds of needs that I have emphasized today. We all knew that it was unlikely that they would find many studies that gave the kind of detail that would be required to compare one study with another and to extrapolate from a study to the general population. However, contributors were urged to note, where it was specified, how the population from which the study derived was defined in terms of demographic characteristics, location, and historical period; how the study defined the narcotics covered; how it defined addiction; what the treatment or arrest history was that made the individual eligible for entry into the study; the length of intervals and the variance in length of intervals between moving from one level of drug use to another; and the degree of overlaps between users appearing in various public records. As expected, they report that much of the data that we would like to have is missing. While that is bad news, it at least gives us clearer information about what we do not know. Their efforts should certainly underscore the need to provide in future studies for obtaining the quality of data in the detail needed to enable our drug policy planners to use our epidemiological studies of heroin addiction as a base for the formulation of policy.

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This research was supported in part by Research Scientist Award DA 0013 and Research Grants DA 01120 and DA 4RG008.

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[1]Detailed results will be found in the Appendix table.

APPENDIX

Multiple Classification Analysis Compared with Cross-Tab Analysis of Effects of Preservice Scales on Post-Vietnam Heroin Addiction

% positive Preservice scales	N	Post-Vietnam heroin addiction (N = 571)		Stages in the development of post-Vietnam addiction: Heroin use in Vietnam (N = 571)		Continued use after return (N = 376)		Addicted after return if use continued (N = 144)	
		3%		35%		20%		25%	
		Cross-tab %	MCA Adj. %	Cross-tab %	MCA Adj. %	Cross-tab %	MCA Adj. %	Cross-tab %	MCA Adj. %
Drug Uae									
0	277	1	1	20	22	15	17	10	10
1	67	4	4	40	45	20	22	45	42
2	207	6	7	64	59	39	36	30	30
Parent Behavior									
0	355	1	2	30	33	27	27	14	14
1	154	7	6	45	35	36	34	42	39
2	62	6	5	55	46	17	20	59	71
Deviance									
0	102	1	2	14	25	30	35	6	33
1	194	4	4	34	34	23	26	44	39
2	168	2	2	44	40	24	24	21	16
3	107	7	4	65	46	40	34	25	26
Demography									
0	56	3	4	14	17	26	20	77	66
1	241	2	2	31	34	17	19	30	26
2	185	5	4	50	44	39	37	23	29
3	67	7	5	73	59	45	42	22	6

Notes on the Spectrum of Opiate Use

ROBIN ROOM

Epidemiological work and policymaking operate to a considerable degree in different worlds and with different agendas, and Lee Robins' thoughful paper, which begins this section of this report, reminds us of some of the difficulties in maintaining relations between the two worlds. Policymakers and program planners are most likely to call on epidemiological researchers when they need a number-in terms of our concerns here, as Robins notes, it would be "the" number of heroin addicts. The number is usually needed to establish the size of a social or health problem as a justification for the size of the programs directed at that problem. By now, the idea that the size of programs should be directly related to the magnitude of the problems they deal with is well established in the public mind. Thus a recent independent policy analysis of federal health budgeting compared the research budgets of the various National Institutes of Health with the ranking of each institute's diseases on indicators of death and disability (Russell, L., Bourque, B., Bourque, D., & Burke, 1974). In the same vein, a foundation for kidney diseases advertised in *Newsweek* on behalf of a greater priority for its problem by showing charts comparing its ranking among diseases for mortality with its ranking for federal funding.

It is beside the point here to argue the merits and drawbacks of a strict proportion between the size of programs and the magnitude of their problems. For present purposes, the point is that the demand for a number commonly arises from the essentially political context of a program agency's external relations. Behind the request for a number is the half-hidden desire that the number be large: big problems justify big programs and big budgets. But not too large, so large that it would strain the bounds of credulity, or invite questioning comparisons to last year's number. In fact, we could probably specify fairly closely for many social or health problems a range of

numbers that would be considered politically acceptable. It may be that the primary explanation of convergence in various estimates of alcoholism rates (Popham, 1970) is that only estimates falling in this credible range would be published and accepted.

This is not to deny the reality or the importance of the issues of priority that are the stuff of the political agenda. The point is simply that the political agenda mixes poorly with the epidemiological. The political agenda needs the simplicity of a single number; the misinterpretations that Robins mentions are inherent rather than accidental. On the other hand, the first impulse of epidemiological workers is to complicate matters — partly, as Robins elucidates, because of the real complications they know only too well, and partly also in an attempt to educate or at least frustrate the process of political simplification. As Bruun (1970) put it concerning alcoholism, "One way to avoid the negative effects of the black/white thinking easily introduced by the dichotomy alcoholics/non-alcoholics is to try to use not only one but two or three measures thereby indicating the vagueness of our definitions. . . . This will force the users to discuss what is behind these measures."

A second concern that frequently brings policymaking and epidemiology together is the program planning function, which demands projections of needs for treatment or services. A common solution to this demand has been to plug in "the number": there is a need for services for a thousand addicts because there are projected to be a thousand addicts in the population. In this case, epidemiologists have sometimes been more confused than policymakers. Certainly the framers of the first Federal Strategy (1973) recognized that not everyone defined as an addict was going to walk into treatment. Epidemiological thinking, however, has often followed the common clinical confusion of the two meanings of "needs": needs as imputed from the outside, in defining those who fit some criterion as "in need of treatment"; and needs as felt and acted upon by the potential clients themselves. Those we define as having problems are not necessarily going to utilize offered services voluntarily or even under pressure. The incongruous situation of a large theoretical unmet need along with empty positions in treatment services has not been uncommon. The number of users of services will be affected by the availability of services, as Robins mentions, by the nature and social ecology of the services, and by the potential clients' personal histories and characteristics. An accurate projection of the potential utilization of services must be based on a sensitive investigation that takes all these factors into account.

Both of these common situations of contact between policy and epidemiology are thus often exercises in mutual frustration. It is presumably a major purpose of the present meeting to transcend this potential impasse and to challenge epidemiological workers, in going beyond "the number," to assess and increase the policy relevance of their work. In the terms of reference for the meeting, and in her paper, Robins points to one classical line of development in this direction: to provide a far more descriptive picture of opiate use as it exists in the population at large, a picture that includes a whole series of numbers for different criteria of use and use-related problems including addiction, and for different subgroups of the total population, and a picture that measures change as well as position, described by Robins in terms of transition probabilities and time intervals involved in moving from one condition to another.

This general line of development must of course be seen as highly desirable. The particular analysis of her own data that Robins uses to illustrate her meaning is

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carried out with her customary methodological elegance and analytical lucidity. But there are questions about the generalizability of the approach Robins takes. In addition, there is another whole path of development that needs to be recognized in relating epidemiology to policy.

Robins proposes as a general strategy to improve the power and sensitivity of prediction by "decomposing the process of heroin addiction into a series of successive stages." In the particular example she uses, a sample of returned Vietnam veterans, the stages are (1) the use of heroin in Vietnam, (2) the use of heroin after Vietnam, and (3) heroin addiction after Vietnam. As Robins points out, the sequence from (2) to (3) is a logical necessity: by most peoples' conception of addiction, one cannot be addicted without having used. Robins argues that the other sequence, from (1) to (2), is also close to being a necessary stage, since the wide availability of heroin in Vietnam "temporarily exhausted the potential market for users." Actually, it is not clear that this presumption fits the data. The four new post-Vietnam users that Robins found represent about 2% of her "general" sample, that is, those who were not detected as drug-positive at departure from Vietnam. In a national probability sample of males aged 20 to 30, only 2% reported using heroin within the most recent year, whether or not for the first time (O'Donnell, Voss, Clayton, Slatin, & Room, 1976, Chap. 3). So it is not clear that the high rate of use in Vietnam actually resulted in a post-return initiation rate among never-users lower than for young U.S. males generally.

Even if we can accept that use in Vietnam was a precondition of use after return, it is clear that this sequence is not in the same conceptual status as the sequence from use to addiction. It is an empirical regularity rather than a logical necessity. Furthermore, it is an empirical regularity in a particular time and place and cohort, and a regularity that has no meaning outside that circumstance. Even if it did have a meaning — say if we converted the sequence into a statement that use at age 20 is a precondition of use at age 23 — the applicability of this empirical regularity to other populations, places, and times would need testing. In our sample of men aged 20 to 30, marihuana use was nearly a precondition of heroin use (O'Donnell et al., Chap. 9), but this empirical regularity would not be expected to apply, say, in Hong Kong or to the relation between marihuana use and opiate addiction among U.S. physician-addicts.

In general, we can expect that sequences that are neither logical necessities nor reflect intrinsic physiological properties of use of the drug will apply only in relatively limited times and situations. In alcoholism studies, there has been a long history of attempts to define sequences and stages of symptoms, stimulated originally by the imagery of Alcoholics Anonymous (Jellinek, 1946). Even in clinical populations or populations of Alcoholics Anonymous members, the sequences tended not to hold up under empirical scrutiny (Trite & Wahl, 1958; Room, 1970). The parts that did hold up tended to be either logical necessities (such as that "first drink" and "first drunk" uniformly preceded other symptoms) or intrinsic physiological consequences of drug use (such as "tremors" and "convulsions" coming uniformly late in a series of symptoms, reflecting a long history of heavy drinking). As we move away from clinical populations to general populations, the phenomena of alcohol problems become much less coherent, and while tendencies in time-ordering can be discerned (Cahalan & Room, 1974, Chaps. 3 and 8), the possibility of necessary stages becomes even more remote. Even what would appear to be logical necessities often fail in general populations; for example, many U.S. adult males report significant interpersonal problems with their drinking without drinking very much (Cahalan & Room, 1974, p. 179).

Given the illegality and thus the tighter social boundaries around heroin than around heavy alcohol use, we may expect somewhat greater coherence among the various heroin usage and problems indicators. Nevertheless, insofar as Robins' proposed strategy revolves around a set of stages in determinative order, each a precondition of the next, it will have only limited applicability. The applications will probably tend to be in situations of logical necessity — for instance, in Robins' own finding that demographic factors predicted heavy drinking but not alcoholism among heavy drinkers, presented in her classic longitudinal study of problem children (Robins, Bates, & O'Neal, 1962, pp. 395-412). However, if we are not so ambitious, and set aside the idea of progressive stages, the general strategy of decomposing the "dependent variable" and looking separately at the predictors of various elements of it must be seen as a crucial part of epidemiological work, which should be on every study's agenda.

Disaggregation of the dependent variable is also a desirable strategy from the point of view of the second major relationship between epidemiology and policy that needs to be discussed. Epidemiological workers tend to see themselves as in essence performing an intelligence function with respect to a dependent variable whose nature has already been defined by others: if you define the problem you want studied, we'll tell you its epidemiology. This perspective presumably carries over from the infectious-disease origins of public-health epidemiology, where the definition of the entity to be studied could safely be left to the clinician or laboratory scientist. In the epidemiology of drug use phenomena, however, we are in a different situation. Addiction or whatever other conceptualization we choose is but one of the possible governing images (Room, 1974), medical and otherwise, for interpreting a set of human behaviors (Room, 1973; Cahalan & Room, 1974, pp. 7-9); and the dominant interpretation at any one time is at least partly a matter of policy and social definition. If the conceptualization chosen is not to be simply a matter of popular prejudice or professional self-interest, the alternative conceptualizations must be subjected to testing for how well they fit the available data on drug use behaviors. In this view, then, the epidemiologist has a responsibility not only for exploring the predictors and correlates of the drug use dependent variable, but also for exploring the nature of the dependent variable itself — in particular, for investigating interrelationships of the various elements that are lumped together in such governing images as "addiction" or "dependence." The epidemiologist can bring to the policymaker news not only on where the beast is to be found but also on the nature of the beast.

The most dramatic recent example of this other function of the epidemiologist is provided by the work of Lee Robins herself. The finding that few of those who used heroin in Vietnam used it back in the United States (Robins, 1974), even where the Vietnam use fit classical criteria of addiction (Robins, Davis, & Nurco, 1974), raised fundamental questions about assumptions concerning the nature of addiction, and about the policies that had been based on those assumptions. "When only a small proportion [of drug-positive Vietnam returnees] is likely to become readdicted, should treatment be forced on all?" (Robins, Davis, & Nurco, 1974, p. 43.)

As Robins and her co-workers noted, the Vietnam veterans study was unusual in the opiate literature not only in terms of the special circumstances of the return from Vietnam, but also in collecting information on heroin use for a substantial number of users in a nonclinical population. In its results, the study opened up the possibility of finding for opiates what had previously been found in alcoholism studies (Room, 1975) — that clinical data, on which most of our received wisdom has been based,

offer a poor window into the nature of drug use and problems in the general population. Thus alcohol problems are far less coherent and more sporadic in general-population samples than in clinical samples — in fact, we may suspect that those extruded out of the general population for treatment tend to be precisely those who have stacked up alcohol-related problems of various sorts in a cumulative fashion.

As a practical first step toward exploring the question of the coherence of aspects of opiate use, addiction, and problems in the general population, it seemed worthwhile to bring to bear here some data from the study of drug use among a nationwide probability sample of men aged 20 to 30, conducted jointly by researchers from the University of Kentucky and the University of California. The description and basic results of this study have just been published as a monograph by NIDA (O'Donnell et al., 1976). In the appended tables are, cross-tabulated in dichotomous form, a variety of indicators of opiate use, dependence, problems, and treatment. There are a number of ways in which these data can be analyzed; since our focus here is on the question of overlap between one indicator and another, we have chosen to show below the diagonal a simple taxonomic coefficient of similarity (Sokal & Sneath, 1963, p. 129), which measures overlap among those positive on one or the other of each pair of indicators. While this coefficient ranges from 0 to 1, where the pair of variables have unequal marginals its upper limit will be the ratio of positive cases on the rarer item to positives on the commoner item.

Table 1 displays the interrelations and overlap from attributes of heroin use on a lifetime basis. The lifetime basis, of course, yields a maximum degree of overlap; a respondent who is positive on two items did not necessarily display them at the same time. Apart from the conditional relationships that are logical necessities or forced by the interview schedule's skip patterns (indicated by daggers), it is clear that there are some fairly strong empirically conditional relationships. Positive status on most of the other items is rare among those who have used heroin less than 10 times or have never mainlined it. Using 10 or more times and having ever mainlined are in fact the most common characteristics of use among those who have ever used. But, except for these two commonest items, strongly conditional relationships are the exception rather than the rule. Most of the small number who have been in treatment for heroin use, for instance, have used fairly regularly, have mainlined, and report having been psychologically or physiologically dependent on heroin, but only one-third to two-thirds of them are positive on most of the other indicators.

In terms of overlap, the strongest non-dagger relationship is between two items that are closely related: reporting dependence, and giving "force of habit" as a fairly important reason for heroin use. Apart from this, the coefficients of overlap range downwards from .63. A cluster of seven items shows mutual overlaps that are all .45 and above: using 10+ times, 100+ times, weekly use, dependence, using from force of habit, interpersonal problems from heroin, and selling heroin. The other interesting overlaps above .45 are between use to get through the workday and each of three items: dependence, force of habit, and treatment.

In terms of lifetime heroin use, then, the general-population sample shows a moderate degree of clustering, primarily around items that could be seen as relating to regular use, membership in a subculture of users, dependence, and interpersonal friction.

The same variables (except for the cost of habit items) are shown for nonmedical use of opiates as a whole, including heroin, on a lifetime basis in Table 2. The other

TABLE 1

Overlap Between Lifetime Charecteristics of Heroin Use in a Nationwide Sample of Men Aged 20 to 30

	1.[a]	2.	3.	4.	5	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.
1. Uaed 10+ timee	76	35†[b]	59†	18†	44†	17†	50	28	42	25	21	43	17	13	30	20	19	10	40
2. Uaed 100+ times	.46†	35	33	14	25	12	20	16	28	10	14	31	10	0	28	14	0	12	24
3. Weekly use In any year	.78†	.54	59	18†	38	18	47	24	38	22	18	40	18	12	38	10	14	17	38
4. Used daily, usually a lot, in any year	.24†	.36	.31†	18	11	7	18	0	13	10	8	15	8	7	18	8	5	8	15
5. Usual habit $10+/day	.54†	.42	.54	.20	49	17†	42	10	28	16	12	27	11	7	28	15	10	13	28
8. Usual habit $40+/day	.22†	.30	.27	.25	.35†	17	15	8	12	8	4	13	8	5	13	8	8	5	13
7. Ever mainlined	.58	.32	.49	.19	.46	.17	84	30†	38	22	18	38	16	13	33	18	12	18	34
8. Usually mainline	.33	.33	.37	.23	.32	.21	.36†	30	18	12	10	10	8	7	17	10	7	11	18
9. Uaed from force of habit	.53	.54	.58	.26	.42	.24	.42	.32	45	23	15	38	13	10	28	18	0	13	27
10. Used to get through workday	.32	.44	34	.29	.27	.16	.25	.27	.47	27	12	23	10	10	20	13	7	15	18
11. Stayed high more than a day	.27	.32	.28	.24	.20	.10	18	.23	.28	.32	23	14	8	8	14	8	5	8	18
12. Physically or psychologically dependent	.55	.63	.61	.31	.40	.27	.42	.34	.73	.47	.26	45	15	12	20	10	11	16	30
13. Health problems from heroin	.22	.23	.26	.20	20	.21	.19	.20	.26	.29	.24	.31	18	8	10	7	5	10	14
14. Job problems from heroin	.17	.23	.20	.28	.13	.19	.15	.19	.20	.32	.19	.26	.23	14	13	7	3	8	12
15. Interpersonal problems from heroin	.50	52	.56	.37	.45	.29	.36	.31	.48	.42	.28	.51	.20	.31	41	16	10	13	28
18. Law problems from heroin	.26	.33	.31	.26	.27	.27	.22	.24	.32	.37	.22	.40	.22	.25	.35	21	5	7	18
17. Benefits from heroin	.21	.21	.23	.17	.18	.22	.14	.18	.17	.10	.15	.20	.17	.11	.21	.16	16	8	13
18. Treatment for heroin	.25	.28	.27	.27	.23	.16	.21	.28	.25	.47	.23	.33	.36	.31	.27	.21	.20	20	13
10. Sold heroin	.51	.45	.55	.33	.40	.28	.37	.29	.45	.35	.33	.53	.30	.27	.51	.34	.29	.27	42[c]
Ever used heroin (N = 148)	.51†	.24†	.40†	.12†	.33†	.11†	.57†	.20†	.30†	.18†	.16†	30†	.12t	.09†	.28†	.14†	.11†	.14†	.28†

Note. N = 2.510. On the *diagonal,* underlined, is the number of men who report each characteristic. *Above* the *diagonal* is the number of men who report both characteristics for each pair. Below the diagonal is Jaccard's coefficient of overlap (Socal and Sneath, 1963, p. 129):

$$\frac{\text{men with both characteristics}}{\text{all men with either characterIstic}}$$

[a]Top row numbers refer to the same characteristics numbered in the leff-most column.

[b]† icdlcates that the occurrence of one variableto conditioned on the other (with minor exceptions).

[c]Excludes two cases who never used heroin.

TABLE 2

Overlap Between Lifetime Cherecteristics of Opiete Use (Including Heroin) in a Netlonwide Sample of Men Aged 20 to 30

	1.[a]	2.	3.	4.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.
1. Used 10+ times	305	75†[b]	112†	21†	71†	33†	83	81	20	48	18	14	47	24	104	20	82
2. Used 100+ times	.25†	75	50	17	30	20	38	27	16	35	11	0	32	16	10	14	30
3. Weekly use in any year	.37†	36	112	21†	58	30	48	31	24	44	17	12	30	22	28	10	50
4. Uaed daily, usually a lot, in any year	.07†	.22	.10	21	18	10	18	13	8	17	7	8	18	10	8	10	18
7. Ever mainlined	.23†	.31	.38	.10†	90	38†	40	24	17	39	18	13	35	10	17	18	40
8. Usually mainline	.11†	.22	.25	.21	.40†	36	22	15	10	22	10	0	10	12	8	13	10
9. Used from force of habit	.20	.32	.35	.21	.33	.26	72	30	16	41	15	12	33	18	19	14	33
10. Used to get through workday	.18	.20	.18	.13	.15	.14	.23	90	18	28	12	12	27	15	38	18	24
11. Stayed high more than a day	.09	.17	.20	.17	.16	.16	.18	.15	35	15	8	6	16	8	8	8	21
12. Physically or psychologically dependent	.15	.30	.38	.32	.39	.35	.51	.23	.22	49	18	13	33	10	13	17	33
13. Health problems from opiates	.06	.23	.15	.21	.17	.22	.19	.12	.17	.30	20	8	12	8	8	10	14
14. Job problems from opiates	.05	.11	.10	.28	.14	.21	.16	.13	.13	.25	.29	16	15	8	3	8	12
15. Interpersonal problems from opiates	.15	.32	.30	.28	.32	.28	.35	.23	.21	.48	.19	.26	56	17	14	14	33
18. Law problems from opiates	.08	.19	.19	.31	.20	.24	.23	.15	.15	.35	.22	.24	.27	25	8	7	17
17. Benefits from opiates	.26	.08	.10	.03	.06	.04	.08	.14	.03	.06	.03	.01	.06	.03	195	8	18
18. Treatment for opiates	.07	.17	.17	.32	.10	.30	.18	.17	.17	.32	.32	.28	.22	.18	.04	21	15
10. Sold opiates	.20	.37	.38	.25	.33	.22	.30	.18	.25	.38	.18	.16	.35	.22	.08	.20	70[c]
Ever used opiates (N = 792)	.39†	.09†	.14†	.03†	.11 †	.05†	.09†	.11 †	.04†	.08†	.03†	.02†	.07†	.03†	.25†	.03†	.09†

Note. N = 2.510. "Opiates" = heroin and/or other opiates. Thus a respondent is counted as having a characteristic (e.g. used 10+ times) if he reports it for either heroin or other opiates. "Other opiates" includes codeine, Darvon. opium, opiated hashish, Demerol, morphine, paregoric, Methadone, Percodan. Talwin, etc. *On the diagonal,* underlined, is the number of men who report each characteristic. Above the diagonal is the number of men who report both characteristics for each pair. *Below the diagonal* is Jaccard's coefficient of overlap (Sokal and Sneath, 1963, p. 129):

$$\frac{\text{men with both characteristics}}{\text{all men with either characteristic}}$$

[a]Top row numbers refer to the same characteristics numbered in the left-most column.

[b]† indicates that the occurrence of one variable is conditioned on the other (with minor exceptions).

[c]Excludes one case who never used opiates.

opiates add very few to the number of positive cases attributable to heroin for several of the items — notably treatment, the problems measures, frequent heavy use, and dependence. Clearly the overlap between indicators for the opiates as a whole is considerably diluted from the overlap Table 1 showed for heroin: ‘in the whole table, the coefficient only rises above .40 for two pairs of characteristics.

Tables 3 and 4 show results for current use of heroin and all opiates respectively in the smaller list of items for which time data were collected. Overlaps are generally quite low, particularly among the various kinds of problems related to drug use. Among current heroin users, a fairly strong overlap remains between regular use, dependence, and interpersonal problems; no other (nondagger) relationship rises above .35. For current opiates use, the highest coefficient is .31, and comparing Tables 3 and 4, it seems clear that even so it is primarily the heroin users that are responsible for the higher coefficients.

Space does not permit more than this brief overview of the results in the tables. In general, the data suggest that, while there is some coherence between a minority of the heroin indicators, the degree of overlap among characteristics of heroin use, and particularly the degree of overlap for opiates in general, is far less than we might expect on the basis of clinical samples. Opiate use in the general population seems to present a somewhat different picture from opiate use in clinical or other “caught” populations. Informed policymaking on heroin and other opiates depends on our further efforts to see and understand the picture in the general population more clearly.

TABLE 3

Overlap Between Characteristics of Current Heroin Use in a Nationwide Sample of Men Aged 20 to 30

	1.[a]	2.	3.	4.	5.	6.	7.	8.	9.
1. Used in last 30 days	15	7	3	2	4	0	2	6	1
2. Used weekly 1974/75[b]	.32	14	6†[c]	4	2	2	2	12	3
3. Used daily, usually a lot, 1974/75[a]	.17	.43†	6	2	3	1	2	6	2
4. Stayed high more than a day	.10	.22	.17	8	3	1	1	4	2
5. Physically or psychologically dependent	.20	5 3	.25	.21	9	2	1	6	1
6. Health problems from heroin	0	.14	.14	.11	.22	2	0	1	0
7. Job problems from heroin	.13	.14	.33	.11	.10	0	2	2	1
8. Interpersonal problems from heroin	.33	.63	.35	.19	.44	.06	.20	17	2
9. Law problems from heroin	.06	.20	.25	.20	.08	0	.12	.11	4
Used heroin in 1974/75 (N = 46)	.33†	.30†	.13†	.17†	.20†	.04†	.04†	.37†	.09†

Note. N = 2,510. For all items except 1., "current" = occurring in 1974 or 1975. Interviews were conducted in late 1974 and early 1975, so the mean time period covered is about 1 year. On the diagonal, underlined. is the number cl men who report each characteristic. Above the diagonal is the number of men who report both characteristics for each pair. Below the diagonal is Jaccard's coefficient of overlap (Sokal and Sneath, 1963, p. 129):

$$\frac{\text{man with both characteristlcs}}{\text{all men with either characteristlc}}$$

[a]Top row numbers refer to the same charaoteristics numbered In theleft-most column.
[b]Asked only of those who used at least 10 times in their lifetimes.
[c]† indlcates that the occurence of one variable is conditioned on the other.

TABLE 4

Overlap Between Characteristics of Current Opiate Use (Including Heroin) in a Nationwide Sample of Men Aaed 20 to 30

	1.[a]	2.	3.	4.	5.	6.	7.	8.	9.
1. Used in last 30 days[b]	71	21	4	5	4	0	2	11	1
2. Used weekly 1974/75[b]	.25	33	7†$_c$	6	6	2	2	13	3
3. Used daily, usually a lot, 1974/75[a]	.05	.21†	7	2	3	1	2	6	2
4. Stayed high more than a day	.06	.16	.13	11	3	1	1	4	2
5. Physically or psychologically dependent	.05	.24	.23	.18	9	2	1	6	1
6. Health problems from opiates	0	.06	.11	.08	.20	3	1	2	0
7. Job problems from opiates	.03	.08	.25	.08	.09	.20	3	3	1
6. Interpersonal problems from opiates	.13	.29	.23	.13	.31	.08	.12	25	2
9. Law problems from opiates	.01	.09	.20	.14	.08	0	.07	.07	5
Used opiates in 1974/75 (N = 275)	.26†	.12†	.03†	.04†	.03†	.01†	.01†	.09†	.02†

Note. N = 2.510. "Oplates" = heroin and/or other opiates. Thus a respondent is counted as having a characteristic (e.g., used 10+ times) If he reported it for either heroin or other opiates. "Other opiates" includes codeine, Darvon, opium, opiated hashish. Demerol, morphine, paegoric, Methadone, Percodan, Talwln, etc., "current" = occurring in 1974 or 1975. Interviews were conducted in late 1974 and early 1976, so the mean time period covered is about 1 year. On (he diagonal, underlined, is the number of men who report each characteristic. Above the diagonal is the number of men who report both characteristics for each pair. Below the diagonal is Jaccard's coefficient of overlap (Sokal and Sneath, 1963, p. 129):

$$\frac{\text{men with both characterIstics}}{\text{all men with eirher characteristic}}$$

[a]Top row numbers refer to the same characteristics numbered in the left-most column.

[b]Asked only of those who used at least 10 times in their lifetimes.

†$_c$ indicates that the occurrence of one variable is conditioned on the other.

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The Race, Class, and Irreversibility Hypotheses

Myths and Research About Heroin

BRUCE D. JOHNSON, Ph.D.

In the introduction to *Drug Use and Abuse Among American Minorities: An Annotated Bibliography* (Iiyama, Nishi, & Johnson, 1976), the authors examine systematically the empirical literature, through the end of 1974, on opiate use among minorities. One of the central themes presented in this essay is the Social Science Frankenstein. This refers to those patterns of discrimination, racism, and classism that have become historically linked with addiction. So many unstated connotations are symbolically linked to the concepts of "heroin" and "addiction" that even the most elaborate and careful operational definitions by researchers are ignored or forgotten by the public, which equates "black" and "criminal" with "addict."

It has been said that the American public doesn't understand anything that can't be put on a bumper sticker. This American penchant for slogans that simplify complex reality is well summed up by the three "myths" briefly analyzed here: (a) The *race* myth-"Blacks are likely to be addicts." (b) The *class* myth-"Lower class persons are likely to be addicts." (c) The *irreversibility* myth—"Once an addict, always an addict." These three myths *represent* distortions of well based research findings: blacks are more likely than whites to be addicts; lower class persons are more likely than middle class persons to be addicts; and those who have ever been addicted are very likely to remain addicted.

While studies of institutionalized addicts are frequently cited in support of these myths, the evidence from the general drug literature is confusing, convoluted, and shows enormous variation by city and region of the United States (Iiyama, Nishi, & Johnson, 1976). However, the final reports of two just-completed research projects provide evidence, which, with certain qualifications, tends to refute these three myths.

The Race and Class Myths

The recent report of O'Donnell (O'Donnell, Voss, Clayton, Slatin, & Room, 1976) provides some of the best evidence currently available to test the race and class hypotheses. To compare with Robins' (1973) sample of Vietnam veterans, O'Donnell and his colleagues selected an equal probability sample of men at each age between 20 and 30 from Selective Service records across the United States; 84% were located and interviewed.

The first Frankenstein in social science data is racism. Although O'Donnell, et al. (1976) do *not* make this interpretation, consider how the data at the top of Table 1 might be interpreted to support the race myth. By computing ratios, blacks are 3 times more likely than whites to try heroin and 4 times more likely to use heroin 10 or more times or to be current (1974-l975) users. Given such a seemingly large propensity for heroin, the racist slogan "Blacks are likely to be addicts" appears to receive impressive support-but only if all of the following conditions are met: (a) the comparison of whites is skipped, (b) the small proportions of both blacks and whites using heroin is ignored, (c) the current use of heroin is equated with addiction, and (d) the rest of the table is not considered.

However, while blacks are more likely than whites to try heroin, the bottom two-thirds of Table 1 shows that *among heroin users,* blacks are no more likely than whites, and perhaps slightly less so, to have recently or currently used heroin. Thus, the propensity of blacks to addiction is explained by the fact that larger proportions of blacks than whites experiment with heroin; but very seldom does such use translate into recent use or addiction for either race.

Liberal laymen and social scientists frequently attempt to explain racial differences in drug use by invoking class explanations. However, Table 2 shows that both race and class contribute about equally to experimentation with heroin. In the top half of Table 2, race differences are significantly reduced only among college graduates. In the bottom half, unemployed whites are almost as likely to try heroin as their black counterparts. For whites, the lower the social class the more the heroin use; among blacks, however, heroin is less strongly linked to class. Moreover, these measures of class (highest education and present employment) are not very helpful in predicting how to prevent heroin use, since they frequently occur long after heroin use has begun. Other variables such as marital status and size of city have a similar postpredictivt effect.

The Irreversibility Myth

Probably the most firmly held belief about heroin is in its supposed addictive powers: those who try heroin are more likely to become addicted; once addicted, always addicted. But both the O'Donnell and Robins studies provide evidence that forcefully contradicts these beliefs.

In the O'Donnell study (Table 1), only 10% of all heroin users had used the drug in the past 30 days. Of those using heroin more than 100 times during their lives, less than 50% had tried it in 1974 or 1975. Clearly, even relatively heavy experimentation does not imply continued regular use for the vast majority of heroin users. Further, O'Donnell (1976, Table 2.10) finds that 32% of the heroin users had used heroin "almost every day" during some part of their lifetimes; but only 1% of these

TABLE 1

Various Measures of Heroin Use by Race, in Percents

Heroin use	Total[a]	Race	
		White	Black
	(2510)	(2103)	(303)
Any lifetime use	6	5	14
Used more than 9 times	1	2	8
Currant use *(any* use in 1974-1975)	2	1	4
Among those ever using heroin	(148)	(101)[b]	(42)[b]
Most recent use within			
24 hours	1	0	2
30 days	10	7	14
6 months	20	18	21
12 months	28	28	26
During 1974-1975	31	34	29
Number of times used heroin during lifetime		% Current users (any use in 1974-1975)	
1 - 9	11 (72)	12[b] (52)	6[b] (18)
10 - 99	54 (41)	55 (29)	50 (12)
100 - 999	41 (17)	33 (9)	43 (7)
1,000 +	50 (18)	50 (10)	40 (5)

Note: Ns are presented in parentheses.

[a]The total column contains 104 persons of Spanish and other backgrounds; their drug use is not presented separately

[b]ho racial breakdowns in these columns were kindly supplied to the present author by Dr. O'Donnell.

Source: O'Donnell. et al. (1976, pp. 15,31,34,37,40)

TABLE 2

Lifetime Heroin Use by Race by Measures of Socio-Economic Status

% Ever Using Heroin

Race	Highest education				
	Less than high school	High school graduate	Some college	College graduate	Total
Black	17 (95)	12 (114)	Text (72)	2 (22)	Text (303)
White	9 (263)	6 (785)	5 (617)	1 (438)	5 (2103)
Total[a]	11 (358)	7 (899)	6 (689)	1 (460)	6 (2406)
	Current employment status				
	Working 30 hrs/wk or more	Students	Working less than 30 hrs/wk	Unemployed	Total
Black[b]	11 (213)	23 (26)	11 (9)	20 (55)	14 (303)
White[b]	4 (1683)	4 (248)	0 (23)	17 (149)	5 (2103)
Total[c]	5 (1980)	6 (282)	6 (35)	18 (213)	6 (2510)

Note: Ns are presented in parentheses.

[a]The total rowis computed from data presented for blascks and whites.

[b]The racial breakdowns in these rows were kindly supplied to the present author by Dr. O'Donnell

[c]The total row is for the entire sample, imcluding 104 nonwhites/nonblackes.

Source: O'Donnell et al., 1976, 19, 22.

heroin users reported their more recent use within the past 24 hours. This means that only 4% of those who sometime in the past used heroin "almost every day" were using it daily.

O'Donnell's findings are directly parallel to Dr. Robins' findings on Vietnam veterans. Table 3 shows Robins' (1973) three major measures of narcotics use and addiction at various time periods and samples. While in Vietnam, almost 50% of those ever using narcotics felt addicted; this lends credence to the irreversibility myth. However, during the first year after return to the states, experimentation dropped sharply. In addition, less than 10% of narcotics users have felt addicted. But most importantly, Robins (1973) reports that *of those addicted in Vietnam, less than 10% (2%* in the general sample and 9% in the drug positive sample) *continued to be addicted at any time during the first year* back-although many continued to use narcotics occasionally. In Robins' (1975) second follow-up, only 29% of those addicted during the first year at home in the United States (N = 21) report continued addiction during the second and third years after return. Furthermore, narcotics use is much less common than the use of almost all other drugs (alcohol, barbiturates, marihuana, and amphetamines), is less often used heavily, and is less often apt to cause problems for users than are other drugs. Thus, both Robins' (1973) and O'Donnell's work suggest that less than 10% of previously addicted (or daily users) are *currently* addicted (or presently daily users). A new bumper sticker slogan is needed: "Once an addict, very seldom an addict."

In the major paper of this section, Robins clearly demonstrates the difficulties of using variables that are presumed to predispose toward narcotic addiction. With respect to the class myth, Robins, Davis, and Wish (1975) find that various measures of parental social class are virtually unrelated to heroin use or addiction. Robins' (1976) data also contain a major surprise relevant to the race myth. Black and white veterans seem about equally unlikely (4% versus 3%) to have'been addicted at any time since returning from Vietnam. Blacks were 21% (54% versus 33%) more likely than whites to have used narcotics in Vietnam, and 25% (49% versus 24%) more likely to continue narcotics use in the States if they had used in Vietnam. However, *among returned narcotics users* in the States, *whites* were 19% (33% versus 14%) more likely to have been *addicted* than blacks. Thus, black veterans are more likely to use heroin, but black users are less likely to progress to addiction than whites. This inverse correlation with heroin addiction is not unique and holds up when variables such as preservice drug use, demography, and preservice deviance are controlled. Those who are least sociologically predisposed to heroin use show the greatest vulnerability to addiction if they begin using narcotics.

Dr. Robins' (1976) explanation of these puzzling findings seem plausible. But we wish to pose an alternative interpretation as suggested by O'Donnell. Within the culture of black America, norms regulating drug use may be shifting dramatically away from tolerating heroin use and addiction, while the white culture's tolerance may be increasing. Among O'Donnell's et al. (1976) subsample of black males born in 1953-1954, only 2% had ever tried heroin, while over 12% of all older blacks had tried it. Among white males, as age decreases, the proportion trying heroin increases; 7% of whites born in 1953-1954 had tried heroin. In short, the youngest black males were the least apt to try heroin. Such a shift in heroin use, if it is occurring, may have inhibited the addiction vulnerability of returning black veterans with heroin experience in Vietnam. This is, of course, a speculative interpretation needing much more careful research.

TABLE 3

Various Measures of Narcotics Use and Addiction among Vietnam Veterans at Different Periods and Samples

Measures of narcotics use	General sample				Drug positive sample			Matched samples	
	Prior to Vietnam	In Vietnam	Up to 1 year after Vietnam	Up to 3 years after Vietnam	Prior Vietnam	In Vietnam	Up to 1 year Vietnam	Veteran Last 2 years	Nonveteran Last 2 years
N	451	451	461	571[a]	469	469	469	259[a]	259[a]
%Using amy narcotics	11	44	9.5	20	25	97	33	17	7
% Using heavily[b]	c	25	4	6	d	86	10	5	1.5
% Felt addicted[e]	d	20	0.7	2	2	74	7.2	1.6	1
% Using in past 2 weeks (current)				2				2	0.7

[a]Robins, 1975 provides statistics from which the percentages in these columns are computed. (All figures are obtained from Robins, 1973.)
[b]Heavy use = Weekly or more than 1 month (Robins, 1973). Heavy or frequent use = Several times a week (Robins, 1975).
[c]Less than 1%.
[d]Not given
[e]Felt addicted = "Felt strung out or addicted;" in Robins (1975), the term "problem" is used.
Source: Robins, 1973, 1975.

Conclusion

This review suggests provocative questions needing further discussion.

1. Using Dr. Robins' (1976) operational definition of "addict," how many of the street hustlers loosely called "addicts" by the press or involved in treatment programs should be classified as addicts?

2. Both the O'Donnell and Robins studies of carefully selected samples of the young U.S. male population find low levels of heroin experimentation and/or addiction/regular use. If these findings can be replicated, how can the apparent discrepancy with studies of institutionalized addicts be explained? Is there something about the official labeling process that makes occasional heroin users into long-term addicts? Or do the general surveys miss, for unclear reasons, hard core addicts who are known to treatment programs? Or are the supposedly hard core addicts in institutions chipping around like those in the general population? Perhaps a way of beginning to answer such questions is to research "street" people in high addiction neighborhoods. Interviewers would select respondents from randomly selected street locations, offer money to complete the interviews, and conduct the survey at a nearby location. Using similar tactics, persons known to treatment programs or having a drug arrest (but presently on the street) could be traced and interviewed. Such tactics might locate young males who are impossible to trace otherwise. Comparisons of such street samples with a general household sample might tell more about the differences between the general population and those who avoid interviewers.

3. Dr. Robins' research suggests three major conclusions: (a) the successful prediction of rare events such as heroin addiction is very difficult, (b) heroin addiction may not be particularly harmful and/or continuous, at least when compared with other drugs, and (c) race and class variables may be positively related to narcotics experimentation but may be negatively related to addiction among users. If these three conclusions are true, what is the best social policy for the government to pursue?

4. Perhaps the greatest research need is to begin investigating how official and public attitudes about heroin can be changed. It is most unlikely that the public will ever hear, much less believe, the new slogans that could justifiably be extracted from the evidence to design new bumper stickers:

"Heroin use usually does not lead to addiction."
"Once an addict, very seldom an addict."
"Blacks aren't much more likely than whites to use or be addicted to heroin."

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249-470 O - 77 - 5

Estimates of User Populations for Heroin and Other Narcotics

Available Methodologies and Their Limitations

Prevalence of Active Heroin Use in the United States

LEON GIBBON HUNT

There are three principal measures of heroin use, which, taken together, give an adequate quantitative description of use in a population:

1. Incidence of first use: the absolute or relative population rate of new users per unit time. Incidence of new use measures *spread* of heroin in the population.

2. Prevalence of active use: the absolute or relative population rate of active users at a given point in time or during a given interval (point or period prevalence). Prevalence measures extent of heroin use in the population.

3. Frequency and intensity of use: the probability distributions of frequency of use and quantity of heroin used by individual users—e.g., 10% might be daily users, etc.; 2% might use 100 milligrams per day, etc. Frequency and intensity distributions, combined with prevalence data, measure *quantity of heroin consumed,* or the size of the heroin market.

Heroin use cannot be understood quantitatively except in terms of the relationships among these three measures. For example, falling incidence of first use may or may not be associated with falling prevalence of active use, depending on the removal rate (cures, remissions, deaths, net emigration). Conversely, rising prevalence can occur with zero incidence as a result of "negative removals" (lapse of cured or remitted users, exits from treatment). The nominal relationship between incidence and prevalence is

$$p_n = p_{n-1} + i_n - r_n \qquad (1)$$

That is, prevalence at the end of the nth period (for example, year) is equal to prevalence at the end of the preceding period plus incidence of new use during the

period minus removals (either positive or negative) during the period. (This equation has no operational mathematical meaning as it stands, since we do not know how p, i, and rare connected, and so cannot solve for p_n.)

Equation 1, though not itself useful for estimating prevalence, shows the futility of heroin market estimates and calls into question their relevance to prevalence: at any time, the heroin consuming population is a changing mixture of users of different experience and consumption patterns. Thus mean dose is a function of time, and in particular of the changing *distribution* of consumption patterns (item 3 above).

Ideally, if we knew the way in which incidence, prevalence, and removals were connected, we might calculate the future behavior, heroin use, as is done in disease epidemiology. For instance, the assumption that spread of a disease is proportional to the number of infectives and susceptibles leads to the well-known simple epidemic model (Bailey, 1956).

$$dx/dt = -bxy \qquad (2)$$

where the change in x, the number of susceptibles, is related to the size of x itself and to the number of infectives, which grows as x diminishes (since $x + y = n + 1$, the population size plus one exogenous infective) and to b, an infection rate. The assumptions underlying the differential equation, 2, include a homogeneously mixing population, continuous infectivity, no removals, etc., none of which characterize heroin use, so that this model does not remotely describe the spread of heroin. Neither do the more complex epidemic models in general use. While it is possible to construct a mathematical system to calculate prevalence (Hunt, 1973), the practical utility of such constructs has not been great.

Thus we are left with the task of *estimating* each of these measures separately, just as if they were independent, but at the same time knowing that they do depend on one another, and will change continuously accordingly. This paper is concerned with the estimation of one of these measures of heroin use, period prevalence, chiefly for 1-year intervals.

There are four general types of prevalence estimates:

1. *Enumerations,* which are counts of positively identified (or suspected) heroin users by various medical, law enforcement, and social service agencies. Enumerations (or registers) are, by definition, both incomplete and biased, since they include only users who are the "business" of the registering agency. For instance, treatment programs count only treated users; police know only criminal users.

2. Surveys, of general or special populations, which attempt to identify the proportion of users (in various categories) in the surveyed population.

3. *Indirect estimates,* which are based on the observation of some condition or phenomenon connected with drug use, but are not numerically equivalent to it. Overdose deaths, serum hepatitis, and property crimes are typical indirect indicators. Indirect indicators are easier to observe than heroin use itself, but prevalence estimates based on them depend for their accuracy on some presumed relationship between the condition being observed and heroin use. In practice such relationships are seldom known accurately, if at all. For example, Dr. Michael Baden's ratio of 1 out of 200 overdose deaths per year among heroin addicts was specific to New York City and

the year in which the ratio was observed. There is no reason to assume that the relationship holds elsewhere at other times, since overdose deaths depend on conditions of the heroin market such as fluctuating purity.

4. *Inferential estimates,* which result from manipulating direct counts in some type of mathematical device that infers, according to a stochastic law, the probable size of the parent population. Inferential techniques demand that restrictive mathematical conditions be met before they will work properly. Such conditions are seldom fulfilled.

This paper adopts the following strategy: current data on enumerated and indirect populations of heroin users are reviewed and their demographies are contrasted. Each population known from a different source is shown to have distinct characteristics, often radically different from other groups that are apparently closely related. (For instance, treated populations of users from different collections of programs differ among themselves, and also from overdose emergency populations.) Such differences suggest that these populations are not completely duplicative, i.e., not merely the same users being observed by different sampling mechanisms. We deduce that each agency (treatment, hospitals, law enforcement, etc.) naturally "serves" a somewhat different subset of the heroin using population, and that there is a larger parent population of users that is only partially glimpsed as a result of the institutional myopia of the specialized agencies. For example, roughly speaking, treatment programs know only treatable addicts, but all users are not addicts, and all addicts are not amenable to treatment.

A comparison of the aggregate size of these enumerated populations with figures from general population survey estimates proves that the former are incomplete. But from other data we know that general population survey estimates are themselves understatements. Similarly we can show that the principal inferred estimates, the Greenwood recapture data based on the DEA register, are also undercounts.

All these disparate data-direct enumerations having different demographies, survey results, and inferred estimates-are combined by systematically using each to correct the others, and to adduce limiting sizes for the total population of active users. Local examples are shown, and national totals obtained.

Characteristics of Users in Treatment

Treatment programs are the largest source of data on unambiguously identified heroin users. There are no good estimates of the number of different individual heroin users who have been treated in the United States during the last decade of heavy use. Making such estimates is complicated by multiple counting of the same persons who were admitted at various times to different programs. Even crude counts of total admissions are lacking. The roughest of guesses might be that about 200,000 different heroin users were treated between 1968 and June, 1975.[1]

Crude Demography

Who were these heroin users? Surprisingly little is know about them beyond the simplest demography-age, race, and sex-and these data often come from relatively small samples.

Age of first use has been remarkably consistent among treated heroin users. Figure 1 shows the age distribution for three samples: addicts entering Federal treatment programs in 1969, 1971, and 1973. Although these data *do not represent the same groups of programs* (as a result of the great increases in Federal support over the period), age at onset has changed little. For all 3 periods the modal age is 18 and about two-thirds of all addicts began use between ages 15 and 21.

The race distribution of treated heroin users varies with the sample being considered (unlike age at onset). The Texas Christian University's Institute on Behavioral Research (TCU/IBR) sample[2] shows a fairly stable pattern of white and nonwhite admissions since 1969:

	White	Nonwhite	Sample size
1969	25.0%	75.0%	1,147
1970	21.4%	78.6%	4,237
1971	24.8%	75.2%	9,922
1972	20.5%	71.5%	11,371
1973	26.7%	73.3%	2,906

However, by contrast, total admissions to the CODAP program (which subsume the TCU/IBR sample and which are much more representative of the nation as a whole) show a significantly higher percentage of white heroin addicts admitted in 1973:

	White	Nonwhite	Sample size
TCU/IBR sample	26.7%	73.3%	2,906
COOAP sample	36.9%	61.1%	25,777

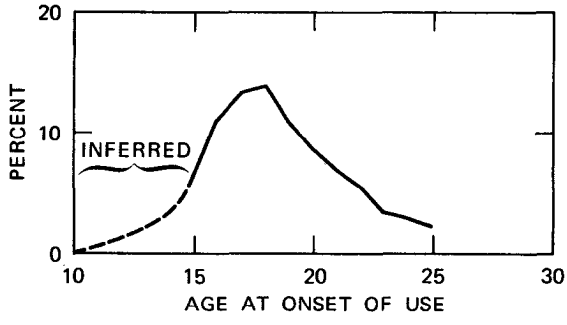
The underrepresentation of whites in treatment programs (relative to their proportions in the general population) is hard to interpret. It does not necessarily mean that heroin use occurs at a lower rate in white populations. Given the specific minority focus of many urban treatment programs (more typically in the TCU/IBR sample), it would be surprising if many middle-class white addicts were accessible to them, and, if they were, that these programs would be acceptable to them.

Sex distributions have a slightly different pattern. The TCU/IBR sample shows a gradually increasing percentage of women among admitted heroin users:

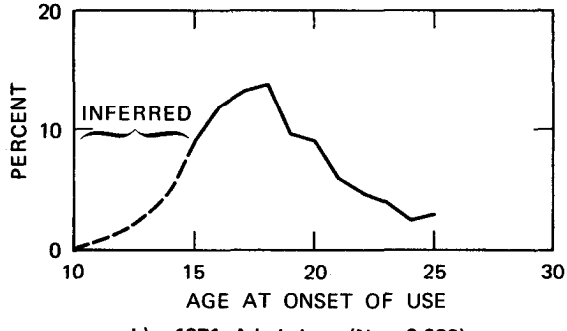
	Percentage		Sample size
	Men	Women	
1989	83.0	17.0	1,147
1970	82.0	18.0	4,237
1971	79.3	20.7	9,922
1972	75.2	24.8	11,371
1973	72.0	28.0	2,906

But, unlike racial distribution, the TCU/IBR and national CODAP samples are fairly similar in 1973:

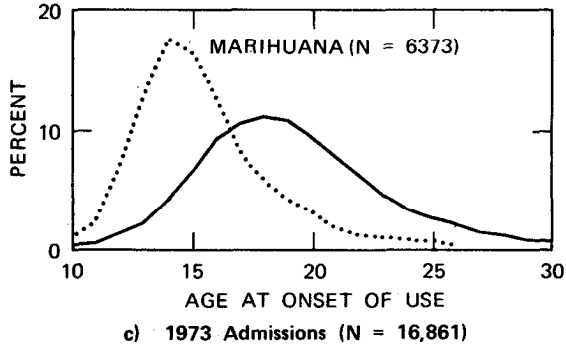
	Percentage		Sample size
	Men	Women	
TCU/IBR sample	72.0	28.0	2,906
CODAP sample	76.0	24.0	25,777



a) 1969 Admissions (N = 1,147)



b) 1971 Admissions (N = 9,922)



c) 1973 Admissions (N = 16,861)

Figure 1. Age at onset of heroin use. Sample of clients admitted to Federal Treatment Programs.

Again, it is doubtful whether these data represent an accurate measure of the relative prevalence of heroin use among men and women, since the same type of biases that influence racial distribution also apply to sex. What may be significant is the gradual increase in the percentage of women shown in the TCU data. While this change might be interpreted as the results of efforts to make treatment more acceptable to women, it could also mean a relative growth in the population of female addicts. Bejerot (1975) has described the increasing use of a drug by women as evidence of its overall penetration of a society. As the percentage of female users approaches 50%, true "endemic" use has developed. The growing fraction of female heroin users in treat*ment* is certainly consistent with Bejerot's observation, and serves to corroborate other data on the spread of heroin cited above.

The Effects of Treatment Modality

As usual, our efforts to understand even the most rudimentary characteristics of users (age, race, and sex) are confounded by the nonrandomness of the user samples from which we work. The difference in race distribution between the samples described above may be multiplied, and indeed grow more radical, as we descend into more local and specific treatment data. Here extraordinary differences among addicts emerge that are related to treatment modality.

In its evaluation of treatment programs, the Johns Hopkins University School of Hygiene and Public Health, Interdrug Project, studied 16 different programs representing 21 different treatment regimens (some programs employed several modalities) and over 12,000 patients throughout the country (Johns Hepkins, 1974). These clients presumably represented the programs' entire intake since their inception.

Though the population is not restricted to heroin users, it does consist principally of heroin addicts. Their characteristics are generally similar to those described by the data discussed above: typical age at first use of heroin was around 18 or 19, and the users are predominantly male. But the race distributions are totally unlike among the different regimens; in particular, white addicts are least numerous in methadone maintenance programs (Table 1).

The contrasts are even sharper among the drug using and criminal histories of the clients in each class of programs. Methadone maintenance addicts are older with longer histories of heroin use and greater arrest records.

Such differences could scarcely occur by chance in a sample of this size. These data, combined with readmission rates, suggest that methadone programs are different from other classes of treatment. They serve principally older, long-addicted persistently criminal, nonwhite heroin users. Their clients drop out and reenter treatment at high rates. These characteristics have the appearance of the stereotypic hard-core addict.

However the characteristics are interpreted in terms of treatment outcome, they certainly describe very different kinds of heroin users. The similarities of programs *within* a class, compared to the differences *between* classes, suggest that the regimens are themselves exercising a selective effect on their clients: young, recent users enter methadone programs less often than other types of programs, and, conversely, experienced, long-term addicts tend to choose methadone programs, perhaps to guarantee their supply of an opioid drug.

TABLE 1

Treatment Categories (Mean Values Within Modalities)

	Methadone maintenance	Therapeutic communities	In- and outpatient regimens	Ambulatory abstinence/other
Regular heroin users	98.9%	93.1%	98.7%	95.5%
Median age of onset of heroin use	19.3	10.5	18.5	19.0
Race distribution:				
White	33.7%	53.7%	65.8%	44.5%
Black	44.0%	41.2%	27.2%	45.7%
Hispanic	21.4%	4.1%	6.3%	9.8%
Oriental/other	1.0%	1.2%	—	—
Male	83.1%	76.0%	61.0%	77.8%
Median age (at entry into program)	27.1	23.0	22.2	20.4
Median length of regular heroin use—years	8.6	3.2	3.6	5.8
Ever arrested	95.8%	39.3%	94.4%	96.8%
Median number of arrests	10.3	4.3	5.0	8.0

If modality (or regimen) does exert this selective effect it is still further proof of the specialness of treatment populations. Not only is there a difference between treatment populations and other groups of users, but users in the different classes of treatment are likely to be uniquely matched to their regimens, hence unrepresentative of any larger population.

Thus, in generalizing from any set of treatment data it is important to include as many classes of treatment facilities as possible to better represent the entire range of addicts who have responded. Conversely it is dangerous to infer general user characteristics from highly selective groups such as methadone clients.

A Related Population

Another quite different group of heroin users is composed of those who seek medical assistance or advice as the result of a crisis caused by a drug. These episodes are collected from hospital emergency rooms and in-patient units, from county medical examiners and coroners, and from crisis intervention centers by the Drug Abuse Warning Network (DAWN), an information system sponsored by the U.S. Justice Department's Drug Enforcement Administration, DEA (1974a) in cooperation with the National Institute on Drug Abuse (NIDA).

DAWN samples drug episodes throughout the country. The structure of the samples is complex, consisting of a "saturation panel" of all emergency rooms in 20 standard metropolitan statistical areas (SMSAs) and all medical examiners in 23 SMSAs, and a "national panel" of 200 emergency rooms and medical examiners outside the 23 saturated SMSAs. Together these two panels are extrapolated to national estimates.

DAWN records any crisis or death involving drugs, but since several drugs are often implicated, it reports also "mentions" of each drug. Most analyses of the reported data are done in terms of these mentions. From July 1973, through February 1974, DAWN reported 9,442 heroin mentions (DEA, 1974a). The race distribution of these individuals having crises in which heroin was mentioned is remarkably different from that of heroin addicts in treatment:

Whites	57%
Blacks	30%
Other	5%

For both whites and blacks, males constitute about 70% of the total. Median age lies between 20 and 29 years for all black/white/male/female subgroups. Someone judging from DAWN data, without knowledge of the much larger sample of treatment demography previously discussed, might conclude that most heroin users are white males, and that nonwhites are represented at only 2 or 3 times their overall frequency in the general population. We know this to be improbable, based on the great preponderance of nonwhite addicts (about 73% in 1973) in the overall treated population.

Characteristics of Criminal Users

Next in size to the group of treated heroin users is the user population known to the police. It includes individuals arrested specifically for drug law violations as well as those involved in other crimes, such as burglary, who are discovered to be users,

either through urinalysis, possession of drugs or drug-using paraphernalia, or by showing needle marks. Besides records of persons arrested, many police departments also maintain intelligence files that include known users reported by undercover agents working in copping areas. Collectively, these groups may be called the population of criminal users. Even though many have not been convicted of any crime, all have been individually identified as heroin users by some law enforcement process.

Criminal users are not necessarily the same persons as those in treatment. Many users entering treatment have criminal records (as high as 98% in some methadone programs); conversely many criminal users have been in treatment, but the overlap of the two populations is not usually high. For example, a recent 5-year prevalence study in Phoenix, Arizona, comparing police intelligence files to central treatment intake records showed only 18% of the treated individuals included in police records (Drug Abuse Council, 1975). This condition is represented in the Venn diagram, Figure 2.

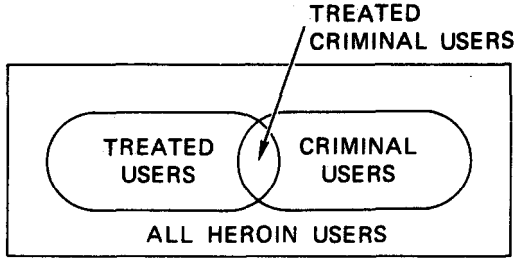


FIGURE 2. Treated criminal heroin users

The general lack of identity (as far as is known) between those two populations has several interesting consequences. It is the basis of the most accurate methods of calculating prevalence of heroin use-the recapture techniques first employed in field ecology to estimate animal populations. More generally, it means that criminal users offer another, largely independent perspective in considering the question of who is the heroin user.

The largest national file of criminal drug users is the DEA's Drug Abuse Statistics System, which is a record of users reported by local police departments. Contributors to this register do not constitute a representative sample of police departments throughout the nation; in recent years reporting has been increasingly confined to a few large cities. Thus the relationship of the DEA register to criminal heroin users in general is much the same as the TCU/IBR treatment sample to all treated users: both have strong big-city bias.

Demography of DEA register narcotics users is given in Table 2. The population is slightly broader than just heroin addicts, as it includes also users of related drugs such as morphine, opium codeine, and synthetic narcotics like methadone, dilaudid, and demerol. Active users are defined by DEA as those arrested within the last 5 years.

Their gross demography (Table 2) is strikingly different from that of treated heroin users. Most surprising is the constant 15% of female users, as opposed to the growing fraction of women in treatment recorded by TCU/CODAP (17% to 28% from 1969

TABLE 2

Active Narcotic Users. Drug Enforcement Administration

	Number of active users					
	12/31/70	12/31/71	12/13/72	12/31/73	6/30/74	9/30/74
Total	68,864	82,294	95,392	98,988	92.416	90,854
Net Change (%)	--	16.32	14	3.63	-6.64	-169
Sex (%)						
Male	84.87	85.38	85.10	84.57	84.41	84.36
Female	15.13	14.82	14.90	15.43	15.59	15.62
Race (%)						
White	51.22	48.97	46.51	45.20	44.21	43.90
Black	48.43	50.78	53.23.	54.54	55.53	55.85
Other	.35	.27	.26	.26	.26	.25
Age (%)						
< 18	.78	1.71	1.46	.74	.97	1.05
18-22	17.58	21.97	23.01	20.37	22.72	23.35
23-30	41.57	42.66	46.01	50.03	49.82	49.34
≤31	40.09	33.66	29.52	28.86	26.89	26.26

Source: System Sciences (1975).

to 1973). Also note race changes: starting in 1970 with nearly equal percentages of whites and blacks, the DEA white population shrank to 44% in 1974, while the TCU addict file contained whites at a steady 25% during about the same period. However, the 1973 CODAP treatment population, which is much larger than the TCU sample, showed whites rising to 38% approaching the 45% in the DEA register.

DEA addicts have grown progressively younger, probably from the inclusion of more and more young arrested addicts. By contrast, the age distribution of first use of heroin in TCU/CODAP remains unchanged. This difference may mean police have shifted their attention from older "hard core" addicts to newer and younger users as time has passed. Alternatively the change probably shows that the older pool of hard core criminal addicts has been, since 1970, progressively diluted by younger addicts whose first use of heroin dated from the big-city epidemics of 1968 through 1972. It is almost certain that the average age of all active users has declined for this reason, and the same explanation is likely to fit criminal users as well.

The demography of the DEA register seems to be different from the characteristics of *all persons* arrested on heroin charges. Total heroin arrests exceeded the population of the register during the early 1970s but have begun to fall in the last 2 years; as shown in Table 3.

TABLE 3

Total U.S. Heroin Arrests. 1968-1974

Calendar year	State heroin and[a] cocaine arrests	Federal[b] heroin arrests	Total
1968	42,328	--	42,328
1969	67,945	--	67,945
1970	107,427	1,104	108,531
1971	114,573	1,223	116,498
1972	92,364	2,159	94,523
1973	—	2,077	67,794
1974	—	1,963	--

[a]U.S. House of Representatives, 1974.
[b]DEA. 1974(b).

Once again, these are not exactly the same individuals as those represented by the DEA register, though here the overlap between the two populations should be much greater than between the register population and treated heroin users. In spite of the closer identity between the two criminal groups, they are not alike. We do not know the exact demography of arrestees, since there has been no analysis of all arrest reports (which is technically possible through the FBI uniform crime data). However, a sample of opiate arrests from New York, Chicago, Los Angeles, Dallas, Washington, D.C., and Miami during the period July through December 1971, yielded 85% males, 62% blacks, 19% whites, and 19% Spanish surnamed (Johnson and Bogomolny, 1973). Table 4 shows the DEA registrants (who have much the same metropolitan sampling bias) at about the same sex distribution but around 5 1% black and 49% white (Spanish surnamed are not shown at all). Age distributions are also different. Those arrested on opiate charges are even younger than DEA registrants and much more heavily concentrated among minority populations.

TABLE 4

Age Distribution of Heroin Users, in Percent

Age	DEA register (12/31/71)[a]	Age	Opiate arrest sample (7-12/71)[b]
Under 19	1.71	Under 19	7.00
19-22	21.97	19-22	25.00
23-30	42.66	23-30	36.00
Over 30	33.66	Over 30	31.00

[a]N=82,294
[b]N=6,844

Characteristics of Survey Populations

Conceptual Issues

Surveys—both of the general population and of special populations-are among the most widely used methods of estimating the size and characteristics of drug-using groups. They are the familiar tool of social scientists; they are relatively cheap and easy to do; and their results have an appearance of quantitative precision. Although efficient in describing some forms of drug use, surveys have not been very successful in estimating numbers and characteristics of heroin users.

As we have noted throughout this discussion, heroin users are, in one sense, criminals, and most of them regard their drug-using activity in this way. They do not willingly discuss it; certainly most do not advertise it or even admit to it unless some compelling reason, such as need for treatment, overcomes their natural caution. Heroin use is obviously not the sort of characteristic that is easy to study in a voluntary survey, where guarantees of anonymity are always suspect.

There is another more compelling technical reason why general population surveys of heroin use are unsuccessful. It is the problem of access. No survey can accurately describe the distribution of a characteristic in a population unless it is based on a probability sample. This condition means that the surveyor must know the probability that any individual (or kind of individual) is in his or her sample. In the simplest design, the pure random sample, the probability for all individuals of being chosen for the sample is the same.

The demands of probability theory founder on the vagaries of real populations. For instance, the first step in designing a probability sample for a survey is to obtain an enumeration of the population. How? There are no lists of everyone. The sample designer inevitably falls back on some surrogate of a complete enumeration, such as telephone directories (which list most telephone subscribers) or households (which physically define collectives of individuals living in houses or other dwellings).

There is no reason to suppose that the distribution of heroin users is the same in these populations as in the general population. Intuitively we suppose just the opposite. If heroin users are concentrated in disadvantaged socio-economic groups, then the population of telephone subscribers (for example) systematically excludes them because telephones are underrepresented (per capita) in lower income groups.

The sampling problem is even more complex when households are used as the sampling frame. There are two questions that affect the randomness of household-based sample designs.

1. Is a heroin user as likely to have a stable, clearly defined address as any other person? The stereotypes of the dysfunctional addict characterized as the criminal junkie or the hippie drug-culture drifter suggest that such individuals would be less likely to live at a determinable location that could be sampled than would a member of the general population. These stereotypes may themselves be of dubious validity, and, even if they do characterize some heroin users, they may not be numerically significant. Thus this effect is obscure, but some data do exist that suggest that heroin users are difficult to find. A sample of 197 street users (interviewed by ex-addicts or professionals working with addicted populations) were followed at 60-day intervals during 1973 in inner city locations in Washington, D.C., Detroit, and Los Angeles. After 6 months, 30% could no longer be located (Resource Planning Corporation, 1974).

2. What is the effect of the distribution of individuals within households? Consider the following situation, as illustrated in Figure 3: if a house is randomly chosen from a block containing 10 houses, and a single individual sampled from the chosen house, then the probability of choosing a person from that block depends on the distribution of persons within houses. If distribution of individuals is uniform among houses, then such a procedure yields a random sample, e.g., if each house contained two people, then each person on the block would have a uniform .05 probability of being chosen, since any house has an equal probability equal (p = .1). However, if individuals are not uniformly distributed in houses, then this procedure does not yield equal probabilities of choice. Suppose one house contained 10 individuals, while the other nine had one each.

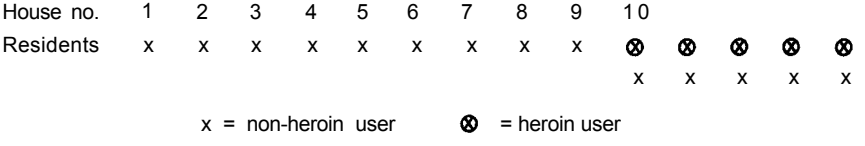


FIGURE 3. Sample distribution of individuals within households.

Each person in houses 1 through 9 would have an equal probability of selection (p = . 1), but the 10 persons in house number 10 would each have a selection probability of .01.

Apart from this nonrandomness in selection (which applies to any household design), there is the additional issue of clustering. Drug users in whose lives drugs play a significant role tend to associate with one another, and may even cluster in domiciles. Suppose, for example, that five of the 10 individuals in house number 10 were active heroin users. Then the true frequency of users in the block illustrated is 5/19. A little experimentation will show that it is practically impossible to design a household sample (of reasonable size) to estimate this frequency with any accuracy.

These numerical examples are not to be taken literally. They are intended to illustrate the principles that limit the accuracy of general population surveys of some forms of drug use. How serious sampling bias may be is not known. For assessing use of illicit drugs they are probably not significant in special population surveys, where individuals can often be enumerated and sampled directly.

In a few cases it is possible to check survey results against independent measures of drug use. Some such comparisons are discussed below.

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Survey Results

Thousands of surveys of drug use have been made during the last 10 years. Most have been nonrecurring examinations of special populations-youth, schools, labor groups, and populations of cities and states. A few have been longitudinal-i.e., the same or similar populations have been resurveyed at later times to obtain changes in drug use in the resurveyed group. A few have been general population surveys that purport to measure drug use in the whole country.

Perhaps the most widely reported general population survey was done in 1972 for the National Commission on Marihuana and Drug Abuse (1973). Based on a nation-wide sample of 2,411 adults (over 17) and 880 youths (12 to 17), the Commission reported that 1.3% of adults and 0.6% of youths had ever used heroin, and that about 0.5% of adults and 0.5% of youths had used heroin in the past year. These rates imply the prevalence in 1972, shown in Table 5.

TABLE 5

Heroin Use Among Adults and Youth, 1972

	Ever used	Current *users* Within the lest year)
Adults	1.817,000	699,000
Youth	149,000	125,000
Total	1.966.000	624,000

Source: National Commission on Marihuana and Drug Abuse (1973)

The Commission report also noted:

> The Commission emphasizes that household surveys do not generally pick up "street" users of cocaine and heroin and therefore underestimate both incidence and prevalence of use. (p. 69, footnote)

In contrast to the data from its national survey, the Marihuana Commission also reported composite results for about 200 school and college surveys representing more than 900,000 students. These yield longitudinal estimates for the percentage of students at various educational levels who have ever used opiates, as shown in Table 6.

TABLE 6

Mean Percentages of Students Who Have Ever Used Ooiates

	1967	1968	1969	1970	1971	1972
Junior high	--	_	2.2	3.55	2.1	4.75
Senior high	0.4	1.7	3.3	3.3	4.0	5.2
College	--	3.2	5.14	4.26	3.4	6.0

Source: National Commission on Marihuana and Drug Abuse (1973)

Apart from the fact that these percentages are larger than the percentages for "youth" reported in the national survey, the interesting property of this table is that percentages generally increase both across each row and down each column. During the same year, successively older students showed higher rates of use, and from year to year the *same level* of students also showed increasing rates of sometime use. Since neither longitudinal (year-to-year) nor transverse (within a year) school populations are homogeneous, these data display a general tendency for sometime opiate use to grow during the period.

The category "ever used" necessarily increases with age if there is *any* continuing new use of opiates. The differential increase from year to year would represent incidence of new use if the populations were either constant or, at least, randomly selected. Thus the decline in percentage of those who ever used opiates among junior high students from 1970 to 1971 is either a statistical fluctuation or else the result of the dilution of the junior high population by a newly (1971) entering class that had fewer drug users.

The incidence of new use implied by these data is presented, in crude terms, in Table 7.

TABLE 7

Mean Annual Increases of Opiate Use for School Populations, in Percents

	1967	1966	1969	1970	1971	1972
Junior high	--	--	--	+1.35	-1.45	+2.65
Senior high	--	+1.3	+ 1.6	0	+0.7	+1.2
College	--		+1.94	+1.12	-0.86	+2.6

Adjusting for the atypical decline in 1971 (which may be due to the sample of 1971 surveys), these increases average to a steady 1.2% increase per year. That is, about 1.2% of the *total population* of junior high through college students began to use opiates during the 1967 to 1972 period. This rate of increase must have yielded at least a 6% rate of "ever used" among this age cohort.

These results are roughly consistent with the age at onset distribution shown earlier (in Figure 1) for users entering treatment during the period 1969 to 1973. Those distributions showed most new use (more than 80%) occurring within junior high and college ages. Here we see a mutual corroboration of the two sets of data on age at onset distribution, as the percentage of opiate use mounts steadily through the school years.

Intermediate between the prevalence figures of the Marihuana Commission's national survey and its composite school surveys are those reported by Johnston (1973) from the Youth in Transition Project at the University of Michigan. A national panel of boys was followed from the fall of 1966 to spring of 1970—typically from the 10th grade to a year past high school. Of the final sample of 1,798, 1.8% had used heroin during high school, and 2.2% had used it after a year out of high school. These rates are comparable to the senior high school percentages of 1.7% for 1968 and 3.3% for 1969 given in Table 7. Allowing for the unknown structure of the Marihuana Commission's 200 survey composite, the results are extraordinarily similar.

In subsequent longitudinal studies (1975) of the same population, Johnston has estimated that the percentage of individuals ever having used heroin increases linearly from 1.1% at age 18 to 5.7% by age 23. Throughout this period, about half of the group of sometime users have had heroin within the last year. Again these results are roughly consistent with the Marihuana Commission's findings, since recent (1972) college populations show about 6% ever used.

A more recent (1974-1975) national survey of the general population undertaken by the Social Research Group, George Washington University, for the National Institute on Drug Abuse (Abelson & Atkinson, 1975) obtained results similar to the Marihuana Commission's. This survey found 1.3% of adults (18 and older) had ever used heroin

and 0.3% had used within the last year in a sample of 3,071. Similarly 1.0% of youths (aged 12 to 17) had ever used, and 0.4% had used within the last year in a sample of 952. These results are statistically indistinguishable from the Marihuana Commission's findings.

Significance of Survey Results

The conceptual problems outlined above, along with the explicit warnings of many survey practitioners, suggest that most surveys systematically underestimate levels of heroin use. It is seldom possible to assess this understatement with any real quantitative precision, but when comparisons can be made between survey results and other, more definitive prevalence measures, the differences are striking.

Chambers and Inciardi's (1971) household survey in 1970 found that about 9% of black males and 1% of white males between the ages of 14 and 34 in New York City had used heroin in the preceding 6 months. During the same period, urinalyses taken in connection with preemployment physical examinations by the New York Telephone Company showed the following percent positive distribution (Dupong, 1971):

	White (N = 7,772)	Nonwhite (N = 3,069)
Morphine	0.4	2.4
Quinine	3.6	12.1

Quinine is the usual diluent of street heroin in New York. It is also relatively more persistent in the body than morphine. (An average dose of heroin is detectable as morphine to thin-film chromatographic techniques for 24 hours or less, while the associated quantity of quinine shows up for 5 days or more.) Quinine is uncommon in similar populations from areas where it is not used to cut heroin. For example, Pacific Bell Telephone applicants from Los Angeles (where quinine is not used as a diluent) show a 0 to 0.3% quinine positive rate. Thus there is a strong implication that most of the quinine positives in the New York City tests were actually heroin users.

If quinine is a valid and persistent surrogate for morphine in these circumstances, the prevalence of *weekly* use becomes 4% for whites and about 14.5% for nonwhites in New York City in 1970, compared to 1% for whites and 9% for blacks reported in the household survey. It can scarcely be argued that the telephone company applicants are a more drug-prone group than their general population counterparts. They are all high school graduates who were otherwise qualified for employment. Thus we conclude that the survey actually understated prevalence of active use by *at least* 50% to 400%, and probably much more, since the urinalyses identified only weekly or more frequent users, while the survey dealt with use in the last 6 months (a larger class).[3]

Characteristics of Users from Disease Populations

Heroin use is sometimes observed as a consequence or correlate of some other condition. For example, serum (type B) hepatitis is transmitted by contaminated needles, in blood transfusions, and injections of all types. Needle sharing among intravenous drug users has led to frequent cases of serum hepatitis in these groups.

A quite different population is found among psychotics who are incidentally drug users. As between treated users and criminal users, both of these groups offer different perspectives on heroin users since they represent highly selective samples of users, some of whom may be otherwise unknown.

Serum Hepatitis

Incidence of serum hepatitis cases has been popular as an indirect indicator of heroin use in spite of the formidable problems of interpretation that it poses. These problems are of two sorts: (a) Serum (type B) hepatitis is hard to distinguish from the more prevalent infectious (type A) cases; accuracy of diagnosis is known to be poor and more important, to be uneven in different places at different times, so that type B cases are differentially underreported. (b) Many serum hepatitis cases are *not* the result of drug use, and, of those that are, some are connected with other intravenous drugs such as amphetamines and barbiturates.

Either problem is sufficient to render hepatitis data doubtfully relevant to heroin use. However, the first problem, that of separating type A and B incidence has been circumvented by noting that the age distribution of cases of the two diseases are sharply different. Incidence of infectious hepatitis, transmitted through the mouth by contaminated food, water, or any object, is highest among children aged five to nine. Serum cases typically occur much later, chiefly between 15 and 29. After subtraction of the "natural" age distribution of type A cases from the total reported, the residue is inferred to be type B (Minichiello, 1974).

A study of the more than 30,000 diagnosed plus inferred type B cases reported nationwide in 1973 by the Center for Disease Control shows that they comprise 79% whites and 21% blacks, and 59% males and 41% females (Minichiello, 1974). The race distribution is strikingly different from that of any positively identified heroin-using population discussed above. It is closer to the racial composition of the general population (about 11% blacks in 1973).

How many of these hepatitis cases actually represented heroin users? There is no direct answer to this question, but recent studies on the distribution of type B hepatitis antibodies in the general population offer some suggestions. Some investigators believe that type B has become virtually endemic in the U.S. population in the last few years: perhaps as many as 8 to 10% of the total population may be positive for type B antibodies, indicating exposure to the virus. The reason for this widespread exposure seems to be that type B hepatitis is really transmitted in a greater variety of ways than was formerly understood. .'Virtually any route of transmission including oral-enteric contamination is possible. So, far from being a disease of dirty needles, type B hepatitis may be spread more often by kissing than by drug abuse.

Empirical data supporting widespread exposure to the type B virus casts real doubt on the utility of hepatitis case rates as an indicator of drug abuse. For example, Phoenix, Arizona probably has had 8,000 or 9,000 heroin users in a population of about a million, within the last 5 years. Thus, the 5-year active prevalence rate is around 1% in the general population. Use is, however, disproportionately concentrated in the relatively small black and Spanish-speaking minorities. If type B hepatitis exposure were a good indicator of intravenous heroin use, we might expect both these groups to show relatively high rates of type B antibodies.

Random samples from volunteer blood donors in Maricopa County (the Phoenix SMSA) in 1974 show extraordinary differences between the percentages of individuals having type B antibodies.[4] For all ages the rates were:

	N	Prevalence
Blacks	337	11.9%
Orientals	231	9.5%
Indians	57	5.7%
Whites	5,592	3.3%
Mexican-Americans	160	2.5%

Since the prevalence of type B antibodies continues to increase with age (due to cumulative exposure), the rates in the principal heroin using groups (ages 20 to 24) are more relevant:

	N	Prevalence
Blacks	176	8.0%
Orientals	29	6.9%
Indians	122	4.9%
Whites	2,420	3.1%
Mexican-Americans	84	1.2%

Thus, the two groups having the highest *rates* of heroin use (as measured by arrests and addicts in treatment), blacks and Mexican-Americans, show the greatest variation in the presence of type B antibodies. These differences suggest that hepatitis case rates in these two groups would be a poor indication of relative prevalence of intravenous heroin use.

Mentally Ill Populations

Prior to the establishment of widespread drug treatment facilities in the early 1970s, mental hospitals and community mental health centers provided much of the public treatment received by drug users. During this period, there was necessarily some mixing of normal and psychotic heroin users, but there is also reason to believe that many normal addicts deliberately avoided such programs because of the stigma of mental illness associated with their clients. For example, Nurco (1969) found that less than 30% of the "hard-core" addicts known to the Baltimore, Maryland City Police Department (between December 1966 and November 1968), had been treated in Maryland state mental hospitals, These addicts were also strikingly different in their characteristics from drug users listed in the Maryland Psychiatric Case Register (though the proportions of heroin to other drug users were not alike in the two groups).

Inferred Prevalence Estimates from Combined Data Sources

Up to this point we have discussed heroin-using populations that were defined by some specific process, i.e., treatment, criminality, surveys, associated diseases, etc. The lack of identity among these groups implies the presence of a larger population that contains each "marked" group as a subset. A simple model will explain this relationship.

Suppose one wished to estimate the number of marbles in a large irregularly-shaped container without actually counting them. One way to do this would be to take a handful of the marbles and mark them in some distinctive way (say by painting them red), return them to the container and thoroughly mix the whole lot, and then take a second sample and count the proportion of red marbles. This proportion would be an estimate of the ratio of the original marked sample to the total population, N:

$$\frac{\text{all marked marbles (first sample)}}{\text{total marbles N}} \approx \frac{\text{marked marbles in second sample}}{\text{total second sample}}$$

This equation can be solved for the size of the total population, N. Of course, the result is only an estimate because the right-hand ratio fluctuates according to chance and seldom exactly matches the left. We believe this model may be roughly applied to estimating heroin users (subject to some statistical limitations): the appearance of an addict in one population is equivalent to the first marking sample, and his or her reappearance in another group is equivalent to the second, the recapture sample (so termed because the marking and recapturing technique was first applied to esti-. mating animal populations, in which the samples were literally trapped). A necessary condition for an unbiased estimate is that the second sample be random, i.e., that any member of the total users have an equal chance of being chosen. This condition is seldom fulfilled in practice.

Greenwood's Prevalence of Narcotic Addicts

Recapture estimates of the number of heroin addicts were first made by Dr. Joseph Greenwood of the Drug Enforcement Administration (DEA) [then the Bureau of Narcotics and Dangerous Drugs (BNDD), U.S. Department of Justice], in 1971. Greenwood used an addict's inclusion on the DEA (BNDD) register during one period as his marking process, and reappearance at a later time as recapture. Because of numerous adjustments required by addict mortality, incarceration, "maturing-out" and so on, Greenwood's method is actually much more complex than the simple example described above, but its essence is the same.

The chief conceptual flaw in this technique is that the recapture sample is not random, that is, the probability of a user being arrested and included in the second sample is not the same for all users. Obviously there is a greater chance of rearrest for a known criminal user than there is for any other addict not engaged in criminal activity (apart from drug use).

The consequence of this higher probability of rearrest of an already arrested addict is to increase the size of the recaptured criminal group, relative to other users. More "marked" addicts are recaptured than would be if the second sample were actually random in the user population. The result is to underestimate the size of the total population. (In algebraic terms we are dividing an integer—the number of marked addicts—by a number between zero and one. As this divisor approaches one, our estimate grows smaller and smaller.) Greenwood (1971) noted this flaw in his original paper:

> The addict with a previous infraction may therefore be under . . . police surveillance. Compared with addicts having a previous report, addicts with no report would seem as a class to have more propensity to avoid being apprehended. (p. 8)

Today we are able, as a result of widespread drug treatment, to demonstrate how much Greenwood's estimates have understated the population of active users. His first figures for the number of addicts were broken down by cities; in 1969 Washington, D.C. was calculated to have 4,600 addicts and San Francisco, 4,200.

By examining treatment data on year of first use, a minimum number of proven addicts, up to a certain time, is established merely by summing incidence. For example, Figure 4 shows year of first use for Washington, D.C. heroin users treated by the Narcotics Treatment Administration. As of October 1974, we can see that *at least* 9,030 heroin users must have been active in 1969, because that many have entered treatment since 1970 when NTA first opened. Thus, Greenwood's estimate of 4,600 users was about half of what *we* now know to have been the *minimum* number of active users in Washington, D.C. in 1969 (since others, who did not enter treatment, are not included in this estimate).

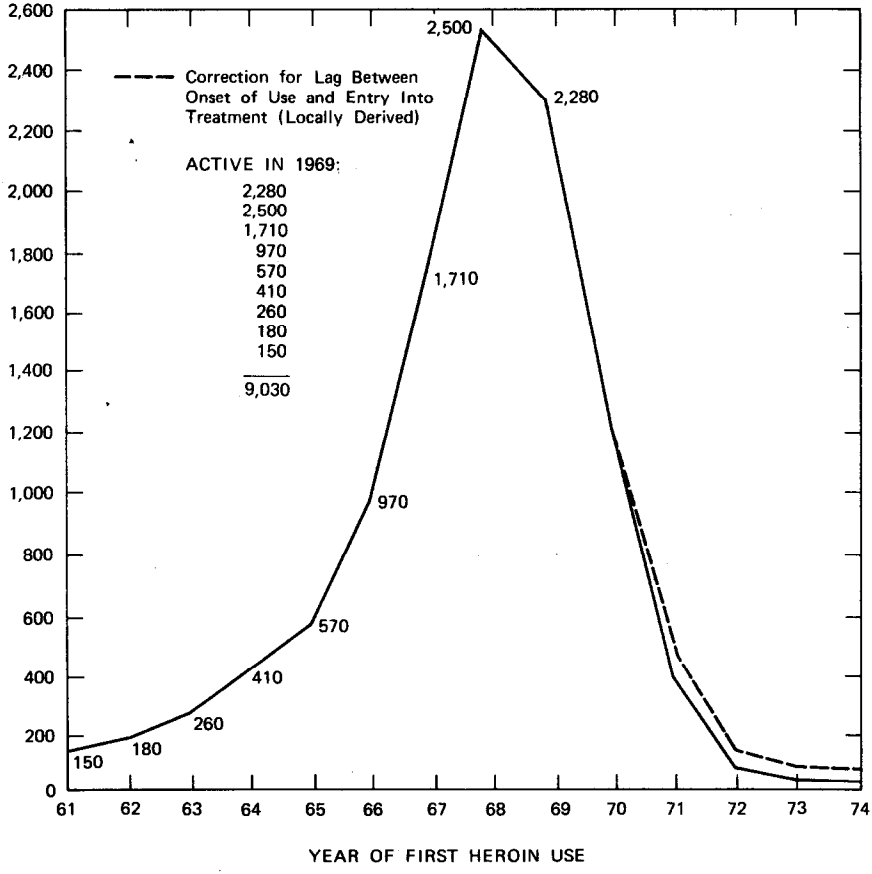

FIGURE 4. Year of first heroin use as reported by first admissions to the NTA treatment program, 1970-1974. 1974 data complete through October 1974 and projected to an estimated annual total (after data from Mark Greene, 1975.)

The same comparison for San Francisco data from Newmeyer (1974) at the Haight-Ashbury Clinic shows about 7,500, of which Greenwood's 4,200 addicts represent about 55%. Thus, if Greenwood's estimates account for about *half of the subsequently proven minimum number of active addicts,* a crude rule of thumb might be to double his figures to obtain a less biased estimate. But even this correction is not sufficient, since other studies show that substantial numbers of addicts are not known to treatment systems.

Hidden Populations

Some attempts have been made either to discover or to estimate the population of heroin users who have not been identified by any institution-such as the police or a treatment agency. These are the same "hidden" users whose presence is implied by recapture estimates.

Project DUSK undertook in 1973 to sample active street users sought out by ex-addict interviewers or professionals engaged in working with addicts. In Washington, D.C., Los Angeles, and Detroit, 137 heroin users were identified, and of these, about half had been arrested but only 30% had been in treatment (Resource Planning Corporation, 1974). While DUSK sampling was not in any sense random (interviewers simply found whatever users they could through personal contacts), the institutionally "hidden" fractions are strikingly high.

From an entirely different perspective, independent recapture samples were used to estimate the 5-year prevalence of active users in Phoenix, Arizona at about 8,500 (Drug Abuse Council, 1975). Of these, only 3,000 were known to the treatment system, and only between 1,500 and 3,000 to the police. Thus, about 2,500 to 4,000 users were at large in the community unobserved by any institution during the 5 years, 1970 to 1975.

Both these results, though purely anecdotal, suggest that about a third of the active users in four large drug-using communities (Washington, D.C., Detroit, Los Angeles, and Phoenix) were identified by their treatment systems at a given time. Thus, while cumulative incidence data from treatment programs require that we double Greenwood's estimates, these "hidden user" data suggest tripling treatment figures, i.e., multiplying Greenwood's numbers by 6.[5] The problem is not so simple, of course, since there is no proof that hidden users are distributed in their dates of onset of use in the same way as treated users. In fact, we know from lag data that most *recent* users would naturally be hidden from treatment, and these might account for a large part of the preceding estimates.

The crucial problem in accounting for users outside treatment is the relationship between the sampling period and local incidence of new use. If the sample is taken well after the epidemic peak (several years) as it was in Washington, D.C., Detroit, and Los Angeles, then lag cannot account for the hidden population. But if the sample occurs during a period of rising incidence, as in Phoenix, many of the users at large will eventually show up in treatment of their own accord, and thus reduce the ratio of the untreated group to total users.

We might conclude that these results are ambiguous, if there were not a second line of evidence (however thin). The understatement of prevalence by general population surveys, as indicated by the New York City urinalyses, will be systematically employed below to test the hidden population ratios.

Prevalence of Active Use in the United States

Sizes of heroin using populations discussed are summarized in Table 8. Even if summed (though they are certainly overlapping to a degree), the sizes of these positively identified groups (treatment populations, arrests, and DEA registrants) do not approach Greenwood's estimates, which are below general population estimates, which themselves are certainly too small for the reasons shown above.

TABLE 8
Indicators of Active Heroin Users or Addicts in the United States
(Thousands, rounded)

	1966	1969	1970	1971	1972	1973	1974
Greenwood's number of narcotic addicts		315	524	559	626	602	725
DEA register of active narcotic users			69	82	95	99	91
Heroin and cocaine arrests	42	68	109	116	95	68	
Estimated heroin users in treatment				40	88	133	115
Marihuana Commission general population survey					824		

If these estimates are corrected as just discussed, adjusting for the known undercounting in each, an entirely different series emerges. Table 9 gives these inferred estimates of active heroin users by year. They are, of course, subject to many sources of error, in particular, to the doubtful generality of the correction factors themselves (based on only a few cities), but at least they are consistent estimates that attempt to account for all the known errors discussed up to this point.

TABLE 9
Inferred Prevalence of Active[a] Heroin Users
(Thousands, rounded)

	1966	1969	1970	1971	1972	1973	1974
Corrected Greenwood estimates	1.167	1,890	3,144	3,354	3,756	3.612	4,350
Corrected general population estimates	1,056	1,640	2,724	2.908	3,296	3.132	3,772

[a]Use within the last war.

Perhaps the most interesting characteristic of these corrected estimates is their similarity, even though they are the result of entirely independent processes. The corrected Greenwood numbers (Table 9) are a consequence of adjusting the original Greenwood estimates (Table 8) by undercounting ratios based on cumulative incidence observed in treatment programs, combined with treatment underestimation resulting from hidden population calculations. In contrast, the'corrected general population survey estimates were arrived at by raising survey prevalence to account for higher levels of use proved by urinalyses of matched populations.

These estimates cannot be taken as very precise, but they are certainly closer to the truth than any of the other measures of active heroin use discussed here. Support for the higher figures comes from crude indications of the enormous size of the U.S. heroin market, as measured by seizures. In fiscal year 1974, state and federal agents confiscated about 5,500 pounds of heroin equivalents, most of which was probably intended for U.S. users. If this seized quantity amounted to 10% of the total market, it would imply about 4.5 million active users at an average consumption of 6,000 milligrams per year (pure heroin). These estimates are, of course, at least as arguable as the figures we are trying to corroborate. (In particular, DEA would probably argue that their seizures represent a larger than 10% share of the market.) They are, however, not unreasonable, and they imply an order of magnitude of users similar to the other estimates. The question is not whether there are three rather than four million, but that the number is several million rather than only several hundred thousand.

APPENDIX

Confidence Limits for Recapture Estimates

If a random sample of n individuals from a population of size N is marked and returned to the population, the probability that a second random sample of size r contains exactly k of the marked individuals is

$$q_k = \frac{\binom{n}{k}\binom{m}{r-k}}{\binom{N}{r}}$$

$$= \frac{n!\, m!\, r!\, (N-r)!}{k!\, (n-k)!\, (r-k)!\, (m-r+k)!\, N!}$$

where $N = n + m$

$k \leq n$

This is the hypergeometric distribution, and it is the mathematical basis of the recapture techniques, since the actual size of the recapture group in the second sample fluctuates according to the probabilities given by Equation 1, assuming the conditions of random sampling are fulfilled.

Equation 1 is also the basis for calculating the confidence limits on a recapture estimate. We know that the best estimate of the population size N is

$$\hat{N} = \frac{nr}{k} \quad ,$$

in the sense that this estimate has the highest probability (i.e., it is the Fisher maximum-likelihood estimate). However, even though it is the most likely value, it tells us nothing of the probabilities corresponding to any range of value in which N might lie. To determine such a range, it is only necessary to find a lower limit, N_1, such that

$$\sum_{i=0}^{k} q_i(N_1) = \sum_{i=0}^{k} \frac{\binom{n}{i}\binom{m_1}{r-1}}{\binom{N_1}{r}} = p_1 \ ,$$

and an upper limit N_2, such that

$$\sum_{i=k}^{n \text{ or } r} q_i(N_2) = \sum_{i=k}^{n \text{ or } r} \frac{\binom{n}{i}\binom{m_2}{r-i}}{\binom{N_2}{r}} = p_2 \ .$$

(In the second case, for N2, the sum is taken to n or r, whichever is smaller.)

For example, if $p_1 = p_2 = 0.05$, the corresponding values of Nl and N2 would determine a 90% confidence interval on the estimate.

In practice, the evaluation of these partial sums of the hypergeometric density function are quite difficult, since they involve the direct calculation of large factorials or else Sterling approximation, either of which is onerous. For ordinary purposes, the asymptotic relation (Feller, 1957)

$$\frac{\binom{n}{k}\binom{m}{r-k}}{\binom{N}{r}} \sim h\phi(x)$$

where $\frac{r}{N} \to t$, $\frac{n}{N} \to p$, $\frac{m}{N} \to q$,

$$h = \frac{1}{\sqrt{Npqt(1-t)}} \ , \quad \text{and } h(k - rp) \to x \ .$$

$\phi(x)$ is the normal probability density for the standard deviate X. Hence

$$\int_{-\infty}^{-x} \phi\,(t)dt = \Phi\,(-x) \approx p_1 \ ,$$

and

$$\int_{-x}^{\infty} \phi\,(t)dt = 1 - \Phi\,(x) \approx p_2 \ .$$

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This paper is based, in part, on material from *The Heroin Epidemics: A Study of Heroin Use in the U.S., 1965-1975,* Part 2, by L. G. Hunt and C. D. Chambers, Spectrum Publications, Holliswood, N.Y., 1976, and on various of the author's researches sponsored by the Drug Abuse Council, the Special Action Office for Drug Abuse Prevention, the National Institute on Drug Abuse, and the Foundation for International Resources.

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[1]This estimate, if such a figure can be dignified by even that term, is based on three types of data: treatment censuses, readmission rates, and percentages of heroin users reported among total admissions. It is more or less corroborated by other data: by mid-1975, about 90,000 different heroin users were known to the Federal treatment system, but Federally-supported treatment constituted only about half of public drug treetment, and represented less in the past.

[2]The TCU/IBR sample includes data from Federally-funded treatment programs in 18 large cities: Boston, New York, Newark, Philadelphia, Cleveland, Washington,. D.C., Detroit, Indianapolis, Chicago, San Juan, Miami, Kansas City, San Antonio, Albuquerque, Phoenix, Los Angeles, San Francisco, and Seattle. All cities were not present in each year's sample. The data are taken from *Monthly Management Reports,* Executive Office of the President's Special Action Office for Drug Abuse Prevention (1973) and *Quarterly Statistical Brochures,* National Institute on Drug Abuse (1974).

[3]This comparison was first suggested by the author in 1974, but was first reported without reference in L. Dean et al., *A Quantitative Assessment of Non-Opiate Drug Abuse,* Institute for Defense Analyses, Report R-201, February 1975, pp. 4-37 4-41.

[4]Personal communication, Dr. James Maynard, Phoenix Laboratory Division, Bureau of Epidemiology, Center for Disease Control, May 1975.

[5]Greenwood argues against the use of these multipliers (2 then 3) as too high for two reasons: (a) the real abusers are likely to be arrested and the exceptions of certain protected, hidden users are trivial (a few hundred);(b) the denominator C in this formula-representing the N common to both first and second capture-are very small for each city. For example, Washington, D.C. had only 11. Thus, Greenwood maintains there is a large statistical error in some of his estimates, and multipliers based on such unreliable values are themselves suspect. The author believes that (a) is not supported by detailed studies of user populations in cities such as Phoenix. There most users were unknown to police (cf. page 47-48).

A Critique of: Leon Gibson Hunt

"Prevalence of Active Heroin Use in the United States"

Leroy C. Gould, Ph.D.
W. Douglas Thompson

Leon Hunt begins his paper, the first in this section of this report, by observing that an adequate quantitative description of heroin use in the United States would include measures of incidence of first use, prevalence of active use, and frequency and intensity of use. Such a description, he notes further, is beyond existing data. Therefore, his paper advances the more modest goal of estimating only one of these measures of heroin use, namely, period prevalence.

To accomplish this goal, Hunt reviews heroin prevalence data derived from selected law enforcement, drug treatment, other treatment, and survey sources. He concludes that each source individually provides an inadequate estimate of the total number of active heroin users. Therefore, a more complete estimate, Hunt argues, must be inferred by combining certain aspects of different indices. Specifically, Hunt begins with prevalence estimates derived by Joseph Greenwood (1971) from Drug Enforcement Administration (DEA) data (arrests) and then employs a "correction factor" for these figures derived from treatment and survey data.

Using this approach, Hunt arrives at United States heroin use prevalence estimates for the years 1968 through 1974. Hunt's derived figures show an increase of over 300% during these years, from 1,167,000 active users in 1968 to 4,350,000 in 1974. These figures, Hunt concludes further, are consistent with prevalence estimates derived from survey data.

Hunt's prevalence estimates are striking on at least two counts. First, as he points out, they are larger by several factors than estimates made, heretofore by most other methods. Second, as he does not point out, they reflect trends in heroin use that are

at odds with trends in prevalence and incidence reported by other researchers (DuPont & Greene, 1973; Greene, 1974; Greene & DuPont, 1974) and by Hunt himself (1974a, 1974b).[1]

How is one to account for the differences in these reports? Has heroin use been increasing or decreasing since 1968? To answer these questions we must examine the procedures used in arriving at the apparently conflicting reports. We will do this by reviewing the definitions, procedures, and conclusions contained in Hunt's paper as well as the procedures used in estimating trends in relative incidence described more fully in other works. We will conclude our remarks with a brief discussion of whether or not accurate estimates of heroin use, incidence, and prevalence can be derived from present data, and if not, what kinds of data would be needed for such estimates.

Prevalence of Active Heroin Use in the United States

Definitions

Hunt's stated purpose is to estimate the prevalence of active heroin use in the United States, in particular for the years 1968-1974. What is active heroin use? It is not necessarily heroin addiction, which is difficult to measure even under ideal clinical conditions and is impossible to measure accurately under the field conditions presently available to drug use epidemiologists. Active heroin use, for present epidemiological purposes, then, involves more simply the concepts of frequency, intensity, and duration of use at some point (or period) in time. These concepts are themselves difficult to deal with in practice, however, because as Hunt observes (p. 62) "at any time, the heroin consuming population is a changing mixture of users of different experience and consuming patterns."

Operationally, Hunt classifies those who have used heroin in the preceding year as active heroin users. We might conclude from his general discussion, however, that "active heroin use" involves more than this. In particular, it involves an ongoing pattern of continued (and we might presume daily) use during the period under consideration.

Hunt does not include duration of use in his definition. He recognizes that there may be periods of nonuse during a heroin user's career with drugs (p. 62), but ignores this observation in some of his subsequent calculations (p. 79). Unless one includes duration of use in the definition of active use, this kind of oversight is easy to make.

Most researchers in the drug field have by now abandoned the notion that any use of heroin inevitably leads to addiction and have accepted the idea that some people, at least, seem to be able to experiment with heroin without getting heavily involved with it. What may be somewhat less well accepted is that certain people, evidently, can also use heroin on a regular basis, even to the point of addiction, for periods of time and then abandon use, either altogether or for varying periods of time. Some involved heroin users spend time in prison or in drug treatment, and others give up heroin use on their own for substantial periods of time. The Robins, Davis, and Nurco follow-up study (1974) demonstrates this for returning Vietnam veterans, while two other as yet unpublished studies have found similar patterns of intermittent use and spontaneous remission among "street people" (Ramos, 1974) and drug treatment applicants (Gould, Forrest, & Kleber, 1975). Altogether, these findings should suffice to warn drug epidemiologists that it cannot be assumed, as perhaps it once was, that heroin use is inevitably a continuous activity.

Procedures

Following his attempt to establish workable definitions of heroin use for epidemiological purposes, Hunt's next step is to show that there are striking differences in the sociodemographic profiles of drug use populations known to drug treatment agencies, law enforcement agencies; the Center for Disease Control (as measured by known and inferred cases of serum hepatitis), mental health facilities, and survey projects. From this Hunt concludes that each of these enumerations or surveys represents different portions of the overall heroin using population. This conclusion is reasonable, with one qualification: the differences between these populations do not mean necessarily that all of them are unrepresentative of the entire heroin using population-one could be representative.

To date there exists little national drug use survey data from comparable populations that could be used to assess trends in heroin use. The survey data reported by the National Commission on Marihuana and Drug Abuse (1973) and cited by Hunt in his paper (pp. 74-75) showing yearly trends for junior high, senior high, and college students from 1967 through 1972 are composites of various individual surveys on different populations in each year, They cannot, therefore, be taken as legitimate survey estimates of trends in the prevalence of heroin use among United States school-aged populations. Neither can the results of the Youth in Transition study (Johnston, 1975), cited also by Hunt (pp. 75-76), be used to measure national trends. Although this study is national in scope, it is based on a single cohort, and thus estimates cannot be made from it of yearly variations within different age groups. Other yearly surveys of comparable school-aged populations (Corder, Dezelsky, Toohey, & Tow, 1974; Lavenhar & Sheffet, 1973; Smart, Fejer, & White, 1973; Whitehead, 1971; Berberian, Kasl, Gould, Thompson, & Kleber, 1976; Glenn & Richards, 1974; Russell & Hollander, 1974; San Mateo County Department of Public Health and Welfare, 1974; Smart & Fejer, 1974) in different United States and Canadian geographical regions have found relatively stable rates of reported heroin use for the years 1968 through 1974, with some indication that there might have been a slight increase in prevalence during the earlier part of the period and some decline in the later years.

These variations over time between data gathered from treatment, law enforcement, or survey sources would seem to confirm Hunt's argument that the different data sources are tapping essentially different segments of the total heroin using population. However, a strong argument could be made that, of all these possible indicators and measures of heroin use prevalence, surveys, especially if they are based on nationwide probability samples, should come the closest to estimating true trends in heroin use. Surveys, at least, are free from such biasing factors as the availability of institutional space and the degree of organizational effort directed at controlling heroin use that plague treatment and law enforcement data. Hunt, however, does not agree, and devotes considerable space in his paper (pp. 72-76) to enumerating the sampling and validity problems associated with surveys. Among these are questions of whether or not respondents will report their drug using behavior accurately on a survey (Hunt assumes that they will not) and whether surveys sample heroin users accurately (Hunt argues that they do not).

Problems of sampling and validity, it should be pointed out, are not unique to the survey technique; these are general social science problems which also apply to the enumerations and indirect estimates based on data derived from these sources. Not

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mentioned in Hunt's paper, for example, is the fact that the data used in enumerations are themselves usually of a survey form; that is, they are gathered through interviews (usually at the time of first agency contact), and thus are subject to the same validity problems Hunt describes as applicable to population survey data. In addition, however, these data are also subject to additional sources of invalidity introduced by the widely varying conditions under which they are gathered and by the widely varying degrees of training and skill exhibited by agency interviewers, who are not usually trained in scientific interviewing techniques.[2] Therefore, it should probably be concluded that all heroin prevalence data currently available are of questionable validity and representativeness. It might also be concluded, however, that these problems are the most serious in the cases of enumerations and indirect estimates. At least survey researchers attempt to use good sampling procedures of definable populations (enumerations and indirect estimates cannot even pretend to do this) and take care to standardize interviewing procedures and to use trained interviewers.

Inferred Prevalence Estimates from Combined Data

Given the problems of sample representativeness and validity inherent in surveys which Hunt describes, and the even more serious problems inherent in enumerations, the scientifically cautious would probably conclude that it is not possible, at the present time, to compute even "ball-park" estimates of heroin use prevalence in the United States (Domestic Council Drug Abuse Task Force, 1975, p. 20). Hunt concluded otherwise, however, and proceeded to use selected data sources to build a rough estimate of the prevalence of active heroin use in the United States for the years 1968 through 1974.

Hunt begins with estimates Greenwood (1971) has made from the DEA register using what is called the recapture technique. DEA data, as Hunt describes it (pp. 68-71) is compiled from "users reported by local police departments . . .; in recent years reporting has been increasingly confined to a few large cities."

The recapture technique is similar to the "indicator dilution" technique proposed by DuPont and Piemme (1973) and to the technique used by Andima, Bergner, Krug, Patrick, and Whitman (1973) to estimate prevalence of active narcotics use in New York City. Hunt's description of the basic idea behind these techniques is thorough although it is less than complete in noting various sources of potential error associated with the method. These include, as others (Glenn & Hartwell, 1975) have pointed out, the assumption that the probability of recapture (rearrest or reapplication for treatment) is random, that accurate estimates exist for the annual rates of death and self-remission of heroin users, that there are no variations in the capacities or effectiveness of law enforcement or treatment agencies, and that reporting to a data source (e.g., the DEA register) is uniform over the heroin using population.

Hunt recognizes the problem associated with the assumption of random probability of recapture and concludes that recapture probably departs from randomness in such a way that Greenwood's figures are an underestimate of true prevalence. As confirmation of this, Hunt notes that Greenwood's estimates for two cities, Washington, D.C. and San Francisco, are well below what one would expect based on estimates available from treatment agency sources. To make the comparison between Greenwood's figures and treatment data, Hunt computes the number of patients in treatment in the years 1970-1974 who reported that they had begun using heroin either during or prior to 1969. This number was approximately twice the Greenwood estimates in

both cities, and it did not even include those heroin users who never came to the attention of treatment facilities. From this, Hunt concludes that actual prevalence must be at least twice the Greenwood estimate and is probably considerably higher, because not all active heroin users will be known to treatment agencies.

There is at least one major fault with this conclusion: it assumes that all persons who were in treatment in 1970-1974 and reported that they had first used heroin in some year prior to 1969 were using heroin in 1969. Unless one assumes that heroin use is a continuous activity from the time an individual first uses it until that person enters a treatment program, it is improper to sum the figures as Hunt has done. We have already mentioned the importance of duration of use in calculations of heroin use prevalence and some of the evidence that indicates that duration of use is not continuous. If it is not continuous, then Hunt's estimate of prevalence in 1969, based on heroin users known to treatment agencies, is an overestimate.

Nevertheless, Hunt concludes that Greenwood's estimates are below those derived from treatment program data by exactly a factor of 2. They are below true prevalence figures by yet another factor-the extent to which clinics do not come to know all active heroin users. To estimate accurately the size of the "hidden" heroin using population, one would need the type of population-wide data that would render indirect estimation procedures superfluous. In lieu of such population data, however, Hunt (p. 73) draws on results from a study (Project DUSK) that involved interviews with 197 active "street" users in 1973 in Washington, D.C., Los Angeles, and Detroit. This sample is subject to seriouspotential bias, however, since individuals were identified strictly through nonrandom personal contacts. Nevertheless, only 30% of this sample reported that they had ever been in treatment, and Hunt concludes from this finding that treatment populations will have had contact with only' about one-third of all active heroin users in the community. Hunt admits that this assumption is extremely tenuous.

Tenuous as it may be, it is also inappropriate, on purely technical grounds, to combine a weighting factor derived from the DUSK study with a weighting factor derived from treatment data. In relating the 1969 Greenwood estimate to the number of treated users, Hunt considers as treated all those individuals who were presumably using heroin in 1969 and who actually entered treatment at some time *during the next 5 years.* On the other hand, when calculating the proportion of current street users who have been in contact with the treatment system, only *prior contacts* with treatment agencies are considered. Combining the two separate weighting factors, then, necessitates an assumption that the number of current users already known to treatment facilities equals the number of current users who will enter treatment in the next 5 years. Why, in the absence of any relevant empirical evidence, Hunt should be willing to equate these two numbers is not at all obvious to us. In fact, it seems quite likely that the relative sizes of these numbers would vary considerably over time and place. Especially in communities where a large segment of the heroin using population began use fairly recently, or where treatment has only recently become available, the number of current users who would seek treatment in the future is likely to exceed greatly the number who have become known to treatment facilities in the past. As a consequence, Hunt's prevalence estimate would be inflated.

Hunt assumes, however, that this would not be the case and bases his assumption on a review of relative incidence curves developed and described more fully elsewhere

(Hunt, 1974a, 1 974b).[3] Since a number of Hunt's conclusions rest heavily on this earlier analysis, it is important to review this procedure here in some detail.

Relative Incidence Curves

Relative incidence curves, as the name implies, do not provide estimates of actual numbers of new users in the population. Instead, what they reportedly do is indicate relative changes in incidence over time. The curves are obtained by plotting the distribution of reported year of first heroin use for a particular group of treatment applicants. (See, for example, Figure 4 in Hunt's paper, p. 80). The general shape of such distributions is assumed to reflect trends in incidence in the general population.

As we have discussed elsewhere (Gould, Thompson, & Berberian, 1975, in press), the use of relative incidence curves requires the assumption that the same proportion of each onset cohort of heroin users enters treatment in any given time period. This assumption is untenable on two counts. First of all, many of the people who began using heroin several years before an entry period under study would have stopped using heroin (through self-remission, incarceration, death, etc.) or would have lost interest in treatment and would thereby not be included in the relative incidence curves. Furthermore, the longer the time between onset of heroin use and the beginning of the treatment entry period under study, the less likely it would be that members of an onset cohort would enter treatment. This means that the apparent increase in relative incidence reported in curves such as that in Hunt's figure may be due to nothing more than the greater probability that earlier onset cohorts would have dropped out of the pool of active users before the time of treatment period being considered.

The second problem with relative incidence curves is that many of the people in very recent heroin-use-onset cohorts will not yet have been using heroin long enough to have experienced the adverse consequences of use that presumably motivate individuals to seek treatment. They, therefore, would be underrepresented in relative incidence curves, and this underrepresentation would be greater for those who had had the least time to enter treatment, The apparent decline in incidence reported in curves such as that shown in Hunt's Figure 4, then, could be due to nothing more than the systematic influence of what Hunt calls "lag."

Neither we nor the proponents of relative incidence analysis have been able to demon-. strate any reasonable correction for the systematic underrepresentation of early onset cohorts. Without year-by-year estimates of the probability of becoming unavailable for treatment, one cannot correct the relative incidence estimates and thus judge their relative magnitudes correctly. There seems to be no viable option, then, but to disregard relative incidence data derived from onset cohorts for years prior to the first entry year for which treatment data are being considered.

Hunt has suggested a procedure, however, for correcting the underestimation of recent years. Unfortunately, this correction is of dubious validity. It is questionable in the first place because it depends on an assumption that yearly lag distributions are the same for successive cohorts of heroin users. Although Hunt has shown elsewhere (1974a, 1974b) that there has been a fair amount of similarity in these distributions in some cities, Richman (1974) cites evidence that they have not been similar in other cities. Hunt's lag correction is also questionable, however, because it is computed on the assumption that willingness to enter treatment is the same for all yearly onset cohorts of heroin users when a treatment program opens. That is, Hunt

assumes that someone who has been using heroin for 10 years, for example, has the same probability of entering a treatment facility when it opens as someone who began using heroin only one year earlier. This, it seems to us, is a tenuous assumption to say the least.

We would argue, then, that in order to draw any conclusions about changes in incidence on the basis of relative incidence curves, it would be necessary first to determine the proportion of each onset cohort that would eventually enter treatment. These proportions could only be determined, however, on the basis of population-wide studies of all groups of heroin users. Since population studies of this kind would yield direct estimates of heroin incidence, the need for indirect estimating procedures would be obviated.

Conclusions

Since Hunt's final estimates of prevalence are based on two correction factors of dubious validity that can be combined only if one is willing to make assumptions which are probably untenable when applied to the questionable estimates made by Greenwood (1971) from law enforcement data that were admittedly not representative of the whole country, it must be concluded that Hunt's final figures are probably too inaccurate to be used for purposes of planning future drug policy. In all fairness, though, we must note that Hunt presents his final estimates in the spirit of ball-park estimates; he does not intend them to be anything more than a rough guess as to real prevalence figures in the United States. It is our opinion, however, that the guessing is too rough. Such rough estimating procedures, it might be pointed out, are particularly problematic when it comes to describing trends in heroin use, since the weighting factors on which the procedures depend may themselves change over time.

Hunt takes some comfort in the fact that his figures are close to estimates derived by different assumptions and weighting procedures from general population survey estimates (p. 82, Table 9). Here, however, we must admit that we are at a loss to determine exactly what Hunt has done. There have been only two general population surveys that have asked questions about heroin use. The one by the Marihuana Commission produced the general population survey estimate for 1972 that Hunt includes in Table 8 and weights, by a factor of 4, in Table 9. The other survey, by the Response Analysis Corporation (Abelson & Atkinson, 1975), is not included in either Table 8 or Table 9 even though it could have been used as the general population survey estimate for 1974. Had it been used, it would have yielded figures that were lower than those used by Hunt. How the figure in Table 9 for 1974 was arrived at is not made clear. Neither is it clear how the figures for 1973 and 1968 through 1971 were derived. They could not have come from general population surveys for these years, because general surveys of the United States population involving prevalence of heroin use were not included in these years.

Where, then, do we go from here? It seems unreasonable to us to devote further effort to developing derived indicators of incidence or prevalence of heroin use such as those presented in this paper. The assumptions on which these indicators rest are so numerous and untestable that the validity of their conclusions must always remain in doubt unless they can be corroborated with actual population-wide studies. With population studies, however, derived indicators become superfluous.

Efforts have been made in the past, and will no doubt continue in the future, to use drug use surveys as a means of measuring the prevalence of heroin use in the population. But drug use surveys, as Hunt discusses, suffer from their own sources of potential bias and error. Whether these problems can be overcome is an important question. Other authors will discuss this question in papers appearing in other sections of this report.

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[1]In his paper Hunt notes (p. 61) that it is possible for incidence to decline while prevalence increases. This can happen if significant numbers of former heroin users relapse to heroin use during a period when incidence is declining. Such circumstances, however, would also result in increases in the average age of heroin users. Since the average age of users appears to have decreased rather than increased during this period (Hunt, p. 70), we must conclude that trends in incidence and prevalence should have been similar during these years.

[2]Even such seemingly solid data as urinalysis results are of questionable validity when gathered under the varying conditions prevailing in many treatment and law enforcement programs and analyzed by the laboratories employed by these agencies (Riordan, Primm, Slobetz, & Wall, 1972).

[3]Specifically, he argues on the basis of relative incidence curves that in most cities the period of peak incidence has passed, and that therefore the passage of additional time would not affect greatly his estimation of the size of the "hidden" user group.

Comments on Hunt's Estimation Procedures

JOHN A. O'DONNELL, Ph.D.

In the main paper of this section of this report Hunt multiplies Greenwood recapture estimates by 6, and survey findings by 4, and arrives at two surprisingly close estimates of prevalence. In the interest of brevity, the focus here will be on the weaknesses in his procedures. Unfortunately, this means neglecting the many values of his paper.

The Greenwood recapture technique may be questioned as a very simple mathematical model that does not adequately represent the complexity of heroin use in the real world. Let us repeat the model. You have a sack of identical white marbles, unknown in number. At one time you remove a handful, count them, paint them red, and return them to the sack. At a later time, say two years later, you remove a random sample. The ratio of the red marbles in that second sample to the N of the sample is approximately the ratio of all red marbles to all marbles in the sack. Since the equation has four terms, and the values of three are now known, it is simple to solve for the fourth, the total number of marbles in the sack.

How does this provide a technique for estimating the number of addicts? The equivalent to all the marbles in the sack must be the total population of addicts at a given time, or the population of addicts in those cities whose police departments happen to be reporting to DEA. The equivalent of marking some marbles red is the labeling of some of that population as addicts by police reports to DEA. At the later time, some years later, the police reports constitute the second sample, and those in that sample who were labeled two years earlier are the red marbles of the second sample.

But addicts are not marbles. Not only is the second sample of addicts not a random sample, as Hunt points out, but there are other differences. Hunt says, for example, that "in recent years reporting has been increasingly confined to a few large cities," which implies that the population of addicts has changed from year to year-the equivalent of changing the unknown number of marbles in the sack between the two

samplings. It can also be expected that there are varying degrees of accuracy between departments in the identification of arrestees as heroin users, and quite probably within departments from year to year. Criminologists have established beyond any doubt the dangers of basing inferences on official crime statistics, and the identification of an arrestee as a heroin user would seem to multiply the dangers, rather than to reduce them.

The major difference is that no one interferes with the mathematician's sack of marbles. It remains unchanged for two years; the total number remains the same, the red marbles stay red, and the white marbles stay white. But within two years, some heroin users cease using, and thus do not belong to the population of users when the second sample is drawn. This is the equivalent of removing some marbles, both red and white, from the sack between the two samplings. The ratio of red to white removals is obviously relevant to later estimates, but it is unknown.

Marbles are also added to the bag, as persons who were not heroin users at the first sample become users before the second. Note that these add only to the number of white marbles; they cannot add to the red, because red means an individual identified as a user before these new people began to use. In an epidemic period this could mean a large increase in the total number of marbles in the sack. The model requires that the two samples be drawn from the same population, but the fact would then be that they are drawn from two different populations.

Suppose that the number of addicts reported at time 1 is 10, and the number reported at time 2 is 30, of whom five were among the original 10. If one assumes that the two samples are from the same population, the equation would be the equation suggested by the model,

$$10/N = 5/30 \text{ , so } N = 60$$

But one might equally well make the assumption that the police arrest a constant proportion of the total number of addicts available for arrest at any given time. Then the appropriate equation would be

$$10/N_1 = 30/N2, \text{ so } N_2=3N_1 .$$

The fact that five of the originally reported men (i.e., of the red marbles) are found in the second sample could mean (a) that the number of addicts remained constant at 60, that the police became much more efficient in arresting them, and that the chance of being arrested at time 2 was the same for all addicts, whether or not they had been arrested at time 1; or (b) that the number of addicts increased from 60 to 180 from time 1 to time 2, that police efficiency remained the same, and that the chance of arrest at time 2 was 0.5 for those arrested at time 1, and close to 0.15 for those not then arrested.

To choose between these possibilities (and the wide range of other possibilities between them), it would seem necessary to have some independent knowledge-or to make whatever assumptions seem reasonable-about changes in the number of addicts, or about police practice in all relevant cities, or about the relative chances of arrest for red and white marbles, or about all of these.

The realistic model, it seems, would be more like this: the sack of white marbles represents the total population of the reporting cities, or of that fraction of the population liable to arrest, however that might be defined. At time 1 the police arrest a sample-though hardly a random sample-from that population; their number, and even individual identities, could be obtained from local police records. These we count, and paint red. Then, from this nonrandom sample the police identify some as current heroin users, by what we hope are reasonably accurate and consistent criteria. These we count too, and identify by painting a blue dot on each red marble in this subset.

A few years later, the marbles in the sack remain essentially unchanged in number. The police then draw a second sample, also nonrandom, and by no means identical to the first sample of arrests. From this new sample they again identify some current heroin users; we hope they use the same criteria as before, and do not classify an arrestee as a current user simply because they know he was one several years before.

This is clearly a more complicated model than the one used in the Greenwood recapture technique, and one that provides no simple equation allowing us to estimate the number of heroin users at the two times. But we would be a lot happier with such a model, because it seems to us to correspond with reality. Unfortunately, we cannot suggest how the possibilities of change in number of those committing crimes, in arrest practices and priorities, and in numbers of criminals and noncriminals using heroin could be built into a model.

Hunt's first procedure is to multiply Greenwood's estimates by 6. This figure is the product of two factors; you multiply by 2 to correct for Greenwood's undercount, estimated from data from treatment programs, and then multiply the result by 3, to allow for calculations that seem to show that only one in three users appears in treatment programs. Let us examine the bases for choosing these two factors.

Greenwood's procedure calculated 4,600 addicts in Washington in 1969, and Hunt concludes there must actually have been at least 9,030 users, because that number, admitted to treatment in Washington in 1970-1974, said they had been using in 1969. Note a minor assumption here. Conceivably many of the 1969 users had been using in other cities, not in Washington. But we grant this is unlikely to affect the argument to any great degree.

The same comparison, for San Francisco, gave figures of 4,200 and 7,500, so Hunt suggests that "a crude rule of thumb might be to double" Greenwood's figures'. Doing so, of course, is to generalize from two cities to the United States-in effect, to make the assumption that reports to DEA are adequately represented by these two cities, or that the ratio of reported users to eventual patients is constant in all parts of the country. That strikes us as a strong assumption. Indeed, the data Hunt cites suggest that one needs to multiply the Greenwood figures for Washington by 1.96, and for San Francisco by 1.79, to reach the treatment program figures. We would predict that if the same procedure were applied to other cities and states the variation among multipliers would be much greater.

Not only does the multiplier 2 represent a generalization from two cities to the country, but also from one year, 1969, to the seven-year period 1968-1974. This seems to us an even less acceptable assumption, especially since the period was one in which both incidence and the number of treatment slots were rapidly increasing, but almost certainly not in step with each other. Rather than using a constant

multiplier, 2, we would suggest a range; it is hardly likely that the multiplier would be less than 1.5 anywhere, but it might be very much higher in some years in cities where treatment programs had not yet been established, or were new. For the country as a whole, this range might be, say, 1.5 to 3.0.

The second multiplier, 3, is used to try to correct for those hidden users who do not enter treatment, or at least have not yet done so.

The data are:

1. A nonrandom sample of 197 heroin users in three cities in 1973, of whom only 30% had been in treatment.

2. A pair of recapture samples in Phoenix, which estimated the five-year (1970-1975) prevalence of heroin use at 8,500, of whom only 3,000 (35%) were known to the treatment system.

The fact that the two percentages come out 30 and 35 seems to us an extremely weak basis to suggest a multiplier of 3. As Hunt himself points out, the Phoenix study occurred during a period of rising incidence, so there were many new cases of use, and of these a large proportion will later show up in treatment. Earlier work by Hunt (1974, p. 19) suggests that perhaps a fourth of those who will eventually enter treatment will do so within a year after onset, while up to 65% will do so within 2 years. If this is even approximately correct, the 35% for Phoenix is a gross underestimate of those who will be known to treatment agencies. Further, the Phoenix data of 1975 are only dubiously relevant to guide the choice of a multiplier for other places in the late 1960s and early 1970s. The other sample, of 197 users, is so small and so ill-defined as to offer no basis at. all.

Again, then, we suggest a range of multipliers. As a guess, we would set the lower limit as low as 1.5. We really find it difficult to see 3.0 as a reasonable choice, but Hunt does, so we will accept it as the upper limit.

For the sake of argument we have accepted, in the above few pages, the Greenwood estimate as the figure to be multiplied. But it should be clear from earlier discussion that we regard such an estimate as randomly falling within very wide limits, depending on how the factors of cessation of use, incidence of new cases, and so on, happen to be operating. We have no logical basis to set those limits, so our choice is arbitrary-intuitively, it seems that the lower limit could easily be half of, and the upper limit double, whatever the Greenwood estimate may be.

Now we too can make estimates. Hunt takes the Greenwood number for 1974, 725,000, multiplies by 2 and then by 3, and arrives at an estimate of 4,350,000. The two extremes of our procedure are 362,500 X 1.5 X 1.5 = 815,625, and 1,450,000 x 3 x 3 = 13,050,000.

Our estimate, in short, would be that from 800,000 to 13 million persons in the United States in 1974 had used heroin within the last year. In one sense, we do not ask to be taken seriously. We realize that to policymakers, to persons who have to plan and budget for treatment slots and law enforcement, such a range is too wide to be useful. They need a much more precise figure, even if it is illusory. If the figure is two million or six million they can plan, and it may not even make a great deal of difference if they accept one of these two figures as correct and it turns out that the other was.

But in another sense we offer the estimate quite seriously. It seems to us to suggest the present state of knowledge, and to emphasize the inherent dangers in the procedures used to obtain the estimate, both of which are concealed in Hunt's single estimate of 4,350,000.

Hunt, however, arrives at approximately equal estimates by a completely independent procedure, and this would seem greatly to strengthen his position. We confess to some confusion about this procedure. He describes it as correcting general population survey estimates by raising them to account for higher levels of use proved by urinalyses of matched populations. The "corrected estimate" for 1972 in his Table 9 is precisely 400% of the Marihuana Commission's survey figure for that year in Table 8. But we do not see how the figures for other years in Table 9 were derived from Table 8, if indeed they were.

This is less important than Hunt's conclusion that most surveys systematically underestimate levels of heroin use and his quantitative assessment of this underestimate. His conclusion that surveys underestimate is based on several considerations, which will be considered in other papers. The assessment of its size is based on one comparison, which may be discussed now.

Hunt uses data from preemployment physical examinations by the New York Telephone Company to arrive at the estimates that 4% of the white and 14.5% of the black applicants had used opiates within the week before examination, while the Chambers and Inciardi (1971) survey in the same year found that 1% of white males and 9% of black males had used heroin in the 6 months prior to interview. Hunt sees a discrepancy between these sets of findings, and concludes that the survey understated prevalence by at least 50% (for blacks) to 400% (for whites).

There are at least three basis on which Hunt's reasoning can be rejected:

1. At worst, the discrepancy is between one survey and better data, in New York City. It seems excessive to generalize to all surveys, in all years, and especially to national surveys. New York is typical of the country in few ways, and drug use is certainly not one of them.

2. As Hunt's figures from the telephone company show, positive tests for opiates were relatively rare; it was tests for quinine that made the total of positive tests high. His conclusion that the survey "understated prevalence by at least 50% to 400%" is justified only on the assumption that all positive quinines indicated heroin use.

Those who did the study rejected this assumption. The author (Dupong, 1971) states:

> The numerous reports of quinine without any other concomitant drug findings created many administrative problems since there were no analytical methods known to us that could distinguish quinine which entered the body with heroin from the quinine taken in quinine medications or mixers. . . . Unfortunately for our findings, quinine, in addition to its use with illegal drugs, is present in many commonly used medications, soft drinks and mixers. Its presence in the urine, therefore, can only be considered as suspicious of, but certainly not conclusive evidence of narcotic drug abuse. In the absence of histories or signs of drug abuse we have not felt justified in rejecting every applicant solely because quinine was found in his urine specimen (pp. 461,463).

When any drug was found in the urine, the applicant was recalled for further examination, interview, and further urine testing. When the applicant was suspected of drug abuse, or drug abuse was proved, he was rejected for employment. There were 347 who were rejected. These could all have come from the 373 positive for drugs other

than quinine, so it is possible that no one with a quinine positive was diagnosed as a drug abuser. Even if all or most rejections came from the quinine group, at least 47% of the quinine positives were not rejected, and, therefore, were not, after reexamination, suspected of drug abuse. The true rate of heroin use among the applicants for telephone company jobs was, therefore, probably much lower than Hunt's estimates, so the discrepancies from the survey figures must be much smaller than those he infers.

3. Even if Hunt were correct in his figures of heroin use among the telephone company applicants, there may well be no discrepancy between them and the survey figures. This argument rests on the age distributions of the two samples.

We cannot construct, from the Chambers and Inciardi report, the cross tabulations needed to produce the findings reported by Hunt, that 9% of black and 1% of white males, 14 to 34 years in age, had used heroin in the preceding six months. We will here assume these figures to be correct, based on some table we have not seen. We will also assume to be correct, though we cannot find it in the report on the telephone company applicants, that all of them were, as Hunt says, high school graduates.

If so, the data indicate that at least half and possibly two-thirds of the telephone company applicants were in the 18 to 24 age groups, as against one-third or less of the survey respondents. Heroin use rates in this age group are much higher than in younger and older groups, so age distribution alone would be enough to account for a higher rate among the telephone company applicants, which would not be inconsistent with the lower rate in the survey. Hunt's analysis means nothing unless a discrepancy can be shown to rest on age-specific as well as sex-specific rates.

Therefore, the factor of 4, by which Hunt multiplies a survey estimate to arrive at inferred prevalence-the method that is independent of his inference from Greenwood estimates-rests on a weak basis. Indeed, we find it curious that he arrived at 4 as the multiplier. Recall that even if we fully accept Hunt's argument about the survey/telephone company discrepancy, it was only for whites that the multiplier of 4 was needed; a multiplier of 1.6 would suffice for blacks. One would expect the multiplier for the nation to be an average of these two figures, weighted by the best available estimate of the proportions of whites and blacks among heroin users. This would produce an overall multiplier of about 2.7. Why, instead of this or some figure close to it, did Hunt choose to apply the white multiplier of 4.0 to everybody, white or black?

Applied to the Marihuana Commission survey of 1972, the multiplication by 4.0 produces the estimate of 3,296,000, which in our judgment is reasonably close to the corrected Greenwood estimate, 3,756,000. But if 2.7 were used, the estimate would be 2,225,000. The first estimate is about 88%, the second 59%, of the Greenwood estimate. Using 4.0 as the multiplier allows Hunt to say "Perhaps the most interesting characteristic of these corrected estimates is their similarity, even though they are the result of entirely independent processes." Using 2.7, he could hardly have said that. We cannot help but feel that, unconsciously, the choice of 4.0 was in part determined by the similarity it would produce, as well as by the data on the survey-urinalyses discrepancy.

In brief summary, Hunt's estimate is based on two independent procedures that produce roughly identical results. But the first procedure is based on dubious data, the Greenwood estimates, and on two multipliers, each of which rests on inadequate

samples The second procedure is based on survey data, but the multiplier is based on only one set of data, on a discrepancy that is not established to exist and which, if it does exist, would suggest a smaller multiplier.

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Survey Data as Contributors to Estimation

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In discussing the possible uses of surveys in estimating heroin or, more generally, narcotics use in the United States, we would like to take an approach that is somewhat different from that taken by Leon Hunt in the opening paper to this section of this report. Hunt focused heavily on the goal of estimating the *number* of users, and on changes in that number across time. Our own bias is that such efforts are doomed to failure despite the increasing sophistication with which they are being made. We believe that the phenomenon of illicit narcotics use is simply not amenable to accurate sampling and measurement. Further, the inaccuracies do not seem in any way reasonably estimable.

We do not see the picture as totally bleak, however. The most important type of feedback we need in this area is whether "the narcotics problem," which we already know is sustantial and worthy of considerable public concern, is getting better or worse as time goes on, and at what rate. We do not necessarily need absolute numbers of users to answer these questions. Being able to monitor the size of some *fixed* proportion of the users — even if the proportion itself is an unknown — will allow us to assess rates and directions of change. If we had such information over time, we could determine whether various historical and environmental events affect narcotics use and, specifically, what effects certain policy interventions have on use.

Given the existing weaknesses in many of the alternative sources of data, we would contend that the survey method presents one of the better alternatives available for the systematic collection of such data.

Suggested Standards for Useful Indicators

We would argue that there are two particularly central characteristics that a national indicator on narcotics use should have. First, it should reflect levels and

changes in narcotics use for the nation as a whole — not just in certain cities, for example, as do some of the alternative systems. Obviously, national surveys fulfill this requirement.

Second, such an indicator should have a consistency of methodology across time, so that method changes would not introduce artifactual fluctuations in our indices of usage. Arrest data, for example, may give a distorted picture because of changes in law enforcement priorities, changes in the police reporting procedures themselves, or mtentional misrepresentation for political purposes. Repeated surveys, on the other hand, can be designed to have identical methodologies over time, since so many of the procedures and data-gathering mechanisms are under centralized control. We emphasize the word "can," because generally they have not had such consistency.

Some Ongoing Series

A few series employing consistent methodology across time have been started, however-two at the national level. Of particular importance are the surveys conducted by the National Commission and recently repeated by Dr. Ira Cisin and his colleagues incollaboration with Response Analysis Corporation (Abelson & Atkinson, 1975). We understand that this series will be continued in the future with a particular concern for maintaining the fundamental methddology, and we trust that it will provide valuable indicators of prevailing levels of drug use.

Another national series, which focuses exclusively on young people in the "high risk" years between ages 17 and 23 was recently begun at the University of Michigan (Johnston & Bachman, 1975). Using identical methodology, the investigators will survey about 16,000 seniors each year and also follow a longitudinal panel from each graduating class for 5 years.

The trend data from these two national series should tell us a great deal about the direction and rates of change in use of the various drugs; and even if there is consistently a 50%, or 60%, or 70% underreporting for heroin, the observed trends still should be indicative of what is happening in the population more generally.[1] These studies will also yield trend data on a number of related factors such as availability of drugs, exposure to use, and the changing social meaning of use. In time, some of these variables may prove to be *leading* indicators of use, in contrast to the concurrent or lagging indicators that most other methods provide.

Problems with Survey Methodology

Having made a quite positive statement for the potential usefulness of surveys as indicators of the use of heroin and other narcotics, let us hasten to add that they are not without their problems. Several of the key problems have already been stated by Hunt: the possibility of systematic underrepresentation in obtained samples, underreporting of use by the heavy heroin users who are successfully captured in the sample, and the low numbers of cases of reported heroin use in most national samples.

The issue of representativeness is being addressed at length elsewhere in this report by Dr. O'Donnell, so we will concentrate our remarks primarily on the latter issues. However, we will return below to the criticism of underrepresentation in surveys to question how important this issue is, even if it is valid to some degree.

Validity of Self-Report Data

Validating self reports of illicit drug use has proven to be an extremely difficult undertaking. Given the strong, systematic ways such variables repeatedly have been shown to relate to other variables with which we would expect them to relate, there obviously is a substantial amount of validity in the self-report data. The technical term for this type of validity test is "construct validity." But one can have good construct validity and still have substantial underreporting and underrepresentation.

How, then, might one go about trying to assess these problems? One approach is to see whether different survey methods have tended to validate one another.

Replicability

A comparison of the prevalence figures reported in four national surveys of drug use was recently made by Cisin and Moss (1975). They found amazingly similar prevalence figures in the four studies despite differences in the way the sample universes were defined (households, Selective Service rosters, and schools), despite differences in procedures (household interviews, mailed questionnaires to an 8-year panel), despite differences in question wordings, and despite the very small subsamples available for some of the comparisons of comparable age groups. The prevalence estimates are not identical, of course; but considering the myriad of differences in methods and procedures, they were surprisingly similar.*

We come away from those comparisons with the feeling that the survey method in general has shown a great deal of reliability (reproducibility) in estimating the prevalence of drug use.[3] The best test of the reliability of prevalence estimates, of course, would be to have exactly the same procedures and methods repeated within a short time interval, but even without such controls (all of which should tend to boost the replicability of the results) a high degree of replicability has been demonstrated.

To say that survey techniques have reliability or replicability in their prevalence estimates of drug use is not to say that those estimates are valid, however; they are simply stable. The data from all of the surveys could be badly biased in one direction or the other — for all drugs or, perhaps, just for particular ones. One attempt was made recently to assess the seriousness of the biases (presumably underreporting biases) to which survey techniques are subject (Cisin & Parry, 1975). More such efforts are needed.

Congruent Validity

Cisin and Parry conducted a small study in which a comparison group of known users was surveyed, using normal survey techniques, to see what proportion would accurately report themselves to have been users. The records of 85 former patients of drug treatment clinics were used to determine whether each was or was not a user of a particular drug; a matched control group of people who had not been in treatment was chosen from the same geographical area; and a double blind procedure was used in which the respondent did not know that he or she was already identified as a "user" by a clinic, nor did the interviewer know whether or not the respondent had been a user.

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The investigators discovered, however, that the external validating information derived from clinic records was far from 100% valid itself. The poor condition of the record-keeping and data-gathering apparatus in the clinics raised doubts about the validity of their data; but the more telling fact was that a number of former patients admitted in their interviews to taking specific drugs that the clinic records did not show them as having taken. The investigators were forced to use each of the two data sources to validate the other, rather than using the clinic record as the definitive information source.

Assuming that a person was a user of a particular drug if either his or her clinic record or questionnaire so indicated, they found that the survey technique detected anywhere from 90% of such users to 41%, depending on the drug. The survey technique did the worst job in identifying previous heroin users: only 41% of the presumed users admitted to use on the questionnaire. Better results were obtained, however, for other opiates, with 74% of the presumed users self-reporting such use.[4]

What do these results mean for the viability of surveys as estimators of heroin use? First, if we assume that we can generalize the results of this very small study to the entire population of drug treatment matriculates, (a big assumption, by the way), then narcotics use among heavy users is (and will continue to be) substantially underreported on surveys, even when the users are captured in the sample. This fact added on to the widely held belief that serious users are likely to be systematically under-represented in survey samples (whether based on households or m-school populations) suggests an even more accentuated underreporting. If surveys do tend to understate the number of regular heroin users in the population, (and to underestimate them to unknown degree), why use them?

The Usefulness of Surveys

As the reader will recall, we have argued that the value of surveys in this area lies in their ability to indicate directions and rates of change, not absolute numbers of users. If a constant 40% of heavy heroin users in the population tend to be sampled and validly detected by a particular survey method, then proportional changes in the number of such users in the repeated samples should be the same as the proportional changes in the number of such users in the population.[5] (As always, change estimates would have to be interpreted in terms of probabilities, rather than certainties, because of sampling error.) So, for example, if a national household survey yields 100 heavy heroin users in one year and 200 in a subsequent year, other things being equal, our best guess is that heavy heroin use has doubled during the intervening period.

The validity of this procedure rests on several important assumptions; however, which are worth noting explicitly:

1. Survey methods must be constant across time to prevent a host of possible method artifacts. This can be done as a matter of policy.

2. In particular, the degree of undersampling of users must remain fairly constant across time. To accomplish this, sampling methods and field procedures can be carefully replicated across time, and response rates should be checked (overall and for relevant subgroups).

3. The degree of underreporting by users who fall in the sample must remain relatively constant across time. There is less that the researcher can do to assure that this condition is met (other than hold the confidentially procedures, etc., constant), since many environmental conditions are beyond his or her control. It may be possible to check the validity of the assumption, however, by trying to measure concealment motives across time to see if there has been any change in them.[6] The Blackford data, cited in Footnote 2 are encouraging on this point.

4. The number of cases identified as heavy users (or whatever user subgroup or group one is trying to estimate) must be large enough to keep the sampling error from overshadowing obseived changes. Clearly, in most national surveys having a substantial number of cases for the purpose of estimating change in heroin use (in particular) with any reasonable degree of accuracy remains a problem. Special sampling will undoubtedly be required if national samples are to be used to monitor such changes. Specifically, one would want to oversample segments of the population, both geographically and demographically defined, that historically have the heaviest rates of heroin use, e.g. youth, blacks, particular inner city areas, etc.

5. Finally, this survey method for estimating change rests on the assumption that the unobserved portion of the user population moves in parallel with the portion that is observable through surveys. Although this assumption has high face validity, it has not yet been tested empirically.

Given that the above assumptions can be made and conditions met, the method of estimating direction and rates of change in heroin and other narcotics use via repeated surveys is a viable one. Clearly it is not without its imperfections, and consumers of the data should be aware of them; but at the moment it would seem to have fewer such imperfections than the major alternatives. In this area of far from perfect indicators, the best strategy is undoubtedly to seek to improve procedures for those methods that appear most promising and, as time goes on, to seek convergence among them. Insofar as there is convergence, one can be reassured. Insofar as there is not, a new iteration of investigation is required to reconcile the apparent contradictions.

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[1]To date, the National Commission series has encompassed too few heroin users to make reliable estimates of change; but ture surveys planned for this series have the potential for enlarged samples from high risk subpopulations.

[2]Blackford's (1975) studies of junior and senior high school students in San Mateo County, California, usevirtually identical methodology from year to year. The percentage of the respondents reporting having used heroin during each 12-month interval from 1971 through 1975 was 3.5%, 3.2%, 3.2%, 3.4%, and 3.0% respectively. These data show a remarkable consistency across time with the biggest difference between any two years being .5%. Considering further the fact that there were probably real shifts going on that could account for most of the differences, these data are all the more impressive in their repllcabihty. (Of course, given that she includes virtually the whole universe in her sample, there can be rather little sampling error affecting the estimates.)

[3]Of the four studies compared by Cisin and Moss, those having the most similar sampling procedure were the George Washington study and the Columbia University study. (Both used household samples in 1974.) They showed lifetime marihuana use among American youths aged 12 to 17 at 23% and 22% respectively. The use of narcotics was not measured in the latter survey.

[4]Of the individuals reported to have been heroin users by the clinics, only 30% admitted such use on the questionnaire. Of those reported by clinics as users of other opiates, 44% admitted such use.

[5]This notion is analogous to the fact that one can estimate the relative size of an iceberg by measuring the observable portion above water. If the observable portion (which is always a fixed percentage of the total volume of the iceberg) doubles over time, we know that the volume of the entire iceberg has doubled, even without knowing what proportion projects above water.

[6]One possibility is to ask at the end of each interview or questionnaire, "if you had used heroin during the last [year], do you think you would have told us that you did if we asked?" Changes in the proportion saying "no" among high-risk populations would tend to indicate an historical shift in concealment motives.

Survey Data as Contributors to Estimation

JOHN A. O'DONNELL, Ph.D.

In the main paper of this section of this report, Hunt argues that surveys systematically underestimate levels of heroin use, and even arrives at the conclusion that findings, at least for heroin use within the year preceding interview, must be multiplied by 4 to be approximately correct. A separate critique of his estimation procedures, which appears earlier in this report, rejects this multiplier.

But this leaves for discussion his general reasons for believing that surveys underestimate levels of heroin use. Some of these are technical reasons. Hunt asserts that the first step in designing a probability sample is to obtain a list of the population. This is true for simple random samples, and may be true for the selection of individuals in the last stage of multistage samples, but is not generally true. Johnston (1976), for example, in a project still in the early analysis stage, with no publications to be cited, has studied a sample of high school seniors without a list of such seniors; a list of high schools sufficed. Texts on sampling devote relatively little attention to simple random sampling, except for illustrations and the development of sampling theory. Most of their attention goes to stratification, cluster sampling, stages of sampling, and the practical and statistical refinements these involve. But by common usage, and, we have always assumed, with mathematical justification, these are all regarded as probability samples.

'It is correct to say, as Hunt does, that "there are no lists of everyone," in the sense that no list is 100% complete. But there are lists complete enough for practical purposes. Selective Service records, for example, were close enough to a complete list of young men that we can claim a sample (O'Donnell, Voss, Clayton, Slatin, & Room, 1976) representative of the total population of young men, in which each man had an equal chance of being selected. In theory, census records or Social Security records could furnish almost complete lists of the population except for

small children. For smaller areas, there is also the possibility of constructing a list from a variety of sources such as utility records, school records, and lists of registered voters.

But we can grant that, for mostpurposes, most surveys will not attempt to obtain or construct a list of the population but will use a multistage sampling procedure of which the household sample is the most common example. The rather improbable distribution of individuals within households discussed by Hunt is really not the problem he makes it seem. Not only is it possible in theory, but it is already common practice to design the sampling procedure to allow for such variation. In both the Chambers and Inciardi (1971) and the Abelson and Atkinson (1975) surveys that Hunt refers to, the first step in sampling was to determine the number, age, and sex of its residents in selected households. Depending on the answers to these questions, a decision based on preexisting rules was made as to whether anyone in that household, and if so, who, was to be selected into the sample and interviewed.

It is true, of course, that not everyone lives in a household, but this can be handled in the same study or another by separate samples of the military, of college students, of persons in institutions, and so on. Hunt asks, and it is a legitimate question, "Are heroin users as likely to have a stable, clearly defined address as any other person?" Some stereotypes suggest they are not, and it would be useful if surveys, treatment agencies, and studies of arrested persons asked this question about the users they identify, so that we could begin to build up some data to estimate the size of the problem. In developing our research plan, (O'Donnell et al., 1976), we were troubled by this question when we were considering using a household sample, and one of the more optimistic staff members suggested that even if the stereotypes are correct, it could increase rather than decrease the chances of locating heroin users; after all, they sleep someplace, and if a user spends a few nights a week with his mother, a few with his wife and a few with friends, each of them might identify him as a household member-so he would have three chances of being selected into the sample. We simply do not know how much of a problem we have in this area.

We do not place much weight on Hunt's suggestion that heroin users are difficult to find, which he based on the fact that, of 197 street users followed at 60-day intervals, 30% could not be found after 6 months. We would first wonder about the competence of the searchers, and how far they cast their nets. Vaillant (1966) was able to locate a large percentage up to 12 years after treatment, and Robins and Murphy (1967), Ball and Pabon (1965), Nurco, in a study not yet completed (1976), and O'Donnell (1969) were able to locate over 90% of narcotic addicts after periods ranging up to 20 years. But more important, the follow-up cited by Hunt is relevant to a list sample, not a household sample. It is when you have a list of persons identified at some point in time that you find yourself looking for them months or years later. With a household sample, you normally interview the respondent within a short time after identifying him or her as a member of the household. The relevant data on the 197 street users would be whether or not they were members of a household at the times they were interviewed.

Another, nontechnical, argument is that heroin users do not willingly discuss or even admit to their drug use. This is the old argument that heroin users are liars, and one cannot believe what they say. Our own experience had led us to a different formulation. Opiate users do lie, when it is to their advantage to do so. If a policeman asks an addict if it were he who threw away the package of white powder on the

street, or if a physician about to begin the withdrawal of opiates asks the user how much of the drug he has been using, they can expect the answers to be lies. But when there is nothing to be gained or lost by telling the truth, our own expectation is that we are more likely to hear the truth with respect to socially disapproved behaviors from the addict than from the average person. The social desirability bias has seemed to us greater among the comparison groups than among the addict groups we have tried to study.

There are some empirical data on this point. Cisin and Parry (1975) have reported on two validity approaches. In one, an index group of known users and former users was compared with a randomly selected group matched on age, sex, and location,. and the index group was more likely than the other to report lifetime and current use of drugs (other than heroin). In the second, questionnaire reports of an index group were compared with their clinic records; for most drugs, but not for heroin, the questionnaire revealed higher rates of use.

In our study of young men (O'Donnell et al., 1976), we have a second sample drawn from areas in Manhattan that had been identified as areas of high use by the New York City register. Among the first 140 men interviewed, the expected high rates of use were observed. The percentages reporting lifetime use were 99 for alcohol, 74 for marihuana, 39 for cocaine, 22 for heroin, and 36 for other opiates. We also obtained from a variety of sources the names of men who had used drugs, and were able to interview 52 of them. We had no data on individual drug histories, so we do not know exactly how many had used heroin or had been arrested, but we do know the percentages should be high. Of these men, 98% said they had used alcohol and 89% marihuana. Corresponding percentages were: cocaine, 73; heroin, 81; other opiates, 62. In addition, 71% said they had had trouble with the law because of drug use, 81% admitted an arrest record, 73% had used a needle to inject drugs, and 54% had been treated for drug use. Underrreporting clearly remains a possibility, but for lifetime use, at least, it can hardly be off from the true number by 400%- or even 40%.

Surveys do have their problems, however. They are cheap and easy only in a relative sense. Among their problems are the difficulty and expense of locating subjects if the sample is a list sample. In any kind of survey there seem to be increasing numbers of people who cannot be found or refuse to be interviewed, so completion rates rarely approach 100%. Statistics texts tell us what we can infer from a probability sample, but they tend to be silent on what you can infer from 70 or 80% of such a sample.

With respect to heroin use, we are dealing with relatively rare behavior, so we need enormous samples to achieve precise estimates. Larger samples in turn require more interviewers if the interviews are to be conducted within a reasonably brief period of time, and the average level of quality of interviewers goes down as their number increases, to say nothing about the added difficulties of training and supervising them and coordinating their efforts. As quality goes down, we are forced to more fixed formats in interview schedules and to simpler questions, because only good, well-trained and motivated interviewers can be trusted to handle complex skip patterns, or in other ways to exercise judgment in the interview process.

Further, we agree with Hunt that surveys cannot be expected to give us completely accurate counts, and undercounts seem more likely than overcounts. Our survey of young men, the Abelson and Atkinson (1975) survey, and Johnston's (1976) survey

gave very close estimates for the age-sex groups they shared, and one would like to interpret this as evidence for their accuracy. But it is clear that the explanation could be the sharing of common systematic errors in reporting.

While we cannot accept Hunt's estimate that any survey finding needs to be multiplied by 4, we do feel it is likely that current use is denied more often than lifetime use, that use of heroin is more likely to be denied than use of marihuana, and that the extent of use may be minimized even when use is admitted. Further, and with equal lack of basis in data, we suspect that past experimental or occasional use by a now upright citizen is more likely to be denied or minimized than heavy use by a current user.

This, of course, implies that we accept the argument for some multiplier of findings in making estimates but reject the idea that a single multiplier can be applied to all of the findings a survey provides. Current regular use of heroin may require the largest multiplier, though we would guess it should be on the order of one-point-something, not 4. Marihuana use is now so accepted by young men that it can be admitted freely, and a multiplier of 1.0 to 1.1 may well be large enough.

But where are these multipliers to come from? Conceivably comparisons like those Hunt makes between the survey and telephone company applicants can provide data on current heroin use, though such comparisons are almost certain to apply only to small areas, not to the nation as a whole. In theory, at least, if confidentiality of both sets of data can be protected, it should be possible to check the responses of survey respondents against treatment and arrest records in a number of cities, but practical considerations would limit the comparisons to cases of recent, fairly heavy, and regular use. We do not see how any conceivable operations, except for their own statements, would make it possible to identify individual survey respondents as having been experimental or sporadic users at some past time, even 6 months to a year earlier. Such use is not likely to lead to arrest or treatment or report to a register; it could lead to emergency room treatment for an overdose, but without knowledge of the relation between frequency of use and overdose, we do not see what we could learn from that.

While one is forced to admit that no one survey is likely to be completely accurate, we see no competitor for providing estimates of past use or of light use as of the time the survey is done. For current heavy use of opiates, one must admit the possibility that other data may exist to fairly firmly establish a correction factor, but such factors have not yet been established and we feel no confidence that they soon will be. Periodic surveys of the same populations, however-and one hopes they will be more common in the future-should give reasonably good estimates of trends, on the assumption that whatever operates to make responses inaccurate remains constant over time. In short, we see weaknesses in the survey as a basis for prevalence estimates, but still see the survey as the best basis available to us.

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This research was supported in part by Research Grant DH01121, National Institute on Drug Abuse.

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Estimating the Incidence and Prevalence of Addiction: Why?

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Researchers carrying out epidemiological studies of drug addiction were recently admonished ". . . to be more concerned with means and less with ends" (Richman, A. & Richman, V. V., 1975a). A review of reports dealing with estimation of the size of the addict population, however, suggests that this is a case of preaching to the converted. Methodology is discussed and defended at great length at conferences and in the professional literature; almost every article promptly elicits a rebuttal that criticizes the validity of the data and the assumptions used in their analysis. Indeed, the controversy that pervades this specialized area of investigation has almost reached the intensity of the debate concerning the philosophies of addiction treatment. The only consensus among academicians regards the rationale for epidemiological research, expressed in cliches such as: "Knowledge of trends in the extent of narcotic addiction is essential for the evaluation and planning of treatment and prevention programs" (Richman, A. & Richman, V. V., 1975b. p. 226).

Superficially, such a premise seems reasonable. Incidence and prevalence studies have indeed played an important role in furthering the understanding of many medical as well as social conditions. It is also difficult to argue with the assumption that such studies *should* be indispensable to effective planning and implementation of programs that address the drug abuse problem. Nevertheless, it does not follow that the theoretical benefits of data relating to the size of the addict population have been realized, nor that such benefits could be enhanced if the data were more accurate. Furthermore, standard epidemiological techniques such as case registries and field surveys, when applied to addicts, are not without considerable potential danger to the subjects. Accordingly, it seems appropriate to reassess the need for research activities that are intended to estimate the number of drug addicts in a community.

The Utility of Addict Registries: A Case In Point

Less than 2 years ago we described the factors that we believed were most important in determining the utility of addict registries: *completeness* of reporting; *representativeness* of the reporting sources; *accuracy* of the data; *capability of matching* reports of the same individual to avoid duplication; and *timeliness* of the analysis (Newman & Cates, 1974).

Since then, however, our experience in directing the operation of the New York City Narcotics Register has led us to conclude that these and other intrinsic attributes of an addict registry's methodology are totally irrelevant to its potential usefulness.

"Utility" of incidence and prevalence estimations is entirely a function of the value that such data have for particular individuals or groups. Generally, those who seek information do so in order to confirm their preconceived biases, or to justify a course of action that has been decided on for extraneous reasons. One example is the demand for data that is generated by the proposed opening of new treatment facilities. Communities generally seek to prevent the establishment of new clinics in their midst and want data to support the contention that the clinics are not necessary; treatment programs planning to expand into new neighborhoods wish to document the unmet need that they believe exists. It is extremely rare that either group changes its position on the basis of contrary data. Simiarly, experience indicates that when data are available that agree with a particular point of view, no one (except the opposing faction) is the least bit concerned with such issues as reliability, completeness, or statistical techniques employed in the analysis.

It is particularly important to recognize that those who are involved in carrying out epidemiological studies of addiction are impotent when it comes to determining the purposes for which the data will be used. Misinterpretation cannot be avoided by merely stating, however clearly and emphatically, the limitations that apply to the information being presented. On the other hand, when available data suggest that a popular and expedient course of action is inappropriate, presenting "the facts" rarely has an impact. The great disparity of the findings of studies using different methods of enumeration generally ensures that data will be found to support any position, and contradictory information is simply ignored.

Conclusion

In a paper delivered at this symposium, Leon Hunt stated: "The question is not whether there are three or four million [addicts], but that the number is several million rather than only several hundred thousand." Other participants will insist that more precise estimates are critical. But, in fact, why should we be interested even in this gross distinction? Who cares whether there are 300,000 or three 'million addicts in the country? In 1970, based on data released by the New York City Narcotics Register, it was concluded that there were approximately 3 16,918 active heroin addicts in the City (Burnham, 1972); would anything have been done differently if, with improved methodology, the figure had been 6 13,000, or 163,000? In retrospect, does it matter whether the "true" prevalence at the time was one-third of the Register's estimate, or three times greater, or precisely the same?

It is small comfort to realize that the lack of agreement on the size of the addict population is inconsequential. Irrelevance is hardly a convincing argument for continuing to allocate resources in order to arrive at more accurate estimates. Objectives

of studies that are intended to measure the incidence and prevalence of addiction must be reassessed in terms of experience. It is necessary to ask candidly what impact such research has had in the past and to question the premise that knowledge, for its own sake, is sufficient justification. In their classic compendium on the subject of drug abuse in America, published in 1928, Terry and Pellens observed: "As a matter of fact, it is not necessary to know the exact number of users or even the minimal extent, to realize that there are a large number [of addicts] and that the problem is serious" (Terry & Pellens, 1970). They would undoubtedly reach the same conclusion today.

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Registers as Contributors to Estimation

IRVING ROOTMAN, Ph.D.

Never ask a ballet dancer to play professional football (Canadian aphorism)

The use of narcotic case registers to estimate the incidence and prevalence of narcotics use in the population is counterindicated by a number of inherent problems that have been outlined by others working in the field. These problems include the following: systematic reporting biases (Defleur, 1975); absence of reports from some sources (Smart & Ogborne, 1974); differences in ascertainment procedures (Richman, 1973); dearth of ancillary data (Richman, Fishman, Bergner, & Patrick, 1971); and outdated information because of lack of purging of the dead, cured, and mobile (Blumstein, Sagi, & Wolfgang, 1973). Nevertheless, some researchers have argued that registers can be used to make population estimates if certain assumptions are made. These assumptions include the following: the death rate among the registered is the same as that among the unregistered (Andima, Krug, Bergner, Patrick, & Whiteman, 1973); underreporting in one locale is the same as in others (Ball, Englander, & Chambers, 1970, pp. 68-78); the population of heroin addicts does not change between time periods (Greenwood, 197 1); and there is an independence between ascertainment sources (Blumstein et al., 1973). Since all of these assumptions are open to challenge, it is our opinion that, for the time being at least, narcotic case registers should not be used for estimating the incidence and prevalence of narcotic use in the population.

Having said that, however, we would like to deal with the question of what kind of sound and useful contribution such registers can make in helping us improve our knowledge of narcotic-related problems. In doing so, we would like to draw on the experiences and work of our unit with a system for collating and tabulating reports received from various sources, namely a narcotic users index maintained by the Bureau of Dangerous Drugs (BDD) of Health and Welfare Canada.

As this index has been described in detail elsewhere (Rootman & Richman, 1975; Thomas, 1975), we will describe it only briefly here. It is a record of the known users of the major natural and synthetic opiates governed by the Canadian Narcotic Control Act. Maintained and revised annually by the BDD since 1955, it is based on information provided by several sources including law enforcement agencies, treatment centers, and pharmacists. Information on three groups of users, "illicit," "licit," and "professional," is recorded on McBee cards. Information is added to existing cards when it is received, and new cards are created when persons are reported for the first time. Cards are dropped if no reports have been received for 10 years or if notification of the death or deportation of an individual is received. Each year the BDD prepares basic statistical tabulations of some of the information in the index (province of residence, sex, age, drug, and report source) by manual sorting of the cards. Efforts are currently under way to make this file machine-readable.

To date, our Directorate, the Non-Medical Use of Drugs Directorate, Health and Welfare Canada, has used the narcotic users index for three purposes: (a) to determine the prevalence of reported narcotic use in Canada; (b) to determine the incidence of reported narcotic use in Canada; (c) to determine the social and geographic distribution of reported narcotic users in Canada. We would like to briefly report some of our major findings..

With regard to "reported prevalence," in a paper just published in the *United Nations Bulletin on Narcotics* (Rootman & Richman, 1975). we examined the reports of the BDD from 1956 to 1973 on *all* narcotics cases in its index in selected years. The following were among our findings:

1. There was a substantial increase in the number and rate of reported "illicit" narcotic users in Canada over the time period.

2. There were decreases in the number and rate of "licit" and "professional" users.

3. The greatest increases in reported prevalence took place after 1969.

Because we felt uneasy about the adequacy and accuracy of data on *all* cases (prevalence) especially when viewed over a long time period, we suggested that the BDD make separate tabulations on *new* cases coming to its attention. We have now examined such tabulations for 1973 and 1974 and have prepared a report describing recent trends in "reported incidence" of narcotic-related problems (Thomas, 1976). Some of our main findings follow:

1. A slight decline between 1973 and 1974 in the overall number and rate of all *Narcotic Control Act* drug users reported for the first time to the BDD.

2. A substantial decline in the number and rate of heroin users reported for the first time.

3. An increase in the number and rate of newly reported users of cocaine and synthetics.

4. Little change in the number and rate of newly reported users of other opiates.

Finally, with regard to the distribution of reported narcotic users in Canada, both the work on "reported prevalence and "reported incidence" have provided significant information. For example, some of the following trends were revealed by the data:

1. There were generally more reported male than female users in all age groups, a trend that increased over the total time period and that appears to be continuing, according to the incidence data.

2. There were recent dramatic increases in the numbers of users in the 20-to-24-year-old group, which has become the group with the highest reported prevalence in recent years. The incidence data suggest a continuation of the latter trend.

3. There has been and continues to be a concentration of known narcotics users in British Columbia, although the incidence data suggest a dramatic recent reduction in numbers and rates for British Columbia and increases in other provinces.

The findings that we have presented here, and which are elaborated in more detail in our other reports (Rootman & Richman, 1975; Thomas, 1975, 1976), are particularly interesting from a programmatic and policy perspective. They provide policy and program people with valuable clues as to how much effort is required to deal with, narcotic-related problems, where and to whom such effort might best be directed, and what kind of problems to anticipate in the near future.

We would not, however, like to give the impression that we accept these findings without qualification, for such is emphatically not the case. We are aware of the limitations of the narcotic users index as a source of information and have discussed them at length elsewhere (Rootman & Richman, 1975; Thomas, 1975, 1976). These include: lack of information on drug use patterns and on factors that affect individual and social risks associated with the consumption of narcotics; the retention of cases where narcotic use has terminated; reporting biases due to changes in the level of enforcement over time or in treatment practices; variability between reporting sources in the accuracy and completeness of information and the differential tendency to take personal and social characteristics into account in the ascertainment process; regional variations in ascertainment activity; and differential time lags between ascertainment by the source and report to the BDD.

It is our feeling, however, that in spite of these problems and the interpretational difficulties that they lead to, it would be self-defeating to argue, as some have done, that such an index is totally without value for epidemiologic purposes. All sources of data, including surveys and participant obseivation studies, have limitations. It is important, however, to recognize their strengths as well. In the case of the BDD narcotic users index, the strengths are that it draws information from a variety of sources across Canada, the reporting from these sources is almost complete, and the tabulations are timely and they differentiate individuals from events. Thus, the index is currently the most comprehensive source of information on reported narcotic users in Canada.

A number of studies offer firm support for the latter statement. For instance, a recent study of Toronto heroin users known to the Addiction Research Foundation of Ontario, the Royal Canadian Mounted Police, or the BDD, found that 64% of the combined total were known to the BDD (Oki, 1972). Similarly, a study of heroin- and methadone-related deaths in Canada from 1970 to early 1972, conducted by the LeDain Commission, found that about half of the cases were known to the BDD as opiate narcotic users (Commission of Inquiry into Non-Medical Use of Drugs, 1972), and a more recent study of narcotic-related deaths in Vancouver in 1973, conducted by our unit (Sanderson, 1974), found that 60% were known.

As a result of these studies, we have considerable confidence in the index as a source of information on narcotic-related problems in Canada and feel that there is much to be gained from continuing the type of research that we have described to you today. We are therefore planning to replicate these studies and are carrying out and planning others such as the following: a study of the reported incidence of narcotic-related problems in specific communities; a study of the retention of persons in methadone treatment; a follow-up of subsequent reports for newly reported cases; and a study of the spread of narcotic use outside the metropolitan areas. In addition, since we feel it is important to maintain a critical attitude toward the index, we are planning to carry out studies such as the Vancouver deaths study (Sanderson, 1974), to assess the reliability and comprehensiveness of the index on a continuing basis.

In conclusion, the extent to which the findings presented here and elsewhere (Richman & Rootman, 1975; Thomas, 1975, 1976) can be projected to provide accurate estimates of the incidence and prevalence of narcotic use in Canada is unknown and perhaps unknowable. On the other hand, as minimal estimates of the extent and geographic and social distribution of narcotic-related problems in Canada, these data and others based on the BDD narcotic users index are currently without parallel. In addition, the index has considerable untapped potential as a tool for epidemiologic research on narcotic-related problems especially if used as a supplement to local studies that employ other methods, and if continued attempts are made to improve its accuracy, reliability, and timeliness. It is our intention to use it in this manner, and we feel that if other narcotics case registers were used in this way, they too would make their appropriate contribution to our knowledge of narcotic-related problems.

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The author gratefully acknowledges the assistance and comments of Eleanor Thomas and Alex Richman as well as the collaboration of the Bureau of Dangerous Drugs, Health and Welfare Canada.

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Psychosocial and Biomedical Aspects of Deaths Associated with Heroin and Other Narcotics

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Reliable and relevant data are scarce concerning the etiology of deaths due to psychoactive drugs. As a result, nationwide efforts to combat an apparently growing use and abuse of dangerous drugs have been seriously hampered.

To begin to obtain the kind of information needed to appreciate some epidemiological aspects of drug-associated deaths, the National Institute on Drug Abuse (NIDA), in collaboration with the Special Action Office of Drug Abuse Prevention (SAODAP), contracted with a research team from the University of California at Irvine. The goals of the resulting project were, broadly, threefold: (a) to develop and test a comprehensive form for recording information on psychoactive drug-associated deaths (Gottschalk, McGuire, Heiser, & Alexander, 1973); (b) to use this form to collect data on 2,000 cases from the medical examiners or coroners in nine major urban cities (Gottschalk, McGuire, Birch, & Heiser, 1976); and (c) to get an estimate of the quality of toxicological investigations carried out in the laboratories of these nine urban reporting centers, with the long-term goal of exploring means of improving the uniformity and accuracy of such analytical determinations so that nationwide surveys in this area might rest on a more valid and consistent foundation (Gottschalk et al., 1975; Dinovo & Gottschalk, 1976, Dinovo, Gottschalk, McGuire, Birch, and Heiser, 1976). The resulting *UCI Reporting Form for Drug-Involved Deaths* consists of about 135 items of inquiry in such data areas as biography, demography, on-site investigation, toxicology, post-mortem, treatment prior to death, and suicide.[1]

For this conference, presentation is limited to: (a) a brief report of some psychosocial and biomedical characteristics of 551 heroin-associated and 302 other narcotic-associated deaths from the sample of 2,000; and (b) intercity differences in the quality of toxicological examinations.

Psychosocial and Biomedical Characteristics of Narcotic-Associated Deaths

Study Methodology

Cases included for study were those in which psychoactive drugs were involved as a primary, contributing, or indirect cause of death. Cases in which alcohol was combined with a psychoactive drug, and in which the combinations were considered by the medical examiners or coroners to play a significant role in death, were accepted.

Limitations of funds and other constraints led to a decision by the NIDA to limit the study to 2,000 cases from the jurisdiction of the medical examiners or coroners of nine specific cities. The cities included such heavily populated ones as New York, Chicago, Los Angeles, and Philadelphia, as well as the less populated areas of Miami, San Francisco, Cleveland, Washington, D.C., and Dallas.

Based on 1970 census figures, quotas were set for each jurisdiction on the basis of the ratio between its population and that of the others. Thus, if City A has twice the population of City B, City A would contribute approximately twice as many cases. Accordingly, cities with the larger populations were targeted to provide more cases for this study (for example, New York, 400, Chicago, 300; and Los Angeles, 300). Smaller cities were assigned smaller quotas, but slightly more than their percentage share in order to achieve a minimal number for purposes of statistical analysis (for example, Dallas, 150; Cleveland, 150; and Washington, D.C., 150). During data collection, certain cities had difficulties meeting these quotas, and some minor readjustments were made. Because of local problems the rate of data collection varied. Therefore, though the cases were chosen consecutively over weekly or monthly time periods, none were selected during some time periods for some cities. Hence, an extended period of time (1972-1974) was required to fill all quotas. However, these samples were considered by the medical examiners or coroners from each of the nine cities to be representative of all psychoactive drug-involved deaths over this time period.

The forms were filled out by personnel who were selected by each office but paid directly by the investigators and monitored by telephone and personal visits of research team members. Each person was paid a very adequate fee per form, and the motivation and talent applied to the task can be considered to have been very high.

When a report form was received from the office of the medical examiner or coroner it was scanned for obvious omissions, and when units of measurement different from those that were recommended were used, they were corrected or converted when possible. A computer program designed specifically to detect a variety of potential errors in filling out the forms was developed and utilized by the research team, and after processing each case was added to a data bank..

The nine cities are not representative of the entire nation, but probably constitute a fair picture of urban America. In combination, these data allow a meaningful analysis of within-city variables such as age and sex. In addition, percentages of such characteristics associated with drug abuse across cities are important and illustrate the regional variability of the drug abuse problem in the United States.

Study Findings

Heroin-Associated Deaths. Of the 2,000 cases, 551 or 28%, were listed as deaths involving heroin or morphine. Most medical examiners did not differentiate heroin from morphine deaths since morphine is the form in which heroin is found in the body. The medical examiners reported heroin as instrumental in the death when direct evidence was present, such as the presence of a syringe and cooker at the site of death, especially if the syringe was left in the body and contained traces of heroin. However, these nine offices were unanimous in the opinion that "practically all" of the cases listed as morphine were heroin-associated.

Role of Heroin in Death. Heroin may contribute to a death in a variety of ways. It may have a fatal outcome owing to accidental misuse, an inadvertent toxic combination with other chemical substances, or may be a deliberate instrument of suicide or homicide. Therefore, a schema for defining and categorizing the role of a drug in each of these types of death was devised. Its application to the study's heroin deaths, presented in Table 1.

As noted, the role of heroin in these deaths was considered accidental or "unexpected" in about 60% of the cases. In 36% of the cases, heroin was specifically the cause of death with no other agent playing a significant role, while in approximately another 40%, heroin in combination with some other potentiating agent, such as alcohol or a barbiturate was the cause of death. It was definitely used as the instrument of suicide in four cases and homicide in an additional four. In 82, or 15%, of the cases it coexisted with homicide by other means (gunshot, stabbing, etc.); the latter finding presumably reflects the criminal environment with which heroin is associated.

Intercity Differences in Heroin-Associated Deaths. As shown in Table 2, among the 2,000 deaths the larger cities had a higher percentage, 20-40%, of heroin-associated deaths in comparison with the 7-13% rate of the smaller cities of Cleveland, Dallas, and Miami. This is consistent with the fact that these smaller cities report a less serious heroin problem.

Demographic Differences in Heroin-Associated Deaths. The majority, 56%, of the heroin victims were in their twenties with an additional 20% in their thirties. Nearly 80% of them were male, and the majority, 57%, were black. Thirty percent of the victims were reported as white. Of the remaining ethnic groups, the Mexican-Americans were most represented, at 7%.

More than 50% of the victims were never married, probably reflecting the youth of the population. Somewhat surprisingly, 47% of the group were listed as employed at the time of their demise, and a relatively small percentage seems to have been on welfare. However, since about 32% of them were reported as unskilled in their main occupational pursuit, it appears that their level of employment was generally not very high.

Heroin, Methadone, and Other Narcotics Involved in Drug-Associated Deaths

Among the 2,000 psychoactive-drug involved deaths, there was a total of 853 narcotic deaths. Deaths due to heroin (including those due to morphine) totaled 64%, and another 32% of the deaths were associated with methadone. Codeine was involved in 12 (1.4%) of these deaths and Demerol in another 11 (1.3%). Dilaudid added only three cases.

TABLE 1

Role of Heroin in 2,000 Drug-Involved Deaths

	N	%
Drug-Induced		
Simple or direct – the drug in question was specifically the cause of death with no other agent playing a significant role.		
Accidental or "unexpected"	150	27.0
Suicidal	4	.7
Homicidal	2	.3
Unknown	44	6.0
Subtotal	200	36
Drug in combination with some other potentiating or synergistic pharmacologic agent. such as alcohol or barbiturates.		
Accidental or "unexpected"	159	29.0
Suicidal	0	0
Homicidal	2	.3
Unknown	65	12.0
Subtotal	226	41.3
Idiosyncratic – an unexpected effect, such as an anaphylactic or immune reaction.		
Accidental or "unexpected"	0	0
Drug-Related		
Drug in combination with some pre-existing and potentially deadly physiologial condition, such as diabetes or chronic heart condition.		
Accidental or "unexpected"	23	4.0
Suicidal	0	0
Homicidal	0	0
Unknown	1	1
Subtotal	24	4.1
Drug in combination with some physical event outside of the Patient's body, such as death by vehicle or gunfire while under the influence.		
Accidental or "unexpected"	6	1.0
Suicidal	3	.5
Homicidal	82	15.0
Unknown	2	.3
Subtotal	93	16.8
Drug in combination with some medical disorder or disease probably produced by drug abuse, such as hepatitis, bacterial endocarditis. or tetanus.		
Accidental or "unexpected"	5	. 9
Unknown	3	. 5
Subtotal	8	1.4
Total	551	100

Source: Gottschalk, McGuire, Birch, & Heiser, 1976.

TABLE 2

Percentage of Drug-Involved Deaths Listed as Heroin-Associated, by City

	Total in sample	Heroin-associated deaths	% of sample total
Chicago	295	123	42
Cleveland	150	19	13
Dallas	100	11	11
Los Angeles	300	141	47
Miami	151	10	7
New York	405	71	18
Philadelphia	199	66	33
San Francisco	250	71	28
Washington, D.C.	150	39	26
Total	2,000	551	27

Besides heroin, thus, the only narcotic of significant fatal involvement appeared to be methadone, which was associated with 276 cases or 32% of the 853. In these cases methadone was the direct and sole cause of death 40% of the time, and about 40% of the time it was combined with other drugs, such as alcohol or the barbiturates. Therefore, it may be said that while heroin is the Number One agent in narcotic-associated death, methadone is a strong second.

Most of these methadone-associated deaths were from New York and Washington, D.C. — 177 from New York and 62 from Washington. For New York this represented 70% of narcotic deaths in the data and for Washington, 60%. Miami, Philadelphia, and Cleveland were in the 20 to 30% range for deaths associated with methadone, while San Francisco, Los Angeles, and Chicago reported that only 0-3% of their narcotic-category deaths were methadonerelated. It is therefore quite apparent that large regional differences exist in the abuse of methadone.

Some of these differences are undoubtedly reflective of real geographic differences in drug availability and drug abuse habits. Another factor that may contribute to this variation may be the failure in some locales to look for or detect methadone in the body's biological fluids. This issue is further discussed below. Our data suggest, in summary, that methadone deaths are, indeed, a significant problem, especially among Eastern cities, and that to some unknown extent the problem may be underestimated because of variations in detection and reporting procedures.

Suicide and Narcotics

An estimate was made of the probability that any of these 853 narcotic-involved deaths was a consequence of suicide. Only 17, or 2% were listed as "definitely suicide," but 25% were thought "possible suicide," and another 19% were considered "suspicious." Thus, in nearly half of these narcotic-associated deaths, suicide was not entirely ruled out. Even though those people who commit suicide by use of barbiturates and similar drugs, as a group, tend not to be addicts or to have the socioeconomic characteristics of the narcotic user, it is apparent that suicide is not to be dismissed as a consideration among narcotic users. There is ample evidence elsewhere that the suicide correlates of despair and depression exist among the narcotic-using population.

Narcotic-Associated Deaths by Race in Selected Cities

Whites exceeded blacks — 41% versus 30% — in narcotic deaths in Los Angeles, with Mexican-Americans at the level of 24%.

In New York this ratio was the opposite, for blacks exceeded whites in narcotic-involved deaths (55% versus 30%); Puerto Ricans added about 8% and Latin Americans another 6%.

In San Francisco, like Los Angeles, whites exceeded blacks (62% versus 30%), with Orientals producing 4% of the narcotic-associated deaths. In Chicago, narcotic deaths were confined almost entirely to whites and blacks — 29% versus 67% — with Puerto Ricans contributing an additional 3% to make up the entire sample.

Racial differences in psychoactive drug-involved deaths in different American cities are sometimes, but not always, correlated both with differences in local population and with the kinds of drugs locally used or abused (Gottschalk et al., 1976). Between the two major drugs, heroin and methadone, the differences between drug-involved death rates for whites and blacks were not very great, each race showing a split of about 60% heroin deaths and 313% methadone deaths. The 26 Puerto Ricans and Latin-Americans showed about a 50-50 split, while all of the 39 Mexican-American deaths were associated with heroin and no methadone or any other narcotic.

Intercity Differences in the Quality of Toxicological Examinations

A proficiency testing program was performed in collaboration with the nine major cities (Dinovo & Gottschalk, 1976). Since the appraisal of drug-associated deaths reported from coroners and medical examiners offices depends, along with other relevant data, on test results obtained from their toxicological laboratories, toxicological examinations are crucial. Differences in laboratory procedures, thoroughness of screening, and limits of detection could result in sizable differences in some details within mortality statistics from various reporting areas. Brief proficiency studies were, therefore, conducted to determine the differences in the quality of toxicological examinations performed by these toxicological laboratories.

Five standard “unknown” samples were sent to each city. Three samples consisted of drugs added to clean urine and two samples were drugs added to a 3% solution of human albumin. Some drugs were repeated at the same concentration in different samples. All five unknown-specimens were designed to contain 6.5 μg/ml of secobarbital to measure the variations in the secobarbital assay for a given laboratory over a period of time. The concentrations chosen for these unknown samples were at low toxic levels to provide a moderate challenge to the toxicological methods. Some samples were sent as complete unknowns, some with partial information (such as “Drugs in this vial are neutrals and volatiles” or “This vial contains morphine and methadone”), and some samples were sent with all drugs contained identified. This program was set up to simulate the actual situation prevailing in offices of medical examiners, where variable amounts of information are available for each case.

Since an agreement was made with each toxicological laboratory that the results for this proficiency testing would be strictly confidential with respect to what laboratory had obtained what results, all reports and findings were coded numerically, and these findings were not identified by city.

All participating laboratories were found to have adequate instrumentation and methodologies to quantitate the psychoactive drugs when they were known to be in the sample. Errors in quantitation, including both false negatives and false positives, were made much more frequently in the process of screening for the drugs than in quantitating them. Overall, a wide variance was found among the nine cities in detection, accuracy, and precision of toxicological analysis. This is likely to be a factor in the contribution of variations in the certification of the cause of psychoactive-drug-involved deaths in these and, by inference, other cities in the United States.

For example, one of the completely unknown samples contained morphine (3.5 μg/ml) and methadone (2.5 μg/ml). Only three of the nine participating toxicological laboratories found and quantitated the morphine, and an additional four laboratories detected morphine in this urine sample but did not quantitate it. Two laboratories did not detect the morphine present in this urine sample. With respect to methadone, the same three toxicological laboratories located and quantitated methadone in the urine sample that quantitated morphine in it, but only an additional two toxicological laboratories detected the methadone. Four laboratories did not even detect the methadone present in the sample. It was of interest to note, also, that although methadone was present in this urine sample in the concentration of 2.5 μg/ml, the range of concentration measured by the three toxicological laboratories that quantitated this chemical was from 2.8 to 10 μg/ml.

Clearly, the lack of uniformity of proficiency or quality control indicates the possibility of, at least, a modest error variance in national death statistics as reported by some cities and introduces some question into the relative accuracy of toxicological evaluations. One must not interpret these results as being overly critical. In a separate study by the UCI group, the same standard samples were sent for analysis to 19 laboratory members of a state association of toxicologists, a mixed group of forensic, commercial, and clinical laboratories. This group produced 33% false negatives, exactly that of the nine target cities (Gottschalk et al., 1975; Dinovo & Gottschalk, 1976).

Summary

Our findings point to sizable intercity differences in the United States among certain psychosocial and biomedical aspects of deaths associated with narcotics. Secondly, narcotic-involved deaths are not purely accidental, but many are motivated by suicidal goals and a smaller percentage by homicidal intentions. And finally, in addition to the errors that have been surmised to occur in estimates of psychoactive drug deaths from heroin and other narcotics owing to inadequate reporting or other shortcomings in data collection, there are biomedical errors due to variations in the quality control of toxicological laboratories.

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The studies described in this paper were carried out in collaboration with research colleagues Jon F. Heiser, M.D., Eugene C. Dinovo, Ph.D., and Herman Birch, Ph.D., all with the Department of Psychiatry and Human Behavior, College of Medicine, University of California at Irvine, and were supported in part by a contract (No. 271-75-3004) from the National Institute on Drug Abuse, ADAMHA, Rockville, Md.

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[1] A copy of the UCI Form (revised October 1975) is available upon request from the authors.

Narcotics Users, Narcotics Prices, and Criminal Activity An Economic Analysis

FRED GOLDMAN, Ph.D.

> It is quite acceptable on common sense grounds that addicts will steal to support their habits. What is not so readily apparent is why every addict does not steal.
>
> Gould, 1974, p. 70

Current Policy Perspective

One of the overwhelming reasons for public concern with the consumption of heroin and other narcotic drugs, and many feel it is the only reason, is the impact of narcotics on criminal activity. Evidently the heroin addict is a criminal and responsible for much of the crime that is committed in urban America, although there are a variety of numbers to be attached to the word "much" (Casey & Preble, 1974, pp. 283-307; Patch, 1973; Singer, 1971). But it is a long step between attributing income-generating criminal acts to persons who consume heroin and other narcotic drugs and establishing that there is a systematic relationship that links the consumption of heroin and other narcotic drugs with income-generating criminal activity. Nearly every researcher of the drug use/crime nexus has found support for the first point (Greenberg & Adler, 1974). Exhortations from the prior literature notwithstanding, we have yet to adequately test or even formulate the hypotheses that would let us judge the second point.

The elements that predominate in the "logic" that the consumption of narcotic drugs leads to income-generating crime are the same elements that are involved in identifying the determinants of drug use in general. Few addicts have legal opportunities to support narcotic habits that cost $25 to $45 a day. Either the addict resorts to income-generating criminal activity to raise the necessary funds, or he reduces drug consumption. Fear of withdrawal, a profound craving, or the sheer pleasure resulting from drug use are often cited as reasons why reduced consumption is an unlikely choice.

The conventional wisdom appears less compelling when "user" is substituted for "addict." All addicts are users but not all users are addicts. Four categories of drug users adopted by the President's Commission on Marihuana and Drug Abuse (1973) are (a) infrequent recreational users, (b) frequent recreational users and circumstantial users, (c) intensified users, and (d) compulsive users. Each succeeding category is meant to describe greater use, in both frequency and quantity per unit of time. Moreover, each succeeding category is assumed to have behavioral content and to represent a decreasing sensitivity of the consumer to a change in the price of narcotics.

The manipulation of drug prices is thought to be an efficacious policy instrument for the reduction of drug use and, by extension, criminal activities associated with drug use. The potential for manipulating the prices of illicit drugs is commonly thought to be within the realm of conscious government policy. In the recent White Paper on *Drug Abuse* (1975, p. 2), for instance, supply-reduction efforts were recommended "to make drugs difficult to obtain, expensive, and risky to possess, sell or consume." According to the *White Paper,* this would require additional law enforcement since the reduced supply and concomitant price increase has the adverse side effect that "crime rates increase as users, attempt to meet the rising cost of scarce, illegal drugs" (p. 3). However, the paper also states that law enforcement should be complemented with treatment, since "reduced drug availability (and intensified law enforcement in a city) increases pressure on drug users to seek treatment" (p. 4). Thus, an increase in price via successful supply reduction will, according to the White Paper, "(1) minim& the number of new users, (2) increase the number of old users who abandon use,…: (3) decrease the consumption of current users, (4) increase the demand for treatment, and (5) increase crime rates" (p. 3).

It is not obvious, a priori, why these outcomes must follow to the exclusion of others. A brief sketch of some economic relationships between narcotics prices, narcotics consumption, and related criminal activity follows.

Narcotics Prices, Narcotics Consumption, and Related Criminal Activity

Expenditures on heroin and other narcotic drugs are identically equal to the price per unit of drug multiplied by the quantity consumed. It follows that the direction and extent of variation in drug expenditures due to a change in drug prices depends on the quantity of drugs that would be consumed at alternative price levels. The measure of this relationship is defined within the vernacular of economics as the "price elasticity of demand" — the percentage change in the quantity of the purchased good, in this case narcotic drugs, divided by the percentage change in its price.

Consumption of heroin is often *assumed* to lead to one aspect of "addict-behavior," a reluctance to reduce heroin consumption when its price rises. This suggests that the price elasticity of demand for heroin is extremely low (certainly less than one). Although a rise in the price of heroin may decrease the quantity of heroin consumed, the total expenditures on heroin will rise. In the extreme, if there is no decrease in consumption when price rises (a price elasticity of zero) the percentage increase in expenditures on heroin will be equal to the percentage increase in the price of heroin. This assumption is an integral part of the "conventional wisdom," which tells us that heroin consumption leads to revenue-raising crime. Increased expenditures require increased income. By limiting the heroin user's income-generating opportunities to criminal markets there is, ipso facto, an increase in the user's illegal 'income.

If the consumer of heroin has excellent, low-cost narcotic substitutes, say, inexpensive street methadone or access to methadone maintenance, then his or her consumption of heroin may be "sensitive" to increases in the price of heroin. In this case, an increase in the price of heroin may lead to a decrease in expenditures on heroin and, concomitantly, a decrease in illegal income.

These assumptions and outcomes by no means exhaust the possibilities that link heroin use and income-generating criminal activity via price changes. We have considered the effects of a price change as if a single user population existed and each consumer responded in a similar manner. Although there are no reliable figures on how consumers are spread across the user categories, the National Institute on Drug Abuse estimates that only one in 10 heroinusers is "addicted." When the occasional user and polydrug user are a substantial part of the heroin-consuming population, the direction and extent of the *net effect* of a heroin price change on income-generating crime will depend on the mix of users, their relative price elasticities, and the way in which they move across user categories as price changes.

As an example, consider two areas with an identical number of users. Let one have a relative abundance of "hard-core" users with an inelastic demand, the other a relative abundance of occasional users with an elastic demand. A rise in the price of heroin, other things equal, will result in a decrease in the quantity of heroin consumed in both areas. However, the area that predominates in hard-core use will experience an increase in income-generating criminal activity (since it will experience a net increase in expenditures on heroin). The same price increase will lead to a net decrease in income-generating crime in the area with a preponderance of occasional users.

Heroin users are assumed to be reluctant to decrease heroin consumption when faced with a decrease in income. To the extent that the consumption of heroin disrupts the user's work schedule and leads to a decrease in legal income, and to the extent that legal income is used to support heroin consumption, it is expected that heroin use will lead to a rise in income-generating criminal activity. That is, the user would increase time spent in criminal activities as a substitute for time in legal activities since heroin consumption is assumed to be relatively less dysfunctional in criminal settings. This suggests, of course, that persons can simply "phase out" legal activities and begin to successfully — and profitably — commit crimes!

One important consideration can be derived from the accumulated observations that many lower level narcotics dealers are, themselves, users. For instance, Hughes, et al. (1971) observed a "heroin copping community" where 34% of the 125 addicts were primarily engaged in drug distribution. Selling heroin and other narcotics is one way of maintaining a continuous supply and a method of financing consumption. If there is a positive and, perhaps, increasing relationship between the quantity of heroin consumed and one's relative state of drug dependence, and a similar relationship between drug dependence and the propensity to engage in income-generating criminal activities (a corollary of the conventional wisdom), then to the extent that heavy heroin users self-select into drug dealer occupations, the heroin-use/property-crime relationship will be reduced. The "pure heroin" habit sizes of the user populations have been quoted by Holahan (1972, p. 289), as: small, 20 mg/day; medium, 50 mg/day; and dealer, 180 mg/day. He also states that "if other factors remain the same, the total social cost of heroin addiction will be less if the percentage of consumed heroin earned from pushing is increased."

It may be that persons who consume sizeable quantities of heroin select out drug dealing; it is also possible that persons who sell heroin consume large quantities of it because of its relatively lower price, higher purity, availability, and so on. The important point for consideration is, however, the extent to which the distribution system "removes" the financial onus from the heavy users and leaves a body of users to which the conventional wisdom does not readily apply. Hughes, et al. (1971, p. 46) found that workers in legitimate occupations "who reported less frequent use and less expensive habits, paid for their drugs largely through their own legitimate income."

A supply reduction strategy that drives up the price of heroin can be expected to *reduce* the total quantity of heroin consumed. Yet, its impact on the number of new and current users and their criminal activity is not obvious.

Define a population of drug users to encompass persons who consume within the entire spectrum of illicit drugs (Pop), a heroin-using subset of these (Uses h), and the total quantity of heroin used per time period (h). Total heroin consumption per unit time is identically equal to the product of the average consumption per heroin user, the relative number of heroin users in the drug-using population, and the absolute size of the drug-using population. Write this as,

$$h \equiv (h/\text{Uses } h) \cdot (\text{Uses } h/\text{Pop}) \cdot (\text{Pop}) . \qquad (1)$$

We may expect the term on the left-hand side to fall with an increase in price as long as all users are not completely bound to their current level of consumption. The three terms on the right-hand side are likely to fall. However, they are not likely to be equally sensitive to a change in price, and an increase in any one of them cannot be ruled out. For instance, if heroin consumers reduce their average consumption, say, by substituting methadone, the number of heroin consumers may be unchanged in the short run. Over a longer period the number of heroin users may increase if such a multidrug experience allows for easy individual management of narcotics use. As the "strung-out" addict disappears, so may the "visual" disincentive to engage in the consumption of narcotic drugs. A similar theme can be developed to account for the potential impact of an increase in treatment. Fear that addiction is lifelong is a deterrent to use; successful treatment programs and easy access to them reduces the deterrent effect and provides the opportunity for leakages of methadone into illicit markets (Levin, et al., 1975). While an increase in price may decrease the quantity of heroin consumed, it is not obvious how the impact would work itself through the several dimensions of users and drug-associated culture which, when combined, yield an aggregate of heroin consumed.

Now consider the impact of a change in the price of heroin on the amount of income-generating criminal activity. It is possible that an increase in price would discourage the entry of new heroin users. Disregarding, for the moment, any criminal activity of current heroin users, criminal activity may be lower than what it would have been in the absence of a price rise if the foregone users are not drawn into the heroin-crime nexus. As Levin, et al., (1975) point out, it is more reasonable to expect peer group interactions that provide entree into an unfamiliar ritual to be the major determinant of initiation. While some current users may continue, indeed increase, their income-generating criminal activities when price rises, others may drop out of use and, perhaps, reduce their criminal activity. However, if the determinants of criminal activity

are independent of heroin use, and if heroin use reduces the criminal's efficiency, then criminal activity may rise in the absence of heroin use! Thus, the net effect of a rise in the price of heroin on crime is ambiguous.

The aggregate amount of heroin-related criminal activity per unit of time (c), is identically equal to the product of the size of the user-criminal population (Criminals/Pop) and the average number of offenses committed by each user-criminal (Offenses/Criminals). Write this as,

$$c \equiv (\text{Criminals}) \cdot (\text{Offenses/Criminals}) \ . \qquad (2)$$

A rise in the price of heroin would have an impact on the user population and then work its way through the variety of income-generating activities.

Although the net effect of a rise in the price of heroin is uncertain, exit from current use and foregone initiation of heroin use may serve to lower what would otherwise have been a larger user-criminal population. To the extent that addicts are more visible criminals, however, they bring attention to the criminal population in general. This raises the risk attached to each offense, thereby lowering the expected return per offense. A decrease in the addict population is likely to raise the returns to non-addict, professional crime and lead to a substitution of nonaddict for addict criminals. Could (1974, pp. 55-77) has suggested that addict criminals may "drive out" the professional criminals by competing with them. Also, addicts when freed from the need to "pursue" heroin may choose not to leave their criminal occupations since (a) alternative opportunities may be lacking, and (b) they will have accumulated a set of skills that are difficult to transfer to legal occupations. Finally, in the absence of heroin consumption, ex-addicts may become more efficient criminals, thus raising the costs they would face for opting into legal activities. The second term on the right-hand side, (Offenses/Criminals), represents the aspect of user-related criminal activity that is most likely to be affected by a price rise. Although the aggregate number of user offenses is dependent on the size of the user population as well as the offenses per criminal, it could be argued that those persons who are to remain viable heroin consumers would have to increase their number of offenses in order to generate additional income. Heroin requirements may not be easily forestalled for some portion of the user population. Their desired level of heroin consumption is a "constraint" and an attempt may be made to generate income until it "exhausts" heroin requirements.

Consider the relationship between the use of narcotic drugs and participation in criminal activities. If all heroin use were financed from income-generating criminal activity, then the dollar amount of criminal activity related to heroin use would be identically equal to expenditures on heroin. We can write this as identity 3:

$$H \equiv C \ , \qquad (3)$$

where H is total expenditures on heroin (h) and C is total returns from criminal offenses (c). This relationship holds, ex post, and has been used to indicate the dollar costs of crime associated with the consumption of narcotic drugs. Casey and Preble (1974, pp. 283-307), for instance, offer an estimate of total drug expenditures by the nation's addict population. They add a subsistence component to it and net out income from legal activities. The resulting figure is then "allocated" among the

alternative forms of income-generating crimes according to estimates of the distribution of the addict-criminal population across criminal specialties. A similar approach is suggested by Holahan (1972, pp. 255-299).

It is inappropriate to interpret this identity as "a given dollar expenditure on heroin *leads to* an equal dollar amount of crime." It does not indicate behavior. Drug use may be a motivating force for some income-generating crime, ex ante. However, the reverse direction of causality between heroin consumption and income is also plausible. Persons who engage in income-generating criminal activities *and* happen to be consumers of narcotic drugs, may spend a portion of their income, perhaps a sizeable portion, on those drugs. In both cases, success in criminal markets is required for continued drug consumption. The question remains, however, whether the consumption of heroin and other narcotic drugs leads to criminal activity, or whether criminal activity leads to drug consumption, or whether both of these relationships exist.

A more appropriate approach is to consider these two relationships as being simultaneously determined: the criminal income of heroin users is dependent on expenditures on heroin and a "vector" of other determinants such as socio-demographic and environmental variables; similarly, dollar outlays on heroin are dependent on criminal earnings and a vector of other determinants. We may also relax the stringent assumption that criminal earnings are the sole source of funds for heroin consumption and include the role of legal income in the simultaneous system. The National Commission on Marihuana and Drug Abuse (1973), after an extensive review of the literature relating drug addiction and criminal behavior, reports that 41% to 66% of the various study populations were employed immediately prior to arrest, incarceration, or treatment. Then a third relationship would be established to determine the impact of heroin expenditures and criminal earnings on legal income. Finally, we draw on the earlier discussion of the role of narcotics selling and include it as both a determinant of drug expenditures and an alternative to legal income and income from other criminal activities. The total system may be written as

$$C = f_1(\hat{H}, \hat{L}, \hat{S}, V_1) \tag{4}$$

$$H = f_2(\hat{C}, \hat{L}, \hat{S}, V_2) \tag{5}$$

$$L = f_3(\hat{C}, \hat{H}, \hat{S}, V_3) \tag{6}$$

$$S = f_4(\hat{C}, \hat{H}, \hat{L}, V_4)\,, \tag{7}$$

where C is criminal earnings and H is heroin expenditures, as before; L is legal income; S is income from drug sales; V_1, V_2, V_3, and V_4 are vectors of other variables. A circumflex over a variable indicates that it is endogenous, or mutually determined within the system; it determines the impact of the other variables and, itself, is determined by them.

It is the impact of $\hat{H}$ on C in Equation 4 that represents the dollar value of crime due to expenditures on heroin. This will be less than that indicated in Identity 3 since we have now accounted for alternative sources of income to satisfy "desired" or, perhaps, "compulsive" heroin expenditures that are simply due to the presence of income — whether from legal or criminal sources, including drug sales.

The argument that expenditures on heroin lead to an amount of crime that will yield those expenditures appears far less compelling when viewed this way. Recent insights

into the varieties of drug use and how users distribute themselves across legal and illegal activities further reduce our expectations of how much crime is *caused by* heroin use. The question of "how much" remains, however, and should be investigated for the varieties of use. It is an empirical question, and a proper framework for its investigation exists. Federal policies directed at supply and demand reduction cannot have predictable outcomes in the absence of such work.

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Heroin Epidemiology and the Demand for Heroin

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The relationship between the number of heroin users (even if estimated precisely) and the appropriate public policy toward heroin is complicated for many reasons, two of which are relevant here. First, the social cost imposed on society by heroin use is influenced by many factors in addition to the number of users. Second, alternative policies will affect the number of users and the social cost of their heroin consumption in ways that are difficult to predict.

One approach to the analysis of heroin policies starts with consideration of the aggregate demand for heroin. For any individual, the quantity of heroin purchased, and presumably consumed, during a given period is determined by a complex set of physiological, social, psychological, and economic factors. Included among these are the price of the drug, the prices of licit and illicit drugs that might substitute for heroin, and the individual's income or ability to raise revenue to purchase the drug. In the aggregate, the total quantity of heroin consumed (in a given time and place) will also be related to these factors. The formation of public policy would be greatly illuminated if we could estimate such a demand function. [Public policies are directed to shifting demand curve by affecting preferences (e.g., through education and counseling), and by lowering the price of substitute drugs (e.g., through methadone treatment centers), and to affecting the point on the curve at which the market operates (e.g., through law enforcement activities affecting the retail prices.)] While the lack of data on this illicit market precludes estimation of completely specified demand functions, some inferences about the demand for heroin can be made from recent empirical analyses.

A demand function for heroin emphasizes several factors relevant to a discussion of heroin epidemiology. First, the role of price (and thus supply) in determining the quantity that will be purchased is made explicit. (A broad definition of price is used

here to include the risk of arrest and "ripoff" as well as the dollar expenditure for purchase of a given quantity of heroin at a given potency.) Second, the possible interrelationships of prices (again, broadly defined) of substitutable drugs are considered. Third, the relationship between the heroin consumed and activities to raise money for its purchase is made clear. The conceptual problems of sorting out these various factors and quantifying heroin demand, use, and "addiction" (somehow defined) are not any different from those of measuring the demand for alcohol, tennis, or automobiles. The major difference is the availability of data.

A data source that provides some insight into the heroin market is a monthly price series for retail heroin. The remainder of this paper suggests what might be inferred about the epidemiology of heroin from such a price series and its use in conjunction with data on criminal offenses.

Estimation of Heroin Price

Consider a model (discussed further in Brown & Silverman, 1974) expressing the total quantity (HS) of heroin supplied in a given city during a given time as a function of the retail price of heroin (P), the quantity (Q) and potency (S) of the average heroin transaction, the activities of law enforcement agencies (L), and the availability of the narcotic to the wholesaler (V). The total quantity demanded (H_D) is a function of the retail price, the number of addicts (N), and the relative attractiveness of heroin (A). Making the usual economists' assumption that the market clears (supply equals demand), $H_S = H_D = H$, we have

$$H = f_1 (P, Q, S, L, V) , \tag{1}$$

$$H = f_2 (P, N, A). \tag{2}$$

In this system of equations, the quantity of heroin both bought and sold (consumed) and the retail price are simultaneously determined — the price adjusts so that the quantities demanded and supplied are equal. Given specific functional forms for Equations 1 and 2, we can solve for H and P and obtain reduced form equations as follows:

$$P = g_1 (Q, S, L, V, N, A) \tag{3}$$

$$H = g_2 (Q, S, L, V, N, A) \tag{4}$$

We cannot measure the total quantity of heroin consumed and thus are not able to estimate Equation 4. The price equation, 3, also contains variables for which measures are not available. It is therefore assumed that the level of law enforcement, availability of imported heroin, number of addicts, and degree of addiction are all relatively constant over the l-month periods. This is especially crucial with respect to the relative attractiveness of heroin (A), which is affected by the price of substitute drugs, the availability of treatment programs, peer group use, and many other factors. Thus, while we cannot separate the effects of the various components of supply and demand, we can estimate the monthly variation in prices due to the total of these forces.

The procedure for estimating Equation 3 uses data on undercover purchases of heroin by federal, state, and local narcotics agents (Brown & Silverman, 1974). It is

recognized that these data have several inherent problems, primarily that agents are operating at different levels of the distribution system at different times. Hence, the potency and quantity (and implied unit price) of the heroin purchased are likely to differ. The procedure adjusts the unit price of the heroin for its quantity and potency to reflect the unit price at which that heroin would be sold when it reached a particular city's streets (that is, potency and quantity levels that occur in typical "retail" transactions). Thus, variations in price induced by changes in potency levels are incorporated in this procedure. Since a number of purchases are made in a typical month, we average the adjusted retail prices to obtain an estimate of the price of street heroin in a city in a month. The biases which are likely in these data (for example, due to the effectiveness of undercover agents) are relatively less important within a given city across time.

External validation of the heroin price series is difficult. Whenever we were able to obtain narrative reports about supply conditions of the heroin market from narcotics agents, the data series produced was in agreement with the narrative. We were able to detect fluctuations reflecting dock strikes of the early 1970s on the East and West Coasts in the series for New York City and Los Angeles, respectively. Other evidence on the validity of the series is contained in Brown and Silverman (1974). More recent data allowed us to document the relative price trends of brown (Mexican) and white heroin as well as the eastward progression of brown heroin as Mexico became the major point of entry for heroin to the United States.

We are thus able to obtain a monthly measure of the relative availability of retail heroin on the streets of major U.S. cities.[1] As noted above, these data alone are not sufficient to indicate whether monthly price fluctuations are due to variations in supply, variations in demand, or some combination of both. Even without such knowledge, however, the price series may be useful to complement other data that vary monthly within a city, such as entry rates to treatment programs, longitudinal surveys or registers of addicts, death statistics, and incidence of hepatitis.

To learn more about the demand for heroin, it is necessary to extend the system of Equations 3 and 4 to include other markets related to the supply of or demand for heroin. To this end, it is useful to consider the relationship between the demand for heroin and criminal activity.

Estimation of the Heroin Price-Crime Relationship

It has been apparent for some time that a relationship exists between the use of heroin and criminal activity.[2] Many people arrested for crime in major cities are found to be heroin users, and those arrested often state they committed the crimes to finance their use of drugs.

The exact nature of the relationship between the price of heroin and crimes committed to support its purchase is not clear, however. If the price of heroin increases, users who finance all or part of their heroin consumption by revenue-raising crime can respond by committing more crimes to pay the higher costs, by reducing their consumption of the drug, or by some combination of the two. A decrease in the price of heroin may have no effect on the level of criminal activity but only result in more disposable income for the user-criminal. Finally, the amount of non-heroin-related crime may adjust to the amount of heroin-related crime so that the overall crime levels are unaffected by the price of heroin.

The relationship between heroin price and crime therefore has two aspects: (a) the effect of price on quantity purchased (price elasticity of demand), and (b) the effect of heroin expenditure — price times quantity purchased at that price — on criminal activity. Both are considered in the model discussed elsewhere (Silverman & Spruill, in press).

Let the monthly level of reported offenses (C^j of type j be expressed as follows:

$$C^j = f^j (D, R, W, t, L) \qquad (5)$$

where D is the expenditure on heroin in the month, R is a set of dummy variables for the season of the year, W is the average temperature during the month, t is a time trend, and L is a measure of law enforcement acitvity.[3] It is assumed that, while the factors affecting crime are many and complex, only a very few of these factors change significantly from month to month in a given city. (The hypothesis of reverse causality, that an increase in crime results in more income that is spent on heroin, has been tested and rejected.)

Equation 5 cannot be estimated until the heroin expenditure variable (D) is quantified. As noted above with regard to Equation 4, we do not observe the quantity of heroin consumed and hence cannot measure the expenditure on heroin. Additional modeling is necessary to estimate the relationship between price and consumption from these data.

What is important for this analysis is not to infer the level of heroin consumption, but rather the way in which consumption changes as the price changes. In terms of the demand curve discussed above, we are not interested in the intercept of the curve but rather its slope. The model assumes that this slope is a function of the price of heroin, the potency of the drug purchased at retail during that month, and the price relative to its level in recent months. (Other factors that may affect the slope of the demand curve include the availability of heroin treatment and prices of substitute drugs,[4] but data on these factors are not available.) Further, it must be assumed that price changes are dominated by changes in supply; if this is not the case, the model is still valid, but the slope estimated is not that of the demand curve.

Thus, quantity consumed and expenditure are assumed to vary directly with a function of current and past prices of heroin and its potency:

$$D \propto d (P, \overline{P}, S) \qquad (6)$$

where P is the relative (to the recent past) price of heroin and S is retail potency (available from the data on heroin purchases).[5]

This model thus estimates the effect of price changes on consumption simultaneously with estimating the impact of the implied heroin expenditure on crime.[6] In effect, we are choosing the slope of the demand curve (Equation 6) and the parameters of the crime equation (Equation 5) simultaneously to best fit the data.

The model has been estimated using monthly data covering a 2½-year period from November 1970 to July 1973 for Detroit, Michigan. Results were obtained for Detroit as a whole and for 41 "neighborhoods" that were constructed to be relatively homogeneous with respect to income level and racial composition.

The major results of our analysis (discussed in detail in Silverman & Spruill, in press) follow:

1. An increase in the price of heroin resulted in increased robbery, burglary, and larceny in the city as a whole and especially in poor nonwhite neighborhoods (see Table 1). A 10% increase in the price produced an increase of 3.1% in total property crimes (see Table '1 for categories included in this definition) in poor nonwhite neighborhoods. In rich white neighborhoods, armed robbery was the only property crime that appeared to be related to the price of heroin.

TABLE 1

Percentage Increase in Crime Associated with 10% *Increase in the Price of Heroin*

Crime category	Poor nonwhite neighborhoods	Rich white neighborhoods	Total city[a]
Total properly crime	3.1[b]	1.6	2.9[b]
Robbery	5.0[b]	6.2[b]	5.5[b]
Armed	5.0[b]	8.5[b]	6.4[b]
Unarmed	4.5[b]	1.9	4.1 [b]
Burglary	5.9[b]	1.0	3.2
Dwelling	5.4[b]	3.1	3.9[b]
Business, other	8.1[b]	-4.5	2.9
Larceny	2.7[b]	-1.2	-0.2
Auto theft	1.3	1.9	1.1
Total personal crime	2.2	5.5[b]	3.5
Murder	5.5	3.4	4.1
Rape	5.7	9.5	7.6
Other sex crimes	-6.3	3.7	2.1
Aggravated assault	2.0	2.8	2.2
Simple assault	0.8	10.0[b]	5.6[b]

[a]Includes the rich nonwhite and war white neighborhoods that are not analyzed separately. See Silverman and Spruill (in press) for definitions.
[b]Significant at the 5% level.

2. Personal crimes were not related to the price of heroin in the whole city or in poor nonwhite neighborhoods (see Table 1). In rich white neighborhoods, simple assaults were related to the price of heroin, a result as yet not explained to our satisfaction.

3. By fitting the model to the property crime data for each of the 41 neighborhoods, we obtained a picture of the parts of the city victimized by those who supported their heroin purchases by crime. The distribution of these heroin-related crimes did not particularly replicate the pattern of high crime neighborhoods (based on number of property crimes per resident).[7]

4. Preliminary analyses of the distribution of heroin-related property crime and residential burglaries showed that neighborhoods with a high proportion of poor people were disproportionately victimized when the price of heroin increased. Once the income factor was accounted for, racial composition of the neighborhood did not seem to make a difference.

5. Under the assumptions of the model outlined above, the price elasticity of demand for heroin is estimated to be -0.3; a 10% increase in price results in a 3% decrease in the aggregate quantity of heroin consumed. Thus, the demand for heroin is relatively

inelastic, but there is a significant overall adjustment of consumption in response to a price change. Of course, this estimate is based on the behavior of consumers who support their purchases by property crimes; other users of heroin (including "chippers" as well as users who finance their habit by selling the narcotic) may be more or less sensitive to the price of heroin.

The implications of this model for understanding and measuring patterns of heroin use, demand, and "addiction" remain to be explored, but some directionsseem promising. The model offers an opportunity to anticipate the implications of large drug seizures for specific types of crime in specific parts of major U.S. cities. The model can also provide a base against which to compare monthly crime statistics, possibly indicating fundamental shifts in the structure of the heroin market. If replicated across cities, the model would produce price elasticities that might provide the basis for measuiing the effect of alternative treatment, education, and law enforcement policies.[8]

It is important to recognize the limitations of this analysis. As formulated here, the model deals only in aggregate variables; estimates of the number of heroin users, average consumption and price elasticity of different groups of users, and individual crime activities as a result of heroin price changes cannot be obtained from this analysis. It is likely that such individual, as well as aggregate, effects will only be estimable when data on the behavior of specific individuals is merged with the market variables discussed here.

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[1]While comparisons of price levels across cities are possible, the factors that vary across cities make such comparisons risky.

[2]Of interest here are crimes against property, committed to raise revenue to purchase heroin — robbery, burglary, and larceny — rather than the crime of possessing and/or using heroin.

[3]Law enforcement activity (L) is measured by the rate at which offenses are cleared by arrest. The estimated equation for C^j contains only lagged terms L_{t-1}, L_{t-2}, so that simultaneity bias is not an issue.

[4]Data on undercover purchases and seizures of other drugs are available and could theoretically be incorporated into this model.

[5]A specific functional form for d(•) is chosen that has theoretical justification and allows straightforward estimation of Equation 5 combined with Equation 6.

[6]Note that the net effects of heroin price changes on crime levels cannot be measured by data on the criminal activities of specific heroin users. The phenomenon under study is distinctly a market one, and we would be unable to identify aggregate effects from focusing on the behavior of a sample of user-criminals.

[7]This comparison may be confounded by biases in the reporting of offenses to the police. This should not be a problem for the estimates of the model presented here (Silverman & Spruill, in press).

[8]This model has been extended (Stoloff, Levine, & Spruill, 1975) to include entry to treatment programs in Detroit, with the finding that a 1% increase in treatment enrollment is associated with a 0.23% decrease in property crime, and that a 1% increase in heroin price is accompanied by a 0.16% rise in crime.

Treated Prevalence

REFLECTIONS ON THE EPIDEMIOLOGY OF HEROIN AND NARCOTIC ADDICTION FROM THE PERSPECTIVE OF TREATMENT DATA

COMMENTS ON SELLS PAPER

THE CONTRIBUTION OF TREATMENT DATA TO EPIDEMIOLOGIC PERSPECTIVES OF NARCOTIC ADDICTION

Reflections on the Epidemiology of Heroin and Narcotic Addiction from the Perspective of Treatment Data

S. B. SELLS, Ph.D.

Treatment for addiction to heroin and other narcotics represents an area of experience that can be examined for its potential contribution to epidemiological knowledge. In approaching this challenging task the writer has attempted to formulate significant questions and to assemble information concerning them from available sources. This effort has fortunately been aided by access to the files of the Drug Abuse Reporting Program (DARP) and the Drug Abuse Epidemiology Data Center (DAEDAC), both research programs supported by NIDA.

This paper addresses questions in three areas. First it attempts to characterize the population in treatment in terms of demographic and background characteristics and patterns of drug use. It appears clear that within the treatment population there are variations in drug use patterns that are associated with differential patient characteristics along dimensions that have epidemiological implications of considerable interest. These will be examined. The second area involves transition between stages and patterns of drug use. In this discussion, three major *stages* of drug use are recognized: initial use, continued use, and dependence/addiction; *patterns* of drug use exist within the second and third stages and reflect the types and frequency of drugs used. The present discussion of transition refers advisedly to patterns rather than stages, partly because we are dealing here with a population of at least continuing users, and partly for two other reasons. One is that although interest points naturally to progression from soft to hard drugs, there are in fact numerous patterns of drug use, and the concept of a single, hierarchical continuum is an over-simplification. And second, movement between patterns of use occurs in many directions and not necessarily in clearly defined steps upward toward heroin. Admission histories and follow-up studies of treatment samples throw some light on questions in this area. Finally, some general comments are included on the effectiveness of treatment as a transitional (exit) path and on its implications for federal policy.

Analysis of Treatment Populations

One obvious question concerns the possibility of estimating addiction rates in the general population from treatment records. This is a complex problem and we are in general agreement with Glenn and Hartwell (1975), who in a recent review included the CODAP and DARP files among other data sources for estimation. They concluded that there are so many unknowns as well as so many sources of error in available data that this is a most difficult task. Apart from issues of definition, admission policies vary widely, geographic units used for population censuses differ from those served by treatment programs, and coverage of treatment programs is biased, particularly in respect to the sampling of agencies not included in Federal networks. Further, admission records are not infallible indicators of participation in treatment; for example, of 43,943 DARP admissions between June 1, 1969 and March 3 1, 1972, 6,074 (14%) never reported back for treatment and represent a questionable category for estimation purposes. And finally, it should be noted that opioid addiction and to a large extent drug abuse are more characteristic of large centers of population than of the rural and more sparsely populated areas.

At present there are no satisfactory multipliers to generalize treatment samples to specifiable population units. Research is needed to evaluate the possibility of developing them. On the other hand, treatment population files constitute a rich source of data concerning demographic subgroups with different histories of drug involvement and differential implications for intervention efforts. How these subgroups of drug users in treatment relate to the larger population that includes addicted individuals regarded as threats to society is a problem that will be considered.

The data for the present analysis are based on the admission records of 27,460 drug users who participated in treatment at 46 DARP agencies located throughout the United States and in Puerto Rico between June 1, 1969 and March 31, 1974, when the DARP was discontinued in favor of the CODAP system. This sample is identified as the DARP (Final) Research Sample, retained after exclusion of nonusers of illegal drugs, individuals who were admitted but did not participate in treatment, and others excluded for substantive reasons related to the DARP task of evaluation of treatment. A summary of the exclusions is shown in Table 1.

Characteristics of the Total Research Sample

Some significant characteristics of this total sample are shown in Tables 2 to 5. The sex, age, and ethnic distributions in Table 2 indicate that over the 4 years during which DARP admissions were reported, there were changes in the direction of increased percentages of women, youth under 18, and whites, with corresponding decreases among men, patients over age 25, and blacks. In the 18 to 25 range the age increases peaked in the third year. We have studied these trends in relation to CODAP figures for three quarterly periods: January through March, 1974; January through March, 1975; and April through June, 1975. The DARP trends are summarized in the first four columns of Table 3, which present percentages for sex, age, and ethnic group in each of the 4 years, showing age in three groupings — under 18, 19 to 25, and over 25. The next three columns show corresponding CODAP percentages for the three later calendar periods mentioned. With respect to sex, the upward trend in percentage of females continued into the first quarter of 1974 and then reversed. The increase in the youth category (under 18) similarly continued into the first quarter of 1974 and then reversed. In the 18 to 25 range, the decline following the third DARP year

TABLE 1

Summary of Exclusions in Composition of the DARP Research Sample

Category	Year of admission: 1969-1971 (cohort 1)	1971-1972 (cohort 2)	1972-1973 (cohort 3)	Total
Total admissions	11,383	15,831	16,729	43,943
less missing reports	2	16	114	132
DARP master file	11,381	15,815	16,815	43.811
Exclusions by Policy				
Agencies w. questionable data	0	723	665	1,388
Nonusers of illegal drugs	101	153	221	475
Admitted but not in treatment	1,833	2,040	2,201	6,074
Subtotal	1,934	2.916	3,087	7,937
Total DARP treatment population	9,447	12,899	13,528	35,874
Exclusions, by research requirements				
Treatment not classified	587	531	527	1,645
Confined pretreatment (not at risk in baseline period)	784	1,013	1,137	2,934
Legal methadone pretreatment (admitted from a maintenance program)	58	1,020	1,415	2,493
Other (no baseline drug use data)	359	424	559	1,342
Subtotal	1,780	2.988	3,638	8.414
Final research sample	7,659	9,911	9,890	27,460

TABLE 2

Sex, Age, and Ethnic Group Distributions of the DARP Research Sample, Percentages by Year of Admission

	Classificationl Category	Year of admission				Total
		1969-1970	1970-1971	1971-1972	1972-1973	
Sex	Male	80	80	75	71	75
	Female	20	20	25	29	25
Age	Under 18	8	8	9	19	12
	18-20	12	17	20	17	18
	21-22	13	18	17	15	18
	23-25	12	18	19	17	17
	28-30	18	18	18	15	18
	31-40	28	18	14	12	15
	Over 40	11	7	5	5	8
Ethnic Group	Black	55	49	47	41	48
	Puerto Rican	8	15	12	5	10
	Mexican-American	8	8	8	9	8
	White	30	27	32	44	35
	Other	1	1	1	1	1
No. patients		1,940	5,719	9,911	9,890	27,480
% by year		7	21	38	38	100

TABLE 3

Percentages of DARP and CODAP Admissions over Time by Sex, Age, and Ethnic Group Categorles

Classification category		DARP admissions				CODAP admissions		
		1969-1970	1970-1971	1971-1972	1972-1973	Jan-Mar 1974	Jan-Mar 1975	Apr-Jun 1976
Sex	Male	80	80	75	71	89	73	74
	Female	20	20	25	29	31	27	28
Age	<18	8	8	9	19	21	18	12
	18-25	37	51	55	49	39	42	48
	>25	57	43	35	32	40	40	40
Ethnic group	Black	55	49	47	41	40	34	35
	Puerto Rican	6	15	12	5	a	a	a
	Mexican-American	8	8	8	9	a	a	a
	White	30	27	32	44	47	54	52
	Other	1	1	1	1	13	12	12
Total admissions		1,940	5,719	9,911	9,890	7,938	42,298	48,530

[a]Includes in Other.

TABLE 4

Pretreatment Baseline Drug Use Patterns of Final DARP Research Sample Percentages by Year of Admission

Drug use pattern	Year of admission				Total
	1969-1970	1970-1971	1971-1972	1972-1973	
1. Daily heroin only	28	30	28	22	28
2. Daily heroin + marihuana	7	9	10	9	9
3. Daily heroin + rnarihuana and cocaine	12	10	8	5	8
4. Daily heroin + cocaine	10	9	10	8	9
5. Dally heroin + barbiturates	7	8	7	7	7
8. Daily heroin + other nonopioida	4	4	3	2	3
7. Dally heroin + poly	12	12	12	9	11
8. Poly	8	5	8	15	9
9. Weekly opioids + nonopioida	7	5	4	3	4
10. Less then weekly opioide + nonopioids	5	8	10	18	12
11. Dally opioids + nonopioids	2	2	2	2	2
No. patients	1,940	5.719	9,911	9,890	27,460

Note. Baseline period is the 2-month period preceding admission. Thr Poly pattern involves some we of three or more nonopioids with no use or less than daily use of opioids.

TABLE 5

Four Categories of Pretreatment Baseline Drug Use Percentage Distributions of DARP Final Research Sample by Year of Admission

Categoories of pretreatment drug use	Year of admission				**Total**
	1969-1970	1970-1971	1971-1972	1972-1973	
1. Dally opioids only	38	40	39	32	38
2. Dally opioids + nonopioids	48	44	41	32	39
3. Less than daily opioods + nonopioids	12	10	8	11	10
4. Ncncploida only	8	8	12	25	15
No. patients	1,940	5.719	9,911	9,890	27,480

Note. The Poly pattern involves some use of three or more nonopioids with no use or less than dally use of opioids.

also continued into the first quarter of 1974 and then reversed. The decreasing trend in the over-25 age range reversed in the first quarter of 1974 and remained stable at 40%. The trends for blacks and whites continued into the first quarter of 1975. Then apparently they leveled off; whether or not the small differences in the second quarter of 1975 signal a reversal is not yet apparent.

Knowing as we do that the DARP is not a representative sample of the total United States drug user treatment population (or even of the Federally supported treatment population) and that the completeness of CODAP reporting is questionable and variable across periods, we must be cautious about the accuracy of trends inferred from these data. They are based on large samples and they represent the only large-scale data available, but the sources of error are obvious. Our own interpretation is that the 20% increase of whites and decrease of blacks since 1969 are valid. The age trends are not clear. One possible explanation is that there was an increased admission rate for daily opioid users after March, 1975, but the data available reject that. Actually, daily opioid use among CODAP admissions is significantly lower than in 1972-1973, the last year of DARP admissions, and continues a trend observed in DARP away from the predominance of opioid use and toward increased multiple drug use. Since multiple drug use is associated with youth but is also more common among whites than blacks, the fluctuations of the age group percentages after 1973 are believed to reflect reliability problems. It is important that the CODAP age group percentages be watched carefully for the quarters following June 1975. We would not be surprised if the upward trend observed for the under-l 8 group up to March 1974 were to reappear.

Table 4 summarizes the most frequent pretreatment patterns of drug use by year for the DARP treatment sample. These patterns, developed by Simpson (1974), reflect associations of reported drug use during the 2-month period preceding admission. The first seven patterns involve daily use of heroin: heroin only, heroin with daily or less frequent use of marihuana, heroin with marihuana and cocaine, heroin with cocaine, heroin with barbiturates, heroin with one or two nonopioids, and heroin with three or more nonopioids. The Poly pattern involves some use of three or more nonopioids with no use or less than daily use of opioids. Pattern 9 involves weekly use of opioids other than heroin, generally associated with one or two nonopioids, and Pattern 10 is less than weekly opioid use with some use of one or two nonopioids. The final pattern involves daily use of opioids other than heroin, generally with some use of nonopioids. In this treatment sample there are no patients who used marihuana only. Most of the daily heroin groups showed some percentage decrease over the 4 years, while the Poly and Less than Weekly Opioid plus Nonopioid groups increased noticeably. These changes in the treatment population reflect concern by treatment authorities for the youthful multiple drug users and a deemphasis of methadone maintenance in favor of drug-free treatments.

To facilitate analysis, the data defining the 11 drug use patterns in Table 4 were regrouped to four composite types of drug use, as seen in Table 5. The first category consists mainly of addicts who use heroin daily or heroin daily with marihuana. The second involves combined use of daily heroin or other opioids with various nonopioids. Category 3 includes the weekly and less than weekly opioid plus nonopioid users, and the fourth is restricted to users of nonopioids only during the baseline period. The size of this final group in Year 4 reflects the influence of one large South Atlantic drug-free program that catered mainly to white teenagers and

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somewhat inflated what nevertheless appears to be a valid trend toward increased frequency of nonopioid, polydrug users in the treatment population. The categories in Table 5 are roughly equivalent to but not identical to combinations of patterns in Table 4.

Correlates of the Four Types of Pretreatment Drug Use

Although some arbitrary decisions were necessary in the classification of the low frequency patterns, it is believed that these four categories of pretreatment drug use (Table 5) reflect differences in degrees and types of deviance and different life styles. Hence they are believed to have differential implications in planning interventions to cope with extensive drug abuse and addiction. Let us now examine some respects in which these four types of drug use groups vary and attempt to form some impressions based on the data presented. For simplicity of reference, we will call the first group *Daily Opioids,* the second, *Daily Opioids Plus,* the third, *Weekly Opioids Plus,* and the fourth, *Nonopioids*.

Table 6 gives a summary of some facets of sex, age, and ethnic differences. The greatest difference is between the first category, daily opioid only users, and the fourth, which consists entirely of nonopioid users. The Daily Opioids group is predominantly male, older, and black, while the Nonopioid group includes substantially more females and is predominantly white and young.. The two remaining groups show reasonable blends of these extreme configurations. Group 2 (Daily Opioids Plus) is somewhat younger and has a higher percentage of whites than Group 1, while Group 3 (Weekly Opioids Plus) is older and includes more males and more blacks than Group 4. The association of blacks with daily opioid use is more consistent than that of whites with nonopioid use.

We will look next at some socioeconomic background data for the four drug user groups. As shown in Table 7, there is a continuum from lowest status, among the Daily Opioids group to highest status, among the Nonopioids group. This is shown consistently for educational level of patient's father and mother and occupational level of father. In this table the separation of the four groups is quite clear.

Table 8 summarizes some observations related to criminality. In selecting these data it was necessary to be mindful of the age differences among the drug user groups and to avoid measures that are confounded with age. Unfortunately the DARP Admission Record did not include baseline data on arrests and convictions during the 2 months preceding admission; the pretreatment arrest and conviction data obtained cover the entire life span and therefore show higher criminality among the older patients. However, the six items in Table 8 are relatively independent of this bias, and the picture that they describe is interesting. The greatest amount of criminality is seen in the second group (Daily Opioids Plus), although the Daily Opioids group is a close second. The predominantly young and white Nonopioids group stands in sharp contrast to the other three groups, but it is suspected that their true level of overt criminality is higher than the figures presented imply. This undoubtedly reflects the well known phenomenon of deference to the white middle class by the law enforcement system. Nevertheless the overall implication is that involvement in illegal activities and involvement with the law enforcement system increase with involvement with opioid use. It is of interest, although not included in the table,

TABLE 6

Sex, Age, and Ethnic Characteristics of Patients
Percentages by Four Types of Pretreatment Drug Use

Classification variable	Category	Type of pretreatment drug use: Daily opioids Only	Daily opioids + nonopioids	<Daily opioids + nonopioids	Nonopioids Only
Sex	Female	23	23	26	35
Age	Under 18	3	4	21	45
	Under 21	18	22	47	66
	21-25	35	39	30	19
	Over 25	47	40	23	15
Ethnic group (% drug use type)	Black	53	55	33	13
	White	20	27	54	80
Ethnic group (% in drug use type)	Black	42	47	7	4
	White	14	33	17	37
No. patients		9.984	10,646	2.714	4,116
% of sample		36	39	10	15

TABLE 7

Socioeconomic Background of Patients

Percentages by Four Types of Pretreatment Drug Use

Classification category	Type of pretreatment drug use			
	Daily opioids only	Daily opioids + nonopioids	<Daily opioids + nonopioids	Nonopioids only
Education—father				
% < grade 7	22	18	11	7
% ≥ grade 10	53	59	68	76
Education—mother				
% < grade 7	17	12	8	4
% ≥ grade 10	61	68	70	83
Occupation—father				
% executive-management	15	20	30	34
% unskilled & semi-skilled	44	37	30	26
None	14	13	10	10
No. patients	9,984	10,646	2,714	4,116

TABLE 8

Criminality among Four Groups of Pretreatment Drug Users
Percentages by Drug Use Type

Classification category	Type cl pretreatment drug use			
	Daily opioids only	Daily opioids + Nonopioids	<Daily opioids + nonopioids	Nonopioids only
% with illegal source of support (major or minor) 2 mos. preadmission	40%	56%	31%	11%
% never arrested	19%	17%	20%	44%
Median age at first arrest (of those wlth 1 or more arrests)	16.4 yrs.	17.9 yrs.	17.2 yrs.	16.8 yrs.
% aver a juvenile delinquent	25%	29%	30%	20%
% with illegal support and legal involvement at admission	20%	31%	13%	5%
% with no illegal support and no legal involvement at admission	35%	28%	35%	56%
No. patients	9,984	10,646	2,714	4,116

that the oldest group (Daily Opioids) had approximately the same numbers of pre-treatment arrests and convictions as the Daily Opioids Plus group, but had spent a higher median number of months in jail.

Tables 9 through 12 show data on previous drug involvement for the four groups. Table 9 reports on the first illegal drug used. Heroin was the first illegal drug used by 31% of the Daily Opioids group, 21% of the Daily Opioids Plus group, 14% of the Weekly Opioids group, and by only 2% of the Nonopioids group. There was no trend for Other Opioids, but the rank order of frequency was reversed for every other drug category, including marihuana, the most popular initial illegal drug. A similar, but much more informative picture for the first illegal drug used daily is seen in Table 10. Here, the differentiation among groups on the basis of heroin, marihuana, and other nonopioids is emphatic. Although this may well change as the Nonopioid group ages, it is of interest that 20% of that group did not use any illegal drug daily during the 2 months preceding admission to treatment.

Table 11 shows percentages for the four groups for the first opioid drug used and the first opioid drug used daily. For the two groups of daily opioid users, heroin is clearly the drug of choice. However, significant percentages of the three groups using nonopioids (including Group 4, described as users of nonopioids only during the pre-treatment baseline period) had previously used opioids other than heroin, such as morphine, illegal methadone, dilaudid, and other preparations, as their first opioid drug. The corresponding percentages for first daily opioid are lower, but still of interest. The distinction between heroin and other opioids as a drug of choice is believed to have implications in relation to life style, particularly in respect to the perilous street life of the heroin addict in contrast to what is believed to be a much less strenuous and hazardous status for users of other opioids. Further, the fact that patients described as users of nonopioids only report some previous use (including daily use) of opioid drugs emphasizes the changing nature of patterns of drug use.

Age at onset of first drug use, first opioid use, and first daily opioid use is summarized in Table 12. For each of these three drug use categories, initiation of use occurred earliest among the Nonopioid group and increased progressively to the Daily Opioids group, which was latest in each instance. Since a substantial percentage (61%) of the Nonopioid group never used any opioids and an even larger percentage (80%) never used opioids daily, the medians were computed for those with a history of opioids use only. The bottom section of Table 12 also compares the four groups in terms of median transition time from age of first opioid and then from first opioid to first daily opioid. Although the Nonopioid users started drug use at a younger age than the Daily Opioid users, those among them who experimented with opioids and those who later used opioids daily were a minority and delayed longer in transition than the Daily Opioid users.

Other differences among the four drug use types were found in the analysis of the DARP sample. Several of them may be mentioned briefly. With respect to pre-treatment (baseline) alcohol consumption, the two groups representing mainly users of nonopioids consumed more beer and liquor than the two groups of daily opioid users. There were no differences for wine. They also reported significantly more serious life problems related to alcohol excess. Within the two pairs of groups, those who combined nonopioid use with opioids, either daily or less than daily, consumed more alcohol than either the Daily Opioids Only or the Nonopioids Only group. This supports the view frequently mentioned that alcohol is part of the polydrug pattern.

TABLE 9

Type of Illegal Drug Used First among Four Groups of Pretreatment Drug Users
Percentages by Drug Use Type

Type of drug	Type of pretreatment drug use			
	Daily opioids only	Daily opioids + nonopioids	<Daily opioids + nonopioids	Nonopioids only
Heroin	31	21	14	2
Other opioids	4	4	4	2
Barbiturates	5	7	7	9
Amphetamines	3	5	7	8
Hallucinogens	1	1	5	9
Marihuana	55	59	55	54
Other drugs	1	2	4	6
No. patients	9,984	10,646	2,714	4.116

TABLE 10

Type of Illegal Drug First Used Daily Among Four Groups of Pretreatment Drug Users
Percentages by Drug Use Type

Type of drug	Type of pretreatment drug use			
	Daily opioids only	Daily opioids + nonopioids	<Daily opioids + nonopioids	Nonopioids only
None	0	0	4	20
Heroin	63	51	31	6
Other opioids	3	4	4	2
Barbiturates	3	4	6	8
Amphetamines	3	5	9	10
Hallucinogens	0	1	3	4
Marihuana	27	33	40	47
Other drugs	1	1	2	3
No. patients	9,984	10,646	2.714	4,116

TABLE 11

Type of Opioid First Used and Type of Opioid First Used Daily
Among Four Groups of Pretreatment Drug Users
Percentages by Drug Use Type

Type of drug		Type of pretreatment drug use: Daily opioids only	Daily opioids + nonopioids	<Daily opioids + nonopioids	Nonopioids only
First opioid:	None	0	0	0	87
	Heroin	91	88	73	17
	Other	9	14	27	18
First daily opioid:	None	0	0	28	84
	Heroin	95	93	85	12
	Other	5	7	9	4
No. patients		9,984	10.848	2,714	4,116

TABLE 12

Age at First Illegal Drug Use, First Opioid Use, and First Daily Opioid Use
By Type of Pretreatment Drug Use

Classitication category	Type of pretreatment drug use: Daily opioids only	Daily opioids + nonopioids	<Daily opioids + nonopioids	Nonopioids onty
First illegal drug				
Mdn. age at onset	17.3	16.6	16.1	15.6
% <18 at onset	59%	66%	74%	77%
First opioid drug				
Mdn. age at onset	16.6	18.4	17.9	17.7
% <16 at onset	37%	43%	52%	16%
% never used	0	0	0	67%
First daily opioid drug				
Mdn. age at onset	19.6	19.0	16.9	16.0
% <16 at onset	26%	33%	26%	6%
% never used	0	0	26%	64%
Transition time (median differences)				
1 st drug to 1 st opioid	1.5 yrs.	1.6 yrs.	1.6 yrs.	2.1 yrs.
1 st opioid to 1 st daily opioid	.8 yrs.	.6 yrs.	1 .0 yrs.	1.1 yrs.

Both of the mainly nonopioid groups were significantly higher than the two daily opioid groups in respect to history of psychiatric treatment. The figures were 11% each for the Weekly Opioids group and the Nonopioids group, compared to 5% for Daily Opioids and 7% for Daily Opioids' Plus. Other differences observed, in respect to patients' education, pretreatment employment and earnings, previous treatments for drug abuse, hospitalization for physical ailments, and sources of support, appear to be confounded with age differences and will not be mentioned. On the other hand, despite the age differences among the four groups, there were no differences in percentages with military service (about 25% in each group) or in length of service for those who were inducted. Seventy-one percent of all those who served in the Armed Forces received honorable discharges. Of the remainder (not honorably discharged) an average of 11% had dishonorable and 18% had general discharges. There were no noteworthy differences in frequency of dishonorable discharges, but the percentages of general discharges varied from a low of 15% for the Daily Opioids group to a high of 26% for the Weekly Opioids Plus group. The Nonopioids group had 22% with general discharges and the percentage for Daily Opioids Plus group was 19%.

A capsule summary of the various statistics we have reviewed is shown in Table 13. These data may appear disarmingly simple at first glance, since there are suggestions on a number of variables of a fairly homogeneous continuum from a pattern involving essentially noncriminal nonopioid use at one extreme, to another of highly deviant, hard-core narcotic addiction at the other. However, this would be a simplistic interpretation and would fail to recognize the discontinuities between the mainly black, low socioeconomic pattern associated with street heroin use and the mainly white, middle to low-middle socioeconomic level pattern associated with opioids other than heroin and with nonopioids. The fact that the percentages of blacks and whites do progress across groups is important, but indicates (a) that race and social class, both highly implicated in the present problem, are not perfectly correlated, and (b) that, as might be expected, the solution is multivariate rather than univariate.

The data presented, limited to variables obtained for a different purpose in the DARP treatment evaluation research, are generally ascriptive rather than descriptive of individual characteristics, but nevertheless permit some guarded conclusions. Considering these data, along with pertinent discussions in the literature (particularly Jessor, R., Graves, Hanson, and Jessor, S., 1968; Gorsuch and Butler, 1974; and Levin, Roberts, and Hirsch, 1975), it appears most reasonable to conceptualize the patterns of drug use and addiction that exist in the community and on the street as individual behavior patterns associated with life situations and life styles of varying degrees of stability. These life styles are associated with various patterns of drug-taking behavior and depend in turn on individual resources, socialization, personality, perceived opportunity, and perceived risk in relation to life decisions, including those concerning the use of drugs. In this context, drug-taking behavior can be understood better in the perspective of the full spectrum of psychoactive chemical substances than by isolated focus on a particular group of drugs, such as narcotics or marihuana, which have received extensive special attention.

In a society that is highly stratified and geographically‘segregated, despite vigorous efforts to overcome barriers to social and economic opportunity, there are variations in access to sources of socially legitimate as well as illegitimate satisfaction. At the same time, individual strivings for satisfaction and self-enhancement are assumed to

TABLE 13

Summary of Discriminating Characteristics of Four Groups with Different Types of Pretreatment Drug Use in DARP Samples

Classification categories	Type of pretreatment drug use			
	Daily opioids only	Daily opioids + nonopioids	<Daily opioids + nonopioids	Nonopioids only
Females	23	23	28	35
Youth <18	3	4	21	45
<21	18	22	47	88
Adults 21-25	35	38	30	19
>25	47	40	23	15
Blacks	53	55	33	13
Whites	20	27	54	80
Socioeconomic level	LOW	LOW	Mid.	Mid.
Criminality	High	High	Mod.	Low
Heroin As 1 st drug/as 1 st daily drug	31/63	21151	14/31	2/6
Marihuana As 1 st drug/as 1 st daily drug	55/27	59/33	58/40	64/47
Other nonopioids As 1 st drug/as 1 st daily drug	10/7	15/11	23/20	32/25
Heroin As 1 st opioid/as 1 st daily opioid	91/95	86/93	73/65	17/12
Other opioids As 1 st opioid/as 1 st daily opioid	9/5	14/7	27/9	16/4
Pretreatment alcohol >4 oz./daily	17	24	31	28
Serious alcohol problems	3	7	13	11
Psychiatric history	5	7	11	11
Military service (% hon. disch.)	74	71	84	88
No. oatients	9,984	10,646	2,714	4.118

[a]All numbers are percentages of Drug Use groups, unless otherwise noted.

be generally strong. As Jessor and his colleagues (1968) have theorized, this leads to differential vulnerability to frustration and differential amenability to illegitimate and risky alternatives.

Considering the social levels implied in the DARP population, a polarity can be observed between *low socioeconomic level street heroin users,* at one extreme, and the *youthful, middle class, maladjusted, nonopioid* users, at the other. The heroin addicts consist mainly of adults who on the average first used drugs at age 17, used heroin at 19, and began daily heroin use before the age of 20. The nonopioid users typically started drug use earlier, before age 16; about 35% of this patient group experimented with opioids before age 18, and about 15% went on to daily opioid use, but not typically street heroin use, before age 19. However, none of these patients used opioids daily during the 2 months preceding admission. As a group, they appear to represent a more recent phenomenon deserving of careful monitoring for some time to come. Unfortunately, the opportunity to track them in the DARP was denied and other comparable data have not been available.

Transition Between Stages and Patterns of Drug Use

The data presented support a number of facts that appear to be widely accepted (Brecher et al., 1972). These are: (a) that there are diverse patterns of drug use, (b) that these vary widely in the population, (c) that individual patterns of use do change, and periods of no use are not uncommon, even among long-term addicts, and (d) that movement occurs from hard to softer drugs as well as in the other direction. During the past 7 years individuals have entered and reentered treatment at early and later stages of drug use and from every sector of the spectrum of drug use patterns. For many individuals, treatment programs represent a mechanism of control that reduces the prevalence of illegal use while they are under the surveillance of their programs. For some, treatment represents an effective exit path. However, the effectiveness of treatment is a separate question that is discussed in the following section.

Our analysis has demonstrated that the distribution of drug use patterns is not random, but lawful and multiply determined, as reflected by the associations with sex, age, ethnic group, socioeconomic status and social class, and other factors mentioned. The list of relevant factors disclosed by the DARP data is limited, however, by the variables included for the DARP objectives of treatment evaluation research and a considerably more extensive network of family, developmental, interpersonal, social, and environmental factors is suggested by the body of related literature that has become available. A detailed analysis of this literature is not feasible in this presentation; however, selected bibliographies on transition and on treatment evaluation can be obtained from the author on request, and annotated bibliographies will be available in the near future from the DAEDAC.

In this discussion we should take note of two recent contributions that developed systematic models of drug abuse and addiction, based on extensive critical review of available research. Gorsuch and Butler (1974) presented a multistage, multiprocess psychological model of the development of nonmedical drug use as a task in the DARP research program, and Levin, Roberts, and Hirsch (1975) developed a sociological model of heroin and the community, in the framework of system dynamics.

Much of the research cited in the above mentioned bibliography consists of studies focused on one or another factor that in given circumstances is related to a particular criterion. Some factors, such as the availability of a drug supply, are extremely important, but piecemeal consideration of even such important factors can generate confusion about the phenomena and the processes involved. Optimal understanding and effective application of knowledge require not only that all of the potentially critical variables be identified, but also that they be united in a systematic model that reflects their system properties and idiosyncracies, if any, their main effects, and their interactions with other factors.

The Gorsuch-Butler Model

As presented in Figure 1, the Gorsuch-Butler model is best described as a stage-oriented, multiprocess developmental schema. It assumes that multiple processes exist both within and between the different stages. Three stages of development of drug-using behaviors are described: initial use, continuing use, and dependency/addiction. For some individuals, the processes at one stage may continue at the next stage. However, this is not necessary, and the model recognizes the possibility of different processes at each stage, reflecting variations among individuals in the perceived importance of different variables.

The chart in Figure 1 is schematic, and only the major, documented variables and pathways are indicated. It is presented here to emphasize the complexity of the problem and to indicate the limitations of such hypotheses as the so-called "stepping stone" path from marihuana to heroin that is now generally discredited, and also the more appealing, but also limited "aging out" hypothesis. Such efforts at explanation suffer from tunnel vision and ignore too many relevant variables. For example, the aging out hypothesis may well represent one exit path, but hardly meets the requirements for a general explanatory principle.

As a psychological, developmental theory, the Gorsuch-Butler model may well be too detailed and individualized to furnish a basis for the planning of intervention programs at a governmental level. It has important potential for further research and theory development and has important implications for clinicians concerned with treatment. It is included as a contribution to the understanding of the initiation and continuation of drug use and the dependence on drugs by individual users and indicates significant sources of variance that are often not accounted for at the sociological level.

At the initial use stage, three sets of processes that have been well defined in the literature provide paths to the initial drug use. These were labeled iatrogenic, nonsocialized, and socialized to prodrug norms. The same variables are assumed to be involved in the three processes, but to operate differently. For example, a strong, cohesive family is expected to produce different effects in the nonsocialized process than in that involving socialization to a prodrug norm.

A factor of major importance in transition from the initial drug experience to continued use is that of maintaining a constant drug supply. The supply not only satisfies immediate requirements for continual use, but also determines the individual's involvement with the drug subculture. Even the iatrogenic user might struggle to cultivate and maintain his source of supply (possibly a physician) within his environment, and this has a profound effect on his association with a particular group, on

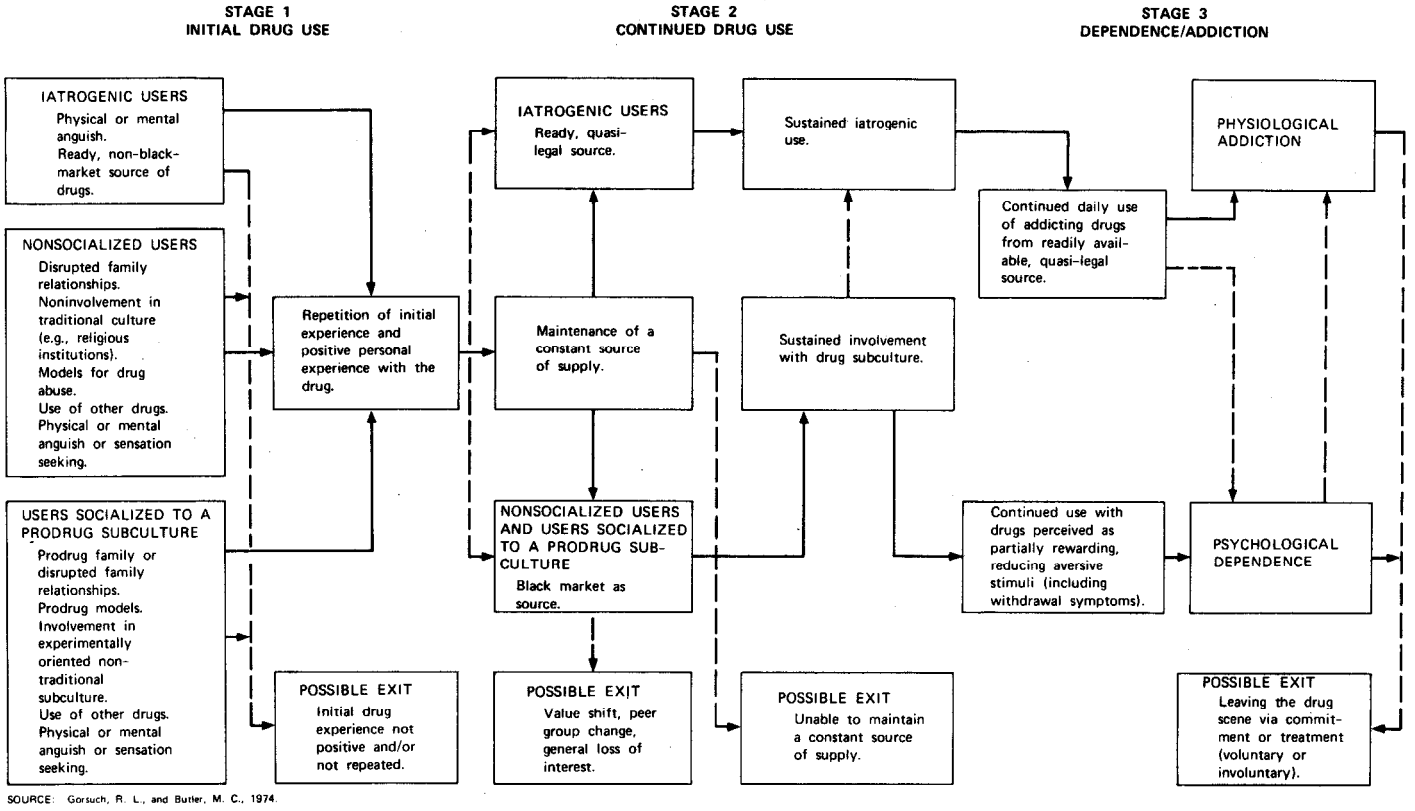

Figure 1. A multistage, multiprocess model of the development of nonmedical drug use.

the group norms that he experiences, and on the subculture in which he must learn to function effectively.. At the final stage, dependence/addiction, the drug behavior becomes relatively independent of the factors that mediate continuing use, except for those behaviors involved in maintenance of the source of supply. This independence is assumed to be mediated by mechanisms of learning and adaptation, or physiological tolerance. Which of these, if either, will occur depends on the drug involved and on the individual's perception of his needs at any time. The distinction between psychological dependence and addiction (physiological) emphasizes that the type of dependency experienced is not solely a function of the drug, but is due rather to the individual's constellation of social influences, previous experiences, expectations, and personal needs.

The model recognizes exit paths at every stage. Discontinuation of drug use could recur whenever the individual-environment interaction lacks sufficient intensity to warrant further involvement, as for example, when the supply diminishes, peer groups change, or personal values shift. It may also be involuntary, as a result of hospitalization or incarceration.

The Levin, Roberts, and Hirsch Model

The data presented earlier, based on the DARP, tend to support the hypothesis that among the population in treatment described as mainly nonopioid users there are many potential heroin addicts, but that the majority will either eventually discontinue drug use or stabilize at some "safe" level of polydrug use, including that of alcohol. This hypothesis cannot be tested by the DARP data at this time, although some useful information is currently becoming available through follow-up studies of substantial samples that are well along toward completion. However, strong support appears to be given by the recently published work of Levin, Roberts, and Hirsch (1975). These authors have formulated a "comprehensive system dynamics model of heroin and the community" in which factors postulated as *attractive* in moving drug users into addiction, and factors that *deter* them from addiction were incorporated in systems equations for simulation research. In this model, poverty, ethnic status, the status of the drug-associated culture, including the number of addicts in the community and availability of drugs, as well as counter-establishment-related emotional problems were cited as attractive factors. Fear of arrest, reflecting the intensity and effectiveness of the law enforcement system, informative drug education, uncertainty concerning the effectiveness of treatment for addiction (which would increase the risk of becoming addicted), and the availability of soft drugs were included as deterrents.

In this framework and proceeding from the data that we have presented, the following statements appear justified concerning the street heroin addicts. This group comes mainly from the low-socioeconomic poverty strata, living mostly in segregated, high density, high crime, socially deprived, inner city areas; they are predominantly members of minority ethnic groups (mainly black, but in some areas Puerto Rican or. Mexican-American), educationally, economically, and socially deprived, and exposed to high prevalence of illegal behaviors, including drug taking and associated activities, that are tolerated at least with resignation in their communities, in contrast to the strong rejection expressed in the greater society. Heroin and other drugs are readily available, often used by role models (e.g., the "stand-up cat," described by Feldman, 1968), and guidance concerning procurement, financing, use, and avoidance of apprehension by police and other authorities is plentiful.

Among youth raised in this type of environmental setting are a subgroup who appear to have the highest probability of embracing the street life of "ripping and running" and of becoming daily heroin users. While a majority of this group have used marihuana as their first illicit drug, a substantial number have started on heroin, and heroin-rather than marihuana is the first daily drug of choice. Although classified as criminal, this life style may well be a meaningful, adaptive pattern for people faced with the poverty, deprivation, rejection, and lack of viable opportunity that appear to be characteristic of the ghetto street addict. Subjected to arrest, imprisonment, unemployment, welfare, and countless dangers and other indignities, it is surprising that they have served in the military proportionately as frequently as the mainly white, middle class nonopioid users, that they have had a higher percentage of honorable discharges than the nonopioid users, and that they include very few individuals with histories of psychiatric hospitalization.

The Nonopioid using patients, while predominantly white and middle class, show considerably more variability than the street addicts with respect to the factors mentioned earlier as attractive and deterrent to addiction, and those in the Weekly Opioid group have higher percentages of older, nonwhite, criminal, alcoholic, and emotionally disturbed, as well as indicators of somewhat lower socioeconomic status than those in the Nonopioids Only group. Considering the converging implications of family situation, economic, educational, ethnic, emotional, and contextual community factors relating to the prevalence and perceived risk associated with addiction, the probabilities that identifiable subgroups of the nonopioid using population will shift to opioids and possibly to street heroin are viable. Our present data do not permit estimation of these probabilities, but perhaps further intensive analysis of the DARP files beyond what was feasible for this presentation and use of the Levin et al. equations might produce useful results.

The data presented and the related discussion should be regarded as suggestive rather than conclusive. They reflect previously completed analyses of DARP data since resources were not available to undertake the more detailed analyses suggested in the discussion. With these caveats, it nevertheless appears that there are many roads to "H," although what has been historically the main highway for low socioeconomic black and other minority ethnic groups is still prominently active. Other roads for middle-class whites and other ethnic group members have been opened. In terms of the tables that we have presented, there is probably a discontinuity between the black (and other minority) ghetto youth and the great majority of the mainly white middle-class youth with respect to their potential for becoming habitual drug users and heroin addicts. At the same time there appears to be a subgroup or perhaps many subgroups of youth outside of the ghetto who are vulnerable to progression from soft drug use to addict status.

Comments on the Data Source

The portion of the daily heroin user population represented in the DARP sample is older than the youth represented predominantly in the nonopioid user groups. This is attributed to several factors. First, the programs that reported to DARP in 1969-1970 and 1970-1971 were limited by legislation to opioid addicts, and it was not until the third year that substantial numbers of nonaddicts were admitted. Table 14 shows the percentages of patients in each year by treatment modality to which they were assigned. In the first 2 years only 5% and 8%, respectively, were in outpatient drug-free treatments, which cater mainly to nonaddicts; these percentages rose to

18% in the third year and to 35% in the fourth. As shown in Table 15, the users of Daily Opioids and Daily Opioids Plus were primarily in methadone maintenance and detoxification treatments, while the two Nonopioid groups were mainly in drug-free treatments. The addicts admitted to treatment in the first 2 years of the DARP included many older individuals for whom community-based, new treatments were not previously available and therefore did not reflect a generally "new" patient group, as did the nonopioid users, particularly in the fourth year. Indeed, the average age of Daily Opioid users at admission was over 33 in Year 1 and declined throughout the 4 years of the DARP. Also, for the entire 4-year sample, slightly less than 50% of the two groups of Daily Opioid users had at least one pre-DARP treatment, while 55% of the Weekly Opioids and 74% of the Nonopioids had no previous treatment prior to DARP admission.

The addicts in treatment in the DARP population (and probably in the CODAP population, although we have not had an opportunity to check this) are believed to be more representative of the populations of the large cities and SMSAs (Census Standard Metropolitan Statistical Areas) in which DARP agencies were located than they are of the United States as a whole. This appears to be true of the sex, age, and ethnic distributions. For example, the black population of cities in DARP Cohort 1 in 1970 was 31%, compared to 11% for the country as a whole, and although Puerto Ricans and Mexican-Americans represent .7% and 4%, respectively, of the total United States population, they constitute substantial components of the populations of San Juan, New York, Albuquerque, San Antonio, and Los Angeles, where DARP agencies were located. Similarly, the DARP age structure is much more similar to that of the respective DARP cities than of the total country. In view of the fact that it is mainly in these areas of high prevalence of addiction that the major problems of illegal distribution, crime enforcement and location of treatment facilities exist, it appears desirable to consider the possibility of developing a standard set of quantitative indices of community context factors that have positive and negative implications for the prevalence of drug use and addiction. Many of these, such as ethnic percentages, socioeconomic indices, employment, housing, crime, and related data, are available, in some cases at the Census Tract level and in most, for SMSAs, from standard statistical sources. Some may be more difficult to obtain, but in this framework, treatment program data concerning new admissions by time period and treatment rosters by demographic classification may be useful. It is expected that such indices would be correlated with prevalence estimates, but in the absence of readily available prevalence data, research along this line might lead to a new method of estimating prevalence.

Addict Deaths

Another issue in the transition area is that of the numbers of addicts who leave the active population each year by death. The DARP research file provided an unprecedented opportunity to study death rates and causes of death among a population of addicts for which a precise population base at risk could be computed. This was the actual time in treatment covered by the DARP reports from admission to termination. Death rates were then computed for actual man-years at risk rather than on globally defined populations, as necessitated in most other studies. Three separate studies, for a sample of 9,276 in treatment in 1970-1971 (Sells, Chatham, & Retka, 1972), a sample of 17,684 in treatment in 1971-1972 (Watterson, Sells, & Simpson, 1974a),

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TABLE 14

Percentages of DARP Patients in Treatment Modalities By Year of Admission

Treatment modality	Year of admission				Total
	1999-1970	1970-1971	1971-1972	1972-1973	
Methadone maintenance	59	58	41	28	40
Therapeutic community	23	18	15	15	18
Outpatient drug-free	5	8	18	35	21
Detoxification	13	18	28	24	23
No. patients	1,940	5,719	9,911	9,890	27,460

TABLE 15

Percentages of DARP Patients in Treatment Modalities by Categories of Pretreatment Drug Use

Treatment modality	Type of pretreatment drug Use				Total
	Daily opioids only	Daily opioids + nonopioids	<Daily opioids + nonopioids	Nonopioids only	
Methadone maintenance	50	51	19	3	40
Therapeutic community	10	17	34	19	18
Outpatient drug-free	11	9	38	88	21
Detoxification	29	23	11	10	23
No. patients	9,984	10,646	2,714	4,118	27,480

and a sample of 23,529 in treatment in 1972-1973 (Watterson, Sells, & Simpson, 1974b) have been completed and were summarized in a recent paper by Watterson, Simpson, and Sells (1975).

The overall death rates were 15 per 1,000 per year in the first year, 12 in the second, and 13 in the third. For the combined 3-year sample, the rate was 13 per 1,000 per year, a level not reached in the mortality statistics for the general United States population until age 57. By contrast, more than half of the DARP patients were 25 years old or younger and only about one-fourth were over 30. In comparison of rates for each of the 3 years, several trends were noted. Over the 3 years studied, the death rates in the 21 to 25 age range increased from 9 to 13, while in the 26 to 30 range they decreased from 14 to 8 per 1,000 per year. The rate was consistently highest (between 20 and 23) for the oldest (over 30) group in all 3 years. For age groups, the 3-year averages were: under 21, 9; 21 to 25, 11; 26 to 30, 10; and over 30, 21. For ethnic groups, the 3-year averages were: Mexican-Americans, 15; blacks, 14; whites, 12; and Puerto Ricans, 11. The death rates were highest for patients in outpatient treatments (18 for drug-free and 15 for methadone maintenance) who were variously at risk in the community, and extremely low (2) for residential, therapeutic community patients, who were minimally at risk.

Since the rates for each of these variables were confounded by their particular composition in respect to the other variables, the specific numbers mentioned are gross. More detailed rates are provided in the technical reports. Two important conclusions are indicated, however. First, when the composition of the population base and the circumstances of the population (e.g., treatment type) vary from year to year, as in the case of even large treatment samples, the death rates may be expected to vary as well. Second, the overall average of 1.3% (13 per 1,000 per year) is probably an underestimate in comparison with that expected for similar addicts not under any treatment or other surveillance in the same communities. On the other hand, since the majority of addicts in treatment appear to have arrived there through some form of direct or indirect coercion, it might also be argued that those who were successful in not getting caught might also be better able to survive. Finally, it may be noted that the analyses of causes of death revealed that very few of the total of 275 deaths studied over the 3 years were for natural causes.

Evaluation of Treatment

As suggested in the recent *White Paper on Drug Abuse* by the President's Domestic Council Drug Abuse Task Force (1975), treatment reflects a major part of the Federal strategy of demand reduction for illicit drugs. This publication indicated that by December, 1974 there were facilities in this country to provide treatment to over 450,000 drug abusers per year. In this final section we will examine the effectiveness of treatment in coping with opioid addiction. This discussion requires some introductory remarks and definitions.

The term treatment has been criticized on the grounds that it reflects the so-called medical model and that this is inappropriate to drug abuse and addiction in that even if individual behaviors were changed, the effects might be expected to be short-lived unless the environmental factors responsible for addiction were also changed. In our opinion, the label medical model is only superficially appropriate to describe a large portion of the treatment system. Indeed, there are clients, who are often called patients, and therapists, many of whom are medical or paramedical; and in

detoxification, as well as maintenance programs, medication is prescribed and administered. However, the atmosphere at treatment programs varies from patient-oriented and therapeutic, at one extreme, to coercive and even exploitative, at the other. In addition, patients receive a variety of services, varying from agency to agency in number, availability, and procedure, that go beyond drug use and encompass efforts to intervene in relation to significant aspects of the total life space. These include economic help, housing assistance, training in employable job skills, job counseling and placement services, educational counseling, medical, legal, and family services, programs related to social skills and value orientations, and individual and group therapies. For many individuals, participation in treatment involves compulsory activities and harsh surveillance quite unlike what they understand as medical treatment. Further, in at least a substantial percentage of cases, participation reflects either direct or indirect coercion, occurring after the individual comes involuntarily to the attention of the authorities as a result of an arrest, an accident or illness, or other critical incident.

Although the Federal strategy, as stated in the *White Paper,* regards treatment as a means of demand reduction, this does not imply a narrow view of treatment effectiveness as equivalent to abstinence. The *White Paper* reiterates previously published policy in that it recognizes the importance of social rehabilitation, implying that treatment clients should be returned to society capable of at least acceptable levels of self-sufficient, unsupervised community living appropriate to the individual's age and sex. This policy formulation can be used as a basis for identification. Briefly it would appear that at a minimum the goals of federally supported treatment would be to eliminate excessive substance use (drugs and alcohol) and associated illegal-riminal behavior and to assist the individual to engage productively in socially acceptable role activities, such as legitimate jobs, school attendance or participation in recognized vocational training, or homemaking.

Such a formulation involves multiple criteria, which, incidentally, are not sufficiently well correlated to justify a simple composite, either of client success or failure or of program success or failure. Further, it is necessary to examine program structure and activities to analyze the results in relation to the services provided. For example, if a program does not engage in vocational counseling, training, and placement, its results in the employment areas may be expected to be less adequate, all other factors remaining equal, than those of a program that has a first-class employment effort. Such differences may also be reflected in program costs.

The phrase "other factors remaining equal" is extremely important because it reminds us that there are substantial variations among individuals entering treatment in their likelihood of realizing desirable outcomes. These variations have been shown to depend on age, ethnic group, history of drug use and criminality, socioeconomic status, and status on the various criteria at the time of admission to treatment (Sells, 1974a, b; Sells and Simpson, 1976a, b, c). The design strategy in evaluation research requires that the effects of these factors be isolated from those of treatment per se. There is also some evidence in the research cited that suggests that, at least in certain situations, different treatments are differentially suitable for particular patients or classes of patients. Finally, with respect to each criterion, the level of expectation that may be reasonable with regard to treatment outcome may depend also on local and general economic and social conditions. Such contextual factors are thus significant in the evaluation of treatment as well as in the causation of addiction.

The DARP studies cited above revealed many interesting facts. First, more people leave all treatments by quitting, often against advice and frequently by just not showing up, than in any other way. Second, at one year and also at the conclusion of DARP reporting, the percentages still in treatment were 4 to 6 times as great for methadone maintenance patients than for therapeutic community or drug-free patients; detoxification is a short-term treatment and is not included in the comparison. The high rates of early quits in the DARP reflect unfavorably on the screening of admissions and the standards for assignment of patients to treatments (Joe, 1973; Joe and Simpson, 1974, 1975).

In the DARP research there were three separate studies of behavioral outcomes during treatment, for three successive cohorts (Spiegel and Sells, 1974; Gorsuch, Abbamonte, and Sells, 1974; and Gorsuch, Butler, and Sells, 1975). *In all three studies there were significant gross changes on all criteria in all treatments in the expected direction of reduction of socially deviant behaviors.* Reduction of drug use, both opioid and nonopioid, was by far the most substantial result and occurred early in the period of treatment for most patients, usually in the first 2-month report period. Reduction of opioid use was greater than of nonopioid use in methadone maintenance treatments, reflecting higher pretreatment levels of opioid than of nonopioid use for these patients, while the opposite was true in drug-free treatments. Changes in therapeutic community and inpatient detoxification were moderated by the isolation from community risk in these environments, but were substantial, as expected, validating the effectiveness of the isolation. Substantial decreases were also reported for the short-term outpatient detoxification treatments on both opioid and nonopioid use. Moderate decreases in alcohol consumption (beer, wine, and liquor) were found in all outpatient (as well as residential and inpatient) treatments. Only very modest improvementswere reported on employment and productive role activities and only in the methadone maintenance treatments; these tended to occur later in treatment than the changes on all other criteria, which were generally seen on the first status evaluation report. Reduction of criminal activities, reflecting illegal activities as a source of support, arrests, and time in jail, was also substantial in all treatments.

Overall, the methadone maintenance treatments showed more effects than other treatments, particularly on opioid use and criminality. Sex differences could be noted on pretreatment scores on some variables, but these were slight and for the most part the changes observed during treatment were comparable for male and female patients.

In all treatments it was noteworthy that most changes observed occurred early, on the first status evaluation report; only productive activities and employment registered change later. In addition to the initial changes from pretreatment levels, which suggest the effects of coming under the surveillance of treatment authorities, further changes were found throughout the first year of treatment on productive activities, employment, opioid use, and criminality. Patient groups that remained in treatment longer improved more than short-tenure patients on these outcomes, in all treatments, although they did not differ from them on admission means. This latter finding also suggests a genuine treatment effect, although its status as an enduring therapeutic change must await the verdict of posttreatment follow-up evaluation.

Ethnic group differences were striking; overall, considering both pretreatment and during-treatment mean scores and the changes reflected by differences between them,

the white sample appeared least deviant and the black sample the most deviant, while the Puerto Rican and Mexican-American patient samples were in between.

The major conclusions of these studies were consistent across cohorts. They indicated, first, that the low correlations between background factors and during-treatment criteria appear to reflect favorably on the effectiveness of treatment, since most patients did well on most criteria. Without significant large variations on criterion scores, there was no opportunity to obtain larger correlations. Second, the relationships that were observed were in part confounded with effects of the demographic and other classification variables with which they were correlated and hence had overlapping effects.

Posttreatment Evaluation

Evaluation focused on criterion measures obtained after patients leave the treatment environment and return to unsupervised community living is essential to the assessment of treatment effectiveness. The critical questions on treatment effectiveness involve evidence concerning the endurance of changes observed during treatment and the extent of these changes from pretreatment levels on various criteria, and also concerning the changes toward specified treatment goals that may be presumed to result from processes set in motion by particular treatment regimens. As we have already indicated, treatment outcomes imply multiple criteria defined in relation to goals, processes, and components of particular programs, and these are assumed to be differentially attainable by different patients as a result of individual differences and varying situational circumstances.

Such treatment follow-up studies as have been published (these appear in the previously mentioned bibliography available from the author) have shown a wide range of results that are difficult to summarize. In general, patient differences in age, ethnic group, socioeconomic status, family background factors, age at onset of drug use and addiction, years of use, types of drugs used initially and prior to treatment, previous criminality, employment history, and situational factors including availability of drugs, have been found by different authors to be related to positive outcomes. In stepwise regression analyses the residual variance attributable to treatment per se, after removing the effects attributable to such patient and situational variables, has generally been disappointing.

How should these data be interpreted? A cynic might say that effectiveness results could be manipulated by controlling patient input. In our opinion if we could do that we would be well off. Indeed, the approaches that we in the DARP program have advocated, of specifying treatments, patient variables, contextual variables, treatment experience, and criteria, are designed to acquire the needed information that would make it possible to maximize the effectiveness of treatments by identifying characteristics of patients who function best in each. At the same time, such research might identify both needed changes in programs to increase their effectiveness and also patient groups for whom no effective treatment programs are available.

At this time the verdict of well designed, large-scale, comparative posttreatment evaluation of drug abuse treatment is not yet available to answer the questions implied by this discussion.

We would like to identify the optimal results, with respect to each of the criteria mentioned, of each specified treatment type and the patient groups for which these

are obtained. Comparative results for different treatments are needed with respect to various patient groups. Unfortunately, these are difficult to piece together from evaluation studies of single programs, which may nevertheless have useful implications for the respective programs. Follow-up studies of the first two DARP cohorts are currently in progress, and it is hoped that they will provide some of the information needed.

Summary

Evaluation of treatment, both during treatment and posttreatment, indicates positive results; however, these involve numerous relationships that require the most careful analysis and interpretation. Detailed discussion of these issues can be found in the DARP research reports (Sells, 1974a, b; Sells and Simpson, 1976a, b, c) and in the evaluation literature. The following comments are limited to an overview of treatment effectiveness as a process of demand reduction for illicit drugs and as a process of rehabilitation for users and abusers of illicit drugs.

1. During the time that patients are under the surveillance of treatment programs the usage of opioid and nonopioid drugs has been controlled with considerable effectiveness in virtually all treatments for virtually all types of patients. Reduction of criminality associated with drug use has also been observed very generally. Variations by treatment type and patient type have been noted, and for some groups-for example, youthful nonopioid users in drug-free programs-the results have not compared favorably with those of other groups-such as older, hard-core heroin addicts in methadone'maintenance programs. Nevertheless, the overall results regarding during-treatment drug use suggest that treatment can be considered a viable alternative to other forms of social control, such as incarceration. We believe that in cost-benefit terms, the outpatient treatments in particular would fare well in the comparisons.

2. Rehabilitation must be considered as well as control of drug use and crime. In the evaluation of treatment, posttreatment evidence of self-sufficient, productive life styles, reflecting processes initiated either as a result of decisions to enter into treatment or of behaviors initiated during treatment must be weighed along with the during-treatment facts. One of the DARP studies (Demaree, Neman, Long, & Grant, 1974) showed that patients who do well in employment generally do well on most other criteria. We must therefore be prepared to evaluate treatments, such as therapeutic communities and the change-oriented outpatient programs, in which during-treatment employment opportunities are limited, on the basis of posttreatment employment outcomes. The initial indications of the Cohort 1 follow-up tabulations are encouraging in this respect, but the data are not yet analyzed.

3. It should be noted, however, that many programs have what appear to be inadequate provisions for employment counseling, training, placement, and associated activities. To the extent that such inadequacies may be shown to result in poorer outcomes, in comparison with those of agencies that provide more comprehensive services, the indications for directions of improvement of treatment services will be clear. Of course, contextual factors must be considered in these comparisons.

4. The analyses of the subgrouping of the treatment population and of transition to more serious drug use, presented earlier, have emphasized the importance of the social context in the initiation and continued use of illicit drugs. Whether treatment

is labeled by the medical model or with any other kind of term, it must be recognized that drug use and addiction are not diseases, but behavior patterns of people who have found ways of satisfying their needs that are disapproved of and rejected by society. Treatment directed toward compliance and conformity that does not assist in finding socially acceptable need satisfaction is probably self-defeating in the long run. Senay and Wright (1972) have argued eloquently that we must understand and help meet the needs of the total person, and this is consistent with the conclusions that are indicated by the treatment evaluation data.

Concluding Note

My colleague, Krishna Singh, a demographer, has analyzed a recent Census report on population changes during the era, 1970-1975, and projected age trends through the next 10 years. As a result of an expected decline in the 10 to 13 and 14 to 17 age ranges, that would reduce the numbers of possible recruits to the drug using and heroin using ranks, he predicts that instead of a continued increase, there will be a decrease in the drug using population by about 1985, regardless of who may claim credit for solving the addiction problem. He considers it safe to expect that natural processes in the population will play a major role.

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The research on which this paper is based was supported in part by contracts HSM-42-69-6, HSM-42-72-132 and Grant No. H81 DA01598, and Grant No. 8 R01 DA01400.

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Comments on Sells' Paper:

"Reflections on the Epidemiology of Heroin and Narcotic Addiction from the Perspective of Treatment Data"

REGINALD G. SMART, Ph.D.

Dr. Sells and the Institute for Behavioral Research (IBR) group at Texas Christian University (TCU) must be congratulated for developing, researching, and managing the Drug Abuse Reporting Program (DARP) system. It is certainly one of the largest epidemiological and treatment projects ever undertaken. It involved some 43,943 admissions on whom data were collected. In all, 46 separate treatment agencies were involved. It is doubtful that there ever was a larger project in the history of drug abuse treatment research The size of this project and the number of papers and reports on the outcomes make any sort of review activity extremely difficult. For this volume Dr. Sells has prepared a paper of some 14 pages that is a distillation of at least several hundred pages of more detailed reports. Obviously, no reviewer can hope to comment on all possible questions raised by the DARP projects. No more than a few can be attempted in the time available. The comments in this review are organized around several questions that have come to mind in reading these reports. It is always easier to raise questions than to answer them, and many of the questions raised have no clear and unambiguous answer. The questions are:

1. What can data from treated heroin addicts tell us about epidemiology?
2. How well does the TCU sample represent addicts coming for treatment?
3. What are the best methods of examining differences among addict groups?
4. What problems do the models of heroin addiction present?
5. What can the TCU data tell us about the efficacy of treatment?

What Can Data from Treated Heroin Addicts Tell us about Epidemiology?

An important concern in Dr. Sells' paper is the question of what inferences about epidemiology can be made from information received only from treated cases. There may be a tendency to infer that demographic changes in the treated group in age,

ethnicity, and the trend towards multidrug use reflect changes in the drug-using population. However cautiously interpreted, this is an area in which many errors can be made. It is necessary for us to remember that we do not know much about the total population of heroin users or addicts. Unless addicts and users in treatment are a large proportion or an unbiased sample of the total numbers of addicts and users, then great fluctuations in the total number can take place without being reflected in the segments in treatment.

At any given time, what proportion of addicts are in treatment? It seems that we do not have very good answers to that question. There are a few suggestions from ethnographic and community studies. A street survey using informal methods in Niagara Falls (Delgaty, 1976) turned up 186 heroin users, many of them daily users, but very few of whom were in treatment. It is also known that the numbers of indexed addicts in Canada increases each year, but that the numbers and proportion in treatment, chiefly methadone maintenance, are decreasing. Several studies in the United States have also shown that chipping and even daily use of heroin is possible without treatment. The early study by Chein, Gerard, Lee, and Rosenfeld (1964) and those of Preble and Casey (1969) indicated that slum dwellers often engage in regular or irregular heroin use while treating or withdrawing themselves without help. Even better indications come from British studies that have compared numbers of surveyed addicts with those in the registration system. A case finding survey made by Arroyave, Little, Letemendia, and De Alarcon (1973) in Oxford showed 20% of 63 "certain' cases of opiate addiction had not been treated [i.e., were not notified (that is, were not reported to the home office)], but that none of the 111 "very probable, probable or suspected" cases had been treated (i.e., were not notified). Blumberg, Cohen, Dronfield, Mordecai, Roberts, and Hawks (1974) interviewed 210 persons seeking treatment for the first time at British outpatient clinics; most reported that they had two or three friends who also fixed but were not in treatment. Probably we must conclude that addicts in treatment, and especially the TCU sample (who were not all addicts), are a small but unknown proportion of the total population requiring treatment. We should be very reluctant then to assume that their characteristics reflect the total group; probably they are less often employed, less socially stable, and have fewer resources than those able to maintain themselves without treatment. Data derived from treated populations aid us in one of the tasks of epidemiology-that of "completing the clinical picture and the identification of syndromes" (Morris, 1964). However, they can mislead us with regard to community diagnosis, assessing the individuals' chances, and examining etiology.

How Well Does the TCU Sample Represent Addicts Coming for Treatment?

There can be no question that the TCU sample is large and well selected from many points of view. It includes most types of patients and all of the important treatment dimensions. However, other difficulties and queries remain. The original sample of 43,943 was eventually reduced to 27,460, or about 63% of the starting figure. Doubtless many of the exclusions were necessary, e.g., those for questionable data or those involving non-drug-users. However, there were 6,074, or 22% of the final sample who were admitted to treatment but did not receive it. Many follow-up and treatment studies include such people. Presumably they all arrived for treatment, were somewhat motivated to take it, but did not. It would be of interest to know why they did not take treatment. Most are probably early dropouts and could be made into an untreated sample. By not including them there is a possibility of

serious bias in the treated group. Treatment agencies should be assessed for their holding power as well as for their effectiveness with those whom they attract. Obviously, if holding power for some agencies is very low their total effectiveness cannot be great.

Another possibility is that the remaining 27,460 are somewhat different from the total arriving for treatment. Dropout rates for heroin addicts vary by treatment type (see Baekeland and Lundwall, 1975, for a review). Typically, dropouts are highest in drug-free programs (up to 82% in the first 10 weeks), lower but also "high" in outpatient detox programs (26 to 68%), and lowest in inpatient detox and methadone maintenance programs. Generally, reviews suggest that the patient who drops out is male, younger, single, living alone, poorly educated, previously unemployed, and has a history of juvenile delinquency and many arrests. He tends to be residentially mobile, to have no previous treatment, and to deny his addiction. By omitting the 6,074 early leavers or dropouts from the study, analysis of the TCU sample may include too few people in drug-free programs and too many socially stable, well motivated, and easy to treat cases.

What are the Best Methods of Examining Differences among Addict Groups?

Analyses have been made of the social and demographic characteristics of various types of users, Nonopioid users start drug use at an earlier age than opioid users. Sells' analysis describes a polarity between "low socioeconomic level street heroin users" and "youthful, middle class, maladjusted, nonopioid users." We are not sure whether a multivariate analysis has been used here (the text does not seem to say) or whether single variable differences are being described. It can, of course, make a substantial difference. One cannot be sure which of the variables is most important, which have unique explanatory power, and what proportion each of them explains. Our own preference is for Multiple Classification Analysis (MCA) of this sort of data. This allows one to examine relationships between several categorical variables and a single dependent variable (drug use), and determines the effects of each predictor before and after adjustment for its intercorrelations with other predictors. Just one example of the use of this technique might illustrate its value. We have used it to predict drunks and heavy drinking among high school students. In the single analysis, age, sex, grade average, father's and mother's drinking, extent of drinking, lack of parents knowledge of drinking, drinking away from home, and drinking in cars were all statistically associated with getting drunk once or more in the past 4 weeks.

However, three variables explained all of the reliable variance, and only three had any unique explanatory value—that is, being a heavy drinker, drinking in cars, and having parents unaware of the drinking. To return to the TCU data, it would have been helpful to know how many variables were examined and which have unique explanatory value in a multivariate analysis.

What Problems do the Models of Heroin Addiction Present?

The TCU paper presents two models of the development of drug using behavior. Both models are essentially "stage-oriented, multiprocess development schema." It is always attractive to have models of deviant behavior. They have heuristic value in that they can guide research in an area that is essentially nontheoretical and short of concepts. One problem is of course to know from what source to derive the

models. Are they to be derived from theoretical concepts, empirical findings, or a combination of the two? Both models presented seem to derive from both sources. As stated in the TCU report, the data do not really permit an estimation of the value of the models in explaining addictions. In fact, it is doubtful whether data from treatment centers could ever be useful in this task. What we need are longitudinal studies of populations at risk for developing addiction with follow-ups and reassessment at several points, e.g., before drug use, during the experimentation phase, after regular use, and after addiction. To date, we do not seem to have any studies that would provide this sort of longitudinal information for addicts. Much of the model building we have is based on short-term studies of select populations or retrospective studies of known addicts. Our models therefore are often attributing causality to what may be contingency or chance, as a result of looking at users at a particular point in time.

What Can the TCU Data Tell Us about the Efficacy of Treatment?

The main purpose of the TCU analysis is probably to provide information on treatment outcomes and on the relative value of different sorts of treatment regimes. In general, the results appear positive and encouraging. There appear to be "significant gross changes on all criteria in all treatments in the expected direction of reduction of socially deviant behaviors." Reduction of drug use was striking and occurred early; other results-e.g., increases in productive activities, employment, decreases in opioid use, and criminality-occurred less often and later. The most optimistic among us would probably take much comfort in these results and believe that our treatment methods are effective and valuable. The most pessimistic might claim that such good feelings are premature, that the design leaves much to be desired, and that it allows for no positive conclusion about treatment.

One major problem is, of course, the lack of a control group of untreated patients. We know that addicts sometimes withdraw themselves, and that their addictions are often fluctuating. Can we be sure that the "treatment" effects found would not have occurred without any treatment intervention? Can we be sure that they are more than random fluctuations in drug careers that have many ups and downs? Spontaneous recovery is a well known phenomenon in all deviant and psychologically disturbed groups. It is well established for neurotics and alcoholics (Smart, 1975) but apparently there is no adequate study for drug addicts. The best available study for alcoholics (Kendall & Staton, 1966) seems to show that treatment allowed only 10% more alcoholics to control their drinking than no treatment. We know that "maturing out" occurs with addicts, but probably that does not account for much change over short periods of time. All of our studies of treatment outcomes, including the TCU one, would be much improved by having no-treatment controls or placebo groups. We do have some controlled studies for methadone treatment (see Henry, 1974, for a review) but apparently none for other kinds of treatment. The TCU study could have been improved by following up some of the 6,074 persons who applied for treatment but did not receive it.

It is notable that most changes, especially in drug use, occurred early, often in the first few months after treatment, and that other changes also occurred fairly early. This, of course, raises the possibility that when patients come into treatment they are at the bottom of a cycle and "ready to improve." In some of our own studies and those of others it appears as if addicts enter treatment at a time when their

resources are depleted. That is, they tend to be unemployed, harassed by the law, separated or divorced, and tired of chasing drugs. Often their social stability is lowest at the point of entering treatment, or lower than it has been for some time. Many have nowhere to go but up. This suggests that posttreatment changes in criminality and tmployment need not be very great to create a statistically significant difference. However, the posttreatment year may not be much better than many of their earlier years. Again the possibility of random fluctuations must be raised.

A further problem is the way in which patients get into treatment. In an ideal design addicts would be randomly assigned to treatments-methadone maintenance, therapeutic communities, and detoxification. This would allow us to be sure that each type of treatment population included patients with similar drug use backgrounds, criminality, social stability, and motivation for treatment. Certainly it would be difficult to randomly assign patients, as all treatments are not equally acceptable and differential rates of dropout might occur. A way of dealing with this is, of course, to match patients given different types of treatment for variables that might be associated with outcome. We wish to separate out the effects of patient treatability and treatment effectiveness. Apparently this has not been attempted in the TCU analysis. The results seemed to show the highest dropout rates among those in detoxification and therapeutic community programs. However, we have no way of knowing whether this is mostly due to the treatment offered, lower motivation, or lower social stability of those selecting detoxification and therapeutic communities as opposed to those in methadone maintenance. One important consideration mentioned in the TCU report is that younger persons were excluded from methadone maintenance by law. Since they are known to have poorer recovery and higher dropout rates, perhaps the holding power of various treatments depends more on their intake than their real capabilities.

The TCU analysis does not allow very detailed statements about what elements in treatment are most important. As is usual in most studies, we have gross categories of treatment intervention-in this case methadone maintenance, detoxification, and therapeutic communities. The methadone maintenance group seemed to show the most improvement. Probably there is a tendency to believe that the methadone itself is a crucial element. However, the report recognizes that most agencies provide services, such as economic help, housing assistance, retraining, job counseling, and legal and family help. Of course, many of these services are provided by agencies that do not offer methadone, but all patients do not receive all of them. It would have been helpful to have had some sort of analysis of the most crucial elements, regardless of whether drugs were given or not. Our own, much smaller studies seem to indicate that the major factor may be getting a job and not whether a drug is given or not. The TCU data would perhaps allow a complete multivariate analysis to investigate which treatment events are most associated with recovery. We would like to know what is the minimal effective treatment for a given modality-e.g., methadone, counseling, job retraining, legal help, and the like. It is also recognized that many of the TCU patients came to treatment under some sort of duress-whether from courts, probation authorities, or families. It would have been instructive to know whether those coerced had better recovery rates than a matched group of those who were voluntary.

In summary, the DARP-TCU study is a large and valuable attempt to provide information on the progress of addicts through treatment. It is doubtful whether such

studies can tell us much about general epidemiology or provide an ideal model for heroin addiction. The study is most valuable in giving us leads for treatment outcome predictors and indications about the comparative value of different treatments. However, the design limitations imposed in studies of this type will always leave open the questions of whether treatment is better than no treatment, and, if better, what elements in treatment are most crucial.

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The Contribution of Treatment Data to Epidemiologic Perspectives of Narcotic Addiction

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This paper is concerned with the uses of treatment data on narcotic addicts in relation to the general level of the state of the art in epidemiology, the contribution of treatment data to epidemiologic methodology, and their potential contribution to policy formulation. Specific attention is directed to the Drug Abuse Reporting Program (DARP) described by Sells in the preceding paper in this report.

Treatment Data as Indices of Morbidity in the Community

Sells' paper begins by dealing cogently and concisely with the question of whether addiction rates in the general population can be estimated from treatment records:

> Apart from issues of definition, admission policies vary widely, geographic units used for population censuses differ from those served by treatment programs, and coverage of treatment programs is biased, particularly in respect to the sampling of agencies not included in Federal networks. (p. 148)

Treatment data could reflect changes in morbidity in the general population *only* if the relationship between the number entering treatment and morbidity in the community were known (Kramer, 1957). Even if there were uniform admission "policies" from place to place, and adequate demographic data were available for the population eligible for treatment, and all treatment programs were included, it would still be necessary to know the numerical relation between the number entering treatment and the number of onsets in the community. To make appropriate comparisons by time, place, or person, this ratio would be needed for various times, places, and demographic subgroups (Richman and Richman, 1974):

Time 1	Time 2
FIRST ADMISSIONS TO TREATMENT	FIRST ADMISSIONS TO TREATMENT
ONSETS IN COMMUNITY	ONSETS IN COMMUNITY

We can only use treatment data to reflect changes in morbidity if we know that the ratio of admissions to onsets has not changed over time. Since the probability of admission varies — over time, among different demographic subgroups, and between places — assessments of morbidity from admissions to treatment must be based on the proportion of onsets who are admitted. If that ratio were known, treatment data would not be needed to infer changes in morbidity.

Characteristics of the Population in Treatment Facilities

The Drug Abuse Reporting Program represents a unique store of data and a major innovation in the assessment of large scale, widespread therapeutic efforts to intervene in the complex problems of narcotic addiction. The development of a systematic approach to describing the characteristics of patients, their treatment, and clinical responses has been followed by larger efforts — the Client Oriented Data Acquisition Process (CODAP) and extension of the methodology by the National Institute on Alcohol Abuse and Alcoholism to federally funded alcoholism treatment programs.

The DARP research sample represents 62% of the population reported as contacting the treatment facilities. Over 16,000 cases were excluded from analysis for the variety of reasons shown in Table 1 of Sells' paper.

Changes in patient characteristics over the 4 years are described as reflecting concern by treatment authorities for youthful multiple drug abusers and a de-emphasis of methadone maintenance in favor of drug-free treatment (p. 153), or, more specifically, as reflecting the influence of patients from one large South Atlantic drug-free program. The DARP data are from diverse programs and do not represent the spectrum of drug use, deviance, or life style. Scattered throughout parts of the United States and changing in composition during the 4-year reporting program, the federally funded programs reporting to DARP reflect shifts in federal funding, local interests, and geographic settings. DARP might be compared to studies of the Smithsonian Center for Short-Lived Phenomena that is concerned with events for which replication is rarely possible. There is no data base equivalent to DARP for which time, place, and person comparisons can appropriately be made.

We do not know whether the current CODAP might be reformatted to represent the treatment programs reporting to DARP in Years 1, 2, 3, and 4, or whether the patient data can be edited to conform to the criteria used in constructing the research file (62.5% of the Master file).

More important is the significance to be attached to differences in the DARP patient categories. The discontitiuities between the four categories of pretreatment drug use are recognized (pp. 158, 161, 166-167) and the confounding of age differences is emphasized (p. 161). It is possible, however, for some readers to consider that the following statement specifically refers to etiology rather than to diverse groups entering unlike programs:

> . . .it is believed that these four categories of pretreatment drug use. . .reflect differences in degrees and types of deviance and different life styles. (p. 154)

Evaluation of the Effectiveness of Treatment

DARP provides a pioneering model for central evaluation of diverse approaches for heterogeneous groups of patients from different social backgrounds in various communities. Some implicit assumptions of the treatment process, patient flow, patterns

of patient care, and patient/modality relationships are not valid. This section emphasizes those aspects of patient care that are especially relevant for evaluation of treatment data.

1. Treatment for an addicted patient rarely follows a stimulus-response model (Figure 1).

2. Patients enter treatment after a complex process involving motivation, attitudes and expectations, screening, evaluation, coercion, and compulsion that will affect the nature, pattern, and course of care. The interaction between patient and. program may result in patients not entering treatment, as Sells emphasizes.

Rather than beginning with entry into treatment, it is necessary to emphasize the screening and selection process that precedes treatment. Accessibility and acceptability should be considered, and the relation of screening to retention and clinical response should be assessed. The intake process and the dynamics of selection affect the composition of the patient population and, therefore, the clinical responses. Differences between those entering and those not entering specific treatment methods should be assessed; case-control methods would be very useful for determining acceptability (to patients) or selection (by staff) within specific treatment programs (Figure 2).

3. The course of patient care is rarely continuous and uninterrupted. Patients who quit frequently return. It is necessary to follow the individual through a number of treatment episodes over a period of time. One should not restrict major attention to the first admission-termination episode, or to the point at which the patient is referred outside the reporting system. (In some treatment settings, patients were not followed from one treatment unit to another. DARP did not include all the treatment units.)

In considering multiple episodes of care or readmission, one should use appropriate actuarial methods. It is necessary to consider readmissions, and even the continuity in programs to which patients were referred. The effectiveness of treatment need not be considered solely in terms of continuous retention within a single program (Richman and Perkins, 1972).

The number and type of readmissions affect the treatment program, its staff, and their therapeutic effectiveness. Patients who have returned can alter the characteristics of groups of ongoing program participants, change staff expectations, and affect the therapeutic optimism of new admissions.

4. The definition of treatment modality, "intensity," duration, and "mix" of treatment was defined by participation rather than by clinicians' intention or prescription. Perhaps in some situations it is realistic to consider only participation; but if we assume that treatment prescription is a logical activity, we should consider whether the clinical intentions of the staff were fulfilled. Patients' motivations and expectations affect their acceptance or compliance with prescribed treatment; patients enter treatment modalities that may not have been initially considered most appropriate by staff. Treatment evaluation must try to determine what the staff prescribed and whether the patient accepted that initial prescription.

Treatment modalities are rarely homogeneous, seldom applied in a therapeutic vacuum, and are usually mixed or affected by different treatment approaches. There are major differences in results between apparently similar programs in adjacent areas, followed over time (Perkins et al., 1975).

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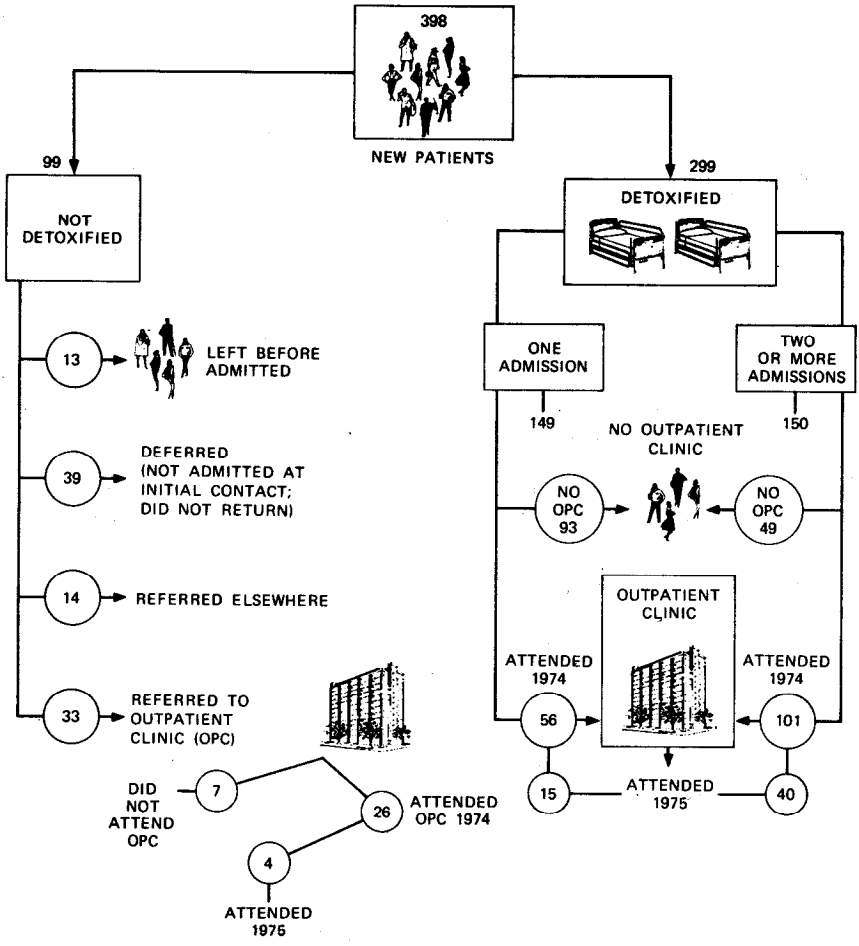
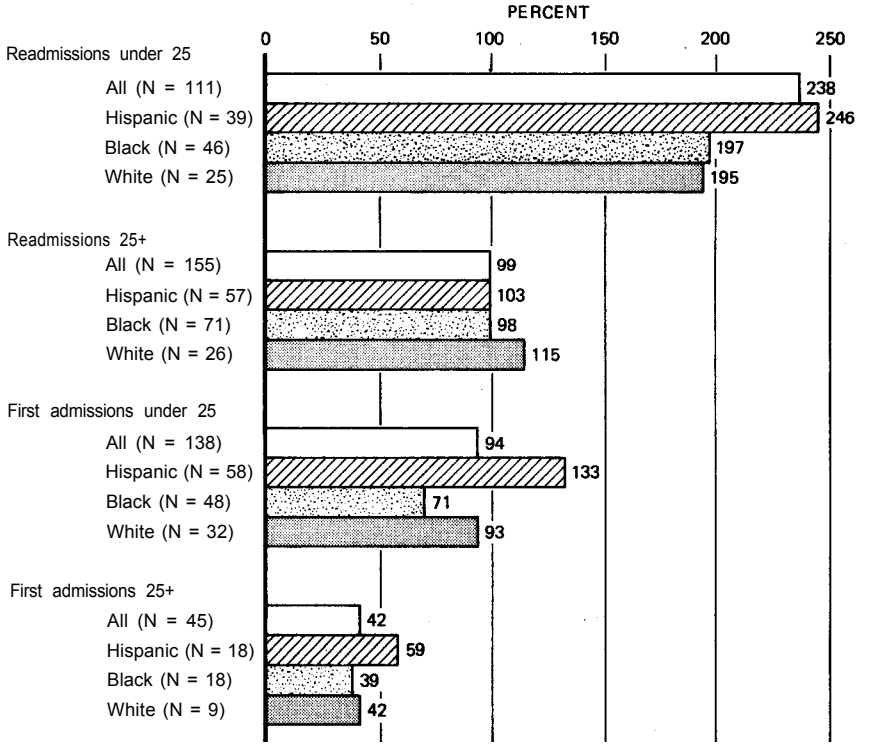


FIGURE 1. Beth Israel Medical Center Alcohol Treatment Program - New patients seen January-March 1974. Nature of contacts for patients up to December 31, 1975, by number of patients

5. Evaluation of initial attitudes and expectations of staff and patients is crucial in evaluating the repsonse of patients to a treatment modality (Richman & Trigg, 1972b).

6. Evaluation of intervention should consider changes in morbidity in the community. It is essential to assess whether intervention has decreased the frequency of narcotic addiction in the community. The General Accounting Office has shown how unannounced urinalyses of servicemen in Vietnam showed no change in opiate usage while urinalyses at embarkation showed progressively decreasing proportions of narcotics (Figure 3).



SOURCE: Richman. A. and Trigg, H.L. Selection of heroin addicts for a therapeutic community — APHA 1972.

FIGURE 2. Relative risk of admission to a therapeutic community from a male detoxification service

Workings of Health Delivery Systems

Hypothetically, treatment programs operate in a rational, logical manner. Applicants are assessed, needs are determined, and modalities prescribed on the basis of visible, objective criteria. Patients enter the treatment to which they are assigned. Treatment approaches do not differ from place to place or from time to time. Patients remain in treatment until the maximum attainable clinical response is achieved.

Since few health delivery systems adhere to these mythical standards, attention must be focused on spelling out the way they actually work; the interrelations between patients, clinical staff, and treatment methods; the dynamics of their interactions; and the ways in which retention and clinical response are affected by the participants and treatment setting.

Sells recognizes that more people leave all treatments by quitting than for any other reason and regards high rates of "early quits" in all treatment programs as reflecting "unfavorably" on the screening of admissions and the standards for assignments of patients to treatment (p. 172).

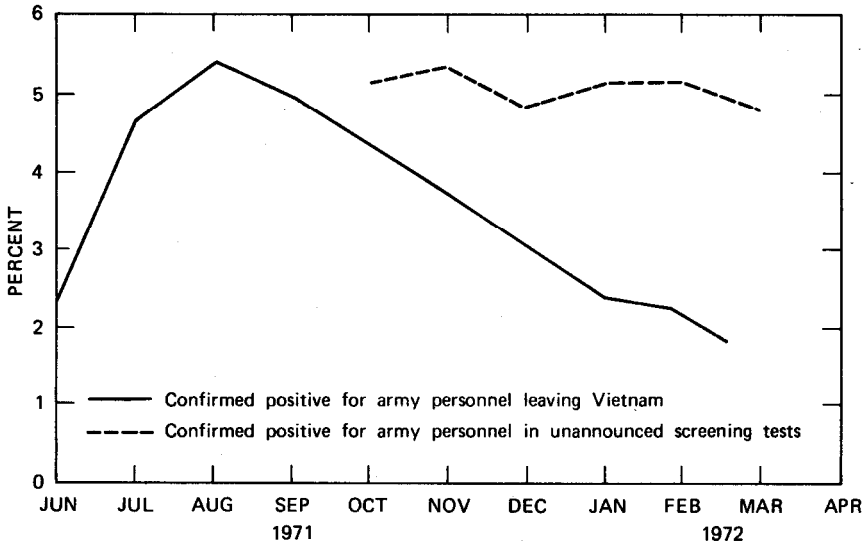


FIGURE 3. Results of urinalysis screening tests for 'Vietnam servicemen. United States General Accounting Office, Report to the Congress, *Drug Abuse control activities affecting military personnel* [B.164031(2)] AUGUST 11, 1972, P. 21. (Prepared from information originally obtained from Department of Defense sources by the Subcommittee on Drug Abuse, Senate Armed Services Committee.)

Patient improvement increases with longer retention in all treatments (p. 174). What are the high-risk factors for quitters by time, person, and place? What points in the treatment process and what patient characteristics are predictive of premature termination? Do we know how to retain those narcotic addicts who are at high risk of quitting in treatment?

Sells details the need for identification of all the potentially critical variables to achieve optimal understanding and effective application of knowledge and that these variables must be united in a systematic model that reflects their system properties and idiosyncrasies (if any), their main effects, and their interactions with other factors (p. 164). Such models of treatment systems are critical for evaluating the response of patient types and treatment modalities. The organizational context of treatment has major impact on the recruitment of applicants, assessment, retention, and treatment. Sells acknowledges the significance of organizational context but does not elaborate on how these factors might be identified and their role assessed:

> . . .in certain situations, different treatments are differentially suitable for particular patients or classes of patients. . .the level of expectation that may be reasonable with regard to treatment outcome may depend on local and general economic and social conditions. Such contextual factors are thus significant in the evaluation of treatment as well as in the causation of addiction (p. 171).

We believe that it is essential to consider the psychological and social aspects of treatment settings, to identify the potentially critical variables in modalities, staff, and patients, and the settings, properties, main effects, and interactions. Sells

recognizes that the distribution of drug use patterns is not random but lawful and multiply determined. The multiple determinants of patient recruitment, assessment, and treatment prescription must be defined and assessed, and their relation to the course and outcome of care must be explored.

Can treatment assignments be based solely on a typology of patients? Far more research is needed, as Sells recognizes, to identify the changes needed in programs to increase their effectiveness and to develop new interventions for patient groups for whom no effective treatment programs are yet available.

Demographic Context

The mortality rates for DARP patients must be compared with those for a more appropriate group than the general population. People living in poverty areas have far higher crude death rates than those living in areas with more adequate incomes. For most cities, death rates for persons living in poverty areas are generally 50 to 100% higher than for individuals in other areas. Violent death rates are 91% higher for whites in poverty areas than in nonpoverty areas, and 55% higher for nonwhites in poverty areas than in nonpoverty areas (Ventura, Taffel, & Spratley, 1976).

A more critical demographic problem is contained in Sells' concluding note, which refers to an expected decline in the 10 to 17-year-old age group in the next 10 years' and a predicted decrease in the drug using population by about 1985. This prediction, based on age alone, apparently overlooks the fact that "street heroin addicts" come mainly from segregated, high-density, inner-city areas, and are predominantly members of minority ethnic groups that are educationally, economically, and socially deprived (p. 175). If the alleged decrease in the 10-to-17 age group had been demonstrated as reliable and were accompanied by a reliable absolute decrease in minority group members aged 10 to 17 living in inner-city areas, the demographer's prediction of declining narcotic use would be more credible.

Data systems for patients in treatment programs must maintain confidentiality. However, without reference to the geographic area of residence (by census tract or ZIP code), it is impossible to compare the characteristics of those in treatment programs with the demographic composition of the area in which they live. It is not possible to assess from DARP data whether intermodality differences in patient populations are due to population differences in the acceptability of treatment or staff preceptions of treatment needs, or whether they represent differences in the characteristics of the population in the region.

Programmatic Relevance of Treatment Data

Programmatic relevance is characteristic of information that is essential for program planning and evaluation and for modifying'program effort in appropriate directions. Treatment programs must be focused on demographic subgroups or geographic areas as well as being directed toward goals of definable changes that can be objectively evaluated.

Wilkins (1965) emphasized that the concept of information has meaning only with respect to a purpose. What does the knowledge enable us to do or decide that would be impossible or impracticable without the knowledge? If the same decision could

be made without the knowledge of "X" as with the knowledge of "X", the utility of the information is zero. It is essential to ask questions that have utility, and that result in decisions that could not be reached without answers to them.

Epidemiologic information is needed for defining target subgroups in the general population; for measuring accessibility of treatment, utilization, and rate of entering treatment by defined (or target) populations; demarcating patient groups at high risk of quitting treatment or dying; and assessing whether selection for treatment is random, purposive, or biased.

Epidemiologic information must be able to enhance decision making for program priorities and allocations, enable policies and 'strategies to be based on adequate problem definition or understanding of the natural history, and specify groups or circumstances where intervention is needed.

Without epidemiologic perspective, it is impossible to detail the location and nature of heroin-related problems that require intervention, to identify the need for additional resources, or to assess whether clinical or social attempts at intervention or treatment are having any impact on incidence or prevalence.

Relation of Treatment Data to Other Measures of Morbidity

Finally, we would like to emphasize the need for correlation of various epidemiologic approaches and methods in one geographic area over a prolonged period of time.

Treatment and survey data and direct and indirect measures of heroin addiction must be correlated on a long-term basis for a defined geographic area. This would involve a common unit of observation, a defined geographic area; different measures and methods would be correlated, their lags and leads determined, and their significance explored. In this focused effort, treatment data would be used to assess the working of health services and would be related to other measures of the extent, distribution, and spread of heroin addiction in the community.

Conclusions

Data from treatment settings are essential for assessing the accessibility and acceptability of health services to patients. However, it is not yet possible to infer the extent and distribution of morbidity in the community from treatment data alone. Much more research is needed to increase our understanding of the relationships between treatment data and other sources of information about morbidity. These interrelations would be best assessed by focusing on a specific geographic area for which data from a variety of sources could be collated and interpreted. This focus would enhance our ability to evaluate the effectiveness of treatment in reducing the incidence and prevalence of heroin addiction, as well as increase our understanding of the course and long-term outcome of treated heroin addiction.

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This review was based in part on research support by Grant Number 00666-03 from the National Institute on Drug Abuse.

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[1]Note, however, that the U.S. Bureau of the Census, Current Population Reports, Series No. P-25 No. 388 Series D Projections (very low fertility rate) estimate, for between 1970 and 1985, a 16.4% increase in whites and 43.4% increase in nonwhites aged 18 to 24.

Consequences of Use: Heroin and Other Narcotics

Consequences of Use

Heroin and Other Narcotics

IRVING F. LUKOFF, Ph.D.

Our charge is to say something about the consequences of narcotic use. Our remarks are restricted to the post-World-War-II American experience. Out of the host of issues, we selected those that may have some possible relevance for policy.

We first venture a brief, schematic review of current epidemiological findings, because we feel this must serve as a backdrop for anything about the sequel to heroin use. We then highlight the inappropriate tendency to minimize the impact that heroin use has on some communities as knowledge of the self-limiting nature of narcotic use for many persons is noted. The view that alcohol and tobacco undoubtedly inflict more aggregate damage is, in our opinion, a myopic one because it fails to locate the problems with sufficient specificity. Therefore, we will look at one community in order to elaborate our view that overall heroin use rates may be a bit deceiving, and that the problem of heroin use cannot be compared to other forms of abuse by the simple criterion of quantity.

Since we were charged to look at the sequel to heroin use, we review, perhaps too critically, the efforts to identify life cycles and various role typological schemes we all find so attractive. We suggest an alternative procedure, and attempt to demonstrate the possibilities of the use of more appropriate methods with a body of data derived from a treatment population. We suggest that the model we use, or other related methods derived from multiple regression techniques, may finally help to bring some order from the fascinating, but somewhat anarchic, literature of life-styles and role typologies.

Finally, we venture into a bit of sociology of science in order to explicate a set of very peculiar facts about the treatment system. Certainly a major consequence of heroin use is the proliferation of treatment programs. We denote only one modality,

methadone, and, even here, do not comment on the many variations in programs. But we do feel that the epidemiological premises of methadone are in need of revision, and that the current advances in our knowledge should be brought to bear on the treatment system.

Epidemiology and the Course of Narcotics Use

What happens to those who finally succumb to the lure of narcotics? Most such individuals have had at least some prior experience not only with tobacco and alcohol, but also with marihuana and a veritable pharmacopoeia of other substances, ranging from glue and other toxic substances to various opiate-laced drugs, barbiturates, amphetamines, and other somewhat less common depressants, psychedelics, and stimulants (Kandel, 1975; Robins & Murphy, 1967; O'Donnell, Voss, Clayton, Slatin, & Room, 1976). Only a small subset ever move on to heroin, currently the most common opiate and the one about which there is most societal and policy concern. More individuals are reported to have used nonheroin opiates. Yet, in most communities they abort nonheroin opiate use, or they transform their allegiance to heroin.

Although the lure of opium and morphine has been long acknowledged -strengthened by the difficulty that identified compulsive users have had in abandoning their dependence – the myths about heroin are at least as firmly established as those developed in the early struggle concerning opium (Terry & Pellins, 1928/1970; Johnson, 1975). Myths not only exist in the public mind, but are also promoted by addicts and the professionals who work with them. For example, the title of one popular book written by one of the more progressive and enlightened members of the treatment community begins, "It's So Good, Don't Even Try It Once." (Smith & Gay, 1972). This title, quoted from a heroin user, certainly represents part of the mythology that is evoked by addicts and provides an important part of the rationale for their encouraging others to share their discovery

Our concern is with the larger shift in our understanding that has developed over the past few years. Many of the current findings in heroin epidemiology had been suggested before the present proliferation of research. Heretofore, the focus of attention was on those kinds of findings that predominated in the professional treatment and criminal justice communities. Although all of the studies, particularly those to which we allude that were completed prior to the early 1970s, were flawed in some aspect, they will not be scrutinized for their methodologies. Their significance is that they provided the basis for the professional and lay public's concept of the "inevitable" sequel to incipient heroin use, namely addiction and crime.

The early systematic studies of users of heroin and other opiates were primarily shaped by a small, prolific, and imaginative group of researchers associated either directly or, at least for research purposes, with the federal treatment programs at Lexington and Fort Worth (Ball&Chambers, 1970; Baganz & Maddux, 1965; DeFleur, Ball & Snarr, 1969; O'Donnell, 1964, 1966; Vaillant, 1966, 1968, 1973). It was a natural place for studying addiction because it was, at the time, the only place where large numbers of opiate users – in the later years, mainly heroin addicts – could be carefully studied and followed for various tenures of posttreatment adaptation. Connected with a government program, the authors had access to official records often difficult for outsiders to obtain, and with persistence and ingenuity they were able to locate discharged patients over long periods of time after their release.

It was from this group of researchers that most of our knowledge of addiction was derived. Important also, in the development of viewpoints about the nature of addiction and the addict, was the treatment effort at Riverside Hospital and the follow-up of results from that program (Alksne, Trussell, & Elinson, 1959).

Certainly, as the follow-up period of these studies increased, the likelihood of locating more individuals who were apparently drug-free increased (Vaillant, 1966, 1973; Duvall, Locke, & Brill, 1963; O'Donnell, 1966). There was also a rigidity in many of the studies; when the slightest indication of drug use was detected, even if it were not compulsive, the individual was, with few exceptions, classified as having returned to addiction (Duvall et al., 1963). The crucial element in all these studies was that they were concerned with drug users who had come to the attention of the criminal justice system, although some were self-committed. Many were charged with a variety of crimes, although a large subgroup was identified only as violators of the drug laws. Nevertheless, these were individuals whose drug use was compulsive, and most of them were often in continual difficulty with the authorities.

The Riverside studies helped to deepen the gloom because a youthful population, presumed to have greater potential of achieving the drug-free state, actually experienced a significantly lower success rate than other groups (Alksne et at., 1959). Despite the agreement of the Lexington-Riverside studies on the course of addiction, empirically derived questions about the inevitability and irreversibility of addiction were being raised.

In fact, before these studies were published, there had been suggestions that the course of addiction was not inevitable. In 1967, Robins and Murphy, who obtained a nontreatment sample of black men in St. Louis, reported, first, that some who had used heroin did not become addicted, and, second, only a few of those who reported addiction (and for whom confirmatory evidence was found in official files) were addicted to heroin at the time of the follow-up. They were not all necessarily drug-free, but they had ceased to use heroin or other narcotics; this was a finding that stood in stark contrast to those of other research results. Perhaps the relatively small number of addicts studied, compared to the earlier cited studies by Vaillant, Duvall, and O'Donnell, contributed to the relatively modest impact the St. Louis study had on the professional community's comprehension of drug addiction. Or, simply because it seemed to defy the experience of those who worked with addicts, it failed to create the effect one might have expected. Currently, the thrust of Robin's findings has been replicated in a number of recent studies.

Winick (1962), with whom the maturing out hypothesis is associated, should also be cited in this connection. His findings were, and still are for that matter, controversial. The evidence with which he had to work was characterized by incompleteness of records and ambiguities of definition. Arguments can be made that most addicts who continue drug use will be recovered by the criminal justice network; alternatively, the high death rates among addicts (Duvall et al., 1963; O'Donnell, 1964) may account for the attrition from the population over time. Despite the attractiveness of the maturing out hypothesis, it received only modest assent in a few quarters. *The dominant wisdom remained: heroin use, once begun, almost always led to addiction; addiction, even if exceptions were noted, almost invariably was irreversible. Even enforced abstinence, whether under treatment auspices or due to incarceration had an inevitable outcome – readdiction.*

In the early 1970s, another group of gifted investigators (Nurco, 1975; Robins, 1973,1975; O'Donnell et al., 1976), operating with different populations, provided a set of findings challenging this dominant wisdom. These landmark studies, many still in progress, have just begun the process of reformulation of widely held beliefs on narcotic use and addiction, although this body of work has yet to receive the attention we would argue it deserves. Certainly in terms of policy towards drugs, particularly narcotics use, there is little evidence that the consequences of these new insights have influenced criminal justice activity, prevention strategies, or treatment – issues we will touch on later.

Baltimore Survey

David Nurco (1975) and his colleagues drew a sample from the Baltimore Police Department Register of 4,069 persons identified as drug users. Their sample, using different ratios for whites and blacks, drew cohorts initially identified to the Register between 1952 and 1971. The criteria for inclusion on the Baltimore Register differ from those of Lexington, and its population is more diverse. Individuals could be listed if they met any one of a number of conditions: possession, loitering with known addicts, as well as having been charged with an assortment of crimes.

In follow-ups (N = 267), the proportion on whom information was obtained was 92% of the survivors In the interviews, the interval being a function of the period since they were first identified, 86% acknowledged that they had been addicted at some period. Among those who were not incarcerated, 57% were nonusers of opiates, 17% were occasional users, and only 7% were regular users of opiates and probably addicted. There were substantial differences between whites and blacks in the sample: only 1% of the whites were addicted compared to 12% of the blacks.

Correspondingly, 67% of whites were nonusers in contrast to 48% of the blacks. Of the whites, 12% were classified as chippers, as were 21% of the blacks.

Vietnam Veterans

The series of reports by Robins (1973, 1975) on the Vietnam veterans is better known than either the Nurco data or, we would suspect, the St. Louis study. These findings have probably infiltrated public awareness, although the full implications are not altogether understood. Two samples were drawn: a general sample of 470 veterans who were returned from Vietnam in September, 1971, and a second sample of 495 (referred to in the report as DEROS positives) who had positive urines during the same month in Vietnam. Of the general sample, 43% had ever used heroin, and 21% reported daily use and reported some symptoms of withdrawal when they ceased taking drugs while in Vietnam. In the 8-to-12-month interval after discharge, only 19% of the general sample, that is, 28% of those who used narcotics in Vietnam, reported any narcotic use; 33% of the DEROS reported at least some use after returning home. Among the other regular users in Vietnam who used for at least 6 months, but who were not identified as having an active addiction in Vietnam, only 4% were identified as addicted on follow-up.

The population selected was one with singular opportunities for narcotic experience, yet the subjects' outcome at follow-up is even more promising than in the Nurco data. Most who try heroin do not become long-term users; all those who use fairly

regularly do not inevitably become addicted; even those who became addicted can curtail drug use (the largest group), or continue drug use on a less than compulsive basis. These findings are essentially confirmed in the 3-year follow-up, although there is the inevitable turnover where individuals move in and out of various stages of drug use (Robins, 1975).

Selective Service Sample

The studies cited above focus on populations where special opportunities existed for heavy opiate and heroin use. A more widely representative population was studied by O'Donnell et al. (1976) and consisted of 2,510 male Selective Service registrants who were born between 1944 and 1954. Although the sample was selected by age and sex to obtain reasonably high rates or reported drug use, difficulties in interpreting rates of low-frequency events such as daily heroin use apply to this sample as they do to all general population studies of rare events.

Heroin use of any quantity is reported by 6% of the respondents, who ranged in age from 20 to 30 years old at the time of the interview. When frequency of use is tabulated, 4.5% report heroin use less than 100 times, and fewer than 1% report use between 100 and 999 times; only one-fifth of the "ever used" group reports using heroin 1,000 or more times. While age of onset may be a factor in the frequency reports, these findings suggest that the majority of those who have ever used heroin become only occasional users or abort use after relatively short periods of heroin experimentation. Nonheroin opiates such as codeine, Darvon, and opium were indulged in by a notably larger group, 31%. The pattern of only occasional use is clearer: 20% less than 10 times; 9% between 10 and 99 times; 2% between 100 and 999 times. Two few reported use of opiates 1,000 or more times to report percentages. These findings suggest, then, that a relatively small segment of the population initiates heroin use; a considerably larger proportion has some experience with other opiates. Only a small proportion of all narcotic users moves to the stage of continuous and compulsive use.

These recent investigations challenge the conventional wisdom of only a few years ago: *The best informed conclusion of the early part of this decade was that heroin (and other opiate) use almost invariably led to addiction. Current epidemiological research examining nontreatment populations documents that while fairly large numbers of persons in the more general community may experiment with heroin or other opiates, they do not necessarily proceed to compulsive use. From the Nurco and Robins studies we also learn that even for those who move on to regular use of heroin or other opiates, continual addiction is not inevitable, so that many regular users either modulate or even cease altogether using heroin or other opiates.* There is abundant evidence that the rate at which this occurs varies with social and age groups.

Community Epidemiology: The Case for Specification

Contemporary epidemiological findings are only now reaching the level of public consciousness where they may have some impact on the course of events. Let us turn our attention to (a) the implications of the overall problem of rates of heroin use; (b) the increasingly well-documented finding that only a small portion of users move on to compulsive drug use; and (c) the accompanying evidence that substantial numbers are also able to forego, often without treatment intervention, narcotic use

after a sustained period of use. These findings raise questions as to whether heroin use is or is not a major problem requiring broad national and community effort.

In many areas of epidemiology when comparisons have to be made, rates are often presented as a function of the total exposed population. Depending on the frequency of occurrence, the phenomena may be presented as a function of various multiples of 10; where events are relatively rare, as in many morbidity statistics, they may be reported as per 10,000 or even 100,000.

Such rates facilitate many useful contrasts even if, as the multiplicative constant is increased, a great deal of attention may be devoted to relatively rare events. As the reader will recall, when one moves to more general populations, the highest reported "ever-used" figure for heroin is 6% for 20- to 30-year-old males (O'Donnell et al., 1976). Only 2% are current users, and if the experience we cited earlier can be extrapolated to these findings, perhaps fewer than a fifth of this group may be using heroin on a regular basis. Only about half of current users may have used it with sufficient frequency to suggest possible addiction. Other household surveys, depending on the population and the year in which the survey was conducted, reported even lower fiiures.

Problems abound with self-reports on heroin use, and as is often noted, street addicts may be less likely to be caught up in the sample designs of most surveys. Although in special populations there is good evidence for high validity of self-reports, there is no basis for extrapolating this experience to more general samples (Robins, 1973, 1975; Nurco, 1975). The likelihood of a conservative bias in these estimates exists partly because of underreporting, but the precise extent of these underestimates is simply unknown. Even if we assume an underestimate that justifies a doubling of the various rates, heroin use involves only a small minority; far fewer become compulsive users. Even without any treatment intervention many of the latter group will terminate use (Robins, 1975).

All these recent findings converge to strengthen the objections of critics who found the concern with heroin use to be excessive. Some of these same writers often suggest that problems'associated with other drugs, such as alcohol and tobacco, should be the primary targets for community effort.

Another view, expressed by Thomas Szasz, is that in a free society one should be able to use drugs since only the user himself is harmed (Szasz, 1970). In a recent paper that draws on Szasz, the authors state:

> Since most addicts are going to use drugs anyway – they have phenomenal survival powers in the face of all obstacles put in their way – we might reconcile our paternalism to let them use the drug as they will. (Waldorf, Orick, & Reinarman, 1974, p. 46)

They go on to state:

> If we had a free society and were less emotional about opiates perhaps we would not need controls. Being irrational and emotional about opiates, we will undoubtedly have controls. (Waldorf et al,. 1974, p. 47)

These citations only serve to illustrate a widely shared view, one that is based on an important distinction between actions that are only criminal in the sense that they affront the morals of society, as distinguished from acts that are dangerous to other individuals or to the community. For many individuals, drug use falls into the category of acts that are private, and therefore should be ignored. At first, it appears

that the recent epidemiological findings we cited would support such a thesis, since relatively few individuals use heroin or other narcotics, and since use becomes self-limiting for most of them.

Let us look closely at one particular community as a case study relevant to this issue. There are other communities that are very much like the one we will examine. We will present two kinds of data: first, information derived from the Narcotics Register; second, data from a community survey conducted in roughly the same area. The community is a high-drug-use ghetto community, predominately black, with all the associated characteristics, including high crime rates, large numbers of female heads of households, and high welfare loads. Despite pockets of well-kept homes and projects, there is a preponderance of slum dwellings.

In Table 1, three different health districts are examined. The first, Bay Ridge, New York is largely white (only .5% black) with some middle income neighborhoods. Median household income was $10,483 in 1970, and only 12% of households were headed by females. The Bedford, New York district is more generally identified as Bedford-Stuyvesant, although our sample is somewhat more restricted in scope. Here 84.4% of the residents are black, and 32.4% of the households are headed by females. Income for households declines to $7,064. Fort Greene is contiguous to Bedford and is the area just over the Brooklyn and Manhattan bridges; some of it is much like Bedford, while other sections still retain some of the older white ethnic and middle class communities. Blacks were 56.4% of the population and 29.2% of all households were headed by females.

The Narcotics Register presents all of the inadequacies of a system that depends on outside referrals for information. In addition, there is not always sufficient information to assume that all individuals reported to the register are necessarily addicted, or indeed, that they even use narcotics; however, some earlier studies suggest that most are involved with narcotics, and a substantial portion are subsequently reported to the register from treatment programs or the police department (Newman, Cates, & Tytun, 1974; Newman, Bashkow, & Cates, 1973).

The register reports only the number of reported users for each Health District. To compute age-specific rates, we apportioned these frequencies in accordance with the age distribution reported for the borough. We did not correct for the lag between onset and first report to the register. Nor did we correct for the more youthful population that generally prevails in ghetto communities. We assume that correct age-specific distributions would shuffle the rates toward the younger years. Since precision is not essential for the points we wish to make, no adjustments were made in the data for these factors.

The rates of heroin use were estimated for different age groups and for each sex for the year 1970, the last period for which we could obtain data by health districts. In Bay Ridge almost all of the drug use is concentrated in the 15- to 24-year-old age group, and is from three to five times as high for males as for females, depending on age. The highest age-specific rate is found among males between 20 and 24 and is about 5% of that cohort, at least for those who are known to the register. For other age groups it is even more modest, although for males 15 to 19, it is 33.5 per thousand, or about 4%. Note that these “official” figures are not very divergent from the reported heroin use figures obtained in more general samples of post-high-school surveys, or from Selective Service respondents, although these figures may identify

TABLE 1

Estimated Age-Specific Narcotic Use Rates per 1,000 for Bedford, Fort Greene, and Say Ridge, New York, 1970

	Prevalence, by health district								
	Bay Ridge			Bedford			Fort Greene		
Age	Total	Male	Female	Total	Male	Female	Total	Male	Female
Under 15	.9	1.3	.4	2.5	3.7	1.2	2.5	3.9	1.2
15-19	22.2	33.5	11.2	72.0	112.3	35.6	75.5	114.7	38.3
20-24	28.3	49.5	10.4	110.0	214.8	37.3	106.5	183.2	39.8
25-44	9.0	15.3	3.0	29.2	56.6	0.7	32.4	58.5	10.2
45-64	.3	.4	.0	1.4	2.7	.4	1.6	3.1	.5

	Incidence. by health district								
	Bay Ridge			Bedford			Fort Greene		
Age	Total	Male	Female	Total	Male	Female	Total	Male	Female
Under 15	.3	.4	.1	.9	1.3	.5	.9	1.4	.5
15-19	6.8	10.2	3.5	25.4	39.5	12.6	27.3	41.4	13.9
20-24	8.9	15.6	3.3	30.0	77.7	13.5	39.5	68.0	14.8
25-44	2.6	4.4	.1	9.7	18.8	2.9	11.0	19.9	3.5
45-64	.1	.1	.0	.5	1.0	.1	.6	1.2	.2

Note: The New York City Narcotics Register provided the number of addicts first reported in 1970 (incidence) and the total as of that year (prevalence) for each health district. These figures were then apportioned according to the age distributlons of incidence and prevalence for Brooklyn. Sex distributions within age groups for all addicts first reported In 1971 (the closest year available) provided the basis for apportionment with age groups for the three health districts. The result was a distribution for each district by age and sex of estimated incidence and prevalence. Corresponding distributions by age and sex were taken from the 1970 census. Rates of estimated incidence and prevalence were arrived at through the following formula:

$$\frac{\text{estimated incidence or prevalence}}{\text{Population baaed on 1970 census}} \times 1{,}000.$$

Source: Narcotics Register, 1972; New York City Narcotics Regisler, 1974; Population According to Age, updated

more "hard-core" drug users, since most of them were reported to the Narcotics Register by the police in 1970.

The significance of age- and sex-specific rates is more dramatically revealed in the prevalence figures for Bedford and Eort-Greene, where there are large black populations. Among males aged 20 to 24, the rate is 214.8 per thousand, or over 21% of the cohort. For males aged 15 to 19, the rate is 112.3 per thousand. Among females, rates are 35.6 for 15- to 19-year-olds, and 37.3 for those aged 20 to 24 years. Rates are higher for each of the other age groups than in Bay Ridge, but do not compare in magnitude to the 15-to-24 age group. Very similar figures are estimated for the Fort Greene area and for Bedford.

Even where we examine "incidence," which is reported as first use for the year 1970, a similar pattern emerges. Rates are four to five times as high for particular age-sex groups in the two ghetto communities as for Bay Ridge. In Bedford 4% of the 15- to 19-year-old males were identified by the register, as were almost 8% of the next age group. Similar proportions were found in Fort Greene and in Bedford.

A community survey was conducted in 1974 in the area roughly similar to Bedford and Fort Greene. Although the boundaries of the sample area did not extend to the outer census tracts of either health district the core areas are identical. Self-reports on a variety of indicators of involvement with heroin are shown in Table 2.

Both data sources – the household survey and the Narcotics Register – are believed to underestimate true prevalence. Our household-surveys sampling design curtails representation from addicts caught up in the criminal justice network and those "street-addicts" devoid of family ties. The evidence we present later suggests that these may be a particular subgroup and possibly not the largest one among addicts in ghetto neighborhoods.

In the 18- to 19-year-old group, no individuals reported heroin use. Among 20-to-24 year olds, however, the figures are similar to those noted in the Narcotics Register five years prior to the survey: 26% of the males report ever using heroin and 4% of the females (compared to about 2 1% and 4% respectively in the Narcotics Register). Reported use rapidly tapers off with increasing age.

The percentage who have casual acquaintances who have used heroin ranges from a low 24% among females, age 40 and over, to a high of 72% for males aged 20 to 24. Most males have acquaintances who are users, as do almost two of five females. When we ask for friends' use of heroin, the figures correspond very closely to those already noted for acquaintances among younger persons, but is somewhat lower for those over 30, especially for females. When the question is asked whether family members or relatives have ever used heroin – possibly a more difficult question to answer – we see that between 11% and 2 1% of younger individuals responded affirmatively; the percentage is slightly less for those over 40. It is significant that while we may expect females to be less involved in drugs, as noted in the previous columns, we see that the discrepancy between the sexes does not exist for reports on family members' involvement in heroin. Females even exceed males under 25 in reporting family-member use, suggesting that these figures may realistically bear on the prevailing situation.

Another way to observe the involvement of the community in heroin use is to see how many of the respondent's friends are reported to be addicted. This is, of course,

TABLE 2

Contact with Heroin Use in an Urban Ghetto Community, by Age and Sex (1975)

Age[a]	% Reporting heroin use in the past/present		% with et least one casual acquaintance with heroin use in the past		% with at least one friend with heroin use in the past/ present		% with a family member/ relative with heroin use in the past present	
	Male	Female	Male	Female	Male	Female	Male	Female
16-19	-		63	42	63	42	18	16
20-24	20	4	72	51	70	46	12	21
25-29	4	3	58	37	45	31	15	13
30-39	3	1	57	33	41	14	14	11
40+	1		29	24	16	9	7	8

	Mean number of reported addicted friends			
	Male		Female	
	N	Average number of friends addicted	N	Average number of friends addicted
18-19	11	1.5	19	1 .0
20-24	50	1 .9	57	0.7
25-29	53	1.4	67	0.8
30-39	101	0.9	124	0.3
40+	133	0.3	205	0.1

[a]The percentages are based on the number of individuals in the age/sex group. The base numbers are presented in the bottom panel

based on the respondents' judgments; however, in a community like Bedford-Stuyvesant, there is possibly more sophistication about drug use than in other neighborhoods. For males (lower panel, Table 2) the average number of addicted friends ranges from .3 for persons over 40 years, to 1.9 for persons 20 to 24. Females report fewer addicted friends. It is 1.0 for 18 to 19 year olds, and only .1 for persons over 40.

Each of these figures probably underestimates the dimensions of the problem. But from the Narcotics Register data we note that the kind of drug use that engages the police can involve over 21% of males ages 20 to 24, and is also high for the contiguous age groups, representing a substantial segment of the population. In predominately white communities, on the other hand, although addiction is not inconsequential, it certainly is within bounds that can be less destructive to the community. We also see, from the community data, that self-reports can identify significant segments of specific age groups that also roughly correspond to official statistics.

Nor is it only the identification of drug users themselves that we observe, for we are also able to direct attention to the fact that most people know addicts. Substantial segments of the populace report that persons they identify as friends use heroin; males, again in the 20 to 24 age group, know on the average, almost two addicts. Finally, substantial portions report that a family member or relative has used heroin. The highest percentage of reporting, 21%, occurs among female respondents between the ages of 20 to 24, but the percentages among most other age and sex groups are only slightly less.

We believe that since there are many communities presenting profiles similar to the one we have examined, a modulated response to the optimistic implications of our new epidemiological insights is indicated. First, rates and bases of heroin experience should be age or residence specific. Given the enormous variability of this experience across communities, national prevalence rates for heroin yield a distorted picture and may lead to a false optimism about the severity of the problem.

Even more important, when we see such large segments of particular populations involved either directly, or through family and friends, with heroin use, then the apparently libertarian approach to heroin use becomes less tenable. Where drug use is sporadic, and engages only a very few individuals, there is some plausibility to the view that people should be permitted to destroy themselves, if that is their wont. From the data we compiled, (and they could be developed for many other communities), we suggest that there is a dialectical process that is operative, even if we only perceive this process heuristically. When more than one-third of Vietnam veterans use narcotics, at least during the period investigated by Robins, or as many as one in four of males aged 20 to 24 in a particular neighborhood, we have a quantitative change that implies a qualitative change, one that no longer can respond affirmatively to the perspective that persons should be free to destroy themselves. Even if only a subset fully engages in compulsive narcotics use, and if of these, many eventually shed their dependence on heroin, the dimensions of the problem force a reassessment, one that focuses not on individuals alone – although they cannot be ignored – but on the impact this has on the communities where heroin use is endemic.

A large proportion of heroin users generally become involved at relatively youthful ages – late teens and early 20s, years during which the rest of their age-mates are finishing school, exploring the world of work, entering the military, getting married – all the activities which are essential for the assumption of adult roles. This truncation

of significant socialization processes can only occasionally be recapitulated at a later age, as the testimony from many studies of outcomes in drug treatment programs amply confirms (Vaillant, 1966, 1968; Lukoff, 1974; Nash, Foster, & Lynn, 1975; DeFleur et al., 1969). Even within the confined context of treatment programs, those who do not assume the normal roles associated with their age group are also the poorest prospects, whether we examine drug-free or methadone maintenance programs. Similar observations can also be made about those who tend to remain drug-free after incarceration or after discharge from Lexington. Even among veterans, those who were earlier involved in drugs and deviance, who didn't finish school, and who were unmarried, present the poorest prognosis for remaining drug-free (Robins, 1975).

Significant segments of the population are literally removed from the creative work of the community. And the communities involved are those that are in the weakest positions with respect to carrying out successful efforts on their own behalf. There is also evidence that suggests that the pool of addicts is disproportionately drawn from the more talented segments of the community, exacerbating the problem (Ball & Chambers, 1970; Lukoff & Brook, 1974; Platt, Hoffman, & Ebert, in press). As the data on the personal involvement with heroin users of large segments of the non-using population would appear to reveal, energies that could be spent on community development are, instead, diverted into social control mechanisms, drug treatment programs, and dealing with the consequences of large congeries of addicted persons, crime, and the attendant social disorganization. What it means for youngsters growing up in these neighborhoods to see on almost every comer of their main thoroughfares the major indigenous industry, drug treatment programs, must also be assessed. Drug use diffused over a large population may be trivial; however, in the context of particular communities the problems become exacerbated in almost any way one chooses to examine the issue.

The Sequel to Heroin Use: Life Cycles, Stages, and Role Typologies

The brief summary of the various patterns of heroin and other narcotic consumption abstracted from the epidemiological literature suggests the complexity involved in attempts to understand stages of drug use. One conclusion from the epidemiological findings just reviewed is that narcotic use does not present a unitary pattern; instead, many diverse and complex patterns are apparent. Yet, we restricted ourselves to the relationship of users to the frequency of narcotic use. We ignored the various sequences through time, the attendant life-styles, and the institutional and organizational dimensions of narcotic use.

A proliferation of schemes has been advanced that attempts to identify various stages of involvement with narcotics, mainly with heroin. There are also typological schemes and social role concepts that attempt to summarize complex patterns that identify significant dimensions of behavior. A legitimate scientific activity is to search behind the complex and shifting "realities" for an underlying structure to serve as a foundation for understanding and control. Despite their long history in drug research, none of these schemes has fulfilled its promissory note.

They have, in failing to account for the full range of use patterns and their correlates, failed to explain the complexities and paradoxes apparent in the drug experience. We believe it is a mistake in the study of addiction to organize such explanations primarily around particular drugs of use or their consumption patterns. While such an

organizing principle can contribute to understanding, the complex phenomenon of narcotic use is not limited to substance properties nor to users' idiosyncratic relationships to those substances. A meaningful schema will deal with the significant aspects of the matrix in which the interaction of substance and user is embedded.

Chambers (1974) develops a behavioral progression typology that identifies a sequence from nonuse to experimentation, to social/recreational use, to committed use, and, finally, to dysfunctional use. He associates these different stages with the quality of their behavior, which progresses parallel to the extent of use from normal, at the time of onset, to social and personal pathology when drug use becomes compulsive. Although the presentation is highly schematic, it is precisely here that one of the basic problems emerges. One can probably classify people into various drug-using categories with reasonable reliability at any particular time. But any simple sequence from "normality" to "pathology" associated with these classifications, if it does exist, is not likely to be very substantial.

Almost all the information on drug users suggests that such a classification of behavior is not likely to account for very much variance. Because such a system focuses on the state of drug use, all other behavior becomes derivative. Even if there is a relationship between stages of drug use and other variables, it soon becomes apparent that these schemes cannot account for the very diverse populations that fall into these drug-taking stages. If there is one simple fact that can be safety summarized from the research literature without fear of criticism, it is that even as behavior is transformed by increasing involvement in narcotics, there is a persistence of other behaviors that preceded any drug use. Such antecedent factors coupled with immediate situational ones provide our best understanding of behavioral correlates of drug use. Since recent epidemiological studies have established that people move in and out of various stages of drug use in all directions, we now know it is unidirectional for only a small subset of narcotics users, and it is not simply a matter of individuals' drug use becoming arrested at particular stages in a progression. Recent epidemiological fmdings establish that people circulate among chipping, addiction, and drug-free status.

The focus on the particular stages of addiction, therefore, is capricious and arbitrary. Such systems have been based on observation of confirmed addicts, those in the treatment system or in prison, who could report on various developments in their own drug histories. Alksne, who authored a framework for studying addiction based on life styles, notes that there are alternatives to the major cycle he identifies (Alksne, Lieberman, & Brill, 1967). However, these alternatives are clearly afterthoughts, not integrated into the series of stages postulated as culminating in addiction, and for selected persons, abstinence at the end of the road.

The process is further complicated by the fact that the population of narcotics users is itself dynamic. If one recalls the two types of opiate users identified by Ball, one soon realizes that the era of the 1950s and 1960s was possibly a simpler one than the current drug scene (Ball & Chambers, 1970). O'Donnell noted that the population entering Lexington was increasingly composed of patients with more extensive criminal involvement, a trend that still exists in treatment populations (O'Donnell, 1966). For example, as shown in Figure 1, when patients in one methadone program were grouped by the time period during which they became addicted, it was found that with each successive time period an increased proportion had criminal records prior to the onset of drug use. These findings, although from only one program, could undoubtedly be replicated in many other programs.

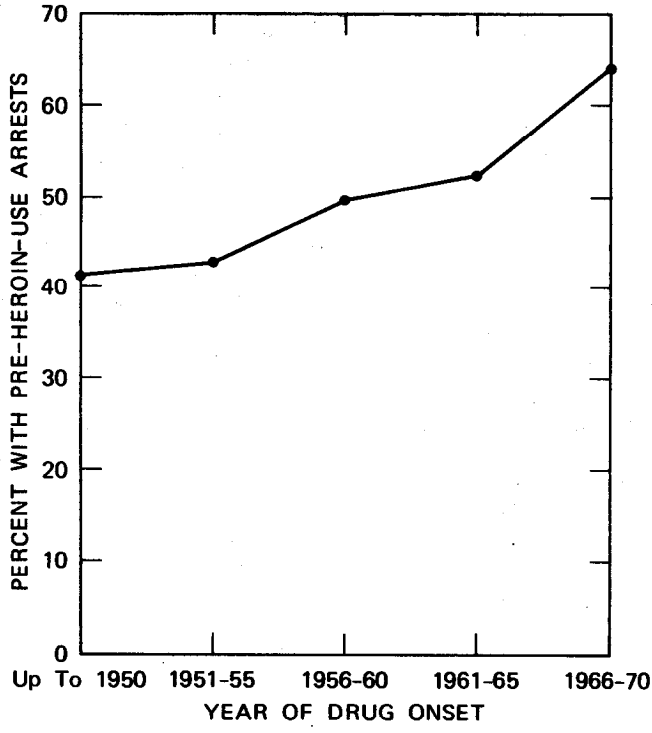

FIGURE 1. Proportion of patients entering a methadone maintenance program with arrests prior to drug use onset. Based on data from first 1198 patients admitted to the Addiction Research and Treatment Corporation in Brooklyn, New York (1969-1971).

Community studies in Bedford Stuyvesant/Fort Greene, New York conducted three years apart suggest a narrowing of reported drug use between whites and blacks (Kleinman & Lukoff, 1975a). The selective service survey of 20- to 30-year-old males by O'Donnell et al. (1976) also notes that successive cohorts within the sample suggest an increased involvement in drugs among whites. Although some caution must be attached to black-white comparisons where frequencies are small, the previously large gaps between the races seem to be diminishing. And we know that the course of addiction and the attendant social disorganization are markedly influenced by the kinds of individuals who move into the drug scene. Even in the sample derived from a police register, such as the one investigated by Nurco, there are white-black differences in the course of addiction, in survival, continuity of drug use, earnings, and criminal activity (Nurco, 1975).

A method that is pervasive in sociological and anthropological research is the identification of various roles that group individuals into different patterns. In drug research, such role systems proliferate, many of them very expressive and, at first glance, analytically powerful. The most successful have been those that focus on particular types of addicts, such as the Cool Cat, or that identify broad classes

reflecting transition stages in the addict population; e.g., the New Junkie, Transition Junkie, and Old Style Junkie (Smith & Gay, 1972). Blumer differentiated the "Rowdy" from the "Cool" groups of drug users, based on congeries of associated behaviors and types of drugs of choice (Blumer, 1967).

Such systems provide some insights into the behavior of particular groups of addicts; no one system is equally powerful for explaining the present day "drug scene." Each is bound by its particularity. All are derived from qualitative observational and participant observation studies that eschew systematic methods. We suggest that the seeming "wholeness" is an artifact of a method that has no need to contend with information that might complicate such schemes and therefore disabuse anyone of their essential correctness. These schemes persist even in the face of fundamental changes in the external reality.

As a prelude to the evolution of a more systematic typological scheme, Nurco & Lerner (1971) proposed a more complex model. The organizing principle was the attempt to define addict subgroups by their relationship to the conventional world. For example, a distinction, based on the user's relationship to the drug marketing system is made among the Street Addict, Dealer Addict, and Shooting Gallery Addict. However, the Female Addict is a separate nonoverlapping category, the assumption apparently being that females play no role in the functioning categories. Other classifying criteria are used: geography (e.g., Suburban Addict), conventionality (e.g., Employed Addict), and finally, treatment status represented by the addict in treatment (who is at least temporarily playing a minimal role in the specialized addict world). Two characteristics of this scheme should be noted: (a) the diverse attributes are not uniformly applied across the scheme, and (b) homogeneity of behaviors within groups is assumed to be sufficient to constitute a typology. This assumption may not be tenable; one has only to examine the addict in treatment to observe an extensive range on almost any attribute of interest.

One of the few, if not the only empirically derived typology in the drug research literature (Meyer, Brotman, & Freedman, 1965) was used in an effort to evaluate a drug treatment program. Using two axes, in the style associated with Robert K. Merton, one reflecting criminality, the other conventionality, four types were derived: Conformist, Hustler, Two-Worlder, and Uninvolved. The information was assembled on an a priori basis, with the result that, in order to avoid empty cells, it was necessary to severely alter cutting points. Here the problem of such typologies becomes immediately apparent. The "purity" of the conception had to be sullied because the patients in the particular program did not fission out neatly into the several clusters. Despite these difficulties, the findings make sense, and provide some interpretive leverage. There are inherent problems in such a priori schemes that must be resolved in order to develop viable classification systems that can be used to study the many themes that might account for the variegated behavior of drug users, or for any impact these various types might have on specific events; e.g., treatment success and altered involvement in drugs.

Several conditions must be met by a successful scheme. First, there must be some evidence that there is a common latent element that identifies the various pieces of information. Just as in scaling, if heterogeneous information is arbitrarily assembled, then various associations with other variables may not be a function of any common element running through the classification scheme, and any meaning attributed to various patterns becomes arbitrary.

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How can the arbitrary basis, characteristic of so many typologies, be avoided? Among the many possible procedures that could be adopted, there is one that could serve two purposes. First, as a typological scheme it selects out various configurations that are independent of each other, and second, it permits the examination of a fairly extensive body of information that can be related to each of these configurations. Canonical analysis can be viewed as a multiple regression technique that, instead of identifying a single dependent variable, permits the assembling of a set of variables as the criterion. The model in effect performs a factor analysis on the criterion set and isolates out various homogeneous sets within the criterion variables. These can then be related separately to the various predictors. Unlike factor analysis, however, it permits the investigator to designate which variables shall serve as criteria, and which variables shall be used to explain the patterns that might emerge in a single operation.

The illustration provided here is drawn from treatment data where outcomes are generally presented on discrete variables. (Is the client retained; did he or she get arrested; is he or she working?) There is the notion that clients can be ordered on some rehabilitation continuum. Insofar as there are very independent clusters, as various role typologies assume, such clusters would be identified in the model that is built upon independent canonical variables.

The success of any model is, of course, dependent on the cogency with which various kinds of information are assembled. In the case of treatment data, there is a generally prescribed set of outcomes, as identified in the row headings in Table 3. These include Retention, Employment, Morphine Positives, Missed Medication and Crime index (based on arrest data). The matrix is interesting for what it says about treatment, or at least this particular program. However, given the low level of commonality in the matrix – a median correlation coefficient of about .19 emerges – very powerful clusters will not develop. Yet, it is possible that variation in outcome configurations related to patient characteristics will begin to provide an approach to understanding. That is, it is possible that with the use of canonical analysis each of these factors that emerge is contingent on some particular aspects of the person's history.

TABLE 3

Correlations among Criterion Variables

	Employment	Morphine positives	Missed medication	1-year-after crime index
Retention	.35	-.18	-.32	-.20
Employment	-	-.21	-.16	-.14
Morphine positives		-	.25	.14
Missed medication			-	.17
1-year-after crime index				-

Note: N=1,198

The results of the canonical analysis are summarized in Table 4. In the upper panel, four significant clusters are identified, each of declining predictability. In the bottom half of the table the most important predictors associated with each of these clusters are presented. The labels are descriptive of the particular patterns revealed in the higher coefficients that cluster together in each of the factors.

TABLE 4
Patterns of Outcomes at End of One Year in Treatment and Predictors of Outcome Patterns

	Canonical Variables			
	1 Marginal retainees	2 Employables	3 Irregulars	4 Heroin-free dropouts
Retention in treatment	.55	.23	.66	-.48
Employment	-.66	.63	-.04	.18
Morphine positives	.12	.33	-.27	-.77
Missed medication	.02	-.04	.96	.24
1-year-after overall charge rate	-.73	-.25	.24	-.50
R Can	.36	.31	.20	.19
Significance	.001	,001	.001	.01

Predictors of canonical variables			
1		2	
Sex	.55	Sex	-62
Onset-entry overall charge rate	-.36	Marital status	.36
Race-ethnicity	-.24	Onset-entry overall charge rate	-.34
Highest grade completed	-.23	Parents at 12	.31
Age at first arrest	.21	Age at admission	.28
Age at admission	.18	Longest period held one job	.27
Age at first daily use	.18	Polydrug use	.18
Longest period held one job	-.16	Race-ethnicity	-.14
3		4	
Race-ethnicity	-.80	Age at admission	-.88
Preaddiction arrests	-.36	Age at first daily use	.56
Age at admission	-.34	Preaddiction arrests	.37
Highest grade completed	-.33	Age at first arrest	.24
Number of moves	.14	Onset-entry overall charge rate	-.24
Polydrug use	-.13	Marital status	-.23
Onset-entry overall charge rate	.12	Longest period held one job	.21
Age at first daily use	.09	Sex	-.19

Note: N = 1.198

Marginal Retainees

The criterion measures that are heavily loaded in this configuration include a lesser propensity to both arrests and gainful employment, and the tendency to remain in treatment. This group tends to be undistinguished from the balance of the patients on missed medication or on morphine positives. While they are less crime-prone, nevertheless on other outcome measures they either do poorly (employment), or are undifferentiated from the overall patient patterns of (a) continued illicit drug use or (b) regularity in picking up medications. Yet, they remain in treatment, as the high loading on retention indicates

As can be seen in the predictor battery for Canonical Variable 1, this cluster is over-represented by female patients. The preprogram crime rates of this group were

somewhat lower than for other patients. They were more likely to be black with low education, and to have been arrested for the first tune at a later age than other patients. This group of patients drifted into addiction and crime at a later age but never acquired the resources – such as education or work experience – to become more than marginal retainees. These were patients who eschewed crime, but were unable to distinguish themselves on other outcome measures.

Employables

This group is very high on employment but the other factors are only weakly loaded; they are only minimally more likely to be retained, a little higher on missed medication, and slightly less likely to be arrested. The overwhelming pattern, however, is the substantial employment loading and the relatively weak loading on other outcomes, suggesting that employment is not heavily dependent on other program parameters. This conforms to the findings in the correlation matrix (Table 3).

These patients are characterized by being male, married, of stable social backgrounds, somewhat less involved in crime prior to treatment, a bit older than other patients, and having had more substantial preprogram employment histories. There is also a weak trend for this group to be polydrug users and slightly overrepresented by black patients.

Irregulars

The heaviest loading for this cluster is the Missed Medication variable. Yet, this group also has the highest loading of any of the clusters in retention and, surprisingly, is somewhat less likely to have urines with indications of morphine. They are also slightly more likely to be arrested while in treatment. They are the only group high on missed-medications, yet they tend to remain in treatment. This confirms the observation that an essential feature of methadone therapy may be consistently violated; however, this may be largely attributed to one of two possible causes; (a) this group may avoid the requirement that they submit urine samples, or (b) that the urine testing program is inadequate.

This group of patients is likely to be black, to have had predrug-onset arrests, to have started drug use at a younger age, and to be less educated.

Heroin-Free Dropouts

These patients are least likely to show evidence of continued drug use; yet, they are the only group that also has a reasonably heavy negative loading on retention, so that they are prone to drop out of treatment. They are less likely to have been arrested during the first year. (Even for those who dropped out within the first year, information on arrests was obtained.)

Although this group is young it is also characterized by having started drug use at a later age. That is, patients were, on the whole, only short-term drug users. While more of them were arrested prior to the onset of drug use, they had fewer arrests between onset and entrance into treatment. They are also more often single, male, and have some history of employment.

Thus, this analysis identified four clusters of patients who accommodated differentially to the several outcome parameters. The modest pattern of association among program

outcomes observed in Table 4 is at least partly explicable by this finding. The patient subgroups are distinguishable not only on the standard demographic variables, but also on different aspects of their social histories, including education, criminal histories, age of onset of drug use, and also on other indicators of socialization, such as residing with parents in early adolescence.

Future analysis might direct attention to typological procedures that identify different addict subgroups. A larger set of outcome variables and perhaps more informative predictor variables might provide additional insights. Within its limitations this very preliminary foray into an uncharted form of analysis demonstrates the inadequacies of conventional typologies as explanatory and predictive devices. This study shows that addicts, even when they come from the same community, cannot be assumed to bring with them the same sets of experiences or to have the proclivity to achieve various goals in lockstep. Indeed, the results suggest that what patients bring with them may be the most important factor in determining achievement of the array of treatment goals – from abandonment of heroin to successful performance in the conventional world. It is possible that a larger set of outcome variables and more informative predictor variables would provide additional insights. The capabilities of this analytic tool should permit reducing the information to a few meaningful patterns, if indeed there is a finite set of such adaptive patterns.

Other methods, such as multiple discriminant analysis, might also be used to identify particular subgroups of addicts. For example, multiple discriminant analysis has the capability of assigning individuals to each pattern. Following this, a variety of other data analytical techniques, not necessarily limited to the use of a battery of predictors, may be applied. In any event, it is clear that whether canonical or multiple discriminant analysis is used, these sophisticated techniques facilitate cashing in on the promissory note issued by typological schemes. These procedures have a goodness of fit to the data of addiction that enable them to summarize actual findings and establish empirical subgroups from pools with wide variance. The results may never be so clean and neat as those derived from typologies based on only a few observations, or those in which individuals or groups are forced into categories established on intuitive logic in advance of empirical evidence. While simple order is certainly an appropriate goal for scientific endeavors, it ought not to be achieved at the expense of a reasonably accurate reflection of the world of experience.

The Institutional Matrix: Methadone Treatment, Science, and Research

An appraisal of the consequences of heroin use, we suggested earlier, cannot be limited to the examination of the actors in the heroin scene. We reviewed several of the efforts to identify types of adaptations of heroin addicts; however, the conclusion we drew was that this was only a prolegomenon to what has to be done in the future. Our examination of a high risk community suggested that where rates of narcotic involvement are high, the problem can no longer be viewed as confined to a collection of individuals who happen to choose a particular mode of adaptation. Instead, it becomes an issue that reverberates throughout the community and influences the community's ability to solve its problems of survival.

There is still another aspect to the consequence of heroin use, the organizational and institutional one, with its concomitant establishment of a complex public and private system to deal with narcotics. Expenditures for supply reduction alone represent over 350 million dollars annually. There are 276,000 treatment slots provided by

federal and local governments and under private auspices *(White Paper,* 1975). A major industry has been spawned to cope with a problem that may involve between one-quarter to one-half percent of the population.[1] To interpret the kinds of sums that are involved, since the overall figures defy comprehension, we limit ourselves to the amount spent on research at the federal level between 197 1 and 1975, almost a quarter of a billion dollars. This does not include large sums of money for research that derive from local government sources or from foundations.

A review of the system that has developed in response to heroin use is beyond the resources at our disposal. To get some handle on at least one important part of this diverse and amorphous system, a review will be made of the relationship between our cumulative information on research and its import for treatment, particularly methadone maintenance. Methadone maintenance is one of the major treatment thrusts that currently engages perhaps 90,000 patients nationwide.

We select methadone because we feel it is an appropriate paradigm for examining the complex relationship between science and large investments in research and the impact this may have on developments in the treatment system. Research on other treatment modalities, particularly the various drug-free efforts, is, on the whole, much less adequate and not nearly so abundant.

We are not here especially concerned with the question of whether methadone works or whether we can attribute successful rehabilitation to the modality, or whether crime is markedly attenuated as a consequence of the expansion of methadone therapy.' Rather, we advance a different set of questions, focused on how knowledge cumulates and is absorbed by those who direct our programs. We also will address ourselves to the research itself and the way it has been used, or, in some cases, ignored.

We might cynically conclude that the failure to absorb contrary findings in an orderly and rational way is simply a consequence of organizational requirements for survival. Where research that is not congenial to the managers of our programs is spurned, it may be rejected only because changes would have to be made in program functioning, in staffing patterns, and – for perhaps the most compelling reason – in the reduction or even elimination of funding. We have no doubt that such processes are at work but they are not the only ones.

We suggest that the processes we observe are at least partly explicable in terms of the very particular context in which methadone therapy developed and the institutional and professional antagonisms that surrounded the inception of the method. The heroin epidemic 'was beginning to flower, accompanied by an increasing crime wave that received abundant attention in the press and was becoming increasingly visible to the general public in many neighborhoods in New York City. At both the city and state levels there were study groups, commissions, and a constant stream of pronouncements. Most of the professional community, however, would have very little to do with addicts or addiction. Social agencies, largely unskilled in the treatment of addiction, failed to respond to the increasing dimensions of the problem. The initial New York City effort, aside from police activity, was to establish a Narcotics Register to provide some monitoring of the dimensions of the problem, and to appoint, as a commissioner of narcotics, a psychiatrist committed to psychiatric modalities for the treatment of addiction.

None of the sociological or psychological frameworks provided any remedies that could be translated into successful treatment regimens. One could talk about solving

the problems of poverty, or about resocialization of disordered characters; neither the will was present to deal with the former approach – if indeed it would have worked – nor were most addicts likely to accommodate to efforts at resocialization through individual therapy or by various group-oriented methods (therapeutic communities).

Many of the early supporters of methadone were in various ways associated with the ill-fated Riverside Hospital program described earlier, perhaps the most substantial recorded failure in the history of drug treatment: at the end of a one-year follow-up they could record only one drug-free individual (Alksne et al., 1959)! Dr. Nyswander, a psychiatrist, was one of the few professionals attempting to deal with drug addicts, and her experience was one of failure. It was at this time that she became associated with Dr. Vincent Dole, who though a physician, was primarily engaged in research at the Rockefeller Institute.

Kuhn (1970), in his classic work, *The Structure of Scientific Revolutions,* reviews the hesitant progress of science whenever particular scientific paradigms encounter problems that are resistant to solutions where the model has previously had brilliant success. But even when a model encounters failure, those who may advance new conceptions or evolve radical solutions are not always embraced. In fact, it may take decades or even centuries – as in the case of Copernicus – before their formulations are finally legitimized by the scientific community. We may be stretching the analogy between science and theories of drug use, but the dismal failures encountered up to the early 1960s may also be seen as the failure of the theoretical underpinnings that existed up to that time; they were very vague, and could only by excessive charity be called scientific schema ensconced in a well-formulated paradigm. Certainly, they lacked the essential feature of a successful paradigm in that the puzzles they were supposed to solve, the rehabilitation of addicts, remained insoluble.

It is in such a context that a psychiatrist could begin to entertain a very contrary set of assumptions about addiction, such as the one we associate with Drs. Nyswander and Dole. Their solution was to maintain addicts on drugs, and they developed a rationale to justify their reformulation. Then, as now, there were those who retained enthusiasm for alternate methods, and who accepted only drug-free status as the legitimate objective of treatment. Yet, in many cases, those who were most hostile to methadone have made their peace with it. As with scientific paradigms, it became necessary to accommodate finally to the presentation of evidence that methadone seemed to work as no other therapeutic intervention system had before.

In addition, the problem of drug abuse appeared to be getting out of hand, and the clamor for solutions was great. And, as the first results on methadone treatment (both the early clinical trials and the early evaluations prepared by Dr. Francis Gearing) appeared to strongly document, a solution was at hand, one that appeared to transform the addict population into working adults who abandoned crime and no longer abused illicit drugs. A solution had been found to a difficult problem where no solution existed before.

To establish the scientific credibility undergirding methadone maintenance, we briefly review (a) the scientific status of the medical evidence that led to the resort to methadone (more specifically, the evaluation of morphine); (b) the theoretical underpinnings of methadone maintenance (including the metabolic disorder theory and the blockade concept); and (c) the critical research directed at methadone maintenance.

These issues are related to the current responses of the major actors in the methadone treatment scene to the critical issues that have been raised in a number of quarters.

The Evaluation of Morphine

Before Dole and Nyswander began their experiments with methadone they had maintained a few addicts on morphine. They found their patients became sluggish and passive and either slept or watched television until they received their next dose. It also seemed that when they developed a tolerance for a particular dosage level they began to demand increased quantities of drugs, so that stabilization was not possible. From this experience they concluded that morphine would not be an effective substitute for heroin, the drug of choice of their subjects.

Neither Drs. Dole nor Nyswander had been pharmacologists, and it would appear that their experience was a trial and error affair. Many people have been, and still are, maintained on morphine at stable dosage levels. Waldorf and his colleagues, in a review of the Shreveport clinic, report that for most patients a "balance" was achieved at 7.5 grains, with some individual variations. Further, the patients worked, and lethargy or inactivity was not a particular problem (Waldorf et al., 1974). The LeDain report (Final Report, 1973) notes that morphine "does not necessarily produce marked sedation, gross intoxication or major impairment of motor coordination, intellectual functions, emotional control or judgment" (p. 308). Duster (1970) confirms this view as well; patients maintained on morphine "are able to lead an otherwise normal life with little change in work habits or ability to meet responsibilities" (p. 113).

There may be very good reasons for using methadone in a maintenance program, particularly because of its relatively long acting quality. What the above citations suggest, however, is that the collective experience of the pioneers was not particularly substantial in at least this branch of clinical pharmacology. Their peculiar results may have been an unfortunate consequence of their inexperience or the particular subjects they investigated. Or, more likely, these addicts were hospitalized and were not expected to work or carry on any normal routines, and their behavior could be explained on these grounds. Thus, it seems that methadone may have been used initially because of the inexperience with narcotics of the principle investigators. In some early experiments at Lexington with methadone the same observations about the reactions of patients were made as those Dole and Nyswander reported on morphine maintained patients!

Metabolic Disorder Theory

One view about drugs that still has cogency is that it might be expedient just to provide addicts with heroin and therefore minimize, if not altogether alter, many of the undesirable social and health consequences associated with drug abuse. Although this view may have been part of the motivation, it is not part of the formal justification for methadone. Instead, Dole and Nyswander have advanced a theory about drug use that maintains that there are no psychogenic origins of drug use, and instead, they insist that addict behavior "is a consequence and not a cause of addiction" (Dole & Nyswander, 1967)

They advanced the explanation that repeated heroin use resulted in a metabolic disorder that was sated only by narcotics or a substance like methadone that behaved

in a similar manner in the system. However, the precise mechanisms were not suggested, and metabolic disorders can involve a very large number of systems in the body. As an explanation it is in a league with those that say "culture" or the "social system" is a cause of something, which is tantamount to specifying almost everything as the cause of something – a trivial formulation.

It is surprising, then, that it took so long for the medical community to challenge this explanation. In 1972, Dr. Avram Goldstein, who also supervised a methadone program, noted that there were abstinent former addicts who could function without drugs, a fact that is incompatible with the premises of the metabolic theory. He further noted that if addiction were a metabolic disease then there would not be any relapses on the part of patients receiving methadone; yet there are episodic relapses, and alternate explanations were offered (Goldstein, 1972). Vaillant also comments on the metabolic theory, that it". . . is almost certainly [an] erroneous hypothesis that heroin addiction, like diabetes, reflects an underlying metabolic abnormality" (Vaillant, 1974).

Yet, attractive explanations manage to survive years after they have lost their credibility. Kuhn notes that when a particular scientific system has been challenged there is a great deal of effort to modify older theories in an effort to maintain the essential structure of the original paradigm. Consequently, a new analogy is now coming into vogue that bypasses the criticisms directed at the reference to metabolic disorders. Addiction is now conceived as a *chronic* disorder which need not invoke the metabolic system (Gearing, 1974; Behari, 1976). At one stroke one kind of criticism is obviated, while the essential structure of the old argument is retained. The abundant evidence that for many thousands of persons addiction is self-limiting is simply ignored. It does not enter into the experiential world of the founders or many supporters of methadone treatment.

Dosage Levels/Blockade/Tolerance

In the early reports, Dole and Nyswander evolved the concept of blockade (Dole & Nyswander, 1965; 1967). They insisted that once a person was given sufficient methadone, a quantity which may vary slightly depending on the individual, he or she could no longer respond to heroin: it would simply not perform its function, and eventually the craving for heroin would be extinguished. As Goldstein noted in the paper cited earlier, Dole confused tolerance with blockade. Only narcotic antagonists can block opiates by shutting off the sites in the nervous system where opiates (or methadone) might have their effect. But there has been no retreat on this issue, and the concept still finds favor among methadone supporters.

This is not merely a quibble, because there are several very important consequences that emerge from the assumption of a blockade effect. First, if the assumption concerning blockade is correct, then it serves as a rationale for high methadone doses. Yet, if there is one cumulative body of evidence that is reasonably clear, it is that high dosage does not result in better outcomes than lower levels of methadone. Goldstein reported in 197 1 and again in 1973 (Goldstein & Judson, 1973), that no important differences in program outcomes could be attributed to dosage differences. Also, as early as 1969, Jaffee, Zaks, and Washington, and then in 1970 and 1971, Jaffee alone reported findings that were consistent with the Goldstein findings; and *again,* in 1973, Schut, Wohhnuth, and File confirmed essentially the same trends.

Each of these experiments contained technical flaws, but there is a cumulative impact from these and other studies not cited here.

Summary data from an experiment on dosage levels carried out jointly by Columbia University and a Yale University Medical Evaluation team support the findings cited above (Lukoff & Kleinman, 1976). Patients were randomly assigned to high (100-mg) and low (50-mg) dosage groups. The experiment was double-blind so that neither staff nor patients knew who was receiving either dosage level. A small number of patients who had side effects (among high-dose patients), or whose codes were broken because the person had to be detoxified, or maintained while in jail, were removed from the experiment.

Two problems should be mentioned: (a) One-third of the patients had left treatment at the end of one year and half of them were gone by the end of 24 months; however, the dropout rates were almost identical for the two dosage groups! (b) Data derived from urine tapes contained some missing or otherwise unretrievable data on regularity of methadone pickups and on urine reports, although the amount of missing data was similar for both groups.

There were no significant differences in crime, retention, employment, missed methadone, or morphine positives. Patients were followed for up to 2 years on medication and morphine positives, and for 4 years on retention. Although patients were randomly assigned, there could still be some interactions between personal qualities of patients and dosage. We therefore performed a regression on retention and on arrests, using a large number of predictors of outcome, with dosage introduced as a dummy variable. The impact of dosage is negligible: the standardized beta weights are .02 for retention and -.05 for criminal activity, clearly insignificant. Along with the results cited earlier and the information we present here, it would seem that dosage does not play the role the methadone model assumes. Different dosage levels neither improve nor detract from the probability of continued drug abuse nor influence any other parameter used to assess the efficacy of treatment.

These cumulative results can have only one implication; namely, that the presumed advantage of high dosage has yet to be demonstrated. Other investigators have observed that high-dose patients abuse a variety of drugs (Stephens & Weppner, 1973; Chambers & Taylor, 1970; Goldstein, 197 1; Hayim, Lukoff, & Quatrone, 1973). There is also a rather odd fact in the debate. Although the programs affiliated with Dole and Nyswander have collected voluminous amounts of urine, and, although they and their colleagues have been among the more prolific publishers of research reports, they have made only very general statements about morphine positives. There is no detailed documentation on drug abuse presented in a decade-long series of reports.

The theory concerning the blockade effect and the consequences attributable to high dose has never been confirmed. But the avoidance of the negative findings we have cited has very direct implications for the population undergoing treatment in high-dose clinics. The statement is constantly reiterated that methadone is relatively benign, that most patients are able to accommodate to it after a short period of discomfort, and that it has no substantial side effects. However, Nash, using a set of questions developed by Goldstein, states that substantial, if declining, proportions report a whole range of symptoms even after they have been maintained on methadone for some period of time (Nash et al., 1975).

Even more important, however, is the relationship of dosage to two particular issues. Many addicts wish to be detoxified and hope to become drug-free. An issue, which we can only raise, and for which we have no answer, is whether high-dosage maintenance, which appears to have no particular advantage, may make permanent detoxification even more difficult, if not impossible, for many patients. This can be especially pernicious when we note that most users of heroin, even those who were addicted, will eventually modulate or even cease drug use. After maintenance on a high dosage of methadone, the cessation of drug dependence may be even more difficult.

A recent NIDA publication reviewed the literature on methadone and pregnancy. We site from the summary:

> It has been firmly established that infants born to methadone-maintained mothers display withdrawal signs at birth. Early investigation revealed neonatal heroin addiction to be more life-threatening than neonatal methadone addiction; but the most recent research results seem to contradict this previous conclusion. Methadone-addicted babies apparently exhibit more severe withdrawal symptoms with a significantly longer duration than heroin-addicted babies. (National Clearinghouse, 1974).

A recent *New York Times* article reported these findings, and several clinics revealed they would reduce dosage levels to pregnant patients. Dr. Dole, in an interview, objected on the grounds that low dosage would only encourage the mothers to seek heroin, which would be worse. Although the credibility of the blockade concept is no longer tenable, the vitality of the original conception appears undiminished.

An accumulation of evidence appears to refute the underlying assumptions made by the methadone pioneers. Yet, many who advocate the use of methadone persist in views where the evidence would appear to call for some revision in their formulations. Instead, we see the model restated, with only slight and insignificant changes.

Evaluative Research

The apparent success of methadone maintenance was dramatically confirmed in a series of reports prepared by Dr. Francis Gearing at the Columbia University School of Public Health (Gearing, 1970, 1974). All previous studies of treated addicts, as noted earlier, were documents of accumulated failures. From the first reports on methadone and a succession of studies over a decade, the evaluations of the pioneer program appeared to demonstrate an ability to retain most addicts in a community-based treatment program, and to promote improved employment, a marked decline in crime, and even improved prospects for longevity. Other programs tended to confirm the gist of these findings, although some evaluations reported less dramatic results. Nevertheless, the documentation prepared by Dr. Gearing provided the credence for the program that, through time, helped to make methadone maintenance acceptable and to vanquish, or at least modulate, those who had been opposed to the concept of drug maintenance. A long-demanded model for innovative programs was followed, wherein a pilot treatment program was evaluated before it was expanded to include other prospective patients.

One discouraging finding about the research activities that prevail in program evaluation in the field of drug abuse is that so much of it cannot withstand scrutiny. A simple example: We (Kleinman & Lukoff, 1975b) assembled all the findings we could on retention rates in methadone maintenance programs, surely a reasonably simple datum. Yet, on close examination we had to conclude that we could rely completely

on only one report, and that others had such obvious flaws or inconsistencies that the figures had to be viewed with suspicion. There are inherent problems in any long-term field trial, some of them not susceptible to easy solution, and we do not propose to ask for conformity to textbook standards that in reality cannot be accomplished. However, even where less demanding criteria are employed there still remains the question of whether the early results can be viewed with credibility.

This problem, it should be immediately noted, is not limited to drug research but applies to the whole field of evaluation research. In an assessment of 152 comprehensive evaluation projects, it was observed that only 10% met minimum scientific standards (Bernstein, Rieker, & Freeman, 1973). Another assessment of a sample of 179 projects selected from 532 studies indicated that only 6.7% were able to achieve their stated objectives; another 34% held some promise in light of contingencies not altogether under the control of the investigators (Minnesota Systems Research Inc., 1973). There is even a published experiment where some individuals who happened on the scene were able to demonstrate that the researchers had never determined whether the stimuli, various educational strategies, had ever taken place! They had not (Charters & Jones, 1973).

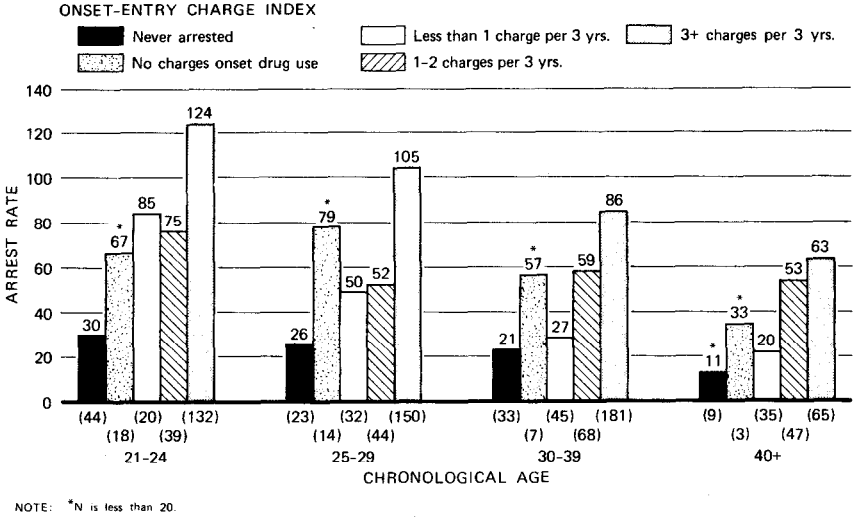
We noted earlier that despite the hesitancy many of us may have about promoting the implications of our research for policy alternatives, that research does sometimes have such impact. I assume the various criticisms directed at Gearing's series of reports are widely known to researchers. I would maintain, however, that the reports still serve as models of programmatic impact, that they reinforce those who are committed to methadone maintenance. They are therefore very important in the field of drug abuse.

Very trenchant criticisms have been directed at Dr. Gearing from the beginning, although they seem to have received very little attention. Perkins and Block (1970) demonstrated the impact of program selectivity on the early program results. Babst, Chambers, and Warner (1971) provided evidence from the same data bank used by Gearing, that clients who more closely resembled the kinds of addicts generally found in jails and on the street did not perform nearly as well as the first reports indicated. They also provided documentation, if any were needed, that more than the very simple tabulations provided by Gearing were needed if there were to be any understanding of what might be happening in the program. Maddux and Bowden (1972) were able to demonstrate that the use of survivor cohorts, where different groups of individuals are contrasted over time, seriously influenced the results presented by Gearing. In *Drug Use in America* (1973), the National Commission on Marihuana and Drug Abuse documented the way in which crime data were incorrectly analyzed – i.e., that the use of man-years in treatment exaggerates declines. Lukoff (1975) reviewed the research performed by Gearing and demonstrated the way in which her reports tended to provide excessively optimistic results, and also questioned her data base, particularly in the area of crime and drug use.

Obviously, these criticisms have had little impact. In the latest reports prepared by Dr. Gearing (1974), she continues to employ the faulty methodological tools that have been her armamentarium since the very first reports. Along with her colleagues at Beth Israel, she seems immune to the barrage of attacks, many of which appear well-founded.

For those who are unfamiliar with the research on methadone, we present a few figures that demonstrate how to show success even if changes that are something less than very important are actually taking place. Observe that we do not assert that no positive changes have taken place in the programs she studied. We are only stating that the presentation of results can, at a minimum, exaggerate positive outcomes even where one is not creating fictitious numbers.

In Figure 2 we present data on arrest rates for the first year after entering treatment from a methadone program in Brooklyn. We present the data for different age cohorts controlled by their preprogram criminal activity. As the results clearly demonstrate, two factors are operative: patients with less preprogram involvement in crime look better after treatment then those who have had more criminal involvement, certainly a nonstartling finding. As patients become older, even where their preprogram criminal activity is similar, their posttreatment crime markedly diminishes. All one has to do therefore, is select older patients, and if at all possible, those who were less criminally involved. There is ample evidence that this happened in the early years of the methadone maintenance treatment program. Where control groups cannot be located, then maturation can certainly account for the diminution of crime.



NOTE: *N is less than 20.

FIGURE 2. Arrest rates first year in treatment by preprogram charge index and chronological age

But controlled experiments are rarely possible in community-based treatment programs, particularly where the population is mobile and deviant, as it is among those who are in drug treatment. The only device to control false results, since patient mortality is a fact of life, is to avoid examining survivors at various points in time. Yet, this practice persists in much evaluation research in drug treatment. To demonstrate the impact this has, in Figure 3 we present two curves (adapted from Kleinman & Lukoff, 1975). The top curve reports on all patients at each point in time who missed 26% or more of their medication. If one examines only that curve, it appears that the longer patients remain in treatment the more behavior improves. In the bottom curve, however, we present only the group of patients who remained in

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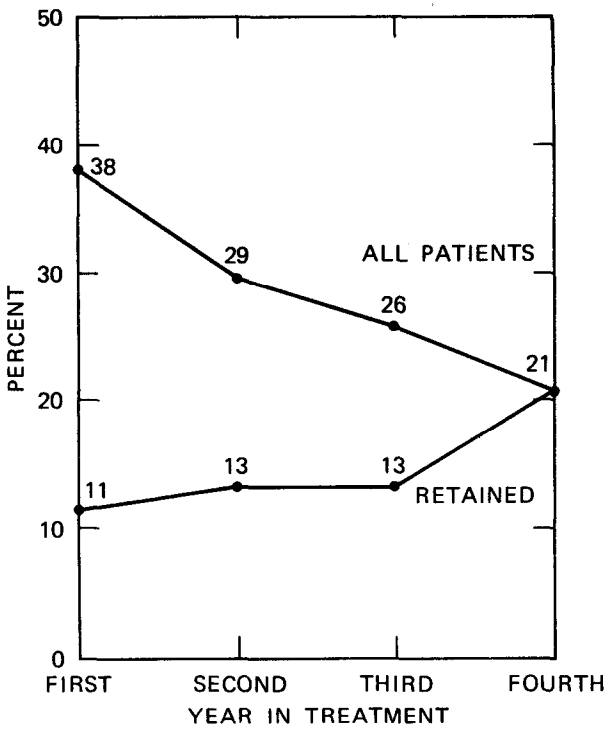

Number of cases				
All patients	1,666	1,011	586	207
Retained only	258	254	244	207

FIGURE 3. Proportion of patients who missed medication 26% or more of the time by year in treatment: for all patients and for those retained three or more years only.

treatment over the four years. Two observations can be made, both of them important: First, those who are retained behave substantially better from the very beginning, so that program impact would seem to be secondary to patient motivation and personal decorum; second, that those who remain actually get a bit worse through time. Yet such behavior is consonant with an aggregate finding that things look better! Nor is this a fluke. In all retention cohorts, patients actually missed more methadone the longer they remained in treatment, although they never quite reached the missed medication levels of the group that dropped out during the first year.

The issue we raised at the beginning of this section concerned the interrelationships between science, research, and treatment. Many explanations might be consonant with the review we have made of the conceptual foundations of methadone therapy and the resistance of some of the major actors in the scene to modify their basic themes, or even to attempt any improvement in the research documentation they present to the public. These are all individuals who were willing to enter a very dark arena, in which failure was the norm before they developed their modality, and to

present that modality to the world. Most professionals avoided any involvement with addicts, and there would appear to have been few rewards for doing so; yet they were willing to take on an extraordinarily difficult task and to persist until they found a solution that seemed to work.

Although they had their share of calumny, they were finally able to convince large segments of the public, including thousands of addicts, that they had a solution to a problem that seemed to elude all prior attempts at effective action. A large part of the opposition had to back away because of their success. They finally became accepted, their work was rewarded by the largesse of local and federal governments, and their fame and their methods entered the international arena. Nor is there any question that the scientific ethos belongs to them as much as it does to anyone else.

If even some portion of the critique we have made bears any relationship to the truth, then some explanation must be offered for the failure of these devoted, competent, individuals to respond to the long history of criticism of their theoretical underpinnings and of their results. There may be room for some cynicism concerning treatment program managers and staff members whose only careers are related to their programs. When these are in jeopardy they may even twist the truth or simply ignore criticism. However, we suggest that Kuhn might provide a more satisfactory explanation for the reluctance of doctors and researchers to change, even if the drug treatment arena is not a scientific field comparable to those such as biology, physics, or chemistry, which have well-developed theories and technologies associated with them. Yet there is a reasonably coherent theory, one that asserts (a) that addiction is a metabolic disease; (b) that a particular drug – methadone – solves the physiologically based need; (c) that addict behavior can be viewed as a *response* to sustained heroin use; (d) that methadone is relatively benign; (e) that illicit drug use can be blocked; and (f) that prosocial behavior is a direct consequence of a regimen of methadone.

Like scientists ensconced in a paradigm, the methadone pioneers had evolved an explanation for a problem and translated this into a technology that seemed to work. Although we may be somewhat skeptical of their documentation and are able to offer some counterexplanations for their early successes, this was not the perspective that engaged them at the time. They are less vulnerable to criticism because they know whence they came and what they were able to achieve. Perhaps the best explanation is that the fact of possible failure is difficult to entertain when there is no competing paradigm that offers a solution to the problems their system is designed to treat. Kuhn notes that new paradigms can succeed only when they demonstrate "they can solve the problems that led the old one to a crisis" (Kuhn, 1970, p. 153). No competing paradigm with a workable technology appears on the drawing boards. Kuhn also observes that scientists must have what is tantamount to a conversion experience before' they can alter allegiances, a rare occurrence, even in the so-called hard sciences. He states.

> Lifelong resistance, particularly from those whose productive careers have committed them to an older tradition of normal science, is not a violation of scientific standards but an index to the nature of scientific research itself. The source of resistance is the assurance that the older paradigm will ultimately solve all problems, that natures can be shoved into the box the paradigm provides. Inevitably, at times of revolution, that assurance seems stubborn and pigheaded as indeed it sometimes becomes. (Kuhn, 1970, p. 151)

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This report was supported in part by NIH grant DA01 103 to Dr. I. F. Lukoff, Columbia University School of Social Work. Additional support came from the Law Enforcement Assistance Administration grant nos. NI-71-046-G and NI-72-008-G. (This financial support does not necessarily indicate concurrence in the statements and conclusions of this paper.) The author thanks Barbara L. Kail for her invaluable assistance in the preparation of this report. Dr. Joan Dunne Rittenhouse (Supervisory Research Psychologist, National Institute on Drug Abuse) provided valuable editorial assistance in the revision of this paper for publication.

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[1]The conclusions drawn from our review of epidemiological research have direct bearing on methadone therapy. Many individuals merely experiment with heroin; others abandon use without treatment intervention. Since there are no clear prognostic indicators to identify these individuals, many can become involved in methadone programs through criminal justice and community pressures. They, therefore, become more heavily invested in drug dependence than they otherwise might. The normal maturation out of drug use that probably characterizes most heroin users can be seriously interfered with. Here, however, we examine a different set of issues.

[2]There are many variations among programs – some adhere to the maintenance schema, and others define abstinence as their goal. The use of other therapeutic modalities within programs also varies, as does the use of variations in dosage levels. We do not review these important variations here but instead focus on the central paradigm of methadone maintenance, its evaluation, and survival, even though we know that all programs do not conform to all facets of the original model.

Critique of Consequences of Use: Heroin and Other Narcotics

William H. McGlothlin, Ph.D.

In the main paper of this section of this report, Dr. Lukoff has provided us with some interesting observations, although some of them fall outside what is normally considered under the topic Consequences of Heroin Use. This paper presents brief comments on his paper and some observations from our own research.

One aspect that is useful to keep in mind is that when we speak of consequences of narcotics use we mean the consequences that result from use in the highly coercive environment in which the addict exists in this country. The consequences would likely be quite different under conditions of social acceptance or toleration. For instance, Dr. Lukoff correctly concludes that recent findings clearly demonstrate that the use of heroin often does not lead to addiction, and than, once established, addiction is not nearly so permanent a phenomenon as was once believed. The role of the environment is an important factor here. Military personnel who became addicted in Vietnam tended to discontinue the behavior once they return to the very different conditions in the United States. Older addicts in the United States tend to get tired of going to jail and stop using. There is no evidence of which we are aware that this phenomenon of "maturing out" exists in countries with a greater acceptance of opiate use. Actually, in some countries opium use appears to be more prevalent among the older segment of the population.

Dr. Lukoff notes that the prevalence of heroin addiction in the general population is not so high as to create a major social problem, but that, on the other hand, it is exceptionally high in the young male minority populations of certain urban areas. He concludes that in these areas heroin use robs the community of many of its more talented members and, further, diverts much of its limited resources away from other needs. In these areas of extreme socioeconomic conditions it is not clear how much of the individual and social dysfunction is due to heroin use per se and how

much to the social policy under which it exists. Health problems, unemployment, and the social and economic disruption caused by addiction-related crimes are closely related to social policy. If heroin were easily available the prevalence of addiction might be even higher, but it does not necessarily follow that the net consequences to the individual and society would be worse.

Dr. Lukoff cannot seem to resist continuing his feud with Gearing (1970, 1974) even where it is not relevant to the subject of discussion. Since some of his criticisms appear to be overdrawn, they require response. It seems true, for example, that Dole and Nyswander (1965, 1967) have evidenced an emotional involvement in their work that probably interferred with their objectivity. But the situation they were addressing was an emotional one. It was not one likely to yield to anything short of a promise of total success. Extremism breeds extremism.

In any event, a faulty theory, the confusion of blockade with cross-tolerance, and the choice of a higher than necessary dose level were not fatal to the successful implementation of methadone maintenance. The evidence does indicate that Dole and Nyswander were correct in concluding that stabilization of the dose level is easier with methadone than morphine. Further, it is not clear that, as Dr. Lukoff concludes, the initial selection of patients with a better prognosis is fundamentally wrong. In fact, such a procedure may be the most appropriate in the case of an experimental therapy. Undoubtedly, the enthusiasm on the part of the treaters also played a significant role in the early success rates.

The most important observation is that, some 10 years later, methadone maintenance is the most successful available means of reducing the individual and social costs of addiction for a large segment of the addict population. Our own follow-up study (McGlothlin, et al., 1976) of persons committed to the California Civil Addict Program provides some evidence on the impact of methadone maintenance. Of a sample of 439 males committed between 1962 and 1964, 132 had enrolled in a methadone program between 1970 and the time of the interview conducted in 1974-1975. During this period (1970 to the time of interview) this group had an average of 21 months on methadone and 25 months not on methadone, not including incarcerated time. Table 1 shows their narcotic use, criminal activity, drug dealing, employment, and alcohol use while on and not on methadone maintenance. The data are expressed in terms of the percentage of man-months involved in each behavior. The majority of those in treatment did report some narcotic use other than methadone, but the reduction in daily use and associated criminal behavior was pronounced. Employment was somewhat higher while in treatment, and alcohol consumption was only slightly higher than when not on methadone. The results are not so promising as those of early reports, but they are still positive.

Some additional comments based on our civil addict follow-up study on the consequences of heroin use are in order. We are just beginning the analysis of the data, but some results are available. One obvious consequence is the high mortality rate of 1.1% per year of exposure, which is similar to that found in several other studies. For those interviewed, we collected retrospective data on several variables from the date of first narcotic use to the time of the interview – an average span of about 18 years for one sample of 439. The data are well suited for assessing the relationship of heroin use and associated behavior because the data collection intervals were based on regular versus nonregular narcotic use in addition to legal supervisory status. This typically resulted in 10 to 15 noninstitutionalized periods – some with daily

Table 1
Comparison of Behavior on and off Methadone *Maintenance for 132* Patients *Enrolled at Some Time During 1970-1975 in Man-Months and Percentage of Man-Months*

	On methadone maintanance	Not on methadone maintenance
Man-months (not incarcerated)	2,774	3,311
Narcotic use other than methadone (%)		
Daily	5	46
2-6 times per week	3	9
1 time per week or less	40	29
None	51	15
Self-reported criminal activity Other than drug offenses (%)	13	42
Dealing (with profit) (%)	3	10
Employed full- or part-time (%)	63	49
Alcohol use (intoxicated 2 or more times per week) (%)	33	30

narcotic use and some with irregular or no use. We probably see more alternating between regular and nonregular use than occurs in most longitudinal records because of the close supervision and urine testing that is part of civil commitment.

Table 2 shows self-reported criminal activity, drug dealing, employment, and alcohol use as a function of frequency of narcotic drug use for the sample of 439 males. Again, the data are expressed as a percentage of nonincarcerated man-months in each category of narcotic use. There is obviously a clear relation between self-reported criminal activity and the level of narcotic use. As in most other studies, we find the majority of individuals were involved in deviant behavior before beginning heroin use – the means ages for first arrest and first narcotic use were 15 and 18 respectively. A few do seem to have begun their criminal activities after heroin initiation, and as a direct result of the cost of the drug. However, from the standpoint of social cost, the temporal order of initiation of heroin use and criminal behavior is not so important as the amount of crime and how it relates to heroin use. As in most studies, we found the crimes committed are largely the theft of property; e.g., burglaries and shoplifting. When the individual was relying on theft to obtain money for drug purchases, the amount of theft was closely coupled to the cost of his drug habit, e.g., three burglaries per week realizing $75 each for a $20-a-day habit. Generally, when the respondent was abstinent or using narcotics one or two times per week, he would report little or no criminal activity for the period – or he would occasionally answer something like “no, not for profit – maybe a little to get something I wanted.” The important point for social cost is not whether the individual is or is not engaging in property crimes but how *much* he is stealing. We do not often see the pattern mentioned as a possibility by Dr. Silverman – that of an addict stopping use but continuing criminal activity at the same level.

As seen in Table 2, employment varies inversely with the extent of narcotic use, although the correlation is not so high as the positive relationship with crime. As Dr. O’Donnell has noted, the causal relationship between employment and heroin use may be in either direction – an individual may not use because of the structure work introduces, or may be able, and have the time, to work because he or she is

Table 2

Self-Reported Crime, Drug Dealing. Employment, and Alcohol Consumption as a Function of Frequency of Narcotic Use in Man-Months and Percentage of Man-Months

	Frequency of Narcotic Use		
	Daily	Irregular	None (or less than once per month)
Man-months (not incarcerated)	23.565	15.173	23,901
Self-reported criminal activity other than drug offenses (%)	59	26	12
Dealing (with profit) (%)	25	8	4
Employed full- or part-time (%)	39	63	76
Alcohol use (intoxicated 2 or more times per week) (%)	20	49	51

not using. We do have a fair number in our sample who work most of the time, keep their daily heroin use at rather low levels, and report little or no crime. This happens more frequently among the Mexican-Americans who make up slightly more than 50% of our sample.

Another consequence of heroin use is a large number of unsupported children. Rarely do the active addicts report any significant support for the children they father.

Not all the consequences of heroin use are negative. Our results show a reduction in heavy alcohol and other drug use during periods of regular heroin use (Table 2). Most respondents also report personal rewards from their heroin use. We ask their favorite drug (77% name heroin) and what heroin does for them. Some say it was fun at first but that later they used because they had to. However, most are quite positive in their assessment of heroin effects, and 25% state they are happier when using as opposed to not using.

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This research was supported in part by Grant No. 5-H81-DA1146 and No. K05-DA70182 from the National Institute on Drug Abuse.

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Some Comments on Consequences of Chronic Opiate Use

Charles Winick, Ph.D.

Dr. Lukoff's paper, which has appeared earlier in this report, has presented a very useful analysis of the need to study the details of the subarea prevalence of opiate use as well as the larger national samples of the population. It also contains some trenchant observations on typologies and a number of vigorous assessments of our use of data on methadone maintenance.

Prevalence

Policymakers and social scientists are especially concerned about that form of opiate use that we can call a *chronic relapsing condition.* It is more a condition than a disease, because it is so heavily mediated by social and ecospace factors that the disease model, as explicated by researchers like De Alarcon (1969) and Hunt (1973), appears to have limited utility without consideration of social factors.

Consider some of the differences in dimensions of opiate use in different communities at the present time (Winick, 1974b):

1. Street methadone is the primary drug of abuse of 500% more users in New York than in Los Angeles.

2. Texas users are almost 300% more likely to be arrested than New Jersey users.

3. New York users are 200% more likely than Philadelphia users to have a legitimate source of income.

4. Detroit users spend 300% more than New York users on their habits.

5. Illinois users are 200% more likely than Washington users to average abstinence periods of more than 6 months.

Such substantial variation in significant aspects of the correlates of opiate use suggest the need to view that use almost as a series of separate social problems that are shaped by local situational forces. For example, narcotic arrests declined dramatically in New York City from 1971 (20,762) to 1972 (11,431) and even further to 1973 (7,566). The largest contributor to the decline was not an epidemiological shift but a policy decision by the Police Department, in March 1972, to concentrate on "quality" arrests (Winick, 1975). This police policy change, which drastically affected street drug activity, recruitment of new users, rates of entrance into treatment, and other phases of the drug "scene," was only operative in New York City and is an example of the kind of social mediator that makes the disease model and generalizations based on national statistics of very uncertain utility.

Life Cycles and Typologies

The same considerations apply to the second theme of Lukoff's paper, dealing with life cycles and typologies. In different settings, different kinds of people will become chronically dependent on opiates. In San Francisco, a typology including the Transition Junkie, who started use in the 1964-1966 psychedelic era, and the New Junkie, who started in 1967, is plausible (Gay, Newmeyer, & Winkler, 1972). San Francisco has such a special place in the history of psychedelics and the hippie trend that events of the year 1967, when the hippie movement officially died, certainly had a unique influence on drug use in that city. The same influence did not, however, apply in other cities, and the typology is less relevant elsewhere.

An early typology in New York City did not find the kind of social-recreational use that has been reported in other cities (Chein, Gerard, Lee, & Rosenfeld, 1964). The special circumstances of New York ghetto life may have contributed to the elimination of this stage.

Urbanologists have developed typologies of cities that could provide a framework within which to interpret a role approach to understanding opiate use. Such a role approach suggests that chronic relapsing drug dependence will be high in groups in which there is (a) access to dependence-producing substances, (b) disengagement from negative proscriptions about their use, and (c) role strain and/or deprivation (Winick, 1974a). This role approach has had considerable predictive value in identifying high prevalence groups in different contexts and countries. It can also contribute to understanding differences in the rate at which chronic users reach a saturation point of "burning out," at which the maladaptiveness of the user role becomes so salient that the "hassle" becomes more important than the satisfaction, and the dependent person stops taking drugs.

The role approach does not imply homogeneity in the kinds of persons who become chronic opiate users. Since there is a self-selection of users in different communities, even though the same general principles are operative the users may be different because the communities or even neighborhoods differ. Thus, we may find a group of active "cool cats" in Chicago (Finestone, 1957) but passive, nonideological users in New York at the same time, although both groups are of similar ethnic, age, and family backgrounds (Chein, et al., 1964).

It is truistic that ingesting the same quantity of the same drug may have very different meanings to an acting-out teenager, a middle-class teenage dropout, a groupie, a nomadic youth, a young person whose major relationship to society is positioned by

being a consumer of the arts, a young dealer-user, a musician, a young person who is involved centrally with a criminal subculture, a "hidden" young user who does not steal in order to get drugs and has been using for several years, a Vietnam veteran, a young person who just began using after losing his or her job, a young user who learned to ingest the substance from fellow workers in a job situation, a current methadone patient, a former methadone patient, or a prostitute. Yet, in an epidemiological tally, all of these different kinds of users might have equal weight because, as one result of uneasiness about generalizing, the tally made little effort to develop typologies of users. Another example of this phenomenon would be taking two cities and discovering that they had exactly the same number of chronic opiate users of the same age group and sex, but making little effort to find out how many subgroups were included in the totals. The study of typologies and life cycles is essential if we are to make epidemiology a dynamic field and go beyond counting.

Methadone Maintenance

Great variability also characterizes the methadone maintenance that is the third subject of Lukoff's paper. Methadone maintenance programs throughout the country vary at least as much as do therapeutic communities. They vary by financing, nature of support, nature of leadership, geographic base, degree of auxiliary services, dosage, institutional locus, emphases, goals, and many other factors. The Dole-Nyswander model that is the target of Lukoff's criticisms actually is no longer even used by Dole and Nyswander; it is now historical. The high dosage, long-term model is not widely followed, and the goal of methadone maintenance programs, by FDA order, is to get patients detoxified within a few years.

Methadone maintenance is actually the most evaluated modality in the treatment of drug dependence. Lukoff implies that a monolithic paradigm was followed in the face of contrary evidence. Actually, the annual National Methadone Conferences that were initiated in 1967 attracted large proportions of the people working in methadone maintenance, and the translation of new ideas into program practice is very prompt. There is no need to wait years for journal publication and ultimate trickling of ideas into the field, since the proceedings are published within a few months.

One unanticipated consequence of methadone maintenance that has changed the nature of opiate use in many urban areas is the general availability of street methadone, which is somehow diverted from treatment programs. In some areas of New York City, illegal methadone is preferred over heroin, partially because of its lower price. If large quantities of Turkish heroin reappear in this country, and its price drops, there will be a kind of Gresham's Law operating, although it is not immediately clear which will become the "good" and which the "bad" drug.

Consequences of Opiate Use

There are many consequences of opiate dependence to the user. They include modification of the self-concept, the relief of physical symptoms, easing of adaptation, shifts in life space and role, self-medication, changes in the use of alcohol, feelings of pleasure, changes in level of sex interest, changes in relations with children and other family members, and the like. Other consequences to the user involve more social aspects of living, such as changes in work patterns, criminal activity, entering

the drug distribution system, arrest, incarceration, treatment, and even death (Winick, 1974b).

How can we begin to measure the consequences of opiate use to the community? We can derive guidance from various techniques developed in the 1960s to determine some consequences of a reduction of various levels in cigarette smoking – 10%, 25%, 50%, 75%, and ultimate elimination of smoking (Committee on Interstate and Foreign Commerce, 1965). These techniques permitted a reasonable approximation of the economic and social consequences of the smoking habit. Another useful approach to the study of consequences of opiate use is the model provided by various cost-benefit investigations of the economics of criminal activity (American Bar Association, 1975). Social consequences – of smoking, crime, or opiate use – may be real and pecuniary, tangible and intangible, direct and indirect, intermediate and final, internal and external. We possess a number of other kinds of technology that can permit a verifiable estimate of the several levels at which opiate use has consequences to society.

One obvious consequence of opiate use is economic. The costs to the community of crime-related activities, including the crimes themselves and the criminal justice system's efforts to deal with them, average $10,775 per user per year (Casey and Preble, 1974, pp. 283-308). The average cost per user of treatment, prevention, and education efforts approximate $1,760 per year, for a total of $12,535 per year. The most conservative figure for the number of chronic opiate users in the United States mentioned at this conference is 500,000. Since there were over 450,000 treatment slots last year, and it is highly unlikely that as many as 90% of all chronic users were in treatment, the 500,000 figure is reasonably conservative. Even this figure leads to a cost of $6,267,500,000 per year for the crime-treatment-prevention-education aspect of opiate use. By way of comparison, this is well over half the total budget for the City of New York.

Another consequence is foregone production, which is loss of many opiate users' capacity to do work, resulting from their shortened conventional working life, reduced life expectancy, and lower productivity. The difference between estimated *annual* earnings of opiate users and what they would earn if fully employed is estimated to be $3,064 per chronic user per year, or a total of $1,532,000,000 with our conservative figure of 500,000 chronic users, and making no assumptions about rate of employment. The Marxist view of foregone production argues that our society drives many superfluous people to opiate use because a surplus population must be generated by the current capitalist economic system (Karmen, 1974, pp. 309-319). It is further argued that many women will be encouraged to enter prostitution to get money for drugs.

The consequence of theft-enforced obsolescence, or the redistribution of goods stolen by opiate users to persons who might not otherwise afford them, permits the latter group to acquire materials, and encourages the victims of theft to purchase replacement products. This form of obsolescence, like fashion changes and planned obsolescence, reduces the threat of overproduction and counteracts the awareness of relative deprivation that could become politically volatile.

Many ethnic leaders have argued that one consequence of opiate use is that illegal opportunities for upward mobility become attractive to minority group members who are unlikely to have access to conventional jobs. They also feel that communities

will be diverted from dealing with central problems of education, housing, and jobs because they are putting money into drug programs. Treatment programs are seen as a cheap and evasive way of buying solutions to complex social problems.

Some observers have pointed to the irony that the "new careers for the poor" movement of the 1960s has largely collapsed, but the democratization of drug dealing has provided careers as dealers for blacks, Cubans, Puerto Ricans, and other minorities. The "new career" that has enjoyed the largest expansion is in the treatment of opiate users, as staff members of therapeutic communities, research assistants in methadone maintenance programs, and the like. Lukoff quite correctly asks us to consider the meaning, to youngsters, of seeing so many drug treatment programs in ghetto areas.

Another consequence of opiate use is its legitimation as a social problem as a result of the creation of the Special Action Office for Drug Abuse Prevention in the White House and its institutionalization as a medical problem by the creation of the National Institute of Drug Abuse (NIDA). These developments of the 1970s underline the tremendous increase in professional, legislative, public, and government response to the opiate problems.

Changes in Policy

It may be instructive to compare two books that were published a decade ago. Schur's (1965) *Crime Without Victims* dealt with abortion, homosexuality, and addiction. Since the books' publication, public and legislative and professional attitudes toward abortion and homosexuality have changed dramatically. But if we compare the content of the chapter on epidemiology prepared by the present author for another California symposium sponsored by the federal government and published at the same time as Schur's book (Winick, 1965) with the content of the papers presented to the current conference, we note that there has been a quantum leap in our knowledge of opiate use in the last decade.

However, changes in policy in dealing with opiate use during the last decade have not at all approached the shifts in policy that have occurred relating to abortion and homosexuality. One reason for the relative lack of policy change regarding drug users is that they do not constitute a constituency of interest group that can be mobilized as effectively as proponents of homosexuality, lesbianism, or abortion.

Another reason is that there has not, except for methadone maintenance, been sufficient success in dissemination of the enormous quantities of data, information, and findings accumulating in the last 10 years, in spite of the valiant efforts of the NIDA clearinghouse. There are many journals, important reports are released in mimeographed format and never indexed or abstracted, the uneven quality of much of the work makes assessment difficult, the nonprofessional background of many practitioners has not oriented them toward dissemination and publication of findings, few libraries make an effort to follow drug publications, and the fractionation of disciplines working in opiate use means that there is no one professional society to which most workers in the field belong and that disseminates authoritative information.

We might profit from the experience of the researchers concerned with studying the effects of television on children. As the result of a concentrated spurt of research activity generated by the Surgeon General's Scientific Advisory Committee on Television and Social Behavior, many studies were conducted, and there was an information explosion. The lack of widely read research journals devoted to television and

the absence of a central professional society made it difficult to keep up with the research data being generated (Rubinstein, 1972). Also, a very wide range in quality characterized the work. The situation in television research was similar to our current situation in opiate research.

As a service to persons working in the field, the "state of the art" was very neatly and ingeniously summarized in three paperbound volumes. One volume summarizes the principal findings, design and methodology, and theory of over 400 reports, each of which is also rated with a Michelin-like star symbol, from none to three stars, to reflect its relative degree of interest at this time (Comstock, 1975). Another volume is a bibliography of over 2,300 citations, with key word indices and 11 specialized bibliographies (Comstock & Fisher, 1975). A third volume describes current research in progress (Comstock & Lindsey, 1975).

These three volumes summarize what is known about television and human behavior. They position each study in an appropriate context and make it possible for everyone reading them to share a common knowledge base. The comparative rating of the studies makes the reader's task much easier.

An analogous series of publications would similarly help to bring order to the chaotic field of the epidemiology of chronic opiate use. It would help to bring students into the subject, indicate gaps in our knowledge, permit identification of the relative merit of studies, in a systematic and valid manner, underscore trends, minimize unnecessary and inadvertent replication, help funding agencies in assignment of priorities, and facilitate the access of researchers outside of a major medical center to the literature.

We have learned a lot, but there is a lot more to learn. An intensive publication program of the kind proposed can help materially in ordering what we know and blocking out the kinds of knowledge we could be pursuing. Such a program can help us to learn more about epidemiology, put our knowledge to better use, and influence policy more constructively. It can help, also, to make the study of the epidemiology of chronic opiate use more attractive to the new generation of students of human behavior and more credible to the community.

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Reflections

Reflections on the Stanford Session

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As is noted in the introduction, the recognition of heroin use as a health problem, a social problem, and a legal problem has shaped government-funded research efforts. In particular, policymakers' concern about the size of the heroin-using population has been reflected in large government investments in the development of an epidemiologic capability, of which the Task Force itself is one demonstration. But as the foregoing papers and the discussion illustrate, scientific inquiries in the area of heroin epidemiology are not defined narrowly. The Task Force goal is not limited to the production of a count or a number; the participants seek to understand developmental processes in drug use, and the antecedent and consequent correlates that characterize drug use.

The contents of this volume correctly represent, in our opinion, the state of the art in epidemiologic research on heroin use. The papers and the discussion are full of facts and opinions, diverse in their viewpoints and approaches, quite selective, and highly personal in the problems they choose to deal with. As is true with pioneers in any endeavor, the authors reflect scattered through their written words the insecurity characteristic of first efforts. In the case of heroin epidemiology, the knowledge explosion of the past decade has, as an unintended effect, created a volatile, if somewhat intimidating, field in which to work. In Winick's words, "there has been a quantum leap in our knowledge of opiate use in the last decade" – and many of the authors of the papers and the contributors to the Stanford discussion sessions are leading figures among those who deserve credit for the quantum leap.

Although the summarization of all this knowledge is a formidable task, its integration – a task beyond the charge to the separate authors – is premature, given the state of the art. Our reflections touch on those notions, models, and thoughts

generated by the Task Force that provoked in us a response or comment. Future commentators will no doubt deal with those worthy pieces not discussed here; for ourselves, we are struck, even months after the session, by its strong call to definition, its effective critique of available methodologies, and its creative identification of new research questions.

To us the basic question raised by the Task Force session is easily stated: in accumulating our storehouse of knowledge, to what extent have we come to grips with the chore of defining our problem? Although our sophisticated view of the word epidemiology permits us to ignore its root "epidemic," it does not permit us to ignore the fundamental requirement. Namely, we must define the disease, the social problem, the undesirable condition that we are trying to understand and ultimately to stamp out, prevent, or at least mitigate.

Because in modern philosophy it is certainly unfashionable to ascribe evil to substances or things, we do not define the drug heroin as the enemy. The existence of alcohol does not cause alcoholism, and the existence of automobiles does not cause highway accidents. As the preceding pages show, we have outgrown the long-standing myth that an individual's first use of heroin leads inexorably to the creation of a dope fiend, a slave to the drug, one who is incapable of escaping from his bondage and is ready to steal, kill, or sell his mother to obtain a fix. But vestiges of the sterotype remain – reinforced in several ways. There are some inescapable facts relating heroin experimentation at one extreme to heroin addiction at the other: (a) there are some cases – perhaps only a few, but enough to provide anecdotal examples – of undesirable consequences from even a first involuntary use of opiates, and (b) first use is a necessary precondition to subsequent use and the onset of problems. Loss of virginity does not inexorably lead to promiscuity, but one cannot attain the latter without the former. With heroin, first use defines the population at risk for problematic use. For those in the public health area charged with prevention, there is some reasonableness to defining the target in terms of preconditions, precursors, or risk-enhancers. In the case of cigarettes for example, it is policy to discourage young persons from smoking to minimize the risk that they will become heavy smokers and thus susceptible to harm.

Our difficulty, of course, arises from our own lack of discipline in differentiating concepts: we tolerate the use of verbal terms, side-by-side if not interchangeably, that simply confuse or mislead ourselves and others. "The problem," "the drug problem," "the heroin problem," "the narcotics problem" – these, we submit, are meaningless phrases and ought to be banned from serious discourse. What is it that we are talking about? Do we have in mind certain specifiable behavior, certain physical conditions, certain psychological conditions, certain social consequences? Let's say so.

At the risk of offending almost everyone, we remind the reader and the Task Force participants of the charge given clearly in the Terms of Reference by Robins well in advance of the Stanford session. Although several of the papers and important passages in the discussion were responsive, regrettably many were not. We come away from the experience with the conviction that the definitional charge in the Terms of Reference may be the part of this document with the greatest meaning for future research. In short, in the field of heroin epidemiology particularly, we owe to ourselves, to other researchers, and to the critical reader, responsible operational definitions. Let us identify a few from a far larger pool of terms that appear in the foregoing pages and that may cause difficulties:

1. *Drug user:* This term conveys no information; it may arouse an affective response in some readers; it may be useful in winning arguments – but we would be better off without it.

2. *Heroin user:* On examination, this term may describe a person who said yes to the question, "Have you ever used heroin?" regardless of the population to which the respondent belongs. He may have said yes in a household survey; he may have responded affirmatively in a treatment setting. Thus, "heroin user" defines entry into a class that must, as Room clearly demonstrates, be disaggregated before the classification becomes useful.

3. *Active heroin user:* This term describes a heroin user (see above) who has met some additional, unstated criterion.

4. *Known heroin user:* This term usually, but not necessarily, describes someone who has met an unstated criterion for membership on a list maintained according to unspecified rules.

5. *Drug-related death:* This term may include anything from an overdose suicide to the presence of some unspecified illicit drug in a pedestrian hit by a truck.

6. *Addict:* We prefer to reserve this term (as we reserve the word "alcoholic") to medical diagnosis describing a physical or psychic dependence (as judged by the diagnostician). This is not to be confused with the self-professed addict who proclaims his addiction as a mitigating circumstance in a criminal trial or who knows what he must call himself to satisfy the requirements for delivery of service.

This brief list, although not exhaustive, suggests repetition of a relevant axiom: *measurement, which is the starting point of epidemiologic research, requires exquisite clarity of definition, differentiation, and precision.* Once we have decided what the disease is – or the behavior that is of concern to us – we are eligible to produce a prevalence estimate or an incidence estimate.

Ignoring the eligibility criterion, at a cost of considerable conceptual and verbal confusion, has resulted in fixation at a preestimation level of crudity often disparaged as the "numbers game." Everyone knows that the first step in epidemiologic research is to estimate the size of the problem: how many persons have the disease; how many new cases occur in each time period? The difficulty with heroin epidemiology today is easy to understand, given the definitional recalcitrance of many investigators: we can collectively provide more estimates than anyone knows how to use. This state of affairs is reflected in the Task Force session and forces us to confront the essential questions: What is right and what is wrong with current estimates? Even more fundamentally: Why do we need these numbers? Any fool can see that there is a great deal of heroin use, that its consequences to the health and well-being of the user and of the society as a whole are indeed intolerable. What difference does it make exactly how many persons there are whose involvement with the drug exceeds some threshold? In many fields of inquiry, the requirement for precise estimates stems from a scientific compulsion: almost no one but an astronomer really cares exactly how far it is from the Earth to the sun; it's a very long way. That's really enough information for almost all purposes. And indeed, if total prevalence and incidence estimates were the only product of the estimating procedures, we suspect that interest would rapidly diminish. But overall estimates are only the beginning: what are the trends across time; is the problem increasing or diminishing; what kinds of people in what kinds of

environment seem to gravitate toward the undesirable? What effects are preventive and palliative programs producing? Answers to these and many other questions of both a scientific and utilitarian nature depend, to a considerable extent, on the quality of prevalence and incidence estimates.

Roughly classified, the Task Force papers illustrate three estimating techniques presently being applied in heroin epidemiology: (a) sample surveys (general and special populations); (b) recapture; and (c) treated prevalence extrapolated to total prevalence.

Sample surveys generally confine themselves to collection of self-reported information on attitudes, self-perception, and behavior. They can provide prevalence and incidence estimates for first use, use within a defined time period, quantity, and frequency; they can provide self-descriptions, developmental histories, and so on. But perhaps their greatest utility with respect to heroin epidemiology does not depend on direct measurement of contact with heroin (because heroin use is so scarce in general populations); rather, they provide information on more popular drugs, availability of drugs, attitudes toward drugs, and presumed precursors or risk-enhancers. The limitations of surveys are well known:

1. Surveys are based on samples, not censuses; consequently any estimates (particularly estimates of rare events) may be subject to random fluctuations.

2. Surveys, to exploit the interpretive power provided by a random sample, are generally conducted in the household population. The unintended effect of this sampling frame is the exclusion of many groups such as the homeless, those in institutions, and members of the Armed Forces, all of whom are likely to exhibit higher-than-average rates of drug use.

3. No survey (including the decennial census) has ever achieved 100% response rate from its predesignated sample. Although high response rates can be achieved at considerable cost in certain specific populations, prudence in cost allocation often dictates the acceptability of lower response rates. The potential bias of nonresponse is unknown.

4. Highly developed interviewing and questionnaire techniques have been designed to convince respondents that the situation is risk free for them. Experimental studies have indicated that such procedures assure some reasonable level of validity in responses. Nevertheless, in surveys, as in all other estimation methods, there is undoubtedly some denial of drug use.

Recapture techniques, popularized by Greenwood, depend on earmarking a known number of members of a population, subsequently drawing a random sample of specified size from the population (a sample in which prior earmarking does not affect the probability of selection), and noting the number of earmarked cases. With this procedure, we have three elements of a four-element equation, and the fourth element (the size of the population) emerges algebraically. As with survey methods, there is little dispute about the correctness of the fundamental theory. Criticisms center around the extent to which requisite assumptions are satisfied; for example, if the earmarking and the subsequent sampling involve arrests, then it seems reasonable to expect that previously arrested (earmarked) persons will have a higher probability of arrest than others.

Treated prevalence extrapolated to total prevalence has long been a dream of epidemiologists including Sells and others whose tine works are represented in the foregoing papers. Because treatment centers and other facilities (such as morgues and police stations) represent central points to which cases gravitate and because such facilities generally keep records, it is not too difficult to place a number on the treated prevalence for a specified condition. Then all one needs is an estimate of the probability that a person with the specified condition will seek treatment or appear at the nontreatment facility and the job is done. The process can involve one step or several, as in the Jellinek estimate for alcoholism. All that is required is that the probability of coming to the attention of authorities be known and that it be reasonably stable across time and place, or that its fluctuations be predictable for available information. There are many variations on this theme, but the fundamental theory simply requires the multiplication of the treated prevalence by the inverse of the probability. It should surprise no one that the technique is widely criticized because, as Smart and Richman state quite well in their papers, the probability of seeking treatment (or being arrested or being designated a drug-related death) is quite variable from one place to another, from one time in history to another, and from one population to another.

Hunt, in combining data from all three methodologies, attempts to correct for the shortfall in all of them. In the end, he courts the same fate as Jellinek: rejection of a thoroughly sound theory because the assumptions could neither be met nor circumvented. As O'Donnell points out in his detailed critique, the application of a single multiplier is not justified by actual field conditions; the application of upper and lower limit multipliers for bracketing purposes leads to wider ranges in the heroin estimate than result from household surveys.

It is quite frustrating to be in possession of three theoretically sound estimating techniques, none of which can overcome practical problems of implementation. It is clear that this Task Force session represents an important step toward acknowledging difficulties common across techniques and proposing solutions to the problem of estimating rare events. For example, Robins offers a challenging conceptual and operational approach. To the extent that the problem behaviors or the problem conditions or the diseases (whatever the targets of epidemiology may be) can be viewed as points in a developmental process and that, conceivably, antecedent points can be defined and the transitions can be identified, the process of estimation can be conceived in terms of the definition of thresholds in one or more developmental processes. Entry into one of the processes becomes the first transition; each threshold is another transition; at each level, we can think of the probability of reversion to an earlier stage, the probability of remaining at the specified level, and the probability of moving to the next level. Certainly these probabilities will vary according to the characteristics of the individual and the characteristics of the environment. Thus, there are three obvious tasks for the researcher: describe the developmental process or processes, estimate the probabilities, and determine the covariates of variation in the probabilities. At the very least, demographic specific transition ratios can be developed. This conceptualization recognizes the multiplicity of problems, or at least a gradation of problems, and specifies the need for the numbers (not the number) that are required for intelligent policy information.

There is no doubt that the well-documented difficulties with estimating prevalence has led a number of investigators to address their attention instead to trends in heroin use. In fact, it is often argued that, under certain reasonable assumptions, change is

more easily (i.e., appropriately) measured than absolute level of use, and that change is, indeed, more important for policy formation and evaluation. Certainly, for drugs of sufficient frequency of use, the mapping of trends is well within the capability of existing techniques.

Despite survey findings of wider heroin experience in the general population than had previously been suspected, the use rate in the general population is so low as to create statistical difficulties. This means that the estimation of trend curves for heroin use is hardly more reliable than the measurement of prevalence. It is possible, as Lukoff's paper implies, that measurement of trends in high-use subpopulations, as contrasted with those in the general population, may be productive of policy-relevant information in the future.

The Task Force and its planning group identified some areas of concern that have important implications for the measurement of prevalence and trends in heroin use. One of the most salient of these is the relationship between heroin experience and the use of other illicit drugs. Specifically, is heroin use the endpoint of a developmental sequence?, Although rare cases of use of heroin as the first illicit drug have been reported, studies of the general population suggest that there is an orderliness to the development of drug experience. Marihuana is generally the first of the illicit drugs: heroin users are, on a highly selective basis, "recruited" from among marihuana users. Given this relationship, it is possible to predict at least two scenarios with respect to future heroin trends.

With a clearly rising trend of marihuana use, if recruitment rates from marihuana to heroin use remain at the current level, it is reasonable to predict a steady increase in the extent of heroin use (which, of course, must be distinguished from addiction). On the other hand, the recruitment rate may drop as marihuana becomes acceptable in broader social circles, whose members continue to disapprove of and lack access to heroin. If so, increased marihuana use will not be followed by proportionate increases in heroin use. We predict that an increase in the normative acceptance of marihuana will see a decline in the function of marihuana as a gateway to recruitment into further illicit drug use. Much research interest has, therefore, focused on increasingly casual attitudes toward marihuana and on the developmental process involving transition from soft to hard drugs.

These papers clearly show that heroin experience is a phenomenon far more complex than had once been thought. There is now some consensus across papers that the term "heroin user" applies to experiences that are both inconstant and reversible. In the last several years, surveys have confirmed each other in the conclusion that self-reported mild use without social or health problems is more common than had been suspected a few years ago. As Robins and others report, there is a good deal of movement in and out of the various statuses associated with the word "user." In the non-clinical populations, commonly tapped in survey research, spontaneous remission is not unusual, particularly among whites. This contrasts with studies based on clinical populations in which relapse to addictive use is common following treatment. These and other contrasts between the general and the clinical populations suggest, as Room clearly concludes, that these two groups experience heroin in diverse ways worthy of study and explanation.

It is by now accepted that the environment, including such factors as drug availability and community economic level, has a strong influence on the extent of use. Whether or not one subscribes to the hypothesis of a drug subculture, we are grateful to Lukoff,

among others, for identifying some truths about heroin that are by now well accepted, at least in the research community. Heroin use is not a homogeneous phennmenon across the nation; it exists in enclaves; it thrives in certain neighborhoods, in certain communities, in certain regions; and these enclaves are only partially defined by demographic factors such as age, sex, and race. Although a great deal of public attention centers on national prevalence estimates, there is a growing realization that such estimates tend to conceal the clustering characteristic of heroin use. The suggestion that Lukoff advances in his paper, namely that community impact in areas of high use is a worthy and socially significant dependent variable, merits careful policy and scientific consideration.

Another future direction for heroin epidemiological research is implicit across several Task Force papers. Concerns with transitions between drugs and with movements in and out of heroin use clearly suggest longitudinal studies, probably designed around high-risk populations whose members have not yet crossed the first threshold at the time of first data collection. But, perhaps, given costs of such research, turn-around time, and urgency of the problem, alternative approaches might better serve more immediate needs at the community level. It is possible to design a series of cross-sectional studies, to sample a variety of subpopulations, each at a different identifiable stage of development. For example, a simultaneous, triangulated study would involve the treatment population, the general population, and a records (agency) study at successive points in time. Undertaking such cross-organization, coordinated "mini" studies on the local level might be useful to discover the effects of quite different contexts on heroin use and to trace diffusion processes. However, investigators need great sophistication to cope with the time lag between initial involvement with heroin and appearance in records.

In closing, we are led to conclude that the kind of constructive thinking that Robins, Lukoff, Room, O'Donnell, and other participants have shown in this Task Force will lead to another quantum leap in the next 10 years. We feel we have a good basis to hope that in 1986 we may again say that "There has been in the last decade a knowledge explosion in heroin epidemiology. . . ."

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[1]We acknowledge our debt of gratitude to the members of the Planning Group for suggesting the parameters of this piece. The interpretations, the errors of commission and omission, and the opinions are, of course, our own. For reprints please write to Dr. Ira H. Cisin, George Washington University, 2401 Virginia Avenue, N.W., Washington, D.C. 20037.

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