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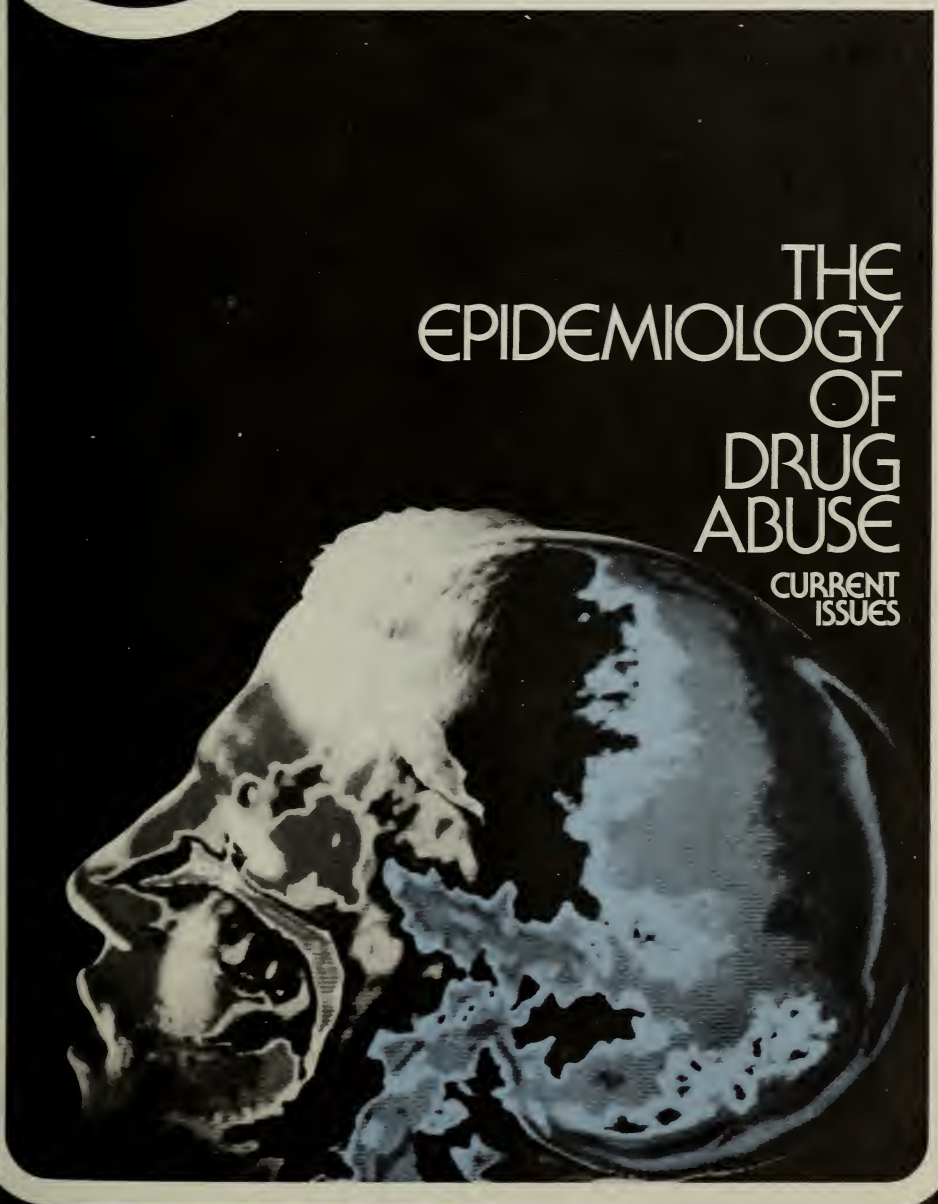

THE EPIDEMIOLOGY OF DRUG ABUSE

CURRENT ISSUES

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

Conference on the Current Status in the
111 Epidemiology of Drug Abuse,
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THE EPIDEMIOLOGY OF DRUG ABUSE: CURRENT ISSUES

Editors:

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March 1977

NIDA RESEARCH MONOGRAPH 10

Psychosocial Branch
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FOREWORD

The epidemiology of nonmedical drug use or abuse continues to be one of the most active research programs in the National Institute on Drug Abuse. It is active in that it has inspired numerous research endeavors, and additionally in meeting continuing and pressing demands for information. This is one of the subject areas that responds most frequently to questions from the public, Congress, and other government agencies. It is also active methodologically. There is no "status quo" in the means of acquiring this knowledge; few if any are satisfied with the techniques now at hand. There is much searching, examining, and speculation about better ways of comprehending the extent and nature of drug use and abuse.

The presentations and discussions in this publication are a fair reflection of the state of the art in 1975, its imperfections as well as its achievements. They should provide an excellent foundation for improved research in the future.

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PREFACE

These papers are edited transcripts from the Conference on the Current Issues in the Epidemiology of Drug Abuse held in Miami Beach, Florida, on November 18-19, 1974. The procedural format of the Conference was informal. A roundtable exchange was encouraged in order to promote the most relevant, stimulating and wide-ranging examination of the issues involved. Participants were invited to speak briefly on a particular topic on the agenda. Their comments in turn became the lead-off remarks for discussion.

The two days' proceedings were tape recorded in their entirety. Edited transcripts of presentations were submitted to each speaker for further editorial review. Discussion sections were edited as well for clarity and relevance. This final report does not represent a verbatim transcript but a joint effort of participants and editors to render an accurate yet concise representation of the Conference.

The editors wish to thank Joseph Romm, System Sciences' Project Director, for his intelligent guidance of the Conference itself and his careful supervision of the project report to its completion. The contributors are also owed thanks for their conscientious editing of transcripts and tendering of finished manuscripts.

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INTRODUCTION

Louise G. Richards, Ph.D.

With this conference in Miami Beach, the epidemiology of non-medical drug use and dependence came into its own. A total of three national meetings had been held specifically on the subject of the extent and patterns of drug abuse.* In the government's classification of research projects, epidemiology had been singled out as a prime focus. At the time of the meeting, there were already two active programs of drug abuse epidemiology, one in the National Institute on Drug Abuse and the other in the Special Action Office for Drug Abuse Prevention, under the leadership of Dr. Mark Greene.** Since that meeting, specialized programs and positions have become firmly established and are now devoted exclusively to drug abuse epidemiology, both in Canada and the United States.

The purpose of the Miami Beach Conference was to review the state of research at the time, identify major problems and gaps, and recommend new directions that should be taken. Geographically, it was midway between Washington and San Juan. Also, conceptually its purpose lay at a point between the extremes of the first national meeting on this subject held in San Juan in 1972, and two later meetings in Washington, D.C. Columbia University's School of Public Health was the sponsor of the first meeting. It was rich in intellectual fare, and created the first community of scholars for this research area. The proceedings of the San Juan conference still stand as the definitive conceptual work on the subject (Josephson and Carroll, 1974). The second and third meetings had practical purposes oriented toward specific plans or projects. Between these extremes, the agenda of the Miami Beach meeting was devoted to evaluation of progress, reminders of the hazards in the territory, and discussion of the precautions to observe if further advances were to be made.

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*The term "drug abuse" is employed broadly here to include all non-medical drug use, with recognition of the fact that not all use results in adverse consequences.

**With the closing of the SAODAP, most of the active projects were transferred to the N.I.D.A.

Because of the emphasis on review and assessment, the group was small and consisted primarily of those who were actively engaged in research at the time. They were and still are the leaders in the field. Full reports of the participants' research are not included here, but references are given to many of the pertinent publications available at that time or in the months since.

EIGHT QUESTIONS

Eight sets of questions were offered to the conference participants as a scaffold for the meeting. These were questions that frequently teased and plagued officials responsible for reporting on drug abuse in the nation and seemed to cry for answers from the research community:

1. The first was an old but still unanswered question: What is the extent of opiate addiction in the U.S.? How much of it is hidden? What proportions are treated and untreated? How much use is there at different levels of frequency and regularity, from chipping to hard-core addiction?

2. How accurate are the sample survey data now abundant? More specifically, how well do they reflect the actual extent of use or abuse, in all groups? Can they accurately chart changes over time?

3. How can the adverse consequences of drugs be measured as they occur among users? Specifically, how many accidents result from drug use? How many hospitalizations, bad trips and deaths? Can the serious methodological problems be overcome?

4. How accurate are data on clients in treatment? Can this information be used for national allocation of resources?

5. Is it possible to predict drug abuse epidemics? Further, can the next new fad be predicted?

6. Can states and cities assess their own drug abuse problems accurately, at a reasonable cost? Is it realistic to try?

7. Are there indicators of drug abuse accurate enough to assess incidence or prevalence? Of those currently used, which are the best?

8. How can one resolve conflicting estimates of the same phenomenon?

OVERVIEW

It is fair to say that the conference participants, if they did not answer these questions, made valiant attempts to address them. At the same time, they commented on other developments of a conceptual or philosophical nature, as would be expected of scientists whose responsibility is to put practical questions in a thoughtful context.

One conceptual issue was the state of operational definitions in this field, dealt with by Elinson; another was the term "polydrug use," covered by James Sample, both in Section I. A number of philosophical questions raised by Nowlis in Section V gave pause to the purposes of the entire endeavor, asking whether epidemiological statistics can actually serve the cause of ameloriation of drug abuse.

The methodological issues covered in Section II reiterated some perennial difficulties in achieving high return rates and preventing bias, but authors Cisin and Robins also conveyed guarded optimism about the possibility of obtaining high quality data from surveys. Johnston and Kandel spoke on survey problems from experience in conducting longitudinal studies over several years' time, commenting on sample attrition, interpretation of changes, and adherence to rules on protection of human subjects. Crawford and Fitzpatrick related their experiences in looking at extent and nature of drug use from a different angle: the observation of users on the street. The shortcomings of validity and reliability often present in household or classroom surveys seem less a problem in observational methods, though these also fail to meet the ideal because they seldom achieve complete coverage of the population or employ standard instruments.

Representatives of special indicator data sources commented on their methodological problems of collection and interpretation. Savitz discussed one of the oldest sources in use, arrests of drug law violators. In connection with arrests, Newmeyer proposed the use of price-and-property data on street drugs, as an important clue to the state of a drug epidemic. Sells dealt with the problems of designing studies of treatment populations and with the possibilities of use of such data when available. Gottschalk presented preliminary findings from a comprehensive survey of drug-related deaths in major cities and Person described the National Drug Abuse Warning Network (DAWN) from which data on other adverse consequences such as hospitalizations are available. Alexander and Minichiello discussing the utility of serum hepatitis figures for estimating trends in drug use, were alternately pessimistic and optimistic on the topic. Finally, Berkowitz threw out a serious challenge to the assumption of accuracy in the urinalysis data used to screen or monitor drug abuse.

Once data are collected from a primary source, whether it is drug users in households, in classrooms, on the street, in physicians' offices, or in military settings, the estimation of extent and nature of the behavior is not automatic. Statistical manipulations of various kinds take place to portray the situation in a conventional but technically correct form that can be understood and used. These problems of relating and extrapolating data were undertaken in Section III. Glenn argued that some efforts to produce point estimates in numbers of persons, characterized in certain ways, had been misleading because the standard error was not taken into account. A second presentation by Greenwood pointed out the possibilities of using addict register data for estimating the total population of addicts, and Rootman commented on a number of uses of the Canadian Narcotics Registry. Lukoff was critical of the procedures used in keeping the New York Narcotics Registry up-to-date and recounted the difficulties he encountered in trying to use the Register for research purposes. Richman, on the other hand, reported on his successful use of the Register in connection with attempts to characterize treated addicts in ecological terms. Sample also described the use of management information on clients in treatment, in this case the nationwide Client Oriented Data Acquisition Process (CODAP), as a research tool.

In Section III, Chambers inserted a summary of major trends from the numerous state surveys conducted by Resource Planning Corporation, as an example of the product of epidemiological research. He pointed to a number of findings showing concentrations of certain kinds of behavior in special populations, and gave an example of how such data can engage treatment needs.

The final presentations of the meeting pointed toward possible new directions for the future as well as toward the accomplishments of the past. As mentioned earlier, Nowlis asked for a broader look at drug abuse than the epidemiological framework provides. Kramer suggested closer collaboration with epidemiological activity in mental health, and McGlothlin urged the merging of information on alcohol use with that of drug abuse for greater insight into trends in substance use. McGlothlin also recommended more longitudinal surveys in this field. Myrick described how the information on government supported research was classified and how one could find out about the types and amounts of research underway.

NEW INITIATIVES

With the opportunity to look back at accomplishments and expectations in Miami Beach, it is surprising how many of the eight questions are still waiting for answers. However, a number of salutary advances have occurred that bode well for the future of this enterprise. Taking each question in turn, the following new initiatives either have been completed or are progressing well:

1. A NIDA sponsored conference on heroin epidemiology was held at Stanford University in February, 1976, the proceedings of

which will be available in the near future (Rittenhouse 1976). Also, a completed study by O'Donnell sheds new light on the patterns of heroin use in a national sample of the population at risk in recent years (O'Donnell 1976).

2. The first study measuring validity of the interview method for incidence and prevalence of non-medical drug use in the general population has been completed (Abelson and Atkinson 1975). The results do provide confidence in the use of interview questions for most substances, the major exception being the measurement of heroin use. The National Survey of 1975 that followed the validity study made possible a third point in time for assessment of trends in use (Abelson and Atkinson 1975). The survey is now on an annual basis with the completion of the 1976 report (Abelson and Fishburne 1976).

3. The research territory of adverse consequences is still in need of development. A new initiative for developing guidelines and questionnaire items for measuring drug use consequences has recently been launched in NIDA.

4. The CODAP source of nationwide data on admissions to treatment has improved its coverage of programs dramatically since the Miami Beach meeting. Quarterly reports of admissions and other aspects of treatment are now available from NIDA (NIDA 1976).

5. A method for predicting future drug epidemics or fads is still primarily a hope and not an expectation, although a number of investigators have been experimenting with techniques for post-diction. Healthy debate goes on over the feasibility of use of treatment admissions data for predicting the incidence of heroin use in the community (Richman 1976).

6. As many as thirty states have conducted surveys of drug abuse to date and undoubtedly as many cities have done so too (Richards and Glenn 1976). The cost of conducting a reliable survey is high, and the expectation of obtaining trend data for local areas is unrealistic, for the most part. Several investigators have explored means of using existing indicator data for assessing need at the local level, but more development is needed (Chambers and Hunt 1976). A beginning has been made toward an index of drug abuse employing survey and census data, but this too needs much more effort devoted to it (Cohen 1974).

7. A good deal of attention has been paid to the question of interrelations among indicators since the meeting (Person 1976). As yet, there are no conclusive answers on which indicator, if any, might be the most accurate. This initiative should eventually result in knowledge highly useful to the field.

8. There is still a visible gap in the estimates provided from drug abuse surveys and from indicators. No one has been able to link one to the other in a useful way, but it is a worthy goal.

The examples of progress outlined above are satisfying indeed, but the unanswered questions do not allow relaxation of efforts to improve the state of the science.

THE "NUMBERS" CRITICISM

Throughout the Miami Beach meeting and at other meetings on the epidemiology of drug abuse, the theme of "number numbness" frequently appears. The theme is expressed in questions of why numbers seem important, and whether they lead to real understanding or merely serve to satisfy reporters and politicians. These queries led to some provocative discussion, and undoubtedly deserved a larger portion of the agenda. Since the time was not sufficient to expand on the theme at the meeting, some comments are offered here.

Non-medical use and abuse of psychoactive substances continue to evoke curiosity, fear and uncertainty, and continue as a topic for political debate. Thus, requests and demands for numbers representing the size and scope of the phenomenon have not abated. The need for numbers has been real and constant and admittedly constituted one impetus for the Miami Beach meeting. It is a legitimate need and the research community is a legitimate source of such information. The objection seems to be that the desire for numbers does not go far enough in providing an understanding of what the phenomenon of drug abuse means for individuals, the society and the culture. The implication is that numbers alone may short-change the contribution that research could and should make to public understanding.

Undoubtedly there is danger in blind acquiescence to requests for simple numbers representing the extent of drug use or abuse or changes over time. Often the consumers of such statistics are not interested in further explanation but only wish to comprehend size. Yet the projects designed for epidemiological purposes are seldom, if ever, limited to a few estimates of size. The surveys commissioned by the government, for example, always include as many explanatory variables as feasible and always report results by sociodemographic correlates, at the least. Providers of estimates should routinely offer adjunctive information along with numbers to serve subtly as a kind of consumer education.

Even the numbers themselves can improve understanding, if couched in the proper language. Numbers never stand completely alone - they are always presented in a semantic context; that is, they are always numbers *of* something. In earlier years of the current drug abuse period (since 1965), numbers of drug abusers were almost always reported as those who had "ever used" a substance, a reflection of lifetime experience. Experts with intimate knowledge of drug abuse knew that those numbers were an exaggeration of the actual "problem." Since then, there has been a slow, but definite movement toward reporting current use along with lifetime experience, and reporting of new users or other measure of incidence

along with prevalence figures. In addition, the reporting from DAWN and other monitoring systems complements the survey data on non-medical use by emphasizing only the "abusive" features. Thus, the types of numbers chosen for reporting and the accompanying terminology in and of themselves, have improved general understanding. It should be the practice of scientists to report as much as possible of this context or qualifying data along with the simple numbers requested by others.

The fact that the epidemiology of drug abuse has come into its own does not mean that it is the whole story of the drug abuse phenomenon. Many psychosocial and clinical studies employing a myriad of variables have been completed or are in progress and these will ultimately have more elucidative importance than studies of the extent and nature of the behavior. Nevertheless, science has always proceeded from description to explanation, and the description is essential. The Miami Beach Conference has helped a number of scientists do a better job of description in the ensuing years.

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I. ISSUES UNDERLYING INCIDENCE AND PREVALENCE ESTIMATES

Status of Operational Definitions

Jack Elinson, Ph.D.

One of those "nuts and bolts" conferences that Louise Richards referred to took place about a year ago. One of the "nuts" present, namely myself, felt that in the discussion about some of the general problems affecting survey research in this field -- specification of the population, sampling schemes, response rates -- the question was raised as to whether there was any agreement on certain definitions of terms used in survey research on drug use. The group agreed that there was no agreement on the way in which sample surveys of general populations defined "drug usage." It was felt that it might be useful to create an *ad hoc* committee to work toward the possibility of achieving more comparable usage in survey research, such that the knowledge gained could be more cumulative than it is now.

Among the several questions posed by Louise Richards, this committee addressed the one on the utility of sample survey data. Utility might be increased if definitions were more comparable. It also addressed the issue of conflicting estimates, which has to do not only with definition, but also with other things. These then are the two major questions being addressed by this committee. Some of those present there -- about ten people -- expressed a special interest in forming themselves into an *ad hoc* committee which has met now about three times. What is being given to you here today is a report of these three meetings, and the extent of work so far.

At the first meeting of this group -- five or six of whom are here today -- the entire day was spent just discussing what drugs ought

to be paid attention to and what drugs should we be worried about, without coming to agreement on that one day. Before the second day's meeting, which took place some months later, it was felt that perhaps a review of existing studies (survey research in recent years) that have dealt with the issue of drug use research be done and be placed before the committee for an overview of the way in which terms are actually used. Accordingly, a research associate and I, and two students, reviewed some 50 studies which met certain criteria: for example, the study had to do with survey research; it had to do with populations of a general sort -- not too special; it had to do with a survey of both users and non-users; these non-users had to be very prominently featured in the population studied; and it had to have resulted in some publication of an open variety, whether in printed form or in progress reports to a government agency (that is, available to the general scientific community, not merely inhouse or agency reports). Some fifty of these met the criteria. You will notice the principal exclusion were studies which dealt with known drug addicts. Definition of terms required in studies of known drug addicts was much more specific, much more refined, and much more detailed than could be applicable to general populations. It was felt that another committee, another group, should be concerned with that highly important population of drug addicts or drug users in treatment, and ways of approaching some comparability. In point of fact there have been such efforts.

Approximately 15 concepts were identified. These were:

- User, ever
- Frequency/quantity
- Onset of use
- Recent or current use
- Interest in trying, maintaining, or changing use
- Typology of use
- History of use
- Polydrug use
- Methods of use
- Conditions of use
- Reasons for use
- Reasons for non-use
- Effects
- Pathology
- Availability

These fifty studies were reviewed with regard to their use of these concepts and terms. Just to show by way of illustration the variation that exists in even the most objective of these concepts, namely "ever use," the following are some of the ways in which some of the studies refer to it. These are among the various questionnaire phrasings: "Did you ever use (selective) drugs?" "Do you now or have you ever used, or do not use now (selective) drugs?" "Which drugs have you taken at least once when they were not prescribed for you by a physician or received during medical treatments, but you use larger doses or more frequently than directed

and that you use with a specific intention of getting high?" (These are all different surveys, not in the same survey.) "Up until today, how many times have you used marihuana in any of its forms -- grass, pot, hash, etc.?" In another phrasing: "Have you ever used this drug or class of drugs?" And an agree/disagree type of statement: "During my lifetime I have used the following drugs without a medical prescription," etc. This report, entitled the *Operational Definition of Terms of Drug Use Research*, is merely a compilation and an examination of the different ways in which terms have been used in recent survey research.

Following the presentation of this report at the second meeting various members of the *ad hoc* committee were assigned the task of taking one of these concepts and working up "recommended" ways of approaching it, with some rationale. The people who were involved, just for the record, were: Denise Kandel on measurement of "ever use" and frequency/quantity, Lee Robins on history of drug use, John O'Donnell on effects of drug use, Charles Winick on typologies of drug use, and Gail Crawford on conditions of drug use. Subsequently, the following were added: Louise Richards on measurement of currency or recency, Lloyd Johnston on measurement of polydrug use, and Mildred Bateman and Roger Meyer on a functional taxonomy of drugs.

We have working papers from the group on these subjects and the next meeting (the third) of the *ad hoc* committee, was supposed to deal with reviewing these papers and coming to some recommendations. Maybe you can anticipate what happened. We were able to deal with only one of the papers in one day, namely the functional taxonomy of drugs. These individualistic investigators undertook their assignment in the way they do their work -- instead of coming up with recommendations, they analyzed the problem further, looked at the way things were conceptualized and suggested other ways of conceptualization. The analysis was full of qualifications, limitations, problematics and so on, which leads us a long, long way from the accomplishment of arriving at some, not "standard," but even some recommended, comparable use of terms in this field.

Prior to the meeting I ended the little report that we did on the operational definitions of drug use with this paragraph which I think still fits even after the meeting: "It is clear that in the current stage of socio-cultural and socio-psychological research on drug use, there is little consensus among investigators as to operational definition of the same concepts and terms. There is of course more agreement with respect to some terms than others." Still, I think Denise Kandel showed us in reviewing the more objective ones, such as frequency of use, that among ten or so investigators there were not two who used exactly the same frequency-of-use vocabularies, although some were transposable into the others. Even in the case of objective experiences, it was remarkable how many different ways concepts or terms are reported. It would seem that at least in such cases a greater degree of consensus among the investigators could be relatively easily achieved without sacrificing either concepts or findings. That is the optimistic statement.

In other cases, especially those which attempt to get at motivation, it is fairly obvious that a consensus would be more difficult to achieve.

We are now preparing for another meeting next month. This time we are allowing two whole days in which we have six more concepts to concern ourselves with. I think we still agree that we do not yet have agreement.

(Editorial Note: The working papers referred to in Elinson's presentation have been published as *Operational Definitions in Socio-behavioral Drug Use Research,* Jack Elinson and David Nurco, eds., National Institute on Drug Abuse Research Monograph Series 2, Rockville, Maryland, 1975.)

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DISCUSSION

Dr. Richards: I should be corrected on the use of the word "Standard" in connection with Elinson's Committee. That was wishful thinking more than anything else. What advice do you have for people who might be in the business of constructing a questionnaire or initiating research in this field? You gave a gloomy conclusion from the work up to this point. Do you have any advice?

Dr. Elinson: The first bit is just to be aware of what others have done. That is partially covered in this document in which someone looked at approximately fifty other studies and put it down -- awareness is first. The second thing -- I am not so gloomy about it as you think because this is really early stuff, as far as epidemiology goes in the field. I have previously worked in the field of cardiovascular disease where it took an international commission perhaps ten years to arrive at some international definitions which people might repair to. The question really is, as I see it, whether or not this is a field in which one ought to strive toward serious research such as that of vascular disease, in which case it will take some time and effort to arrive at some consensus. Is it worth that much? Or should it stay primarily pretty much where it is -- the field of journalistic polling, the sociological fashions in research? I mean this is what's going on in these years. Of course, the answer may be someplace in between; that's about where I stand.

Dr. Robins: One of the problems we were hassling with was the problem of "current?" The definition varies between two weeks to a year. Some people when they speak of a "user" are talking about use within the last year. Some are talking about anytime in their lifetime. Some are talking about a shot of something in the last 48 hours. These obviously are not comparable. The problem is comparability from one study to another.

Dr. Kramer: You don't get involved in dosage or level of use and so on?

Dr. Robins: That is another problem. That is more refined than the one concerning use within an amount of time. The amount of time is never the same from one study to the next.

Dr. Johnston: If you are trying to address a particular question and standard measures that exist are not going to do that, obviously you are going to develop measurements appropriate for that. On the other hand, I think that a lot of people develop measures and really do not care much whether it is measure A, B or C; they all sort of address the same general point the person wants to get at. A provision that a set of standards be recommended in the field would allow, for example, a number of studies all using "A" to do a better job on integrating their findings. What the committee is trying to do is come up with recommendations that I think make sense as general measures, where it is possible, and I suspect in some of those areas we will conclude it is not possible, that is, the investigators will have two different kinds of purposes.

Dr. Richman: In regard to the question of "abuse" in our New York City area -- "use" brought low response; "try," "have you taken" -- brought high response. Surveys should employ verbs other than "use."

Dr. Robins: We had the same experience. We scratched out "use" every time we used it. Quite often this is misunderstood. "Taken" is still innocuous; however, next month that may not work.

Dr. Richards: I remember a really nice illustration of this softness in definitions in a study of marihuana taking or use, about 1968, which asked about people who had used it one or more times -- those people fell into the "ever used" group, of course. This included some experimenters, and some with more frequent use. Then a couple of years later, someone did a study which, because marihuana use had increased so much, categorized people who had used it once or twice as the "non-user" group. This shifting may not be going on extensively now, but you feel that you are walking on eggs when you compare these kinds of results.

Dr. Sells: I would like to make a few comments about some other variables that might be relevant to a study of standard definitions. One is the use of a manual of jargon that may accompany interview questionnaires where the language varies widely. A second question involves the method of data collection, e.g., self-report questionnaires; they may yield results quite different from interviews. And finally I would like to ask Dr. Elinson what criteria he uses for deciding on the appropriate definitions to adopt.

Dr. Elinson: On the issue of a manual of jargon, I think this useful for educational purposes for the investigator. Such a manual can get rather thick, especially if you go into other languages, and other localities and over time. We have been having trouble trying to adapt an English questionnaire which we use in the U.S. to Puerto Rico, then back from Puerto Rico to high schools in New York City where there are Puerto Ricans, Dominicans and Cubans. The word for a pack of cigarettes becomes an issue worthy of an academy of Spanish language. If it's for educational purposes, I say fine. Let people be at least aware of the different kinds of jargon.

On the business of self-report versus interviews, we will be coming out with a report shortly on just this issue, at least in high schools where we are concerned with the same question.* We thought we were getting much higher yields of use of drugs in self reports and questionnaires and honest questionnaires in classrooms in high schools than we were in the interviews as reported in household interviews around the country for people in this age group. It turns out however, that when we tried it out in the same city using the same population, the same sample, and these two different methods, at least with the questions that we used at that level, there were no significant differences.

On the issue of criteria, i.e., how are criteria in science developed for selection of measures? How did one finally decide to use a platinum meridian bar tested at a certain temperature located in Paris to be a measure of length of a defined sort? It came about through an authoritative scientific body with consensus of that scientific body. We are nowhere near that. That is why I differ from Dr. Richards' characterization: that is, it is pessimistic. It is just a start and whether it's worthy of this amount of effort, is a question I put before all of you.

It's not the accuracy that we are dealing with so much as comparability where one investigator says that there is a relationship between A and B, and the second person says there is not. We would be able to look at these two statements and appraise them in terms of the fact that they are both talking about "A" and they are both talking about "B," whereas if the second one is talking about "A" and "B," then they are both true. Go back and solve Dr. Sell's criterion in which you look at the data of each and every investigation, and it becomes an epic in itself, a poem, a piece of art, a piece of sociological interpretation, a journalistic report. Each is judged on its own merits, and everyone somehow arrives at a belief in what is, or an understanding of what is going on. By lack of cumulation, the basic scientific question is lost in the terms of a unique phenomenon. If we can move toward cumulation, we need to see some kinds of things that we can say are comparable. They can be equally inaccurate. Precision should only be precise for a purpose. You want also to avoid creating straightjackets, standards, dogma. We don't want this to become something which every applicant or grant application must use or else they won't get their grants.

Dr. Kramer: Since you are dealing with epidemiologic issues, there are a few epidemiological indicators we are talking about. I think these must be defined so you know what you are getting and comparing and what these measures in two different populations might indeed mean. If I might put the three or four terms on the board it might help clarify some of the issues involved, because this problem of "ever use" or "ever tried" is comparable to the concept that

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*Anne Zanes and Euthenia Matsoukas. Comparison of reported drug use in self-administered questionnaires at home and school. Article submitted for publication, 1976.

was introduced into psychiatric epidemiology which is called *lifetime prevalence*. Then you also have the other forms of prevalence which would be the *point prevalence* and we have another concept called *interval prevalence*. All of these things are functions of the rate at which the thing that you are looking at will occur, which is called *incidence*. They are also related to a variable called *duration*. Lifetime prevalence simply means, just generally speaking, how many people have ever had a history of doing something. If you go into a population as of today, you would ask how many people had used a given drug. It is a function of the incidence and the duration which is also a function of mortality in a population. I was trying to think of a drug that might have a zero lifetime prevalence in a live population -- cyanide is one. Go into a population and ask how many people have ever used sodium cyanide or potasium cyanide and you'll find a zero prevalence. Here you get into the concept of mortality, because in certain populations you may find that people are dying off more rapidly than in another population for a variety of reasons -- whether using drugs or not -- so this duration does have an effect. Also if you are looking at institutional populations versus the general population you have another problem. Because if you have everybody who has ever used a drug in prison and you are doing a population survey, you would never find a lifetime prevalence in the general community. The same thing is true of any kind of condition that has a very high fatality rate. If you go into a population where anybody who had developed an abnormality was killed off, you would find a zero prevalence in that particular population. Point prevalence means how much do you have as of a given point in time? And that would mean -- are you looking for people who are actually using the drug as of today or used it in some prior interval, etc. So as of a given point in time, interval means what it says -- how many people were doing something within that interval? That could be one year, six months, one day or whatever it may be. It depends on the question you are asking. Incidence would mean the rate at which people are first using a compound or whatever it may be within some defined interval of time. Duration gets involved in any of these things because it relates to how long are you interested in studying this, due to the fact that prevalence is a function of interval times duration. I do not see how you can get away from raising the question of what it is you really want to measure. If you're measuring it in two different groups, and you get lifetime prevalence, you want to determine whether it is due to differences in incidences or duration and also take into account the fatality rate. So I do think you have to define which epidemiological indicator you want to use and why you want to use it in order to get comparable measures. I think it's important to keep these measures in mind because they have led to a lot of confusion in psychiatric epidemiology. If you get into societies where people have a very high fatality rate, for any reason -- for example in dealing with putting people in institutions and institutional care being terrible, and people dying off very rapidly -- you are getting into that kind of a comparison. So I do urge you to think through the kinds of measures you are trying to get in order to determine what you want to do in terms of standardizing your definitions.

Dr. Greenwood: I am interested in both of these problems of prevalence and incidence and I think I understand what you have been saying. And, I would like to tell you my definition. Prevalence: in a given, stated interval of time it is the number of people who have that property sometime during that interval. Incidence is the same, practically: it is the number of new ones who come in during that interval. That is unequivocal, too. Both of the past investigations I have done have been based on the theory of probability; it is basically the probability that a given person with a property will do so and so, e.g., get arrested, etc. The basic assumption is equal probability in equal time for all addicts.

Dr. Gottschalk: I want to address myself also to the question of definitions. What is a drug? Is alcohol a drug? What is abuse? Was the drug prescribed? Was the prescription made out to the person or someone else? Are drugs to be limited to the illegal which in my opinion are just a small portion of the problems related to abuse. There needs to be some clarification of what constitutes drug abuse.

Dr. Elinson: This Committee has not dealt with the issue of abuse but we hope to be able to say something about the medical or non-medical conditions under which drugs are used. One further question with reference to a given concept: Even after agreement has been reached on its definition, how do you operationalize it? Again, this practical issue varies from investigator to investigator and the need for consensus exists.

Concept of Polydrug Use

C. James Sample, Ph.D.

Charged with the responsibility to present a paper on polydrug use following a paper by Dr. Elinson on Operational Definitions, I had hoped for some assistance in defining what is meant by the term polydrug use. Currently used definitions of polydrug use or abuse cover a wide range, with no real consensus favoring any one definition. These definitions range from the NIDA treatment definition of use of an illegal drug or drugs other than the opiates to a definition more common to the literature reflecting use of many drugs without necessarily a strong preference for any particular drug. At the risk of adding still another definition to the long list, I will use the term polydrug abuse to mean the use of more than one drug, excluding heroin, which are used simultaneously and with a frequency of use of at least once per month. Regular use of heroin, regardless of how many other drugs are also used will not be classified as polydrug use for my purpose here. It is recognized that this definition is no better or worse than many others. The definition of polydrug use will vary depending on the subject matter and purpose for which it is being used. The intention of this paper is to establish the trends in polydrug use as defined above, utilizing as many data sources as possible.

Other than various surveys that have been done, there is no currently available data source that accurately or appropriately addresses polydrug use. The Drug Abuse Warning Network (DAWN), for example, may be the only data source with the potential to accurately reflect trends in polydrug use. However, because of the concentration on drug mentions rather than episodes, it is not possi-

ble to obtain recurring information on the combinations of drugs being used. Medical Examiner reports from the DAWN system showed that of the 933 drug related deaths during April, May and June of 1974, 69 percent involved use of more than one drug. These data are consistent with reported drug abuse treatment data reported below, but should not be considered to represent polydrug users.

The Client Oriented Data Acquisition Process (CODAP) provides current data on the combinations of drugs used by clients as they enter treatment. With any self-reporting system it is often difficult to distinguish between minor differences in the interpretation of responses. The largest problem of concern here is the distinction between historical use and concurrent use of various drugs. Clients upon admission to treatment have a tendency to overstate their drug problem. In the revised CODAP system we have attempted to be more specific in obtaining responses as to their current drug use. This will be covered below.

Table 1 provides the primary drug of abuse on admission reported under CODAP between April 1973 and December 1974. Most noteworthy is the decline in the relative importance of heroin admissions and the increasing importance of marihuana and hashish as the primary drug problem of clients admitted. The drug categories of illegal methadone, other opiates, barbiturates, and amphetamines have maintained the same relative importance. Clients admitted for alcohol abuse have nearly doubled in relative importance but still account for only 5.1 percent of the clients admitted to treatment under CODAP. This provides a rough description of the trends identified by CODAP since its inception in 1973.

TABLE 1

PERCENTAGE DISTRIBUTION OF REPORTED PRIMARY DRUG USE
APRIL 1973 THROUGH DECEMBER 1974

CODAP Reporting Quarter	Heroin	Illegal Methadone	Other Opiates	Barbiturates	Amphetamines	Alcohol	Marihuana/ Hashish
Apr. - Jun. 1973	62.9	0.9	1.8	7.0	4.0	2.7	12.2
Jul. - Sep. 1973	62.1	1.4	2.0	6.1	5.2	2.6	11.5
Oct. - Dec. 1973	55.4	1.4	1.8	6.6	4.5	3.3	14.7
Jan. - Mar. 1974	55.5	1.3	1.7	5.9	4.5	4.7	15.3
Apr. - Jun. 1974	58.7	1.3	1.5	4.8	4.0	4.6	15.0
Jul. - Sep. 1974	56.7	1.2	1.6	4.8	4.0	5.1	15.2
Oct. - Dec. 1974	48.7	0.4	2.4	5.1	5.6	5.1	21.1

SOURCE: CODAP Quarterly Summary Reports

Opiate use of clients admitted to treatment has generally not been in combination with non-opiate use. Tables 2 and 3 provide data on the combinations of opiate use and non-opiate use for the quarters April through June 1974 and October through December 1974, respectively. There is little difference in the relationships shown for these two quarters. In each case, more than 95 percent of the reported heroin use is as the primary drug problem.* The use of illegal methadone and other opiates is either as a primary drug or as a secondary or other drug to heroin. As shown by the last column of Tables 2 and 3, opiates are not generally used as a secondary or other drug to the non-opiate. Where these are reported as secondary to non-opiates, I must assume that they are used less often than once per week to be consistent with the definition expressed above. This is an assumption and cannot be supported by available data at this time.** However, this assumption may not be unreasonable. Consistently, 20 percent of the reported heroin users have reported use of this drug less often than once per week.

TABLE 2

COMBINATIONS OF OPIATE AND NON-OPIATE USE
(CODAP Admissions, April through June 1974)

Category	Total Reported Use	Used as Primary	Used as Secondary or Other Drug with an Opiate as Primary	Used as Secondary or Other Drug with Non-Opiate as Primary
Heroin	18,707	17,859	223	625
Illegal Methadone	1,812	409	1,302	101
Other Opiates	1,600	448	983	169
		(PERCENT)		
Heroin	100	95.5	1.2	3.3
Illegal Methadone	100	22.6	71.6	5.6
Other Opiates	100	28.0	61.4	10.6

SOURCE: Calculated from the CODAP National Summary Report for April through June 1974.

*This is subject to the limitations presented in another discussion in this volume entitled "Institutional Data -- CODAP" (Section III).

**The revised CODAP system, however, does have the capability to test this assumption.

TABLE 3

COMBINATIONS OF OPIATE AND NON-OPIATE USE
(CODAP Admissions, October through December 1974)

Category	Total Reported Use	Used as Primary	Used as Secondary or Other Drug with an Opiate as Primary	Used as Secondary or Other Drug with Non-Opiate as Primary
Heroin	8,584	8,341	79	164
Illegal Methadone	272	71	187	14
Other Opiates	885	404	399	82
		(PERCENT)		
Heroin	100	97.2	0.9	1.9
Illegal Methadone	100	26.1	68.8	5.1
Other Opiates	100	45.6	45.1	9.3

SOURCE: Calculated from Table 8 included in this paper.

Tables 4 and 5 provide an estimate of polydrug abuse based in the definition expressed earlier. Data are provided for the periods April through June 1974 and October through December 1974 and are also based on CODAP reports. Although there has been some shifting in the relative importance of some drug categories between these periods, the estimate that 22 percent of CODAP admissions fall into the category of polydrug users as defined for this paper is consistent for the two quarters. In each case, 8.8 percent of the defined polydrug users report marihuana or hashish as their primary drug for which treatment was sought. Discounting the marihuana clients as probably not in need of drug abuse treatment -- which is also the current NIDA policy -- leaves approximately 14 percent of treatment admissions classified as polydrug users in need of treatment. These are listed by reported primary drug type by Tables 4 and 5. The specific combinations of non-opiate drugs used will be discussed below at which time we can further define the need for polydrug treatment.

TABLE 4

POLYDRUG ABUSE WITH NON-OPIATES AS THE PRIMARY DRUG
(CODAP Admissions, April through June 1974)

	Total Clients with Primary Drug	Percent of Total Admission	Percent of Admission Reporting Only Primary	Percent of Admission Consistent with Definition of Polydrug Use
Alcohol	1,385	4.6	2.4	2.2
Barbiturates	1,474	4.8	1.0	3.8
Amphetamines	1,216	4.0	0.6	3.4
Cocaine	301	1.0	0.2	0.8
Marihuana	4,574	15.1	6.3	8.8
Hallucinogens	778	2.6	0.4	2.2
Psychotropics	222	0.7	0.4	0.3
Inhalants	234	0.8	0.4	0.4
Non-RX Over the Counter	60	0.2	0.1	0.1
TOTAL	10,244	33.8	11.8	22.9
TOTAL LESS MARIHUANA	--	--	--	13.9

SOURCE: Calculated from the CODAP National Summary Report for April through June 1974.

TABLE 5

POLYDRUG ABUSE WITH NON-OPIATES AS THE PRIMARY DRUG
(CODAP Admissions, October through December 1974)

	Total Clients with Primary Drug	Percent of Total Admission	Percent of Admission Reporting Only Primary	Percent of Admission Consistent with Definition of Polydrug Use
Alcohol	866	4.9	2.8	2.1
Barbiturates and Other Sedatives	1,217	6.9	1.9	5.0
Amphetamines	954	5.4	2.2	3.2
Cocaine	178	1.0	0.4	0.6
Marihuana	3,619	20.4	11.6	8.8
Hallucinogens and Psychotropics	549	3.1	0.9	2.2
Inhalants	281	1.6	0.9	0.7
Non-RX Over the Counter	84	0.5	0.3	0.2
TOTAL	7,748	43.7	21.0	22.7

SOURCE: Calculated from Table 8 included in this paper.

Although a clear understanding of the current drug use patterns is important in order to establish the size and type of treatment resources needed, knowledge of trends or changes in trends is also important. Prior to the implementation of CODAP the most complete and respected drug abuse treatment data base was the Drug Abuse Reporting Program (DARP) operated by Dr. S.B. Sells. The research results of this effort have recently been published in a two volume series.* In an analysis of polydrug patterns, Sells and Simpson define multiple or polydrug users as:

> Users who reported using at least three of six drug classes (excluding heroin and other opiates). The polydrug users who reported no use or less than weekly use of heroin were placed in . . . (a "poly" classification), while those who reported more frequent use of opiates were placed in . . . (a pattern termed "poly plus opiates").**

* S.B. Sells, *The Effectiveness of Drug Abuse Treatment*, Volume I, *Evaluation of Treatments* and Volume II, *Research on Patients Treatments and Outcomes*. Ballinger Publishing Company: Cambridge, Mass., 1974.

** S.B. Sells, Volume 2, pp. 180-181.

These definitions differ from the one used here, but, at admitted risk, I will draw some comparisons. Table 6 sets forth these comparisons, some of which are more valid than others. The most valid and the most frequent category in both reporting systems is the use of heroin alone. The DARP system for the period June 1969 through June 1971 records this category of use as representing 28.3 percent of admissions, CODAP for the quarter October to December 1974 is 29.6 percent of admissions. No drug use reported under DARP was 6.9 percent of admissions whereas CODAP shows this at 2.9 percent of admissions. The use of heroin and cocaine or marihuana does not seem to have changed much over the last four years. It is difficult, with the currently available data summaries from the revised CODAP system, to accurately calculate similar information from the DARP summaries when three or more drugs used in combination are defined. Therefore, I have attempted to define rough ranges for these two cases. In each of these cases shown on Table 6, the DARP estimate falls within the range estimated from CODAP. The definition of polydrug used by DARP is too different from that used here to draw any really valid comparison.

TABLE 6

COMPARISONS OF DRUG USE PATTERNS
(percent of admissions)

DRUG USE TYPE	DARP 6/69 - 6/71	CODAP 9/74 - 12/74
Heroin Only	28.3	29.6
Poly and Opiates	13.3	10.6 - 17.5*
Heroin and Cocaine	8.8	6.9
Heroin and Marihuana	7.8	10.5
Opiates plus Non-Opiates	7.8	<15**
Heroin, Cocaine, Marihuana	7.3	6.6 - 12.5***
No Use	6.9	2.9
Polydrug	4.9	--

*This range was estimated based on data from Table 8 included in this report. The higher estimate is based on the number of opiate users also reporting other drug use. The lower estimate excludes opiates as secondary or other drug use.

**Only the upper limit of this category can be calculated with current outputs from the revised CODAP system although the system is capable of providing this data.

***This range was calculated from the source document from which Table 8 included in this report was calculated. The lower estimate assumes that, in CODAP terminology, cocaine was used as a secondary drug and marihuana was used as the other drug. The higher estimate relaxes this assumption by including heroin and cocaine and heroin and marihuana.

Admittedly, these comparisons are not precise and with a few exceptions are subject to interpretation. However, I believe that the data do show significant consistency in that it is unlikely that there have been major changes in drug use patterns between the periods covered by these two data systems.

I mentioned earlier that clients have a tendency to overstate drug use at the time of admission to treatment. Tables 7 and 8 establish this trend. Table 7 provides reported drug use combinations regardless of whether or not any frequency of use of the secondary or other drugs at the time of admission is reported. Table 8 provides the same information, but requires that the secondary or other drugs are used at least once per month at the time of admission. As can be seen from comparison of these two tables, the use of only the primary drug and no others increases from 41 percent (Table 7) to 55 percent (Table 8). More dramatically, the use of heroin alone increases from 48 percent to 62 percent of all heroin admissions or from 23.6 percent of total admissions to 30.6 percent of total admissions. This relationship holds for most of the primary drug problems reported.

The picture of multiple drug use of non-opiates is primarily one where marihuana or hashish is reported as one of the drugs being used (Table 8). These combinations, in order of relative importance consist of:

Drug Combinations	Number	Percent of Total Admissions
Marihuana and Alcohol	1,056	6.0
Amphetamines and Marihuana	716	4.1
Marihuana and Hallucinogens	666	3.8
Marihuana and Barbiturates	666	3.8
Barbiturates and Alcohol	253	1.4
Amphetamines and Alcohol	224	1.3

Based on the definition of polydrug abuse set forth above, polydrug users in need of treatment currently account for 13.9 percent of total admissions (Table 4). However, this 13.9 percent estimate was based on the assumption that the primary drug problem was a non-opiate, excluding marihuana and that there was also an additional drug problem. As can be seen from the drug combinations listed above and from Tables 8A and 8B, marihuana is listed in highest frequency as the secondary or other drug to the non-opiate primary drugs reported.

The current treatment population, on the basis of these data, may therefore be characterized as:

- consisting of 50 percent heroin users of which 62 percent use no other drug at the time of admission.

TABLE 7 A

COMBINATION OF DRUG USE REPORTED AT ADMISSION TO TREATMENT: TOTAL DRUG PROBLEMS REPORTED

(October - December 1974)

PRIMARY DRUGS	TOTAL PRIMARY	SECONDARY AND OTHER DRUGS -- TOTAL REPORTED DRUG USE*												
		NONE	HEROIN	METH.	OTHER OPIATES	ALCOHOL	BARB.	OTHER SED.	AMPH.	COCAINE	MARIJ.	HALLUC.	INHAL.	O-T-C	OTHER
NONE	521	521	--	--	--	--	--	--	--	--	--	--	--	--	--
HEROIN	8,341	4,033	--	254	583	590	1,134	182	518	1.183	1,795	362	12	67	27
ILLEGAL METHADONE	71	13	38	--	11	2	16	5	2	3	7	6	0	6	0
OTHER OPIATES	404	132	91	7	--	23	101	32	46	18	68	34	3	4	2
ALCOHOL	866	314	28	0	2	--	113	45	69	11	330	60	6	9	150
BARBITURATES	878	151	80	7	31	203	--	95	190	38	349	133	11	16	46
OTHER SEDATIVES	339	110	8	0	10	84	36	--	48	12	109	24	2	6	6
AMPHETAMINES	954	152	73	2	32	187	164	44	--	59	497	234	9	3	22
COCAINE	187	31	34	1	5	19	38	4	28	--	75	33	1	1	11
MARIJUANA	3,619	1,302	104	7	52	879	529	133	478	108	--	636	58	25	533
HALLUCINOGENS	549	65	24	2	14	81	127	32	135	30	327	--	7	1	1
INHALANTS	281	144	5	0	0	39	25	7	18	3	89	13	--	5	0
OVER-THE-COUNTER	30	19	2	0	00	3	3	2	0	0	3	0	1	--	1
OTHER	84	42	16	0	2	9	7	0	8	3	13	5	1	1	--
TOTAL	17,115	7,029	503	280	742	2,119	2,293	581	1,540	1,468	3,662	1,540	111	144	799

* Drug use reported at admission may have been discontinued prior to admission.

TABLE 7B

COMBINATION OF DRUG USE REPORTED AT ADMISSION TO TREATMENT: TOTAL DRUG PROBLEMS REPORTED

(October - December 1974)

Percent

PRIMARY DRUGS	TOTAL PRIMARY	PERCENT OF SECONDARY AND OTHER DRUGS BY PRIMARY DRUG CATEGORY -- TOTAL REPORTED DRUG USE *													
		NONE	HEROIN	METH.	OTHER OPIATES	ALCOHOL	BARB.	OTHER SED.	AMPH.	COCAINE	MARIJ.	HALLUC.	INHAL.	O-T-C	OTHER
NONE	521	100	--	--	--	--	--	--	--	--	--	--	--	--	--
HEROIN	8,341	48.4	--	3.0	7.0	7.1	13.6	2.2	6.2	14.1	21.5	4.3	0.1	0.8	0.3
ILLEGAL METHADONE	71	18.3	53.5	--	15.5	2.8	22.5	7.0	2.8	4.2	9.9	8.4	0.0	5	0.0
OTHER OPIATES	404	32.7	22.5	1.7	--	5.7	25.0	7.9	11.4	4.5	16.8	8.4	0.7	1.0	0.5
ALCOHOL	866	36.3	3.2	0.0	0.2	--	13.0	5.2	8.0	1.3	38.1	6.9	0.7	1.0	17.3
BARBITURATES	878	17.2	9.1	0.8	3.5	23.1	--	10.8	21.6	4.3	39.7	15.1	1.3	1.8	5.2
OTHER SEDATIVES	339	32.4	2.4	0.0	2.9	24.8	10.6	--	14.2	3.5	32.2	7.1	0.6	1.8	1.8
AMPHETAMINES	954	15.9	7.7	0.2	3.6	19.6	17.2	4.6	--	6.2	52.1	24.5	0.9	0.3	2.3
COCAINE	178	17.4	19.1	0.6	2.8	10.7	21.3	2.2	15.7	--	42.1	18.5	0.6	0.6	6.2
MARIJUANA	3,619	36.0	2.9	0.2	1.4	24.3	14.6	3.7	13.2	3.0	--	17.6	1.6	0.7	14.7
HALLUCINOGENS	549	11.8	4.4	0.4	2.6	14.8	23.1	5.8	24.6	5.5	59.6	--	1.3	0.2	0.2
INHALANTS	281	51.2	1.8	0.0	0.0	13.9	8.9	2.5	6.4	1.1	31.7	4.6	--	1.8	0
OVER-THE-COUNTER	30	66.3	6.6	0.0	0.0	10.0	10.0	6.6	0.0	0.0	10.0	0	3.3	--	3.3
OTHER	84	50.0	19.0	0.0	2.4	10.7	8.3	0	9.5	3.6	15.5	6.0	1.2	0.0	--
TOTAL	17,715	41.1	2.9	1.6	4.3	12.4	13.4	3.4	9.0	8.6	21.4	9.0	0.6	0.8	4.7

* Drug use reported at admission may have been discontinued prior to admission.

TABLE 8A

COMBINATIONS OF REPORTED DRUGS USED AT ADMISSION TO TREATMENT:

SECONDARY AND OTHER DRUGS USED AT LEAST ONCE PER MONTH

(October - December 1974)

PRIMARY DRUGS	TOTAL PRIMARY	SECONDARY AND OTHER DRUGS -- FREQUENCY OF USE IS AT LEAST ONCE PER MONTH AT ADMISSION													
		NONE	HEROIN	METH.	OTHER OPIATES	ALCOHOL	BARB.	OTHER SED.	AMPH.	COCAINE	MARIJ.	HALLUC.	INHAL.	O-T-C	OTHER
NONE	521	--	--	--	--	--	--	--	--	--	--	--	--	--	--
HEROIN	8,341	5,239	--	184	395	492	745	140	274	823	1,391	224	7	45	9
ILLEGAL METHADONE	71	34	18	--	4	2	14	5	1	3	5	4	0	3	0
OTHER OPIATES	404	215	61	3	-	18	68	24	28	10	55	22	2	3	2
ALCOHOL	866	490	13	0	0	--	79	36	45	6	275	32	3	9	1
BARBITURATES	878	343	46	5	18	174	--	79	135	27	284	88	9	12	2
OTHER SEDATIVES	339	164	2	0	6	68	28	--	29	8	93	11	1	4	3
AMPHETAMINES	954	384	20	2	12	149	118	29	--	36	409	136	4	1	4
COCAINE	178	73	19	0	2	16	30	4	19	--	57	21	1	1	2
MARIJUANA	3,619	2,057	42	5	33	781	382	99	307	65	--	391	46	19	10
HALLUCINOGENS	549	165	11	2	10	71	89	24	102	19	275	--	7	1	1
INHALANTS	281	163	1	0	0	37	17	5	13	2	80	5	-	3	0
OVER-THE-COUNTER	30	22	1	0	0	2	2	1	0	0	2	0	0	-	1
OTHER	84	59	9	0	1	7	5	0	6	0	9	4	0	0	-
TOTAL	17,115	9,408	243	201	481	1,817	1,577	446	959	999	2,935	938	80	101	35

TABLE 8 B

COMBINATIONS OF REPORTED DRUGS USED AT ADMISSION TO TREATMENT:

SECONDARY AND OTHER DRUGS USED AT LEAST ONCE PER MONTH

(October - December 1974)

Percent

PERCENT OF SECONDARY AND OTHER DRUGS USED BY PRIMARY DRUG CATEGORY -- FREQUENCY OF USE IS AT LEAST ONCE PER MONTH AT ADMISSION

PRIMARY DRUGS	TOTAL PRIMARY	NONE	HEROIN	METH.	OTHER OPIATES	ALCOHOL	BARB.	OTHER SED.	AMPH.	COCAINE	MARIJ.	HALLUC.	INHAL.	O-T-C	OTHER
NONE	521	100.0	--	--	--	--	--	--	--	--	--	--	--	--	--
HEROIN	8,341	62.8	--	2.2	4.7	5.9	8.3	1.7	3.3	9.9	16.7	2.7	0.1	0.5	0.1
ILLEGAL METHADONE	71	47.8	25.4	--	5.6	2.8	19.7	7.0	1.4	4.2	7.0	5.6	0.0	4.2	0.0
OTHER OPIATES	404	53.2	15.1	0.7	--	4.5	16.8	5.9	6.9	2.5	13.6	5.4	0.5	0.7	0.5
ALCOHOL	866	56.6	1.5	0.0	0.0	--	9.1	4.2	5.2	0.7	31.8	3.7	0.3	1.0	0.1
BARBITURATES	878	39.1	5.2	0.6	2.1	19.8	--	9.0	15.4	3.1	32.3	10.0	1.0	1.4	0.2
OTHER SEDATIVES	339	48.4	0.6	0.0	1.8	20.1	8.3	--	8.6	2.4	27.4	3.2	0.3	1.2	0.9
AMPHETAMINES	954	40.3	2.1	0.2	1.3	15.6	12.4	3.0	--	3.8	42.9	14.3	0.4	0.1	0.4
COCAINE	178	41.0	10.7	0.0	1.1	9.0	16.9	2.2	10.7	--	32.0	11.8	0.6	0.6	1.1
MARIJUANA	3,619	56.8	1.7	0.1	0.9	21.6	10.6	2.7	8.5	1.8	--	10.8	1.3	0.5	0.3
HALLUCINOGENS	549	30.1	2.0	0.4	1.8	12.9	16.2	4.4	18.6	3.4	50.1	--	1.3	0.2	0.2
INHALANTS	281	58.0	0.4	0.0	0.0	13.2	6.0	1.8	4.6	0.7	28.5	1.8	--	1.1	0.0
OVER-THE-COUNTER	30	73.3	3.3	0.0	0.0	6.7	6.7	3.0	0.0	0.0	6.7	0.0	0.0	--	3.3
OTHER	84	70.2	10.7	0.0	1.2	8.3	6.0	0.0	7.1	0.0	10.7	4.8	0.0	0.0	--
TOTAL	17,715	55.0	1.4	1.2	2.8	10.6	9.2	2.6	5.6	5.8	17.1	5.5	0.5	0.6	0.2

- consisting of 21 percent that list marihuana as their primary presenting problem. Of these marihuana users, 57 percent report using no other drug at the time of admission.

- consisting of very few polydrug users in need of treatment as this term is defined in this paper.

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II. PROBLEMS IN DATA ACQUISITION

Surveys of General Populations

Ira H. Cisin, Ph.D.

My subject is data acquisition in surveys of general populations and my objective is to remind you of two important and unpleasant facts:

First: if your objective is to estimate the prevalence of a phenomenon in a population, a samply survey is the only game in town; there are no substitutes for a survey based on a rigorously designed random (probability-based) sample; and

Second: the job of conducting a sample survey is full of problems; and the problems seem to be getting worse as time goes by.

It should not be necessary in the year 1975 to issue pious pronouncements on the necessity for random sampling. The statistical theory is clear: only a random sample can provide unbiased estimates for the population; and only a random sample can provide the researcher with the power to make probabilistic statements about the relationship between sample estimates and population values. We do not pretend that any one random sample will accurately reflect a population value; we do assert that only a random procedure can arm us with knowledge of the probability of being wrong -- and by how much. None of the shortcut inexpensive methods can give us that much power.

Because of the expense of random sampling, the search for less expensive substitutes has been unremitting. We hear about quota samples, about "modified" probability samples (which are comparable to

a "modified" tomcat), and about the ingenious application of multivariate techniques to aggregates of clinic patients or other volunteers in an attempt to use what is readily and cheaply available. Without laboring the point, I think it is unnecessary to remind this audience that all such procedures involve the bias of self-selection or the analogous bias of selection by interviewer and thus provide estimates that cannot legitimately be defended.

Some quite sophisticated researchers argue that the requirement for random sampling is less important if we are studying relationships than it is when our primary interest is in population estimates or projections. This argument has a kind of specious appeal until we remember that it is true only under certain restrictive assumptions or conditions, which include perfect homogeneity within strata and strict linearity in the relationships being studied. There may be circumstances under which these assumptions are tenable, but I have never encountered those circumstances.

So the escape routes are cut off and we are stuck with the requirement for honest-to-goodness random sampling in surveys. Let us turn now to some of the problems we are encountering in trying to carry out such surveys in the field of drug use.

The first of these problems is characteristic of all sample surveys these days -- the lack of cooperation from respondents. In years past, sample surveys were a novelty; people were flattered and pleased to be asked about themselves; the interviewer was welcomed into the household. Now we seem to be suffering from a new atmosphere of suspicion and distrust; doors are double-locked; the interviewer is an invader who, for nefarious reasons is abridging the privacy of the selected respondent. You are welcome to speculate on the reason for this change, but the fact is that the novelty has worn off, the flattery doesn't work and the interviewer needs some salient justification beyond the researcher's curiosity. More and more we hear: "What's in it for me?" from our respondents.

As recently as the early 1960's, we were able, with diligent effort and a lot of money, to produce a 90 percent response rate in a predesignated random sample of the general population (I am not talking about special populations, where special pressures can be brought). Certainly we were worried about the biasing effect of the missing ten percent, but we consoled ourselves with the fact that they couldn't change our estimates by much. At any rate, 90 percent was about as high as our response rate ever went.

Now, with similar effort, we can produce a response rate of about 75 percent in the general population and considerably less in certain definable segments of the population. When we remember that the ones that get away -- the non-cooperators -- are quite likely to include some of the most interesting cases, we may well worry about the quality of the population estimates that we make based on the cooperative respondents.

The rapid decline in the rate of cooperation in the general population is of great concern to all those who conduct surveys; the market research people, the statistical association, the sociologists and the public opinion research association are all casting about for solutions -- but so far all I have heard is a lot of groaning and mutual sympathy. So much for the overwhelming problem of non-cooperation.

In the field of drug use, those who conduct surveys have a special worry. We know how to do sample surveys of household populations. If the people would cooperate, we know how to take a sample, we know how to contact them, and so on. But it seems obvious that the household population may be a relatively unimportant target population in the field of drug abuse relative to the floating population. And we don't know how to do sample surveys in what may be the most important population of drug abuse, the floating population.

The next problem to which I would like to draw your attention is the problem of conceptualization in the planning of survey content. One of the first discoveries I made in the field of drug abuse is that there is a very large and varied list of behaviors that we are concerned about. Merely inventing the term "drug abuse" does not create a homogeneous concept. When we set out to study the incidence and prevalence of drug abuse we ask ourselves: what behavior qualified as drug abuse and what behavior does not qualify as drug abuse? By the time we finish our list of things that qualify -- the variety of human behavior that we are regarding as problematic, that we are regarding as deviant -- we have a very long list. This is a practical problem. We have such a long list of different things that we are studying that we cannot study any part of the list in any great depth. Doing a big national survey is expensive. When you are dealing with face to face interviews in the household population, you don't have three hours in which to collect data, or your response rate is going to go down even lower. You have perhaps 40 minutes to an hour. If you have a very long list of things you are trying to ask about, about all that you can do with respect to each kind of behavior is ask primitive, simple questions; and that is what we are doing. We can't have breadth of coverage of long lists of varied behaviors and go into any one of them in any depth.

I have one other conceptual problem here that may be of interest to you: we face head-on into the question, what do you mean by current use? We can tackle the problem of annual prevalence and one month prevalence and so on. What exactly is current use? If I say I am not now smoking a cigarette, I intend to very shortly and I did a little while ago -- what interval between past use and next use is required for a definition of current use? I can only say that we made a stab at this definition by defining current use in terms of having used the substance within the month and not intending never to use it again. Unless you have both these components and you simply ask "Are you a user?" -- I think you have an ambiguity. Look out for the problem cf current use or so called regular use. I suspect that self reporting on these particular words is very unreliable.

Finally one of the problems that pervades the drug abuse field in this kind of research -- interview research -- is the problem of validity. We have made some attempts to assess the validity of these behavior reports. It has been a very discouraging exercise primarily because the psychometric concept of validity requires a criterion, i.e., another measure which is assumed to be more valid than the candidate measure we were using. If you detect a certain circularity in the definition of validity, your quarrel is with the psychometricians. In any case, we tried two kinds of validity studies -- one a group validity study and the other an individual validity study, and in both cases we are dependent on clinic records for our criteria. This was a revelation in terms of the operation of the clinics. We find ourselves sharing the feeling that Binet had when he first invented the intelligence tests and validated them against teachers' ratings. The relationship was not too good, so he said, "That must mean that my test is very good because the teachers' ratings are very poor!" I have the same feelings about our questionnaires -- they must be very good because they are imperfectly correlated with the clinic records. Why is this? We know the clinic records are rough. Clinic records contain fictitious names and non-existent addresses. Many of the clinics that we have dealt with do not routinely take drug history, so if the patient comes in and is strung out on some particular substance, there is nothing in his record to indicate any of the other substances he has used. In our validity study which involved a double blind situation -- the interviewers didn't know that they were interviewing clinic patients and randomly selected matched cases -- we found that our relationship with the clinic records showed approximately an equal number of errors of the first kind and errors of the second kind (as explained below). To be specific, in the case of marihuana we found much more marihuana smoking than the clinics had a record of. The clinics had apparently not asked for that. In the case of heroin we found primarily errors of the first kind; that is, we found that half of the people in our test group who had a clinic record of heroin usage admitted to heroin usage on the questionnaire; a smaller number gave us heroin usage and had not given it to the clinic. In the case of cocaine, on the other hand, (again apparently the clinics do not ask about it) we found a lot of cocaine use that was not part of the clinic records. Now I don't know how to make an overall validity statement out of this. One gets the feeling that if you are interested in prevalence the questionnaire studies are going to give you approximately the same prevalence rate as the clinic records; that is, for the same group of persons, clinic patients, you are going to get in questionnaires approximately the same prevalence rate on the average across drugs as you would get from the clinic records, but they won't be exactly the same people. Now ponder that for a moment and ask what happens in terms of the correlates of various kinds of drug use.

Having presented some of the problems in collecting data on state and national surveys of the household population I conclude that it is a very discouraging picture. But I say again that these frustrations must be viewed as challenges, since there is no alternative to the sample survey. If you want data that you can comfortably

use, rather than bits and pieces which don't fit together; if you want the kind of projectable information that only a samply survey can give you, I recommend that you continue doing this kind of sample survey but accept the fact that you must take the results with more grains of salt perhaps than you are accustomed to using.

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Surveys of Target Populations

Lee N. Robins, Ph.D.

For those of you who don't know, let me review what I've been involved in, and then I'll try to give some general thoughts about problems of surveying on drug issues and tell you very briefly about some of our solutions, such as they were. I got into this business a long time ago because I was interested in doing follow-up studies on chidren, not really because I was interested in drugs. One of the things we asked about in a follow-up study of black school boys was how much drug use they had had by the time we interviewed them at the age of approximately 32 years. That turned out to be the first time, I believe, that anybody had asked a general population -- not in treatment, not known to the police -- about their drug behavior. Although we really didn't have a very big sample, it was so unusual to have asked about this topic that this got me into the drug business.

What I've done since then is a follow-up study of Vietnam veterans in which we interviewed approximately 900 Army enlisted men who had left Vietnam in September 1971, after they were back in the States between 8 and 12 months.* We are now in the process of reinterviewing most of the same men again. We're also interviewing a comparison group of non-veterans who are matched for age, education,

*Robins, Lee N.: *The Vietnam Drug User Returns: Final Report.* Contract No. HSM-42-72-75, SAODAP. Government Printing Office, Washington, D.C., September 1973.

region of the country, and, hopefully, eligibility for service. In other words, we are trying to get people as much like the Vietnam veterans as possible, except that they didn't go into service. We're interested in finding out whether veterans' drug use at the present time is any different than it would have been if they had never gone into the service at all.

Let me say very briefly some things I think about interviewing about drugs and see if you agree with them. The first thing I'd like to say, at the risk of making an extreme statement, is that just getting incidence and prevalence out of a survey of drug users is not getting very much for your money. Not only is it very expensive to go out and interview people, but I would also argue that you never really want to know those figures. Things change very rapidly in the drug field and these figures are true as of one moment in history; they don't necessarily tell you how much treatment you should plan for or even how much drug use there will be next year. What is much more interesting are the relationships among variables within your population. The only time it is worth simply counting the numbers of people who use drugs is when you have some simple, cheap technique for resurveying the same population over and over again so that you can study trends. For the kinds of surveys most of us do, just doing a one-time count of cases seems not terribly rewarding. Patterns of drug use, not only in this country but throughout the world, seem to be going through rapid changes in the availability of drugs and in attitudes towards use. If you interview different age-cohorts, you get very different rates of exposure to drugs and different attitudes, and you also find that people living in one area have had quite different experiences and opportunities to use drugs from those in another. Where there are historical changes in progress, history moves at different paces in different places. New York City in 1969 is quite different from Great Falls, Montana, in 1969 but may be much like Great Falls in 1976. It is very hard to generalize from information about a particular time, cohort, and locality. One reason this is so is that very different people use drugs when drug use is a rare phenomenon than when it is a common phenomenon. To a certain extent that statement is tautologous because if more people are involved, some of them have to be different people. But there is more to it than that. The *meaning* of using drugs changes when it becomes something that a large portion of the population does instead of a rare and unusual event. Different types of people are recruited to drug use when it becomes commonplace.

The other thing that seems to be true of drug abuse in our experience is that the age of risk of beginning drug use is rather narrow. Most studies find very few people beginning drug use after the age of 30. Thus, if you want to estimate life-time prevalence and your respondents are past 30, they are pretty well through the age of risk. On the other hand, if your respondents are adolescents, you don't know exactly where you are regarding the age of risk, because the risk period keeps changing. It has been observed that the age of beginning use seems to get lower every year. As a result, you don't know how to calculate how much of the age of risk an individual has been through if he is much under 30. It may well be that

if drug use becomes more acceptable, the risk period will also be extended at the upper end. As some middle-aged Americans seem to have discovered the sexual revolution, they may also be discovering illicit drugs as time goes on. Then the age of risk will be changing at both its ends over time.

One problem in studying any form of deviance -- and drug use is no exception -- is that the group that society has most concern about is not the occasional deviant, but the regular and consistent deviant (in this case heavy users or drug dependents). This seriously deviant group is always a very small segment of the population. This presents problems in choosing an unbiased sample and still getting enough cases who have the behavior of interest to be able to study its natural history in a natural setting.

The next problem is one mentioned earlier today, which is that there are problems both of ethics and of scientific validity when you collect information about illegal and disapproved behaviors. On the one hand, you are very concerned about possibly harming the individual from whom you are collecting the information, and on the other hand, since he knows that you could damage him in some way, you have reason to doubt that he is going to tell you the truth.

Another problem that was also mentioned earlier is that drug users tend to develop special languages with which those doing the study or at least those interviewing for the study may not be totally familiar. This lack of shared language may impair the communication between the researcher and the subject. And, since there is a great deal of negative public sentiment about drug use, interviewers may share these biases. The interviewers' bias may make them reluctant to ask respondents about behaviors that they (the interviewers) disapprove of. In most human interactions, and interviewing is no exception, people try to keep relationships friendly. They like to ask about things that people are willing to tell them so that they can maintain what social scientists like to call "rapport." (Other people call this "friendly feelings" toward anybody you are interacting with.) If the interviewer is embarrassed or reluctant to ask questions about drug use, or if he fails to understand the respondent's references to drugs because he does not know the language, the study will underestimate the prevalence of drug use.

The final issue, which I think is a very imporant one, is that there is a strong negative relationship between the seriousness of an individual's deviance and the ease of reaching him as a respondent. When the availability of respondents is related to the very issue that you're interested in investigating, you cannot assume that failures to locate or refusals have not biased your results. Serious drug abusers tend not to be home in the evening when you knock on the door -- they're either dead, or in jail, in a hospital, or out on the street hustling drugs. This makes it hard to reach them through ordinary survey techniques.

I think it probably sounds now as if it is impossible to do surveys about drug abuse. Having been pessimistic, I now would like to play Pollyanna and say that it is really not all that hard -- there are some solutions at least. Let's talk first about the problem of historical changes in availability and the narrow age of risk. It seems to me that unless one has money to study enormous samples, one is better off working with somewhat homogeneous samples with respect to their access to drugs and to the age and background variables. It is hard to design a study that can apply to all parts of the population at once, without being very superficial. Which questions about drug experience appropriate to a 25-year drug pusher in New York would make sense to a 60-year-old farmer in Iowa? In studying Army enlisted men who were in Vietnam, we had a sample that was extremely homogeneous. Its members shared nationality, sex, occupation, age, and location during a specific moment of history. This solved many problems of questionnaire design. The difficulty is that once you choose a homogeneous sample you lose generality -- you don't know to what extent you can extrapolate from your homogeneous subpopulation to the population as a whole. What I feel at this moment is that you're better off first working with a carefully defined homogeneous sample and then trying to replicate your findings about factors influencing drug use in very differently defined but equally homogeneous samples -- to see if your findings hold with different kinds of populations in different places, at different times -- rather than trying to solve everything in one study.

The next question is how to get enough really serious users in any general population. One way is to see to it that the homogeneous subpopulation from which you are sampling is a high risk population, but in doing that you severely limit your ability to generalize to the general population. A better solution is to over-sample, at a known rate, the group within your population which you think is going to be a high risk subsample and then weight it back in. This is what we did in the Vietnam study when we took equal proportions of a general sample and of a sample of people identified as drug users at the time they left Vietnam. The second sample was simply a blow-up of that 10 percent of the general sample we could expect to have the highest rate of use after their return. Then, by knowing the proportion identified as drug users at departure in the general sample, we could weight our large high-risk sample back into the general sample. Thus we simultaneously had an unbiased general sample and a large subsample of heavy users in whom to study the progress of relapse and remission after return.

Solving the ethics problem requires some kind of super-inviolable arrangement for confidentiality. It may well be that the most serious users you are interviewing don't care very much about confidentiality, because many of them will be known to the police and have records here and there. We found that the people who cared the most were the interviewers -- and me. Interviewers are typically young, anti-establishment, dewy-eyed, and very sensitive about ethics. That last characteristic applies to me as well. I felt in undertaking this study that I had to be willing to go to jail rather than compromise confidentiality, but I didn't relish incarceration.

In fact, nobody so far has shown any interest in finding out anything about any individual we interviewed, but I felt we had to be protected against every contingency. So we had a very elaborate arrangement -- a double number system -- in which we sent interviews without any number on them except on the mailing envelope to Canada, where they were given a new number, so that the link between the name and the number identifying the interview was broken. Our Canadian contact didn't have the name, only a list of two numbers: the original number associated with the name and the new number they chose. In the U.S. we had a list of names and the first number, but we could not get from the first number to the second number without going back through Canada. This system not only guaranteed protection of confidentiality, it also meant I didn't have to worry about whether interviews were locked up, because there was no way that anyone reading them could tell to whom they belonged, even if they had my list of names and ID numbers. I found that a very relaxed situation.

The problem of the relative difficulties in gaining access to drug users for interview as compared to non-users is a serious one. The methods used not only in this study but in our earlier studies in which deviant outcomes were of interest are not very commonly used, but I would like to recommend them. They are avoided because they are considered to be very difficult. However, I didn't find them so. They involve using an earlier roster as a basis for sample selection and then a reasonably relentless effort at follow-up.

In this study, the Army made a roster for us which contained every enlisted man who had left Vietnam during a certain month. Because appearance on that roster depended only on departure from Vietnam, and not at all on behavior after return, the sample we chose from that roster was not at all biased in terms of whether or not they used drugs after they got back. We could have biased our sample of respondents against men who used drugs after return by failing to interview the hard-to-interview cases, but at least we would know that we were missing them. That is the advantage you don't have in an area survey, where, since you don't know what your base population is, you don't know whom you have missed. When interviewing civilians, you can get the same total count by using early school or birth records. (If you use high school records, you will have lost those too deviant to finish elementary school.) Whatever its source, access to some sort of roster made up before the behavior of interest has occurred guarantees that whether or not that behavior occurs later cannot influence whether or not the person is in the sample. The other advantage in starting with a roster made up before the behavior has occurred is that you can count deaths, which you can't do with area surveys.

Once you have a sample that is unbiased with respect to drug behavior, you still have to avoid bias due to the fact that it is harder to contact the more deviant members of your sample. What we did was, first, to pay the respondent for his time and second, to set no limit on call-backs; each case had to be pursued until located and a definite "yes" or "no" obtained. (Incidentally, once

located, deviant subjects don't seem to be any more likely than others to refuse an interview.) Locating virtually all subjects sounds harder than it is. While the maximum number of call-backs we had was eleven, most interviews were completed on the first visit by setting up appointments ahead of time. The fact is -- at least in the old days and I am beginning to think this is only true of the past -- by the use of various and sundry records, it was pretty easy to find out where somebody was. What we have found in the last few months, however, is that many sources that were public records and open to us as ways of locating people have now been closed. One useful source we used routinely was drivers license registrations. These are now available only to police officers, after having traditionally been public records. There are many other such examples. Even in recent studies, however, our completion rates for follow-ups of cases selected from a roster have been higher than rates in most area surveys. The reasons are two: first, it is much easier to ask neighbors for information when you can name your respondent and his close relatives than when you want the "young man who lives in Apartment No. 3." People are used to answering questions about individuals identified by name, not by dwelling unit, so long as you can convince them that you're not a bill collector -- and that is easy when you can say you have money for the respondent. So communicating with neighbors and neighborhood shops and using reverse telephone directories to call people who were former neighbors is considerably easier when you are looking for somebody whose name and age you know. Second, I believe both having a name and offering to pay for the time to answer reduce refusals, because your prospective respondent doesn't think you might just as well talk to a neighbor as to him.

We tried to solve the problems of the specialized language of drug abusers and interviewer bias through extensive training and by providing interviewers with a lexicon of technical and street names for drugs. In the first Vietnam follow-up, most of our interviewers had never before seen anybody who had used heroin. They were just plain scared. Their fear was further intensified by one of the people we brought in to train them. He was a young psychiatrist who had been treating addicts in the amnesty program in Vietnam. He told the interviewers that the drug addicts they would interview might well be sociopaths who would try to get money out of them, and that they had to be extremely careful. *His* viewpoint had been biased by his own experience, which was almost entirely with men who had volunteered for treatment in order to avoid standing trial for serious non-drug offenses. To overcome the interviewers' fears, we had them interview addicts during the training period in a protected environment, rather than going out to the addicts' homes, as they would during the study proper. They discovered that the veteran addicts who came from treatment programs to be interviewed in the office were mostly nice kids, for whom they felt enormous sympathy. This experience changed their view of what a drug abuser was, and instead of being terrified, they felt warm and friendly toward their respondents. This abandoning of fearfulness turned out to be justified. In com-

pleting 900 interviews, we had only one case in which an interviewer had reason to be afraid of a respondent.

While the training sessions enable the interviewers to see addicts as people instead of monsters and to learn the drug vocabulary, there was still a question as to whether they would get honest answers from the users. We tried to test validity in every way we could. We tested the validity of our estimate of point prevalence of use by taking urines at the end of the interview, to provide an independent measure of whether men were currently using drugs. We got a test of the validity of our interview-based estimate of the prevalence of heroin in use in Vietnam by reviewing Army records to see whether the respondents had been known to the Army as drug users while they were in Vietnam. We have been pleased with the results of our tests of validity. It turns out that almost every drug user is willing to tell you the truth so long as nothing is riding on it. Addicts may be more honest with interviewers who are going to go away and never come back again than they are with the doctors they see in drug treatment programs. What the doctor thinks is going to influence what happens to them, but it doesn't matter to them what *we* think -- so they can afford to be candid.

AUTHOR

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DISCUSSION

Dr. Gottschalk: Dr. Robbins, where did you get the names of the drug users and how did you get them as of before leaving Vietnam? I thought those data were confidential.

Dr. Robins: This was funded by the Department of Defense, so they had an interest in the study and had access to the data.

Dr. Kramer: I would like to ask a few questions about the age of risk. In some way this magic number of 30 years seems to have gotten into these discussions. When interviewing people of 30 years to ask them retrospective questions, you may indeed have gotten all the information there is about their patterns of drug use. But it depends on the kind of drug you are interested in. If you are dealing with the way in which people might start using drugs as a result of a chronic disease which took place after age 30 -- diabetes, cardiovascular diseases, cancers, and so on -- you run into another problem. This population may have different patterns of drug use, side reactions, or perhaps an abuse that resulted from the prescribing habits of a physician. Therefore, it seems to me this kind of clarification is needed in terms of the kind of problem you are studying.

Dr. Robins: If John O'Donnell were here he would tell you about his study of Lexington patients in the 1930's, where he had a lot of older people who were drug addicts; having been given morphine by their doctors, they had become addicted. But that has pretty much disappeared. It happens rarely now -- doctors have gotten smarter; although there are a few people who get addicted this way, they are few and far between.

Dr. Sample: I would like to ask Ira Cisin and Lee Robins a question. Dr. Cisin gave a somewhat pessimistic viewpoint in terms of the 75 percent completion rates on interviews. Lee Robins, with unlimited follow-up -- really unlimited cost considerations -- was able to track, locate, and interview almost everyone. Johns Hopkins in 1972 did a study of the NARA Program, Narcotic Addict Rehabilitation Act. This follow-up and evaluation study had very low rates -- completing interviews of about 50 percent of persons found not suitable for treatment at NARA and about 65 percent of the people who had completed treatment at Lexington. I have not read the reports, but, as I understand the results, they took a

sample of the not-founds in the first study, went after them, found most of them this time, and found that the sample of the not-founds did not differ significantly from the sample of the founds. What I would like to ask Dr. Robins is, in the persons that you did not find initially, where it took 8, 9, 10, 11 follow-ups, did you analyze those cases particularly to see if they differed in fact from the ones who were found early?

Dr. Robins: Yes, we compared people who were interviewed the first time around *vs.* people who needed four or more visits and there are very striking differences, not only in terms of drug use but in their total social adjustment.

Dr. Sample: Dr. Cisin, a question then. Have we ever considered taking a sample of the not-founds and going after them in a more intensive way to test that hypothesis?

Dr. Cisin: But then you need to sample the not-found of your sample of not-founds. There is an infinite regress model available for more and more intensive effort on the "not-founds." You have raised several questions though, and let me comment very briefly on a couple of them. First, the decline that has been going on in cooperativeness over the past 20 years is characteristic of the general population. It is still possible with special populations to produce rather high response rates and indeed we did in our special population and I suspect Room and O'Donnell will in their special population. In special populations the success rate is better for a variety of reasons that we need to go into now. In the general population, there has been a significant decline in cooperativeness. To back up what Lee Robins said about the ones who "got away," in the study that Cahalan and I did of American drinking practices about 10 years ago, we did not subsample the non-cooperators and go after them intensively. As I have said, you do not find them all and, therefore, you do not really know what you have when you subsample the non-cooperators. What we did do was along the lines of what Lee Robins suggested; we compared those who were easy to get to those that were hard to get. This was a survey in which we went all out; as I said, it was 10 or more years ago, and we indeed got over 90 percent of the predesignated respondents. We did an analysis of the drinking behavior based on how hard it was to get this interview -- the number of calls, the amount of time, the amount of effort that went into the interview, and indeed, we found the same sort of things that Lee Robins found; not only did the "hard-to-get" differ from the "easy-to-get" in background characteristics, but they indeed differed on the criterion variable considerably. There may be cold comfort in an occasional study that finds no difference between the "hard-to-get" and the "easy-to-get," but it is improbable.

Dr. Johnston: I think another point which is relevant here is that the type of sampling that is used in the study may have something to do with the results. That is, there are different procedures for drawing a household sample. One is called quota sampling, which means you get someone off of a block or thereabouts,

and another is more specific because you are going after a particular individual in a particular household. If you are using quota sampling, in essence, you are getting the people who are easy to get, the ones who are there. Quota sampling is therefore, likely to give you different estimates than other types of sampling. That is usually "finessed" in reports.

Dr. Cisin: This is not the year 1935. We are talking about probability based samples which produce unbiased estimates and that sort of thing. If you duck into what some people call modified probability samples (which, as I mentioned, is like talking about a "modified" tomcat), you are dealing with a very different thing that has nothing to do with the population in the study. I for example, worked as an interviewer when quota sampling was all that was being done in this field, and I can guarantee you that I never interviewed anybody who owned a big dog!

Surveys of Special Populations

Eric Josephson, Ph.D.

Looking around, I can see I am one of about a dozen or more survivors of the conference in San Juan that Louise Richards mentioned. The proceedings of that conference have finally appeared in book form, approximately 18 months after it took place. It is an interesting question whether that makes it ancient history.*

For some time I have been associated with Jack Elinson and a national study of Drug Use in nearly two dozen high schools in the United States. This study has had many ramifications. I do not propose to talk in detail about it, but rather to attempt my hand at discussing certain methodological and substantive issues which have emerged from our own experiences and those of others. I might also add, by way of background, that at Columbia University a number of us, Kandel, Lukoff, Elinson, and I have recently established a new Center for Socio-Cultural Research on Drug Use, with a grant from NIDA. Sometimes we have thought of calling it the Center for Adolescent Drug Use, but that is not our only concern.

I am staggered by the topic next to my name because it says "Special Populations (Adolescents and Other)." It is difficult enough to try to reach some consensus on the definition of adolescence --

*Josephson, Eric, and Eleanor Carroll, eds. *Drug Use: Epidemiological and Sociological Approaches.* New York: Halsted Press, 1974.

just what "other" means I do not know. I have been trying to think of some of the other populations we might also talk about -- infants, terminal cancer cases, people studying drug abuse, pre-adolescents, post-adolescents, etc. In listening to Jack Elinson talk about the problems that his S.A.O.D.A.P. group is having in defining terms in drug research, I feel we have equal problems in trying to define "adolescence," although again, I know that is not our purpose here. In many United States drug studies we arbitrarily define adolescence as the age group 12 through 17 years, i.e., the high school years. This is, I stress, arbitrary. One issue that could be considered here, for example, has to do with evidence that physiological maturation is taking place earlier in the life cycle; one of our colleagues in Puerto Rico may have some data on the relationship between maturation and drug use. Then of course, there is another way of looking at it. From a sociological point of view, it can be argued that adolescence has been prolonged as increasing numbers of people spend more and more years in college and univeristy. I do not know if "adolescents" on this agenda means college students as well. However, this is related to a point that Lee Robins made just a moment ago --about getting information on the starting point or initiation into the drug experience -- since it is in the college years that many begin such experiences.

I do not know if it is possible in a few minutes to provide a state-of-the-art report on the research that has been done in the United States on adolescent drug use, however adolescence is defined. Glenn and Richards' recent compendium has abstracted many of the high school and college studies that have been done.* The Shafer Commission did the same in its two reports. There have been a great many such studies, perhaps too many. Maybe we should declare a moratorium of a year or so on school studies. In fact, looking at the compendium, it appears that there have been more studies done in high schools than in colleges. Not only do they vary enormously in quality, in the definitions they use, and the methods they apply -- they also vary widely in their findings and interpretations. It is worth noting too, that some of these studies have been done by the kids themselves. I cannot think offhand of any other presumed health problems or health-related problems in which those experiencing them have actually conducted research about themselves, but I know of at least one high school study which was planned, conducted and analyzed by high school students. It is one of the better ones, as a matter of fact.

So in the last ten years there have been hundreds and hundreds of such high school studies, most of them done in selected communities, selected schools, or selected counties. There have been relatively few national studies, however. I think ours is one of few, al-

*William A. Glenn and Louise G. Richards. *Recent Surveys of Non-medical Drug Use: A Compendium of Abstracts.* Prepared for NIDA by Research Triangle Institute, 1974.

though it is not a sample survey of high schools. One of the few systematic sample surveys is the one Denise Kandel has been conducting in New York State, and she may wish to discuss that later. It was difficult for the Shafer Commission to try to draw inferences about national trends based on this bewildering variety and diversity of many hundreds of high school studies conducted at different times, in different ways, for different purposes.

As regards national data on trends in adolescent drug use there have been few studies. Two were conducted by Response Analysis for the Shafer Commission in 1971 and 1972.* In addition to our own study of selected schools, we have so far commissioned three such studies and are now starting a fourth small scale national sample survey of adolescents in which questionnaires are administered to youngsters in their homes. Looked at together, the surveys conducted by Response Analysis and our own, particularly in regard to marihuana, show that in the period 1971 through 1973 there was no significant increase in the proportions reporting "ever" having used marihuana. The data we have and the data collected by Response Analysis show that in that period, 1971 through 1973, there was apparently stabilization in the proportion of youngsters age 12 through 17 who said they had ever tried marihuana; the figure was approximately 15 percent during that three year period.

I think it is worth pointing out that while the concern of this meeting is so-called "drugs of abuse" and I also suppose specifically the illicit drugs, we have to be reminded that various studies show that if one is interested in the drugs of choice among adolescents, they are alcohol first, tobacco second, and marihuana third.

In the study of drug use many problems of data collection are not special to adolescence or any age group, e.g., reliability, validity, maintaining confidentiality, protecting the anonymity of respondents. Some are however, and some I suspect may become more serious.

One has to do with the issue of parental consent. From our experience in several states, we found that in many communities the schools which were our sites for administering the study -- the school principals acting in *loco parentis* -- made it possible for us to reach all those in school on a particular day who were willing to cooperate with the study which we were doing. But in several communities where the law of the state required that informed parental consent be provided before the study could be administered, this presented serious difficulties, not so much because parents were unwilling to provide that consent but rather because of the breakdown in communication when youngsters were given slips to take home to their parents. Youngsters sometimes forgot to give their parents the slips, or the parents forgot to sign them, and

*Since this conference, two further national studies have been conducted, by NIDA, one in 1974 and one in 1975/76.

sometimes the youngsters forgot to bring the slips back so that in at least several schools selected for our study the loss of potential respondents was considerable. In our experience this does not seem to reflect any unwillingness on the part of parents to have their children participate in such studies. However, the consequences of increasingly stringent requirements of informed parental consent for minors to participate in such studies remains to be determined. It could present very serious difficulties for any investigator who wishes to reach a fairly large population of adolescents.

Some of the other methodological problems presented in studies of adolescent drug use are by no means unique. Perhaps most of the high school studies that have been done in the United States have been of the cross-sectional type, i.e., done only once. There have, however, been a number of trend studies done in particular communities, such as the annual study done for several years in San Mateo County, California, as well as the panel studies in which we and Denise Kandel are separately engaged.

On the substantive side there are a number of issues which I think should be considered. One has to do with the age curve of drug use. What I have in mind here is the notion that experimentation with illicit drugs begins for some in adolescence (i.e., 12-17 years) but does not reach a peak until after the age of 17 or 18. In limiting oneself to the study of this particular age group, one is missing many who have yet to begin their experimentation with drugs. The question as to whether the starting age is falling or not remains to be determined, though in some parts of the country the data show that it has indeed fallen. But this does not answer the question as to what happens after the adolescent period. Thus, an accurate picture of the age curve may require a wider range for study.

This is a strong argument for going beyond trend and cross-sectional studies and following-up on adolescents as they reach adulthood. Relatively few such studies have been undertaken so far, although some are planned. Again, since the use of drugs for some begins in the adolescent period but not for all, one cannot by some such age limited population study get the full picture of the pattern of drug use over time which is involved with increasing age.

Another issue has to do with the question of faddism. By reconstruction we can detect such fads -- for example, what appears to have been the fad of interest in glue-sniffing a few years ago! With regard to marihuana, it remains an issue of debate as to whether this is a fad which will pass in time or whether it will become a permanent part of the drug-using scene in the United States. Will today's adolescents continue to use marihuana as they grow older?

I suggest too that most studies of drug use that have been conducted with adolescents are not really studies of drug abuse in

any sense of the term that I can think of. Most drug users in the 12-17 age group are experimenting, although, no doubt drug abusers by some criteria can be found in this population. What has been of concern, I suppose for political reasons, is public concern about the drug problem which focuses not only on the fear of crime but also has to do with parental concern about the presumed ill effects of drug use on their children. Because of this concern, many of us have been under considerable pressure to do what really amounts to a nose-counting operation. I think what the public wants is to know how many young people are using these "bad" drugs. And next, of course, is how to stop them from doing so. Of course that is not of primary interest to many researchers. Some investigators have gone considerably beyond those questions in trying to understand the processes of drug experiences for young people -- the factors that contribute to different kinds of use or non-use, as the case may be.

There are just a few other points I would like to make. One has to do with the need for comparative research, by which I mean studies of ethnic groups which are likely to be missed even in large-scale national surveys. However, I am also thinking of the desirability of conducting studies in other countries. At Columbia, we have recently collaborated with a team in Britain to conduct a study of secondary school students' drug use and attitudes towards drugs in one town in England. Preliminary results indicate that as regards illicit drug use, English adolescents are far behind their American contemporaries -- at least in this particular community.

Looking at Louise Richards' list of eight topics, I feel that many of them do not apply to adolescent drug users, especially as regards opiate addiction. Few adolescents are that heavily engaged in drug use and fewer still have experienced adverse consequences, let alone treatment.

Perhaps the most important issue with regard to the continuing monitoring of adolescent drug use in which some of us are engaged is Louise Richards' question as to our capability of predicting new epidemics of drug use. I suspect that what is needed are surveys on a much larger scale than any undertaken so far since epidemics usually start with just one case. Most surveys that we do are not likely to detect any such beginning or initiation of some new pattern of drug use.

As to indicators which would be most useful in trying to predict and to help us understand drug use -- the experience of a number of us, particularly Denise Kandel's research, shows that if you are looking for powerful predictors of drug use among adolescents, knowing what their friends do perhaps predicts as well as anything else, if not better. Of course, the interesting sociological and philosophical question is whether it is the friends that choose the drugs or the drugs that choose the friends.

AUTHOR

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DISCUSSION

Dr. Eichberg: There is a problem that I am sensitive to regarding faddists, and I think it is particularly important with adolescents. An example that I will use is the problem of developing a questionnaire. Recently when I was at a school, I found that the drug most commonly used was "flying saucers." Now, what do you do? Do you list every individual drug that young people might be using or do you group the drugs? If you group the drugs, you run the risk of grouping it improperly, perhaps losing some information. If you don't group the drugs, then "flying saucers" in a different community may be a different drug. With regard to that particular drug, I called several drug programs. Some of them had never heard of it. One drug program had analyzed it as P.C.P., and another drug program had analyzed it as high doses of Valium. So what do you do with a situation like that? With adolescents it becomes more of a methodological problem than with adults, perhaps.

Dr. Elinson: This is not really a response to that, but I am reminded that in our study that I mentioned before we used a fictitious drug and we gave it the name of "El-Jo's," an acronym for the first two letters of the names of the principal investigators, and, some of the kids in some of the schools weren't fooled for a moment. As a matter of fact, we could not, in going back to these schools in the second wave, use the same fictitious drug because they had become intrigued by it. By which I mean that at least in one school as I recall, banners appeared in public, advertising this fictitious drug. The next time we had to ask about another fictitious drug although we could not be sure it was so fictitious.

Dr. Eichberg: I am wondering what to do with that kind of situation because the incidence of the "flying saucers" at this school is apparently higher than the incidence of the use of anything else. And we don't know what it is. They just said "flying saucers," and described the effects; some kids had overdosed on it. I have a feeling there is no consistency in terms of what it is. They don't know where it is coming from.

Dr. Gottschalk: This is a question Dr. Josephson alluded to. In most school systems that I know about where it is necessary to get informed consent, we have written a letter to the parents, elimi-

nating the problem of whether or not the kids take the letter home. But while you know that using the form letter approach would be O.K. with the school system, you only get a certain percentage of responses. Is it easier to get a biased sample and how much can you take?

Dr. Josephson: I mentioned our experience in which in several schools in one state, we were required to get parental consent and we did not do it by mail. (Perhaps we should have.) We did not pursue further the methodological question to determine just what bias may have been introduced into that particular population of respondents. I am sure some was. In most cases in our experience, since this is a selection of schools, the selection could very well have been influenced this way. The principals themselves or the school officials acted *in loco parentis*; they provided the consent. Now the question I raised is how long can this go on? I don't know. In view of some of the developments that have to do with confidentiality and consent, it may be increasingly difficult and precarious for school officials to undertake this responsibility.

Dr. Gottschalk: It doesn't seem to be possible on the West Coast.

Dr. Lettieri: On the comment that Dr. Gottschalk made: At least a beginning attempt has been made to grapple with the problem of paper and pencil or self-administered questionnaire *vs.* interview data in a grant awarded to John Griest. What he is doing is essentially a methodological study. He is tapping the same population in a small town (actually Madison, Wisconsin), asking the people to fill out a questionnaire, paper and pencil, self-administered. He is also doing an interview and has a third variant, computer generated questionnaires. He is setting up computers in various locales including parking lots, for example, where anyone can come. They literally see a question on a computer screen and punch in the answers. He is testing human intervention and machine intervention; when the product is completed, we might discover that there is not that much difference.

There is some overlap of the same people; and with the computer, that poses a special problem because they don't give their names. There is, however, an observer around to see that someone is not misusing it or continually sitting on it or playing with it. The computer technique has been used as standard intake procedure at the University Hospital. Any student who comes in with any kind of problem at the hospital sits down not with an interviewer to take the history, but with the computer. The computer instantly codes out 12 copies in different formats to go to the nursing staff, doctor staff, etc. They found that to be a neater way of keeping records. They did VD questionnaires that way, checked it against paper and pencil and discovered no difference in that kind of response. So it is an attempt to find if it works with drugs.

Dr. Richman: In the surveys involving interviews, especially for drug abuse, there seems to be an increasing tendency to match the interviewer to the ethnicity or culture of the geographic area in which the survey is occuring. I wonder if there have been any

methodologic approaches to assessing the expectations of the interviewer with regard to the responses that the interviewer gets from the ethnically-matched respondents.

Dr. Elinson: There have been such studies in fields other than drug use. I am not familiar with any that have been done in the area of drug behavior.

Dr. Josephson: We have, with matched groups of high school students, administered questionnaires in the classroom setting and in a household setting. Both were really self-administered, although in one, obviously, even though no names were asked, the respondent knew that his identity was no secret because the interviewer had gone to the house in order to leave the questionnaire.

In the classroom setting, we tend to try to match interviewers and respondents both ethnically and culturally and in terms of dress and costume as well. In other words, to get young people in high schools to cooperate, we tended to recruit and train -- "coordinate" as we called it -- slightly older, usually college age individuals, presumably closer in life styles to high school students than ordinary household interviewers. As a matter of fact, in one high school we were almost thrown out because one of the coordinators came in such outlandish dress that the principal did not want the student body to be corrupted by this individual!

Dr. Elinson: I think it is fair to say that there has not been any systematic evaluation of interviewer effect in studies of this kind.

Dr. Robins: We tended to use black interviewers with black respondents, but we used very few black interviewers with white respondents. We used lots of white interviewers with black respondents. They were sort of accidentally assigned. They happened to be in the right part of the country at the right time. We used both men and women and we looked for age differences, racial concordance, and sex concordances -- and none of them made marked difference in terms of the validity of the answers we got.

Dr. Chambers: We've done about 30,000 interviews. Where we have tried to check interviewer effects, we found ethnicity to be an important variable although probably not as important as sex was. Female interviewers typically get considerably fewer refusals than males do. When you start knocking on doors in the middle of the afternoon, male interviewers could not get in to do the interviews and females could.

Dr. Robins: I think it depends on how you do it, too. All of our respondents had received letters, so they were expecting somebody. It wasn't entirely the first effect, the first impression.

Dr. Chambers: I am still convinced that the strongest variable is geography. Individuals who live in the region in which they are doing the interviewing have always worked out better than people

who have come in from outside to do the interviewing.

Dr. Gottschalk: There have been pretty good experimental studies with small groups that indicate that there are interviewer effects. If it happens in these small experimental studies that are pretty tight, you can fairly safely generalize that it probably happens in these larger ones.

Dr. Elinson: No. I would not agree with that at all. Very special situations are not the same thing as what happens in the field. A lot of other variables affect special situations which influence what happens between the interviewer and the respondent.

Dr. Gottschalk: Let me rephrase my assertion. On the basis of other studies, experimental studies, it would be wise in these studies to try to standardize, to keep the same interviewer, so as not to vary the race or ethnic background of the interviewer over time in a longitudinal study, because one might generalize from these other studies rather than be haphazard about this.

Dr. Lukoff: We did a screening interview to find parents who were involved in deviance. We matched the interviewers with the ethnicity of the area to which we were sending them and all we discovered was that two of our interviewers, one white and one West Indian black, were getting much more cooperation in screening interviews. So, we sent them to the other areas and we discovered they got much better results than the original people. The interviewer effect was probably independent of ethnicity, age, etc. They were better at gaining access and getting people to admit certain kinds of things in a very short space of time. We did not give them a long lead-time. There were also field surveys in communities where we could not get most of our white interviewers to go where they were assigned. So, what you wanted to do did not make any difference. You had to get the interviewer who was willing to go into the neighborhood. You could not really randomize the effect scientifically.

Dr. Abelson: I would like to bring up a point which is of particular concern to us in a current drug study and others which has not been mentioned yet today, and is at least as much an influence as the interviewer on the possible outcome. Our concern is about setting up a situation where the respondent is physically and manifestly guaranteed confidentiality. The elaborateness of the situation which involves the respondent having the return envelope into which he or she puts the materials, various assurances of confidentiality and disclosure throughout the interview trouble us to the extent that we don't know what kind of perspective it provides for a respondent who might not otherwise consider it.

I thought you might be interested in a couple of brief procedural outcomes from the past two national drug studies. In both instances parents or eligible adults were interviewed first, and then if the face-sheet data showed that there were one or more eligible youngsters, age 12 through 17, in the house, one of them

was pre-selected for interview. We asked permission of the adult who had been through the process to allow us to interview the young person. Our overall completion experience in both of these studies was over 80 percent with the 12-17 age group. I am sure it was no less than 85 percent of the parents who said that it was up to the young person as to whether or not they would be interviewed. Then also in the de-briefing of respondents on these other studies, we asked some questions in the self-administered mode that had to do with dissembling, exaggeration, or understatement of experience with drugs during the course of the interview. Our findings were that six percent of the respondents reported that, in fact, their drug experience was different from what they had told the interviewer or indicated on the questionnaire.

The next question we tried to get at is the direction in which this misrepresentation took place. It divides almost exactly down the middle, with about one-half the people saying they exaggerated and the other one-half saying they did not report the reality.

Dr. Josephson: May I just add that with regard to the question of confidentiality, I agree that this raises serious questions when you are looking for use of hard drugs. During pilot studies we conducted, we were also testing a technique of self-generated code numbers so that we could match a panel of students in the second wave. In the earlier pilot study with matched groups, we asked some of their names, asked the second group for self-generated code numbers, and the third group for nothing at all. Our expectation was that the highest level of drug use, and I am not talking about the use of hard drugs, but of drugs such as marihuana, would be reported by those who were asked for no information whatsoever, and the lowest level of use would be by those who were asked for their name. We did not find this. In fact we found no significant difference, and actually a somewhat higher level of use reported by those who had actually been asked for their name.

Surveys—Longitudinal Studies

Lloyd D. Johnston, Ph.D.

I will address problems in data acquisition and interpretation in longitudinal studies. This topic really cross cuts many of the issues already discussed, so I will concentrate on those which are *unique* to longitudinal work.

Let me mention what I think are the major values of the longitudinal approach. In terms of measuring incidence and prevalence per se, the longitudinal design is not the best approach. Probably our best measures of incidence and prevalence come from cross-sectional surveys. Obviously, each longitudinal study starts with a cross-sectional survey, but after that first time point, panel attrition introduces error of some magnitude into the estimates. The unique power of the longitudinal design is its capacity to make temporal connections between drug use (or drug-related attitudes) and other variables -- antecedent correlates (possible causes), simultaneous correlates (possible symptoms), and subsequent correlates (possible effects).

The time intervals over which the follow-ups in the longitudinal surveys occur have a lot to do with whether the relationship between variables looks like a simultaneous relationship or whether it looks like a sequential relationship. The longer the interval, the harder it is to get variables that relate one antecedent to another. I will not go into the methodological problems of deciding what are antecedent conditions and what are simultaneous ones. The fact is that a variable that has an antecedent relationship to another variable -- perhaps expressed in terms of a

cross-time correlation -- usually also has a simultaneous relationship (correlation) with that same variable. There is a whole literature on trying to extricate the "truth" from these complex sets of relationships.

ASSESSING MATURATIONAL CHANGES

Among the major uses and perhaps the most important use of longitudinal studies has been the assessment of the maturational process per se; that is, changes that occur with aging generally. While this type of research undertaking may be less complicated than the search for antecedent conditions, it is not without its difficulties. The most serious one is that maturational changes observed among people who grow up in a particular time period may not be replicated among those growing up in a different period.

So far at this conference we have talked about the two stages of development that have received the most attention in the drug literature, namely, the high school years and the college years. Eric Josephson has already mentioned a number of studies which have occurred looking at longitudinal changes during the high school years -- his own work, Denise Kandel's work in New York State, and some work that I've been doing at the Institute for Social Research at Ann Arbor. As far as I know, there has not been that much longitudinal work on normal populations at the earlier years of early adolescence or pre-adolescence, nor has there been much serious longitudinal work on drug use in the adult years.

I might take a minute to tell you a little more about our own work related to the high school and college period. It is called Youth in Transition and involves a nationwide sample of some 2000 young men whom we began studying when they were sophomores in high school in 1966. In the intervening eight years that we have been following them, they became the high school graduating class of 1969 and many comprised the college graduating class of 1973. The assessment of drug behaviors and attitudes was a secondary purpose added to the study in 1970. At that time and in 1974 we secured information about drug use during the high school years and during the five years subsequent to high school. (During the subsequent years, these young men went into a number of different situations, college being one, but also military service, work, unemployment, and so forth.) The results of the 1970 survey are published (Johnston, 1973) and we just now are beginning to analyze the 1974 data. I do have some early trend data on what happens to drug use between the ages of 19 and 23 for this particular cohort, however. Generally drug use increases substantially through that age period. Increase in marihuana use is the greatest, and regular marihuana use increases quite substantially: about ten percent of this nationwide segment of 23-year-old males said that they had attained daily marihuana use sometime in the prior year. In the cases of the more serious illicit drugs, it looks like there was a peaking and drop-off in regular use during the five year interval after high school.

DIFFERENTIATING MATURATIONAL, COHORT, AND SECULAR CHANGES

As I have already mentioned, age related changes such as those found in the Youth in Transition study may not be replicted in other age cohorts (i.e., in people born in other years). To differentiate enduring maturational patterns from cohort and secular changes requires that a number of age cohorts be studied. Almost no work of this type has occurred so far. Exceptions are work done by Greene and DuPont (1974) and a study now being done by O'Donnell and Cahalan on males aged 20 to 29. Both of these works, however, are quasi-longitudinal since they use retrospective data.

There is a new repeated-cohort, longitudinal study now being launched by myself and Jerald Bachman under SAO and NIDA sponsorship. It is entitled Monitoring the Future: a Continuing Study of the Lifestyles and Values of Youth. Since we are still in the process of getting ready for the first data collection in April, 1975, I don't have any results to report. We expect to be picking up a new high school class of seniors each year, nationwide, and then following them for a minimum of five years longitudinally. We will be looking at a number of things -- incidence and prevalence of drug use among American high school seniors would obviously be one of the products we would try to report on a systematic basis over time. But we also will be looking at maturational changes in the college years and attempting to separate them from cohort and secular changes.

ASSESSING THE EFFECTS OF SOCIAL ENVIRONMENTS

In addition to assessing maturational changes in drug use, longitudinal studies are often used to examine the impacts of various social environments and life events on drug use. Lee Robins' study of Vietnam veterans returning to civilian life (Robins, 1974) is an example of this approach. The work that we are doing in Youth in Transition and in Monitoring the Future also involves an attempt to look at the effects of social environment, since some of our young people go through college, others through military service, some into the civilian work force, and so on. Some other important life events that occur during this period are marriage and parenthood, and we expect to be examining their impact on drug use, as well.

ASSESSING PLANNED INTERVENTIONS

Another possible use of longitudinal studies is to assess the effects of planned interventions. Drug education is one type of planned intervention, and there have been studies in that area with "before and after" longitudinal measures. There are also some longitudinal studies attempting to assess the effects of treatment. I understand that David Nurco is doing a long-term 20 year follow-up on heroin addicts who had been through treatment; Saul

Sells is also working on follow-ups of people out of treatment.

QUASI-LONGITUDINAL DESIGNS

I might mention a couple of quasi-longitudinal designs that are sometimes used, and I think they have some pitfalls. One of them is to attempt to look at maturational change not by following a particular group over a period of time, but by contrasting two groups that represent the beginning and the end of that period. For instance, we could compare the drug use levels of freshmen and seniors at a particular college and conclude that the differences reflect changes that occur during college. This is a dangerous assumption, however, partly because of the nature of cohort effects -- one class may simply not be like the other -- and partly because of unknown losses in the population between those two periods (due to dropping out, for instance).

Retrospective reporting is another method to put together longitudinal data after the fact. It is certainly better than having no data at all on earlier periods, but obviously, there are problems. Some variables are simply not amenable to measurement after the fact, particularly psychological measures. Even for factual events, it is known that respondent recall tends to be quite bad. Therefore, retrospective data is certainly less desirable than true longitudinal data. On the other hand, true longitudinal data requires a substantially greater investment of time and money.

SOME OTHER METHODOLOGICAL PROBLEMS

Some of the problems of data acquisition in this area have already been touched upon. In order to do a longitudinal study you have to have identifying information which allows you to get back to your respondents. This makes the protection of respondent confidentiality a considerably more difficult problem in longitudinal studies than it is in other types of studies; and when self-incriminating information is being secured, such protection is very important. I think it is easy to underestimate the difficulties of creating a protection system that will really give respondents the protection they are usually promised. Fortunately, it is now possible for drug investigators to get rather sweeping protection of their data by applying to the Drug Enforcement Administration for a Grant of Confidentiality.

Another obvious problem in longitudinal studies is tracking down respondents for follow-up interviews, and the difficulties increase as the time interval gets longer. One thing that I learned early in this game is that the post office does not keep forwarding addresses for more than a year; it is post office policy to destroy them at that point. Some of the obvious means of tracking through the mail, therefore, are simply not possible. But there are other procedures -- contacting neighbors, using credit agencies, and securing in advance the names of parents or friends who would know

the respondent's whereabouts. When youthful populations are being followed, parents are probably the best intermediaries.

There can be a pretty high cost incurred in follow-ups. One faces a trade-off between the monetary costs of increasing response rates versus the accuracy costs of retaining smaller proportions of the panel. Even when respondents are located and are willing to cooperate, it can be very expensive to get interviewers to those who have moved some distance away in the intervening period. Mailed questionnaires and phone interviews, of course provide somewhat less expensive alternatives for follow-ups than personal face-to-face interviews. However, then there is a question of how comparable the data may be to that gathered with face-to-face interviews.

Another concern, in those longitudinal studies in which the investigators return to respondents a number of times, is maintaining respondent interest and cooperativeness. In the Youth in Transition study already mentioned, we went back to the respondents five times. They were paid money on each occasion except the first, starting with $2.00 and ending with $10.00 on the most recent occasion. They were also sent newsletters periodically. We managed to get a very high response rate throughout the study -- more than 70 percent over an eight-year period. However, since there was no experimental manipulation, it is not possible to determine how much of the panel retention can be credited to having such procedures.

It has already been mentioned that there is a selective loss of people who are heavier users -- the people who are in some ways, the most interesting in this type of research. We know from our own data that the population which moves residencies more frequently also tends to have a higher incidence of drug use than average. But in the case of the normal population in the Youth in Transition panel we found that re-weighting the data to correct for differential loss rates really did not make that much difference in our overall incidence and prevalence estimates. In any case, I reiterate that the incidence and prevalence data are the less important of the longitudinal products and the relationships between variables over time are the more important; and I think it is still possible to get quite valid estimates of relationships even with some attrition in the sample.

One final note. There is an apparent dilemma presented by the fact that the large samples are needed to accurately investigate rare events in the population, such as the use of certain drugs, yet the cost of intensively following up large numbers of people could become prohibitively expensive. The optimal solution may be in the use of a mixed strategy; that is, one in which one works with large samples with limited follow-up efforts in order to secure enough cases to explore relationships; but also has a smaller subsample which is followed up very intensively to elicit a high response rate, and against which the representativeness of the larger sample can be calibrated.

CONCLUSION

As I have tried to illustrate with this very brief overview, the power of longitudinal research can be very great and much needed in a field like drug research, with its constant search for causes and consequences; but such research is also exceptional in the number of methodological and practical problems it presents. Let the buyer and seller both beware.

AUTHOR

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DISCUSSION

Dr. Rootman: Although I would not want to derogate the use of getting information on drug use, I think that much greater attention should be paid to the question of getting information on drug-related problems. I feel that surveys up to this point have been focused on the former and not the latter and I feel that there is some potential with using survey methodology to get relatively useful information on drug-related problems as well.

Dr. Johnston: I think probably most of us would agree with your general point which is that things other than incidence and prevalence are of importance -- perhaps of more importance. In the work that I reported on "Youth in Transition," we did in fact look at the history of both academic grades and junveile delinquency other than drug use over time, as a function of drug use. Basically, what we found was that an involvement short of addiction did not seem to have any effect on academic performance nor any effect on juvenile delinquency.

Dr. Robins: My study has involved asking interviewees about withdrawal symptoms, unemployment, treatment, whether they feel treatment has helped them or not and whether they feel drugs have made a difference in their lives.

Dr. Gottschalk: We have been having a problem in longitudinal studies with regard to the effects of different interviewers and the slightly different interview biases. Secondly, in any kind of interview you have the possibility of their positive or negative biases with respect to the response of the interviewees. That is, they want to make themselves look good or they may want to make themselves look bad. Is there anything that your group attempts to do in either of these cases?

Dr. Johnston: With regard to looking good or looking bad, Eric Josephson mentioned putting a fictitious drug into the questionnaire; this has been done in other surveys as well. The responses of those who want to look bad and say that they are using drugs which they really are not using has been very low -- about one percent say they have used whatever the drug is. Although we did not test this item in this particular study, I have been convinced by the other studies I have seen that we do not get much exaggeration in drug use.

I think whether or not you are dealing directly face to face with an interviewer has relevance here. Although we had interviews in this study, the drug data *per se* were gathered as self-administered questionnaires which the interviewer never saw. The respondent knew the interviewer would never see it because he sealed it up himself. So I think there was a minimum of wanting to impress someone involved in the situation.

Surveys—Relational Studies

Denise Kandel, Ph.D.

There are several issues I would like to address. First is the problem of informed consent. I am afraid that this is going to become an issue of increasing restrictions in our work. I know that there are some regulations being contemplated by NIH recommending that any study on children seven years and up, the upper boundary not being specified, would require informed and written consent of both the child's parents. Obviously, this would completely prevent any kind of large-scale surveys. In my own case, in 1971 when I started a longitudinal study of adolescent drug use in the state of New York, I was faced with very stringent requirements. The requirements, interestingly enough, were not set up by the schools which I contacted in the course of my work, but by my own institution, the Psychiatric Institute, which is supported by the New York State Department of Mental Hygiene and is affiliated with Columbia University. The Clinical Review Committee which reviewed my study specifically requested that I obtain *written* consent of the parents of the adolescents in the study, or approximately 10,000 parents. I pointed out that this would completely destroy the study and I might as well return the money to NIH. The compromise solution was for me to notify each parent of the study via certified mail, return receipt requested, and to give the parent an opportunity to refuse his child's participation in this study. This was helpful since instead of requiring positive consent, we relied on refusals. We also put a copy of the questionnaire on file in the principal's office of each of the sample schools so that when the parent received the letter, he indeed had the opportunity of informing himself by going to the school, look-

ing at the instrument and seeing whether or not he wanted his child to participate. Out of interest, I tried to keep track of how many parents actually took advantage of the opportunity of going to the school to look at the questionnaire. I think that six out of 10,000 parents did so. Three percent of all parents sent back slips requesting that their children not participate in the study. However, in one New York City school the rate was much higher (14%). The principal indicated that there was a very conservative and closed group of Italian families who had all decided not to participate. These refusals created a lot of complications when we did the field work since we had to take these children out of the classrooms. Some of the youths were embarrassed, they wanted to participate but could not do so. It is very hard to know to what extent parental refusals biased the sample. No information is available on the youths excluded because of parental refusals.

The issue of the protection of human subjects also intrudes in our work to a great extent. Because the behavior inquired into is illegal, identification and linkage of records at one point in time and over time has to be done without the use of names. In my own case, I am not only involved in a longitudinal study, but also in a relational study in which I am trying to put the behavior of a particular respondent in his social context and want to study closely his interpersonal relations. In this particular study, I had to relate the answers of a particular adolescent to those of his parent and his schoolfriend without the use of names. We relied on self-generated identification numbers, a method increasingly used in longitudinal drug studies (Haberman et al., 1972; Groves, 1974). In the particular procedure we used each adolescent was asked to construct a number for himself based on the middle letters of his first and last names, his date of birth and the last two digits of his phone number, and, in five schools, a number also for his best friend in school, identical to the number which the friend constructed for himself/herself. Most adolescents (94%) were willing and able to construct a number for themselves. Some of the numbers were incorrect or incomplete. However, individuals could often be matched on the basis of other information. Having previously carried out a similar relational study on adolescents, parents and schoolfriends in which names were used (Kandel and Lesser, 1972), I was able to evaluate the loss of cases involved when matching is done with codes rather than names. It is substantial. For example, 92 percent of the adolescents could be matched to a best friend in school in the earlier study as compared to only 46 percent in the present one. Matching on the basis of self-generated identification code numbers is a compromise procedure inferior to the use of names. Not only does it reduce the overall rate of matching, but it potentially introduces a bias in the resulting matched sample. Students who do not provide a correct code and cannot be matched are more likely to be drug users than those matched. For example, 24 percent of the students who could be matched to themselves at Time 2 reported marihuana use at Time 1, compared with 41 percent of those not matched. This confirms the point made previously by Drs. Johnston and Josephson; the people that are lost in surveys are the heavy drug users. Of

course, the rationalization is that since we are interested in processes related to drug use, we assume that the heavy users that we did include in the sample behave similarly to those we did not contact. I am not sure that this is a correct assumption. In any event, the data are not available either to prove it or disprove it.

The other point to which I would like to address myself, is one that Lee Robins made in connection with heavy drug users. She stressed the problems involved in locating those cases, and suggested on the basis of her experience that once you have located them, you are able to get useful information.

Our experience has been different. In our school studies, we were very much concerned about the drug use of students absent from school on the days we conducted our surveys, and carried out two studies to assess the levels of drug use among these youths. In the first study, we obtained the names of absentees in three schools and conducted household interviews. We stopped the study after 60 such interviews -- only four adolescents reported having ever used marihuana. We realized that these youths were not reporting any drug use. We analyzed the responses to see whether the conditions surrounding the interview affected the responses. None of the factors, whether the interview took place in the home, with or without parents being present, or outside the home, seemed to make any difference. We carried out a second study modelled after a design developed by Drs. Elinson and Josephson. We contacted another group of absentees, asked them to come to a neutral place and gave them anonymous questionnaires in a group situation. Out of the 238 students we contacted, only 44 (or 18.4%) self-selected themselves to appear (Kandel, 1975). This is a tremendous loss of cases. But this time, we did get reports of drug use: 12 percent, for instance, reported to have used heroin. This is a very high proportion: exactly twice as high as the rate of use reported by the students included in the regular survey in these three schools. However, analyses of the data made it apparent that the self-selected absentees underrepresents the heavy uses in this group of youth. Whereas all high school studies report that drug use increases with age, among the self-selected absentees participating in our study, the *highest rates appeared among the freshmen*. It became very obvious that the juniors and seniors, especially the boys and the black students, did not respond to our invitation to participate. Therefore, I am pessimistic about the possibilities of getting useful data on heavy users using traditional epidemiological surveys. It may be better to use participant-observation in the streets, as has been done by Hughes and Crawford, or other unobtrusive methods.

AUTHOR

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DISCUSSION

Dr. Lettieri: The phrase we are now using for that technique is written "dissent" as opposed to "consent" and there are some draft bills around that pertain to that.

Regarding the non-respondents, I would like to mention one of the techniques used in the contract with Hempsted N.Y. Research and Evaluation, Inc., by Richard Block. With a sample of 9,000 students in over 70 school districts in nine states, Block wanted some record of non-respondents. He asked the teacher at the time that the questionnaire was administered in the high school to fill out a sheet of paper; there was a left column which listed all the names of the students and there were several columns to the right which listed the sex, the age, the grades, the race, and whatever else the teacher happened to know. The teacher filled it out on the whole class and then put a check mark whether or not the student was present that day and had handed in a questionnaire. The teacher then tore off the list of names and gave the investigator all the demographic information. He could then get a feel for any kind of selective non-response rate if there was any, at least on some demographic variables.

Non-Survey Observational Techniques

Gail A. Crawford, Ph.D.

I have been asked to introduce the topic of non-survey, observational techniques in drug abuse research and to discuss some of the problems involved in carrying out eipdemiological field studies. By observational techniques I am talking about a combination of methods for collecting longitudinal or ongoing data on target groups or individuals in the natural setting. Among the most noteworthy of these techniques are direct observation of, and informal conversations with people "on the street," as well as more formal or structured interviews with members of the population of interest. A number of researchers also use indigenous fieldworkers either as informants or as guides to facilitate their own entry into the street drug using population.

There is considerable range in the types of observational studies, from analytic, descriptive accounts of what the addict's life on the street is really like, to natural histories of drug using groups over time, to studies that approach field experiments in drug abuse control. Our own work has been primarily in the latter category, and in this presentation I want to talk about some of the problems and issues we have confronted in carrying out field studies of Chicago heroin addicts.

We have developed an epidemiological field team structure that combines exaddict fieldworkers with trained social scientists to gather longitudinal data on heroin prevalence and incidence trends at the neighborhood level. Our fieldworkers are generally patients or counselors in the drug abuse treatment system. Most were high-

status dealers in the target areas and already have the trust and respect of addicts on the street. Fieldworkers act as participant observers to identify active drug users and record information on the functioning and dynamics of local heroin distribution systems or "copping communities." The social scientist's role in this team structure is to make periodic site visits to verify fieldworkers' reports and to conduct structured, personal interviews with members of the target population to gather additional epidemiological data.

The first kinds of problems I want to talk about are those that involve locating active drug users in the community and, having located them, collecting basic demographic and drug use information. Usually it is not difficult to locate drug using groups in a particular area. By talking with knowledgeable people, such as counselors in drug treatment programs, patients or police, one can identify the sites where users buy and sell drugs and where they "hang out" or congregate. However, having identified the major "copping areas" (or heroin distribution sites) and hangouts within a neighborhood, how can the researcher gain access to the active users who frequent those areas?

In our research we make use of indigenous ex-addict fieldworkers who have established ties with addicts on the street; in effect, they are abstinent members of the local heroin using subculture. The legitimacy of the fieldworker's researcher role stems largely from his affiliation with the drug abuse treatment system. As he begins to explain to addicts on the street the nature of his work, he can point to the treatment program base to insure the confidentiality of all information collected and the nonpunitive, medical orientation of the project. The only incentives offered to active addicts for their participation in the research may be the possibility of later being given special access to a preferred treatment facility or an opportunity to participate in a paid interview. When asked, "What's in it for us?", the fieldworker must be honest; he should make no promises regarding the provision of treatment services or of interview money unless they are a virtual certainty. However, he can point out that participation in the research is neither time-consuming nor demanding on subjects; generally it involves nothing more than occasionally talking with the fieldworker on the street. Therefore, participation does not constitute a significant intrusion or threat to the addict's way of life.

I might point out that even our best fieldworkers have limited access to some kinds of drug users. Fieldworkers are likely to have established relationships with people who were using drugs when they themselves were on the street; it is more difficult for them to identify and gain access to new, young heroin users who may not "cop" every day and therefore are less visible. To partially resolve the problem of identifying new young addicts and experimenters, we make use of a "snowball" technique. In their everyday conversations with addicts on the street, fieldworkers attempt to gather information on new heroin users known to the respondents. Similarly, in conducting formal interviews with active addicts, we

collect data on the process of heroin spread -- specifically, who initiated the respondent to heroin and whom he in turn may have initiated. The respondent's help may then be enlisted in arranging interviews with addicts or experimenters not previously known to the fieldworker.

I want to discuss briefly some of the problems involved in actual data collection. Observational studies rely on repeated measurements or recordings over time. Their success, then, is contingent upon the ability to maintain a good relationship with the target population. For example, the researcher or the fieldworker who makes too many promises to subjects in the beginning is likely to be in an uncomfortable position later if he cannot deliver. Other things that can jeopardize ongoing data collection are simply natural or uncontrollable events in the community. Thus, police pressure on local copping areas or pressure from violent street gangs can cause the addict population to temporarily leave a community until it "cools off." Also, individual members may be removed from the community as they enter treatment programs, are arrested or die. Because our work relies on following a particular group of addicts over time, perhaps a year or more, we try to insure that as people are lost, as attrition occurs, we can obtain at least second-hand information on what happened to them.

Another factor that can be of great importance in observational studies is simply the weather or the season of the year. In a city like Chicago field studies are not much fun in the wintertime with two feet of snow on the ground. When it is cold or rainy, people disappear from the streets. Instead of hanging out at public places to which fieldworkers have easy access, they stay home or they cop their drugs and immediately "split." So there may be problems in maintaining contact over time with the same group of people. If the fieldworker has established good relationships with individual subjects, however, he is more likely to be able to follow them into a variety of places, including private places, and to collect second-hand information from multiple sources on subjects he does not see on every visit.

Next I want to talk about some of the problems arising from specific methods of data collection in observational studies. Our research methodology requires that the social scientist members of the team regularly visit local heroin distribution sites in the company of the fieldworker to make independent observations and evaluations. There are, of course, some places to which we are denied access and some kinds of activities that we are not allowed to witness. We are then dependent upon the fieldworker's description of these places and activities. Another question concerns the reactive effects of our presence on the street. Do people for the most part talk and act freely when we are around? Over time these kinds of problems probably diminish. Gradually, we tend to be accepted (or sometimes tolerated) by most addicts on the street to the extent that we are often surprised by the kinds of information people freely volunteer. Nevertheless, there are some areas where we simply cannot make lengthy field visits. For example,

we are currently involved in researching heroin spread in two black inner-city areas. One of these areas is relatively safe; a white researcher can go out with a black fieldworker, spend time hanging out and talking to people. In the other area, even in the company of the fieldworker who knows the local addicts and can reassure them of our motives, we may run into problems. We can spend a certain amount of time hanging out or conducting interviews, but beyond that people become suspicious or begin to worry that our presence will "bring down the heat." We have learned to take our cues from the fieldworker. When he says that people are getting "uptight" and that we had better get out, we do. In these communities we have to make frequent field visits for shorter periods of time in order to minimize the disruptive effects of our presence.

Our methodology relies on using fieldworkers not only as guides for the researcher in the field but also as a primary source of data collection. The advantages of having exaddict fieldworker-informants include their access to people and places to which we would be denied entry and the minimal reactive effects of their presence. On the other hand, there are some potential disadvantages to relying on fieldworkers as instruments of data collection. For example, our fieldworkers keep daily log books or field notes of local events, such as changes in police pressure, heroin cost and availability, which might be expected to affect heroin prevalence and incidence trends. It takes a good deal of time and effort on the part of the researcher to teach fieldworkers what kinds of data, what kinds of events are of interest and how they should be recorded. Problems can arise if the fieldworker relies on his own preknowledge of what is occurring rather than actually going out to talk to people and observe what is happening. To minimize these problems we have developed a working partnership between the researcher and the exaddict fieldworker. We as researchers must also make regular field visits to talk to active addicts so that we are not too heavily dependent upon the fieldworker's reports. The more time we spend in the field, the more people we talk to, and the more we find out about their daily routines, the more likely we are to arrive at an accurate and well-rounded picture of the social organization and functioning of neighborhood heroin distribution systems.

I would like to say a few words about our efforts to conduct formal interviews with addicts on the street. We found this to be necessary for at least two reasons. First, some of the things we are interested in, such as the process of heroin spread, are not observable. Because first use of heroin is not amenable to direct observation, we have to use more formal interviewing techniques, in other words, asking subjects to reconstruct the circumstances of their initiation to heroin use. Structured interviews also enable us to check on some of the information reported by the fieldworker. By comparing what the fieldworker says about the subject with what the subject says about himself and what other people say about him, we can increase our confidence in the reliability of our information.

Trying to conduct interviews with active heroin addicts is probably difficult in any situation, and the problems are compounded when one tries to do paid interviews on the street. In any field study the researcher or the fieldworker has to adjust his own schedule to fit those of the subjects. He must make himself constantly available if he is to track down people for interviews. We have found that fieldworkers often can prearrange interviews with individual addicts for a certain day and a certain time, but we must still maintain a good deal of flexibility. In some cases, of course, addicts who have a good "hustle" are not interested in making $10 for a one-hour interview so that we have to rely on the street status and skills of the fieldworker to pull them in.

I want to end with a few remarks about problems with reliability and validity of observational data. One criticism that is aimed at many observational studies, and one that perhaps is often deserved, is that they fail to make a strong case for their conclusions or interpretations. The researcher may not define the population base or the period of observation; he may fail to specify the number of people observed or interviewed, the number of times a particular event was observed or the sources of his data. As a result, the reader may be unable to judge for himself the extent to which the sample is representative or the findings generalizable. Also I think there often exists a kind of distrust of so-called "soft" or descriptive data. The researcher presents his interpretations of the addict's life on the street or of the social organization of the addict subculture, but one wonders why the researcher's word for what is going on should be accepted. There is no easy answer to this question, and whether the criticisms are warranted is, of course, dependent upon the particular study. However, careful attention to methodological details and the use of multiple observers and methods of data collection can help to reduce problems of reliability and validity. In our work, the exaddict fieldworkers and the addicts themselves are the "participant" observers; the researchers are outsiders -- nonparticipant observers. By supplementing field observational data with structured interview data, we are able to obtain a wider range of information and to cross-check the accuracy of any one source.

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Non-Survey Techniques—
Sub-Cultural Groups

Joseph P. Fitzpatrick, S.J.

The study I am involved in and that I am going to use as the example to illustrate the larger discussion that I will suggest in a few moments, is a study of Puerto Rican addicts and non-addicts in the East Tremont section of the Bronx, New York.

We were not convinced that either the psychological, the medical or the statistical reporting on the character of drug abuse or the incidence or prevalence of drug abuse or the epidemiology of drug abuse was really getting to what was actually happening. We chose a neighborhood with which we are very familiar -- it is just south of the University actually -- in that part of New York City which is in advanced slum conditions and in an area which is rapidly changing. It is predominantly Puerto Rican with a smaller percentage of blacks, and a tiny number of residual whites who are still around. We sought an anthropological approach -- to look at that neighborhood as a human dynamic experience and to look at what really is going on, and what the life experience of these young people is -- and to ask in that context, what the experience of drug use or drug abuse really means. In that context can we say anything about the epidemiology of drugs, can we say anything about the treatment of drugs, can we say anything about the prevention of the use of drugs?

We confined our study to the Puerto Rican population for two reasons. One, it simplified what we were doing because actually, although the blacks and the Puerto Ricans are living together on the same blocks and the same houses, they are two very distinct popula-

tions. There is very little interaction between them. Walk along East 180th Street and at one corner you have the blacks and two corners up, you have the Puerto Ricans; they live in two quite different worlds. The character and quality of their cultural and social lives are different. We selected the Puerto Ricans for a second reason: for many years, I have been very much involved in studies of the Puerto Rican experience and I had extensive contacts with the Puerto Rican community which facilitated access to the population we wanted to study. The problem that Gail Crawford mentioned regarding access to a population that you want to study is extremely important.

We wanted to look at a youthful male Puerto Rican population -- we eliminated the women also to simplify the study -- in a situation where they are in the process of adjustment to life in New York City and in a slum situation, and to see if we could find out really what is going on. We chose a method of analysis, a method of study that a colleague of mine, John Martin, and myself had developed in previous years in our studies of delinquency; it is what we call structural analysis. It consists of a threefold interdisciplinary method. One method is very extensive area analysis. In other words, we staked out an area of the city and we sought to study it, to gather all the possible information about it. We have been developing information on this area for years, so that we know it very well and we have many contacts in the area. That was the first dimension. What we were seeking really was an insight into the dynamic interaction that goes on among the Puerto Ricans in that area and then examine the use of drugs in relationship to that. The area analysis was first. That is a continuing process. It not only involves the accumulation of all the demographic data we could assemble from the census material, from the city planning offices, from the schools, from the agencies in the area; it also means continuing to update our immediate knowledge, our observational knowledge of the area because of changes every day. Every day we go down there another building has been burned out. The place looks like Dresden the day after the bombing; there are entire blocks where every apartment house is abandoned and burned out. It is really a pitiful and sad looking situation but it is there. Therefore, we must continually update the knowledge of the area. Most important from the viewpoint of area analysis is our information from the subjects. We learn from them what they are doing, where they live, where they interact with others, what is the pattern of their life as it unfolds in that area, and we feed that back into a continually increasing knowledge of the area. The knowledge of the area becomes an extremely important context for an understanding of drug use.

The second dimension of the method is what we call sociogenic case history, which is simply a history of the young person with a focus not so much on the personality variables, on the character or quality of his personality, but in terms of the total experience that he has gone through in the social context in which he has lived, the cultural background from which he comes, and the experiences that he lives through day after day.

This neighborhood, for example, is next to an Italian village. There is a small, very stable, very well put together Italian section that borders immediately on the study site. The conflicts between Puerto Rican and Italian communities have been severe in both directions. As a result the social condition of hostility, conflict, respecting various turfs and so on becomes very much a part of the life experiences of these young men -- their experience with the police, for instance. The sociogenic case history seeks to incorporate an understanding of the cultural background from which the subjects come and of their attempts to adjust to life in New York City.

Thirdly by situation analysis, we seek to get from our interviews with them as clear a presentation as they can give as to how the process of either using drugs or avoiding the use of drugs took place. In the careful accumulation and analysis of the data from this threefold method, we hope to achieve a very good perception of the dynamics of life in that area. In this context, we hope that we will be able to explain a little more clearly what the use of drugs, the abuse of drugs, or the avoidance of the use of drugs really means to that population.

The formalities of the study call for interviews with 50 addicts and 50 non-addicts. The study is not a survey; we had to spend a great deal of time, about six months, just getting a feel and a sense for the neighborhood, seeing if we could locate where the real sources of drug action in the area were. Critical to this was our ability to establish contact with significant people in that area, i.e., significant informants. Some of them were people that I had already known. I am a clergyman and have been in close association with the parishes in that area. There are a number of natural leaders in the parishes who have close contacts with many of the young people. The value of this kind of contact is credibility. How can you convince these young people to talk to you? What would motivate them to talk to you? Why would they be willing to spend a couple of hours in an interview with one of your interviewer? What are they going to get out of it? Who are these people anyway? These neighborhood contacts were one basis of credibility.

Secondly, the University was another source of credibility. The University is close to the area and the fact that this was a Fordham University study brought with it a great deal of prestige and interest in collaborating with a university project. We still have not met anything like the "Town *vs.* Gown" conflicts that some of the universities around the country have faced with their neighboring minority groups, such as Columbia's continuing problems with its neighbors. The Bronx has not had that kind of problem. An association with the University apparently means something to these young people and they are willing to collaborate. Most important of all was one of our contacts, one key person who plays a very critical role as an informal kind of leader and a center of information on activity in the area. I think he knows not only every addict in the area or every person who is using drugs

but he must know everybody else too. It is incredible what influence this man has. He used it to assist our study.

Our street interviewers are three tough street-experienced Puerto Ricans, completely bi-lingual and very carefully trained as interviewers. They are not uneducated street kids; all of them are college graduates and one of them has an M.A. But they are still close enough to the street to have the experience of the streets and the ability to effectively contact people of their own kind. They can very easily adopt the style which is common among the young Puerto Rican men in the area. This question of effective contact is crucial.

By the end of the study, we will have completed interviews with 50 addicts and 50 non-addicts. There is no attempt to reach a representative sample; what we are looking for are the strategic centers of activity. We have already begun to identify those, trace them out and move towards them. We establish contact with the addicts or the non-addicts who are related to those significant centers and ask them to agree to an interview with one of our interviewers -- that is really the way that it has proceeded.

Technically, there are two stages in the accumulation of the data. Once we have established effective contact with a person who is willing to be interviewed and collaborate with the study, we administer a fairly long interview schedule, a carefully prepared schedule. The first thing that they do is fill that schedule out.

We had some interesting experiences with this during the pilot period of the project. We began by giving it to subjects and asking them to take it home and fill it out if they wished. But we were not getting them back. We then used a second strategy: when the interviewer met a subject he would give the schedule to him and ask him to take an hour to fill it out in the presence of the interviewer. That did not work very well. We are now following a third strategy: the interviewer has a copy of the schedule and the subject has a copy of the schedule and they go through it together. If the subject wants the interviewer to mark down his answers, the interviewer will do it. If he prefers simply to answer the question and record it on tape, the interviewer fills out the spaces later on. We are very flexible in this -- the object being to get the interview schedule completed, every item filled in. This provides us with comparable data. On many of the questions, the information we accumulate is the standard information found on questionnaires or interview schedules employed in surveys. We developed the interview schedule on the basis of an examination of a wide range of schedules and interviews that have been used in previous studies of addicts.

Much more important, however, after the schedule is completed, is a lengthy informal interview with the subject. This provides a great deal of elaboration which does not come through on the schedule as it is filled out. Every interview is taped. Four professors who are working on the study review the tapes. We review

the interview schedules and then we interview the interviewer. This we find to be very important. The interviewers prepare a report explaining how they contacted the subject, describing the circumstances of the interview and adding observations of their own. But we have found that the most valuable insights into the area come not from the schedules, nor from the taped interviews, but from our interview of the interviewer after we have gone over the schedule and the tape. We spend considerable time with the interviewers, and remarkable insights are gained in this process, things which they saw or noticed or know, but which they did not realize would be significant in this kind of a study.

These are the methods by which we are accumulating the data: the schedule provides the systematic data that will be comparable for all of the people we interview; the informal interview on the tape and then our interview with the interviewer provide the more qualitative data. We will put this information together, analyze it and, hopefully, by the time the study is finished we will have some significant insights into the whole pattern of life that goes on among these Puerto Rican youth in that area and be able to explain more clearly than it has been explained before what meaning drugs have in this context. Is there any specific relationship between a background of Puerto Rican culture and the process of either becoming addicted or avoiding addiction? These are the kinds of questions we are really hoping we will be able to answer.

With that in hand what does this mean to the kinds of questions that were raised for us concerning the utility of survey sample data. In other words, what is the value of these data or any kind of ethnographic study of the epidemiology of drug use or drug abuse? Our data will be limited in area and limited to a particular population and a particular cultural group. There is no doubt that what we learn from this population may have little meaning for a population in Westchester or Nassau County. The youths we are studying do not consider the use of marihuana as drug use. They all use it. It is part of a pattern of polydrug use; they are into all kinds of things, from alcohol down to all kinds of pills, to methadone. The epicenter of the drug market is in a park across from a methadone center. Some of them ask, "Why don't they put a pipline across the street, it would save us a lot of trouble," because so much methadone is leaking out of the center into the park where they are marketing the goods. In terms of epidemiology I think the value of our study is found in this: when we get the kinds of scientific data that are made available through the processes that Dr. Elinson was discussing, these data must be supplemented by much more intimate knowledge of particular areas. I think the value of what we are developing is the insight it will give to a particular area. Secondly, the particular findings from the Bronx will not explain what goes on in Bronxville or Scarsdale, but a similar method used in Bronxville or Sarsdale will explain a great deal more about Bronxville or Scarsdale than the statistical reports on incidence or prevalence.

Thirdly, in terms of prevention and rehabilitation, I think that we will be able to provide a number of insights into the way cultural backgrounds become related to drugs that will help us a great deal in trying to clarify what kinds of programs may be effective in preventing drug abuse and what kinds of programs may be more effective in the rehabilitation of people who become addicted.

Let me conclude with one brief comment. As we gather more and more knowledge of this population of Puerto Rican youths which I am studying, we continue to read of the preoccupation of the nation with the problem of drug use and drug abuse. Once you begin to deal with this population however, and gain an insight into their lives, you begin to realize that in the context of the enormous other problems that they are facing, the use or abuse of drugs is a relatively minor issue. I think this may say something very important to us as we try to deal with the different characteristics of drug use and drug abuse with different populations in the nation.

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Institutional Sources—Arrests

Leonard Savitz, Ph.D.

This discussion addresses itself to problems of arrest data relating to the problem of drug abuse. There are obvious and considerable uses that can be made of criminal justice system records and data with regard to the problems of drug addiction and drug abuse -- such as, the association between criminality and addiction, the relative criminality engaged in by drug users *vs.* that of non-drug users, comparative costs to society of the several criminal populations. Under some conditions the data will be available to the extent that one can use and perhaps develop temporal relationships, i.e., the sequential order of which came first, drug use or criminality.

The first problem that seemingly has to be determined is at what point in the criminal justice system do we intervene and gather our data. It seems logical and perhaps useful to secure data at three points in the processing system, each producing quite distinctive populations. We can select out, first of all, arrestees when they first enter into official criminal justice processing, that is when they first arrive at a police processing facility. This would produce the largest numbers of "criminals" and have the lowest case mortality. On the other hand, this group has the lowest probability of subsequently being *convicted* for the crimes for which they were charged.

The second point of data collection is at that point at which arrestees (and convicts) are diverted from the criminal justice system into some treatment program and still remain under some degree

of law enforcement control. This can occur shortly after arrest, or at a number of pre-trial and post-trial junctions of the criminal justice system. This population represents those arrestees and convicts who enter into varying diversionary systems which operate in various locales.

The third point of investigation could be at that point in the criminal proceedings when individuals are found guilty of their crimes and formally become *criminals*. The numbers here are the smallest and differential administration of justice can produce peculiar criminal populations, but they do represent the group which is most surely criminal.

For the city of Philadelphia it was our considered judgment that the best group to examine for the particular purpose we had in mind would be arrested persons at the time of their entry into processing within the criminal justice system. Once the decision is made a second problem arises: what is the full range of data which is *normally* and *naturally* collected and therefore available at this particular junction? There are, first of all, police records which are normally and naturally generated for all arrestees. They are also available with not much difficulty.

Typical police records, and there is some evidence that Philadelphia is not unique in this regard, are of three types. First there is the brief police statement on the current direct charge: this very simple document contains relatively little information beyond a description of the crime and is used largely for record keeping purposes. Secondly there is also a detailed police report on the current charge completed by the arresting officer which gives a great deal of information about the criminal event. It is extremely difficult to secure this particular document usually because it is of enormous use to the Prosecutor in determining what type of case he has and what witnesses he has and what evidence is available. The third body of data that the police have and which can be made available is an Extract of (previous) Criminal Records, containing, in considerable detail, all previous arrests, all previous charges, convictions, and imprisonments, both within and beyond the local jurisdiction.

Considering drug charges, certain specific problems do arise with the use of police records. First of all, arrest records are filled out by arresting officers who aren't terribly concerned about certain types of details which do not have pragmatic implications. Therefore the extract of criminal records, for example, will normally state that the individual has been arrested for private possession or sales of drugs without specifying the type of drug. The second problem with the use of the official records, particularly when drug charges are involved is that the police and the district attorney often choose convenient charges rather than technically correct charges. The individual therefore may be charged with possession of drugs or the sale of drugs or, in Pennsylvania, possession with intent to traffic. This last offense is frequently used by the police and prosecutors rather than going through the elaborate procedure of attempting to prove that a sale did in

fact take place or that there was an offer to sell; conviction for the intent to traffic brings penalty almost equal to a sale conviction. Generally police-related data fail to give any detailed demographic, social, economic or work-history data.

In many large metropolitan jurisdictions there exist some pre-trial services branches within the criminal procedure. That is, there are agencies involved with bail and court bail interviews typically take place for virtually everybody who was arrested. The purpose of the pre-trial interview is to secure data for determination of whether or not the arrestee should be released on his own recognizance (R.O.R.). This type of interview given by the trained employee does gather an extensive body of data on family history and relationships, educational history, occupation history, income and wealth status, military history, residence, and migratory history. Many of these bits of information are verified by direct contacts with individuals who can substantiate what the arrestee has reported in the pre-trial interview. The court bail interview also secures some details as to criminal history which cannot be found in any other body of data; that is, information is secured not only on previous adult arrests and convictions, but also on all juvenile delinquencies and juvenile institutionalizations. One additional feature of the Philadelphia court bail interview is a series of items concerning *current drug use*. That is, the arrestee is asked if in fact he is currently a drug addict. If he answers yes, then a variety of other questions are asked of him concerning the types and amount of drugs he uses, his treatment history -- current and past treatment, and whether or not he desires drug treatment.

In June of 1973, the court bail interview was changed and a new item was added in which the arrestee is asked if he is not currently an addict, whether he was ever once an addict. This is an extremely interesting piece of information. While there are obviously reasons for the arrestee to dissemble about current drug status (it might be in his best interest to deny being an addict to avoid being stigmatized, or to lie and state that currently he is an addict, which would then permit exit from the system by some currently operating diversionary mechanism), this seems to be no reasonable grounds for false reporting of exaddict data.

There is a third source of information frequently available at the time of arrest and processing and that relates to the presence of a mass urine screening program.

We are using these three different sources of arrest data to deal with several problems. The major problem being dealt with is how to operationalize the concept "drug users" in the criminal justice system. One group which can be thought of as "drug addicts" are people who take heroin, methadone, morphine or other narcotics and are further defined on the basis of one or more identifying criteria. They have either (1) self-confessed they are an addict, and/or (2) produced a positive urine and/or (3) their current arrest charge is possession or sale of narcotic drugs. We also use one confirming criterion, which is the history of a previous arrest

for possession/sale of narcotics. (Note: if the individual has had only a past charge of possession/sale of narcotics, that person is not classified as an addict.) It is therefore possible to identify drug addicts by 1, 2, 3, or all 4 criteria; there are 14 combinations possible. Thus far, just by examining this population of drug addicts, we have found there are somewhat different distinct populations. That is, those individuals who are identified as drug addicts on the basis of positive urine are somewhat different from those drug addicts who are identified on the basis of current drug charge.

Secondly, the number of people who are identified by all individual criteria are themselves somewhat different. Positive urine alone we have found, much to our surprise, identifies uniquely only 10 percent of individuals who may be identified as drug addicts in the criminal justice system. That is, if the mass urine screening did not list, 10 percent of all identifiable addicts would be lost. It is, in our judgment, a very cost-ineffective technique for identifying drug addicts. It is true that regardless of the cost, urine identification is often an absolute imperative in some jurisdictions where the judiciary and sometimes the prosecutor believe this is the only infallible identifier.

Another curious finding not easily explicated involves the varying degrees of certainty we have about addicts. Drug addicts can be identified by as little as a single identifier or by two, by three, or by all four criteria. The higher the number of identifying criteria the more certain we are that the person is an addict. We find that individuals who have been identified as drug addicts by only one criterion had far fewer previous arrests than those identified by two criteria, and those with two had a less extensive history than those identified by three identifiers. Thus the greater certainty that the arrested individual is a drug addict, the higher his arrest rate will be.

On the average it was found that 17 percent of all persons arrested were identifiable as current drug addicts. Current addicts had on the average 7.2 previous arrests while non-addicts had a mean prior arrest rate of 3.5. That is the total arrest population of 6,800 persons (in a 3 month period) had 54,638 prior arrests and 44 percent of these were that of current addicts.

The second group that can be identified from police records, in addition to those who are addicted, are individuals engaged in the use of non-narcotic drugs as identified by the urine screening or by current arrest charge. These account for 6.5 percent of all arrestees. There is a third population of exaddicts, individuals who define themselves as having once been an addict but do not self confess to current addiction or produce a positive urine or have a current charge of possession or sale. This group is approximately 6 percent of the arrestee population. And finally, or course, a fourth residual population of non-drug users exists.

We are interested, of course, in comparing the quality and seriousness of crimes committed by variously defined addicts, by abusers,

and by non-users. This can only be done, in our judgment, by the development of some technique that can quantify and assign specific values to the seriousness of previous and current criminal activities. We are, therefore, finishing a redevelopment of the Sellin-Wolfgang Scale of Seriousness which assigns a standardized numerical value or score to each offense based upon physical injury or property damage. This was originally undertaken in 1960. We are updating it in 1974 in order to determine the stability of scores after 14 years and to utilize statistical techniques such as A.I.D., and to ascertain scaled values for drug offenses.

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DISCUSSION

Dr. Robins: You said that 7 percent of the addicts would be missed if they had not had a urine test. What percentage of them were positive?

Dr. Savitz: If we tried to identify drug addicts and the criminal justice system did not have a mass urine screening, it seems that we would not pick up 7 percent; we would pick up the other 93 percent on the basis of self-admission or current charges.

Dr. Robins: Of all those who tell you that they are addicts how many of them have positive urine?

Dr. Savitz: We are looking at that single criterion too. We are looking at people who say they are addicts and produce positive urines, and those who produce positive urines and deny that they are an addict. We have 14 different groups of them right now, but we have no results yet.

Mr. Romm: In the TASC study we did, we found that there was a 15 percent loss as difference between those that would be identified as drug addicts or drug abusers by their own admission and those who were then verified.

Dr. Robins: So 85 percent of those who admitted it would have had a positive urine?

Mr. Romm: No, the other way around. We would have lost only 15 percent of the positive urines by self-admission. However, when one challenges the value of the urine test itself, this 15 percent variability gets lost in the "noise" level, I think. Dr. Berkowitz will address this issue during his presentation.

Dr. Robins: How many of those arrested were addicts?

Dr. Savitz: Sixteen percent, or about one out of six. Add to that an additional 6 percent who identify themselves as exaddicts and you come up to 22 percent who are, or were addicts. I think the 6 percent is a staggering figure.

Dr. Sells: I have heard the tendency to arrest people for drug use varies according to social and socio-economic levels. The appre-

hension of middle class drug users, for example, is a very different kind of problem. How much of a bias does that create?

Dr. Savitz: It depends on the drug. In Philadelphia and, I think, probably in other states, there are quarrels between the prosecuting attorney and the police on the *bone fide* nature of an arrest. The police will arrest in any case where there is sufficient evidence to indicate that this person is involved in some illegal drug use. The District Attorney operationalizes this, calling it a minimal type of arrest. That is, if he had two or less joints, the case is thrown out, it does not even enter the processing in the earlier stages and no records are kept. This is in considerable conflict with the police, who say it was really a good arrest and there was good evidence. The District Attorney says technically good, but it is too small to bother over. With middle class kids it's often the case of having just a few joints. Even the use of demeaning behavior with the police will get different results.

Dr. Gottschalk: I would like to make one comment in connection with Dr. Savitz's conclusions about the finding of morphine or heroin in urinalysis. I have often wondered about how carefully those specimens were collected; if someone else's urine were used, it might get a false negative. Secondly, it has not been studied as well as it should; for example, how long after taking the shot will it appear in the urine? It is likely to be present in the urine if the person recently took an injection, but if it has been several days, as for some users who are "chippers" or who are doing it with rather long intervals, you might not find it. I think that percent should be taken with some caution before a conclusion is drawn, because there have not been enough good studies. My presentation will deal with drug-related death and toxicological findings which indicate a rather disturbing amount of errors, either false negatives or false positives, even when you provide what you would think are the best toxicological survey groups in cities, with *known* amounts of morphine or other substances.

Institutional Sources—Treatment

Saul B. Sells, Ph.D.

The topic assigned to me involves data acquisition for epidemiological research, from treatment sources. From the viewpoint of epidemiology, treatment data must be regarded as collected for other purposes and, therefore, the use of treatment data for epidemiological research is a secondary use of such data. I wish to emphasize this at the outset, and will try to develop some points related to it as I proceed. It should be noted first that treatment samples are not random samples of the user population. They are not only biased, but they appear to be biased in systematic ways that need to be understood. Unfortunately the relation of the portion of the user population in treatment to the total user population is not known, but systematic biases are suspected. Within the population in treatment there is a dependence between treatments and the characteristics of the individuals assigned to those treatments. This could be a serious problem for anyone who attempts to use treatment files without awareness of the systematic variations that can occur.

Another problem that is characteristic of treatment agencies is that their records are usually very limited and unsystematic. The aversion of clinicians to keeping records is probably too well known to deserve further mention here, but this does emphasize a need for specially-designed reporting systems. The Institute of Behavioral Research (IBR) at TCU has had the privilege for the past six years of operating the DARP, the Drug Abuse Reporting Program, started under NIMH. Right now we are engaged in a follow-up study of samples of this population and we are finding that for a number

of the agencies that reported, the carbon copies of the reports that were sent to us are their only systematic records of their patients. There are other agencies, of course, that have maintained comprehensive patient records, but these are exceptions and the importance of systematic records and reporting programs cannot be overestimated.

Next there are problems of defining study samples; one of these involves the accuracy of entry and discharge records, and in particular the dates of these events. In our research involving treatment agencies, for example, we found initially an almost unlimited number of local conventions for deciding who would be considered a patient and who would not. There are some people who go through the "revolving door" without leaving very much of a trace. Others go through intake, but stay only a few days. And then there are still others for whom we have records for a full five years, without interruption. When one looks at samples in terms of standard definitions, the question of how to define a patient to be included in a sample is an important one. For some of our purposes, such as calculating retention rates and evaluation of outcome, the base on which these are calculated is naturally extremely important. But in any case, the treatment sample is not given simply because of files that may be available.

There are next a number of important legislative and ethical considerations concerning confidentiality and also issues concerning the effects of further contacts on former patients that are typically encountered in follow-up studies; the latter reflect major concerns of treatment staffs and should be treated seriously, particularly when the surveys are made by third parties rather than by the treatment staff themselves. In our files, the records were all recorded by agency code numbers which were meaningless without the codes. We merely required that the same I.D. numbers be used on every report. This was done to guarantee confidentiality, but involves several limitations that are worthy of mention. One is that when patients transferred from one treatment center to another, they could not be tracked, because the only I.D. was this unique number. The second is that now, when we are engaging in follow-up studies, we have to go back to the agencies to obtain the identities of the patients that we are attempting to follow. And I might mention that in the course of this, we have found that within the past year, a large number of different kinds of superordinate agencies have been created so that the agreement to disclose the I.D. for the purpose of the research may be enmeshed in complex new state laws, in state regulatory agencies, and in municipal and county agencies, so that the problem of negotiating to get this information is becoming increasingly more burdensome.

I would like next to mention that the design of a reporting system is in itself a problem; if it is designed for the purpose of evaluation of treatment, it may not satisfy the needs for administrative oversight or for epidemiological study. The concept of a

system to serve all purposes seems to me to be about as logical as a speech for all occasions; neither of these has proven to be effective.

Notwithstanding these problems and the difficulties, however, I believe that there are values in the use of treatment sources for epidemiological study. It is possible, for example, to fill in a matrix of populations in treatment according to a set of classification variables so that trends in the composition of the samples over time can be studied. In many cases it is possible also to note the changes in relation to particular problems; for example, in the case of our samples we have good reason to conclude that in a number of cities the lowering of age over time probably reflected the saturation of the available pool of people eligible for treatment that overstated the apparent change in the composition of the population of drug users. The older addicts who had been using for a long time were in many cases the first to enter treatment at a number of agencies, and later on the population was more heterogeneous. This occurred notwithstanding the fact that over four years of admissions we found a number of interesting changes -- a lowering of the over-all age of the population, a shift from about 18 percent to 24 percent females in the cohort samples, a change from about 55 percent black to about 45 percent, with a compensating increase in whites, a change from about 55 percent using heroin daily either alone or in conjunction with other drugs to a much lower percent, and a continuing increase in proportions of patients defined as polydrug users. Whether or not these changes reflect changes in the total drug using population, is a matter of speculation, but there are ways of examining support for such a hypothesis particularly when you can replicate samples regionally over a large number of cases.

Treatment data can also be used in designs using treatment and other population samples as a basis for estimation of death rates and related phenomena. In our studies we have published three reports in three succeeding years upon the differential death rates of addict samples. The advantage of using populations in treatment is that time at risk can be accurately tied down and therefore when one takes into account demographic factors such as age, sex, and race, drug use patterns, and other factors that might be of interest, including type of treatment, one can obtain very accurate rate estimates. At the same time, it must be acknowledged that people in treatment or under the surveillance of treatment agencies are much less at risk than typical "street addicts" and that as a result, the rates obtained for treatment samples are undoubtedly underestimates. Nevertheless, this information is of very great interest.

Finally, because of the extensive background data often obtained from people entering treatment, such as we obtained in the DARP program, it is possible to study problems that are related to epidemiology in the same way that Gail Crawford and Father Fitzpatrick indicated, namely by looking at correlates and different relationships among variables. We have reports on development of drug use patterns and on concomitance of drug use with alcohol; these data

indicate very clearly that the present widespread belief that alcohol use increases in methadone programs is unsupported and perhaps based upon biased samples in clinical settings.

I have also been intrigued by another result that has broad implications. As you know, in designing evaluation studies one can approach the problem in several different ways. One approach that we have followed has been to compare different samples, reflecting patient types and treatment types on criterion measures reflecting outcomes over time; this represents the conventional analysis of variance and analysis of co-variance designs. Recently we approached the problem in a somewhat different way by comparing patients classified into different outcome groups. For example if you take employment as a significant outcome you can classify patients in various treatments into a number of different groups representing different patterns of employment outcome. In this study of a methadone maintenance outpatient sample we used ten outcome groups defined as follows. The first were individuals who were high in their pre-treatment levels of employment and remained high throughout treatment. Then there were four groups that were at moderate levels of pre-treatment employment and who throughout the course of treatment varied in one case by increasing their employment, in a second by decreasing, in a third fluctuated, and in a fourth remained steady at a moderate level. There were four who had low levels of pre-treatment employment and were again classified as increasing, decreasing, fluctuating, or steady. And then there was a group who was unemployed before entering treatment and remained unemployed throughout treatment; that made ten. That, incidentally, was the largest group. We classified samples in this way on a number of different criteria. What we found that I say intrigued me very greatly was that although when we look at outcomes for treatment groups without this kind of classification there appears to be no correlation at all among outcomes in treatment -- that is, one may increase in alcohol use, decrease in alcohol use, decrease in drug use, and go any other way in unemployment, etc. -- we found that those groups that improved in employment during treatment improved on everything else. Now the significance of this to me is that employment may be a much more important variable in organizing the life and the adjustment of patients in treatment than is ordinarily considered. As we view the treatment effort across the country the success of the entire overall treatment program in getting employment for patients has been very poor. It is entirely possible that if a greater effort were made in this direction, the overall payoff of the treatment program might be much greater.

I will make two final comments. First, the results of our studies have been available for the most part only in very limited editions of technical reports and therefore have not been widely read and, I noticed recently in a number of new books, have not been cited. I feel that we have corrected this. Two volumes published by Ballinger Publishing Company, a subsidiary of Lippincott, have ap-

peared within the past month.* These cover the results for the first cohort of DARP patients for 1969 through 1971. There will be a second set of volumes for the second cohort probably within the next six months and our work is proceeding on the third cohort at this time.** Second, the use of this file for further epidemiological studies will shortly be available to other investigators in addition to those in the IBR by its incorporation in the Drug Abuse Epidemiology Data Center which was set up last year, under a SAODAP grant and continuing under NIDA, in which survey data covering a wide range of studies and institutional files are being compiled and prepared for just this kind of research. This is described in the brochure on the data center located in the IBR at Texas Christian University.***

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*Sells, Saul B., ed. *The Effectiveness of Drug Abuse Treatment*, Volumes I and II, Cambridge, Mass.: Ballinger Publishing Co., 1974.

**Sells, Saul B. and D. Dwayne Simpson, eds. *The Effectiveness of Drug Abuse Treatment*, Volumes III and IV, Cambridge, Mass.: Ballinger Publishing Co., 1976.

***_The Drug Abuse Epidemiology Data Center (DAEDAC)_, (Grant No. DA4RG005), Texas Christian University, Institute of Behavioral Research, October 1974.

AUTHOR

Saul B. Sells is Research Professor and Director, Institute of Behavioral Research, Texas Christian University, Fort Worth, Texas.

DISCUSSION

Dr. Richards: I have a question that was not included on the agenda: I am curious about experience with unobtrusive measures. The only one I have every heard of is the use of cigarette papers to estimate the use of marihuana. I thought maybe this group might have had some thoughts or some experience with other kinds of measures which would add to those mentioned before.

Dr. Sells: In our studies the amount of poor reporting that we received from different agencies correlates very highly with the number of indices concerning the effectiveness of those agencies.

Dr. Lukoff: Poor reporting can be functional if it is deliberate and not accidental.

Dr. Sells: Occasionally we had deliberate poor reporting, but then we brought in Washington to deal with the problem and they did it effectively; the cooperation throughout was magnificent. And yet we could see where projects were poorly staffed with no back-up people, no facility for training and so forth; poor reporting results.

Dr. Romm: And you dealt with some of the better programs that are being supported by the federal government. In our experience with CODAP we have found that those programs which were disciplined by Saul Sells and his group are good reporters in CODAP.

Dr. Eichberg: Are you talking primarily about programs that are dealing with opiate adddiction?

Dr. Sells: No, perhaps primarily but not by design. When this started in 1969 the federal support was only for opiate addiction and two years later new legislation broadened it. The data we have reflect the shift; but even today heroin is by far the major drug that constitutes the problem.

Q: Regarding comparability between institutional sources and other -- to what extent can these findings be generalized?

Dr. Sells: This is one of the reasons why I have said that the data system designed for one purpose does not always serve another purpose as well. Ours was designed to study the effects of differ-

ent treatment, not even to evaluate particular programs, and therefore we have some sampling problems in respect to how well the samples we have represent the universe from which they are taken. With regard to generalizing our data, I feel we could not generalize our data for almost any of the cities from which we have received reports. In San Antonio, there are two or three treatment agencies now and we have one reporting unit. In St. Louis, I do not know how many there are, but we only have one. In each case, we are getting data from that city, but we are not sampling that city.

Dr. Richman: Nor are you sampling the treatment modality.

Dr. Sells: No, but we are replicating our sub-samples of it so that if we can get some degree of robustness in the estimate from a chunk of three or four samples from a modality, then we will pay more attention to it than if they are not consistent.

Unfortunately, the samples that we had to deal with were given to us by fiat and we have no control over that.

Dr. Gottschalk: I understood Dr. Sells to say that his data indicated that increased occurrence of alcohol and methadone use was not validated. How wide was your sample? Was it local in Texas or across the United States?

Dr. Sells: This is a sample in the third year of our program which totals 15,500, from 38 cities including Puerto Rico. Believe it or not, there were not cities in Texas in the sample.

Dr. Greene: There is also some data out of treatment programs in Washington, D.C., which suggest the same thing; increased rates of alcohol use observed were compared with admission alcohol use rates at some point later in treatment. It turned out that what was missing was that pre-addiction rate of alcohol use. What they were doing was returning to what constituted probable or normal use in their own community.

Indicators of Drug Abuse— Drug-Involved Death

Louis A. Gottschalk, M.D.

I am going to talk about studies that relate to drug-involved death. Our research group in the Department of Psychiatry and Human Behavior, University of California at Irvine, was involved first in the development of a uniform reporting and recording form for psychoactive drug-involved deaths. In the first year of our study, we surveyed 20 cities in the United States for such deaths, specifically through the Offices of the Medical Examiners and Coroners. We found out, as was suspected, that the recording and reporting system in the United States for drug-involved death was rather haphazard and unsystematic. So we developed a recording and reporting form of about 16 pages set up so that it could be filled out by the medical examiners or coroners and the various other professionally trained people in those offices who are qualified to fill out the various parts of this form. There was a trial run or pre-testing of 300 of these forms to check on the understandability of the items, the consensus with which agreement could be reached,

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*This report represents the collaborative research of Louis A. Gottschalk, M.D., Frederick L. McGuire, Ph.D., Eugene C. Dinovo, Ph.D., Jon F. Heiser, M.D., and Herman Birch, Ph.D., from the Department of Psychiatry and Human Behavior, College of Medicine, University of California at Irvine, Irvine, California 92664, and the Orange County Medical Center Research and Education Foundation, Orange, California 92668. This study was supported by a Research Contract Grant (#HSM-42-72-139), 1972-1975, from the National Institute on Drug Abuse.

and so on. The items in the form included the diagnostic system used on the death certificate, that is, the International Classification of Diseases system for reporting the mode of death, which falls into the classifications of direct and indirect and combined causes of drug-related deaths that may not be directly due to the drug itself. Items also dealt with noting those causes of death in which predisposing, underlying physiological conditions were already present, e.g., a heart disease which, with the addition of a drug, may push the person to die or may lead the person to a fatal accident or to jump off a bridge. Other items addressed what may have been seen by the medical examiners or coroners as related to a drug-involved life style, that is, a bullet hole in the head of the deceased due to involvement with people in the drug field. For the purposes of this study, drug-involved death included any type of psychoactive drugs -- analgesics (pain killers), narcotics, barbiturates or other sedatives, psychomotor stimulants, and so on.

The next part of the study was involved with getting from nine cities a total of 2,000 of these forms filled out; there was a certain quota from each city, based on the percentage of drug-involved deaths out of the total number of deceased persons processed by each office per year. The cities involved include New York, Chicago, Philadelphia, Washington, D.C., Cleveland, Dallas, San Francisco, Los Angeles and Miami; the quota related partly to the population and the number of cases that are processed; for example, the quota from New York City was 400, the quota from San Francisco was 200. That does not reflect population entirely, but the number of cases that go through their medical examiner's or coroner's system. Also, the quota was partly related to how many they could process. You may not realize it, but, in general, a coroner's case or a medical examiner's case is any deceased person who did not have a physician in attendance at the time of the death or up to four weeks before the death. But when it comes to how thoroughly these cases are studied, there is quite a great deal of variance. Most cases, however, that we reviewed were cases where there were post-mortem and toxicological studies done. We are at the halfway point, and some of the data that I am going to show you are going to give you preliminary psychosocial and biomedical analyses of about 1,000 drug-involved deaths.

Another part of our study of drug-involved deaths, we felt, should be a proficiency testing of the toxicological laboratories of these medical examiners and coroners, and we got them to agree to do this. We have good rapport with this network of professionals because we have worked with them for some time. They agreed to accept five unknowns in blood plasma or urine for qualitative and quantitative assay. Some of the unknowns had only one drug in them, some of them had four or five. We knew exactly what was in each, and, besides preparing these unknowns, we sent them to another reference laboratory to see what percent of these chemical substances could be found by an independent biochemical laboratory. Also involved, and this is a very important part of such a study, is the determintion of the many ways to measure and analyze these different psychoactive chemical substances: some of the factors

that can produce variance in the reported amount of drugs found in the body and, hence, considered to account for the death might not only be the method of extraction used to obtain the drug from the biological fluid in which it is located but also the particular assay method used. Some of the assay methods are not specific or precise, whereas some of them are quite specific. In the process of narrowing down the exact quantitative amount, the laboratory has to go through a number of steps to finally get the specific amount. Some of the assay methods used (for instance, the fluorometric method) measure not only the parent drug but many of the metabolites of the parent. Other assay methods are more specific and measure only one chemical substance (for example, gas chromatographic or radio-immunoassay methods). Hence, different assay methods will give different quantitative results on attempting to measure the same drug in a specimen.

What I am going to show you will give you some idea of the kinds of data that can be generated from the drug-involved death reporting form my research team has developed. It is going to take us quite a while to analyze all our data. You can imagine the many questions we have. They cover not only data about findings at the site of the death (on-site investigations) but any pre-treatment procedures that the person was undergoing (whether medical or psychological), the so-called "psychological autopsy," post-mortem findings, toxicological examinations (not only screening tests and procedures used but the specific quantitative findings). The drug-related deaths studied in our survey occurred in 1972 and 1973 and, for some medical examiners, ran into the beginning of 1974. One other thing that you should know about: these are all top-rated medical examiners, but they are busy and they have a limited budget; so there is a problem to get them to fill out these forms on their own bugets and to find the time in their own busy operations to do the things they are supposed to do. So we have to pay them a certain amount to fill out our forms and, in some instances (for example, in New York), we eventually had to send a research assistant there to get all their raw data and assemble it and put it down.

FIGURE 1

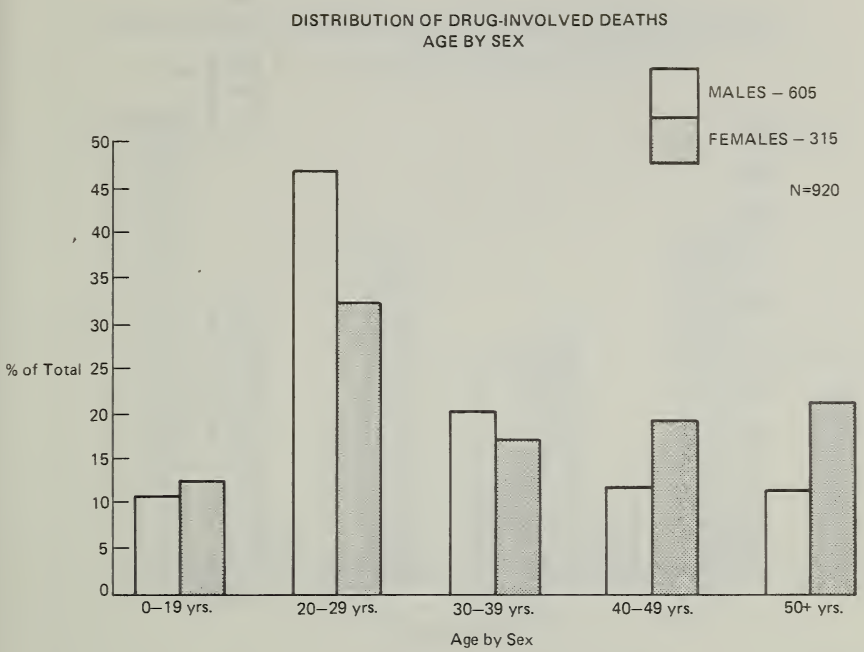


Figure 1 is comprised of data from 920 cases. One way of looking at this figure demographically is to observe that drug-involved deaths (across all drugs) seem to be a condition that occurs maximally at ages 20-29 years and at a higher rate among males than females. Over 50 years of age, females show a higher percentage of occurrence than males.

This figure includes any cases that would come through the medical examiner or coroner system, regardless of cause. These could be suicides, homicides, medical therapeutic misadventures -- whatever the medical examiners and coroners decided were drug-related are all lumped together here.

FIGURE 2

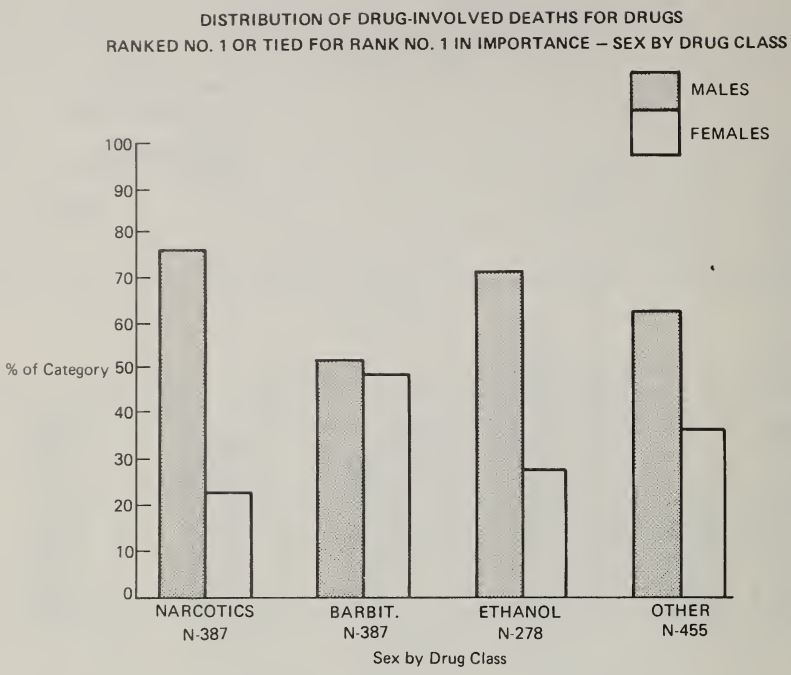


Figure 2 will show that, for narcotics deaths, there are many more males than females; with barbiturates, there is not such a sex difference. By the way, on ethanol-related deaths, we do not collect cases where alcohol alone was the cause of death. The death had to be due to alcohol with some other psychoactive pharmacological agent.

FIGURE 3

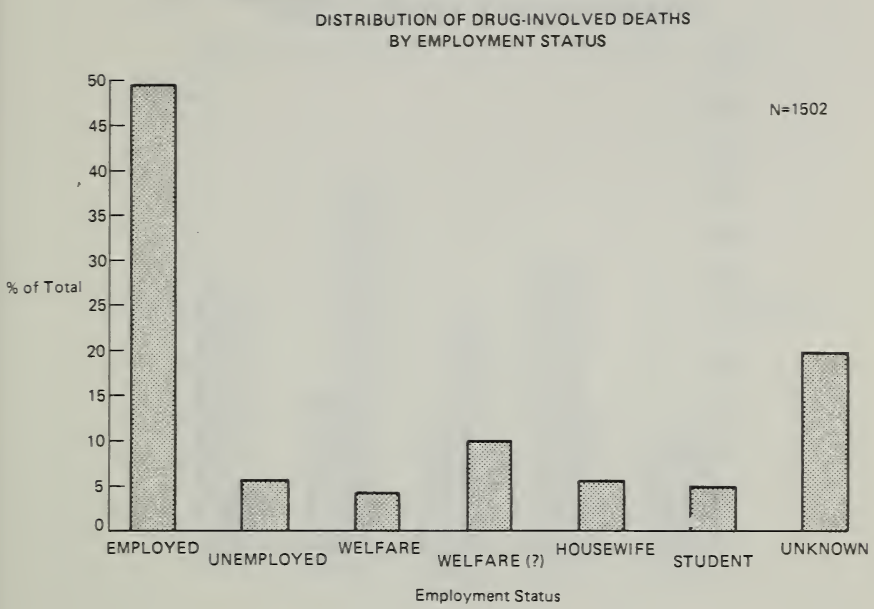
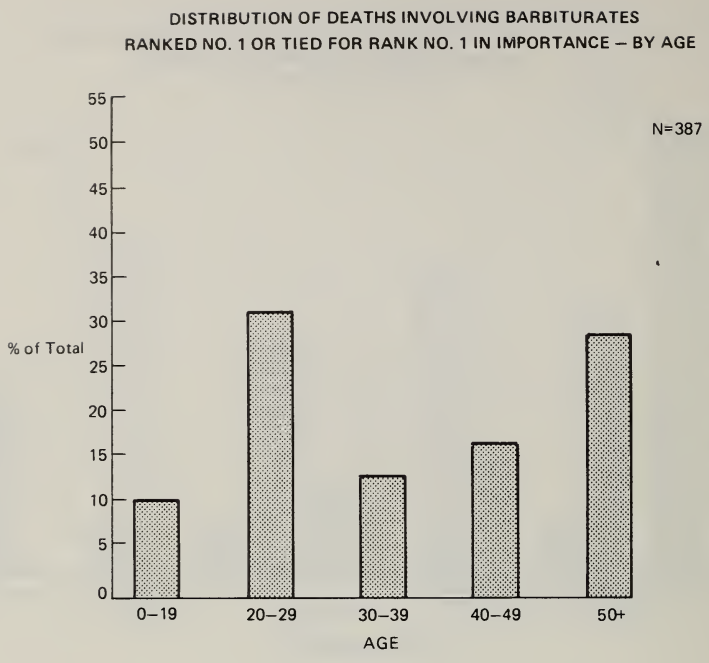


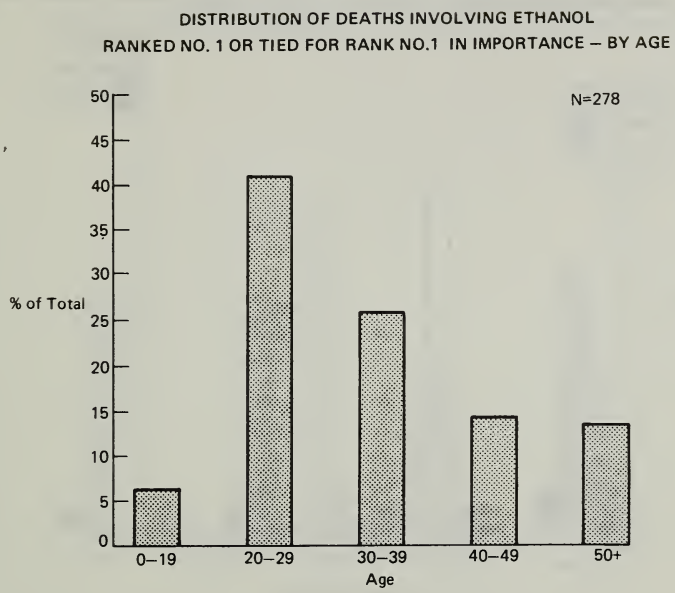
Figure 3 shows an interesting relationship between employment status and drug-involved deaths. For some reason or other, a relatively high percentage of the decedents were employed; a surprisingly small percentage were unemployed.

FIGURE 4



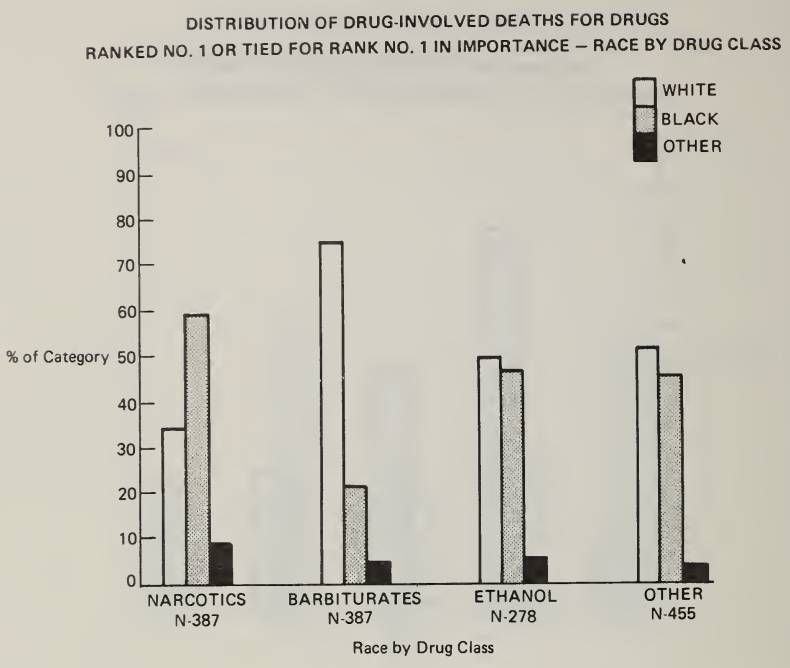
Here is another incidence graph. Again, the modal age range for a fatal consequence due to an overdose of a barbiturate is 20-29 years, but after the age of 50, barbiturates become an increased factor associated with death, obviously from suicide.

FIGURE 5



Here are the deaths involved with ethanol. The coroners had to rank what they thought the death was due to. Polydrug use, with or without the use of alcohol, is fairly common. So if the coroners found a number of drugs in the biological fluids of the body, they had to draw an educated conclusion about which drug was the major factor.

FIGURE 6



Here is indicated something about racial differences by drug class. Among deaths due to narcotics, blacks are relatively high. Among deaths ascribed to barbiturates, the whites are very high.

FIGURE 7

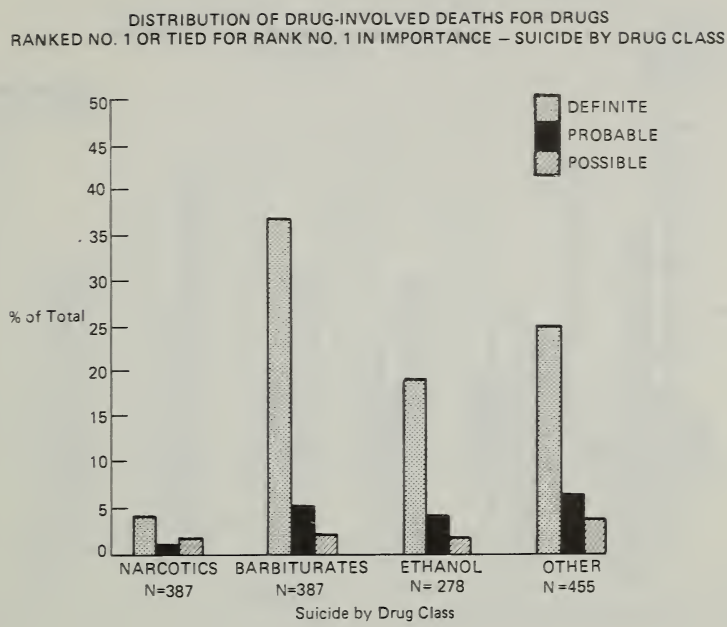


Figure 7 shows suicide by drug class. As you would expect, barbiturates are relatively highly associated with drug-related deaths, very highly, as a matter of fact. We have a breakdown of the likelihood of suicide in terms of definite, probable, and possible, and we have also incorporated some of Shneidman's distinctions in these respects -- that is, whether the suicide was "intentional" or "subintentional," but these distinctions are not diagrammed on these figures.

FIGURE 8

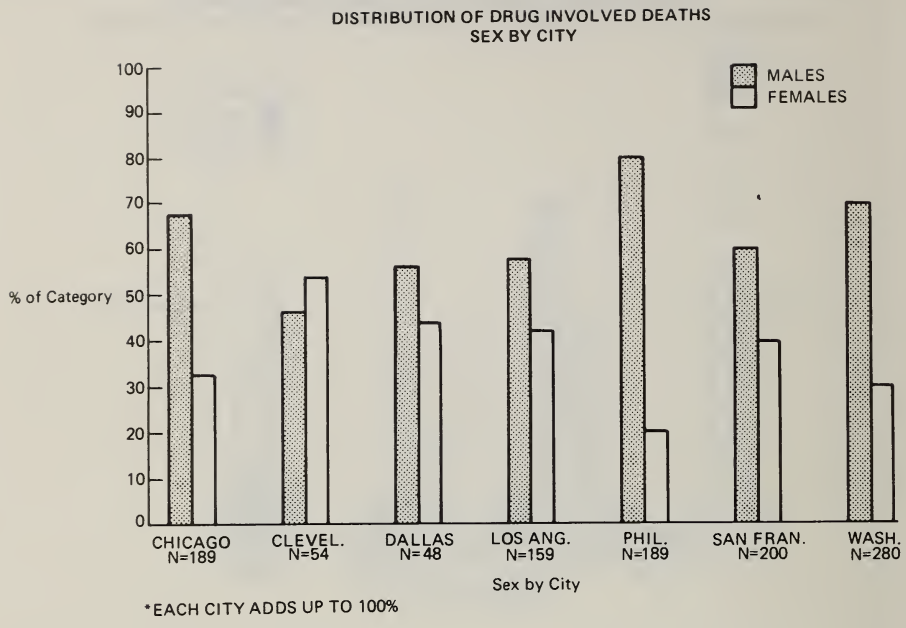


Figure 8 shows a breakdown of drug-involved deaths in terms of sex differences for different cities. The patterns for each of the seven cities represented are somewhat different. You will notice the N's are different this time because we have not yet collected all our data. Also, we have not included data from New York or Miami. You will notice that, for 1973, in Cleveland the female deaths are a little higher than the male deaths, but this difference may not hold up after we have finished collecting the full quota of drug-involved deaths from Cleveland in 1973.

FIGURE 9

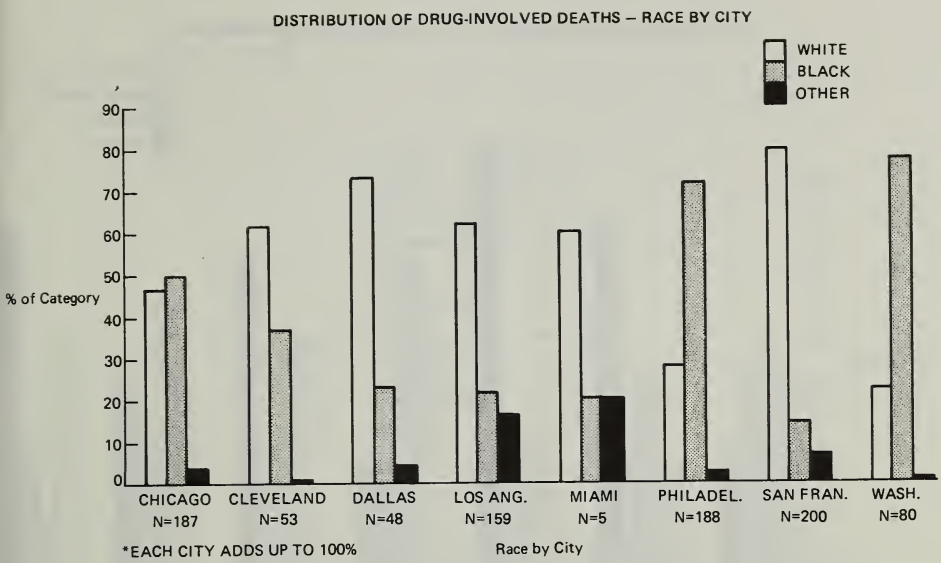
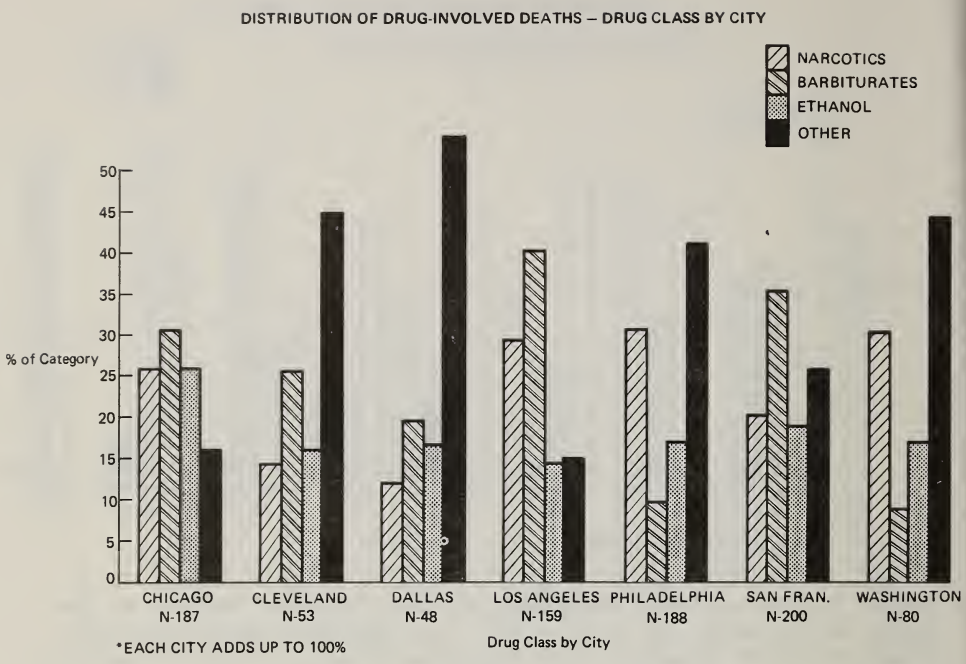


Figure 9 shows another breakdown of drug-involved deaths in the racial differences for each city. The profiles would certainly illustrate what somebody has already said here, that all greater American cities have somewhat different drug abuse patterns. Maybe we do not yet have enough data to draw any conclusions, but some of the differences are marked and look as if they will hold up, for instance, the different racial patterns of drug deaths in Philadelphia and San Francisco.

FIGURE 10



Here in Figure 10 we see the distribution of various psychoactive drugs by class, as they were perceived to be the major cause of death by the medical examiners. For San Francisco, barbiturates were relatively high, whereas for Philadelphia, narcotics were high.

FIGURE 11

Of the 1,562 specific drugs mentioned in these 924 deaths, the frequency by rank is as follows:

Heroin	22%
Alcohol	19%
Secobarbital	8%
Pentobarbital	6%
Quinine	6%
Phenobarbital	6%
Methadone	5%
Amobarbital	3%
Propoxyphene	3%
Valium	3%
Tuinal	2%

Figure 11 is a quick run-down of all the drugs mentioned in the reporting forms as being involved in the death in these 924 case reports. In many cases, several drugs were mentioned. We found that heroin was the most frequent psychoactive chemical substance mentioned, with alcohol the next most frequent, even though cases involving the use of alcohol could be included only if it was involved with another psychoactive drug. Secobarbital and other barbiturates were rather highly involved. We also see that a drug like propoxyphene (® Darvon), mentioned in 3% of cases, and this drug is claimed to have an increasing association with drug-involved deaths.

Figures 12-19 are presentations of a different kind of data. They are more biomedical, but they will be of interest because I do not think there is anything quite like these data reported anywhere, and we cannot aspire to an accurate epidemiological understanding of drug abuse patterns until we begin to acquire valid criteria for the human toxicological effects of these drugs and the body concentrations of the drugs that are absolutely necessary to produce a fatal outcome. The assumption is unwarrented that we know, with certainty, what drug levels are fatal or, in combination with other drugs or pre-existing medical conditions, can lead to death. The wide ranges of drug concentrations recorded here and the frequency distribution of these drug levels serve as a descriptive base line for comparison with other studies and for further validation.

The figures show the range of morphine or other pharmacological substances found in the blood, urine, or stomach contents and the breakdown of the percentage of distribution of these drug concentrations.

FIGURE 12

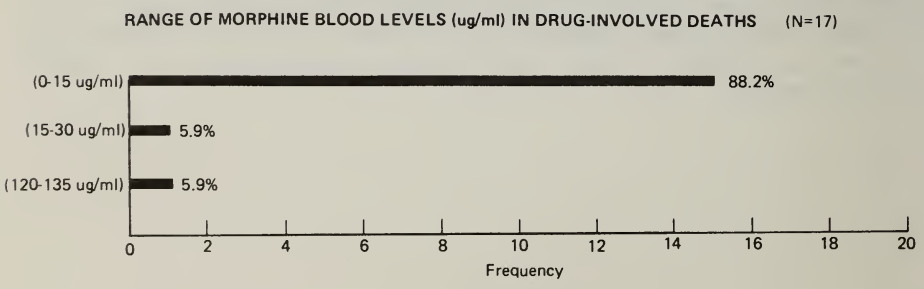


FIGURE 13

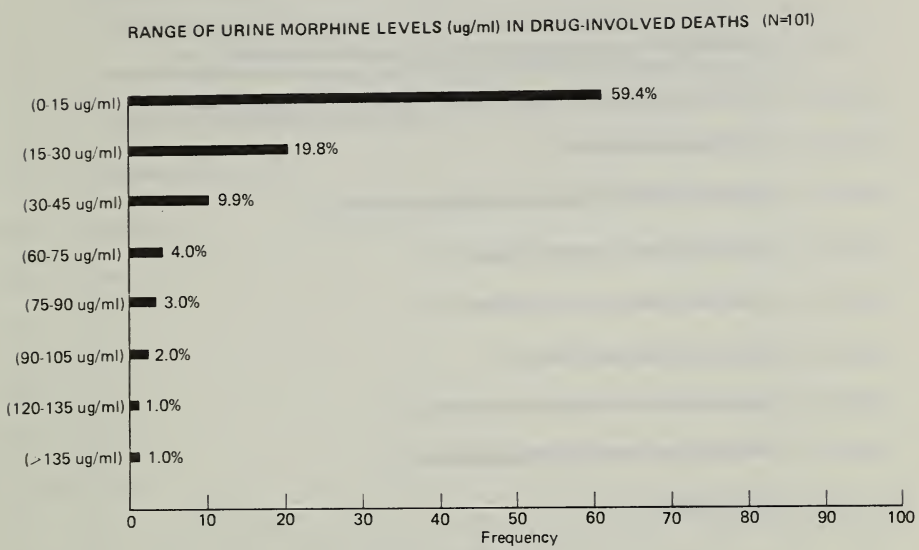


FIGURE 14

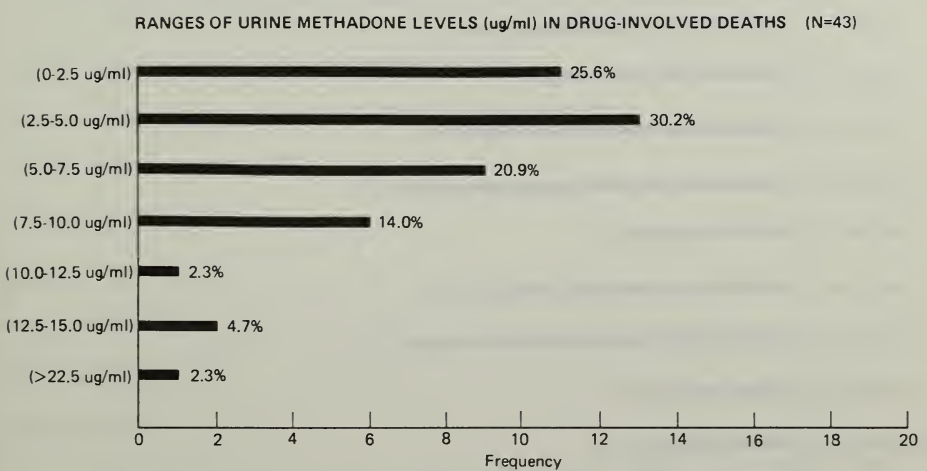


FIGURE 15

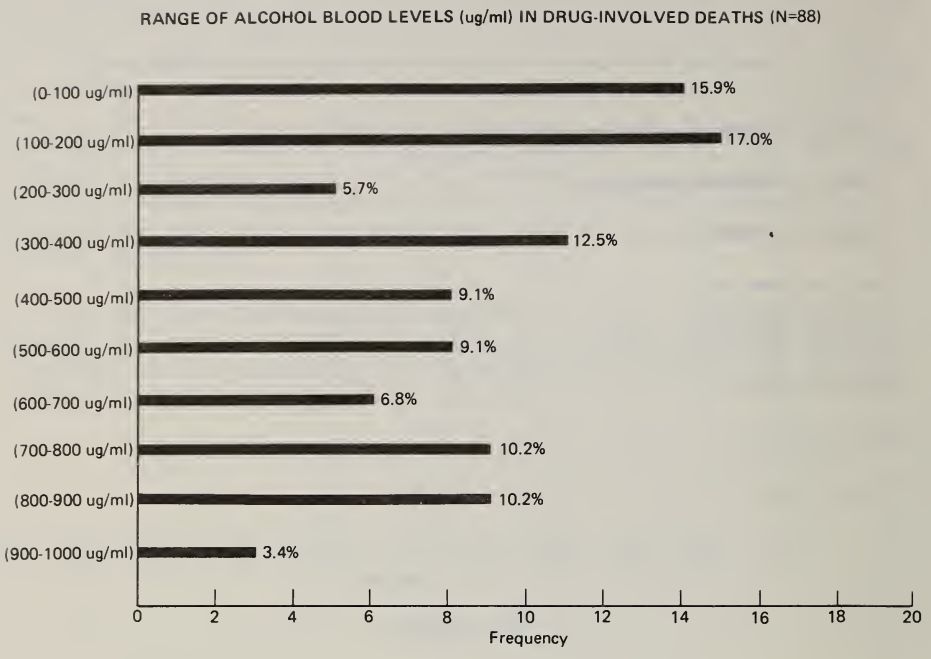


FIGURE 16

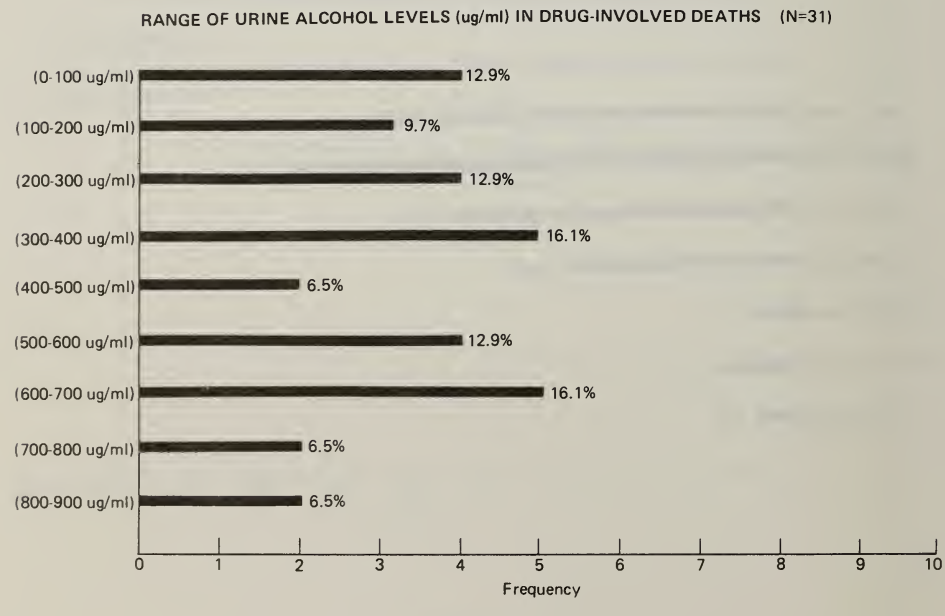


FIGURE 17

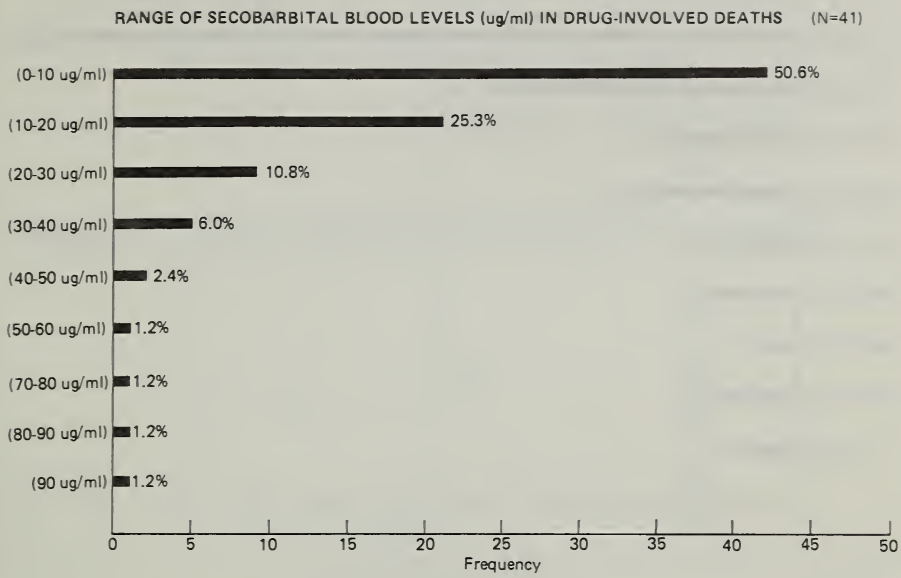


FIGURE 18

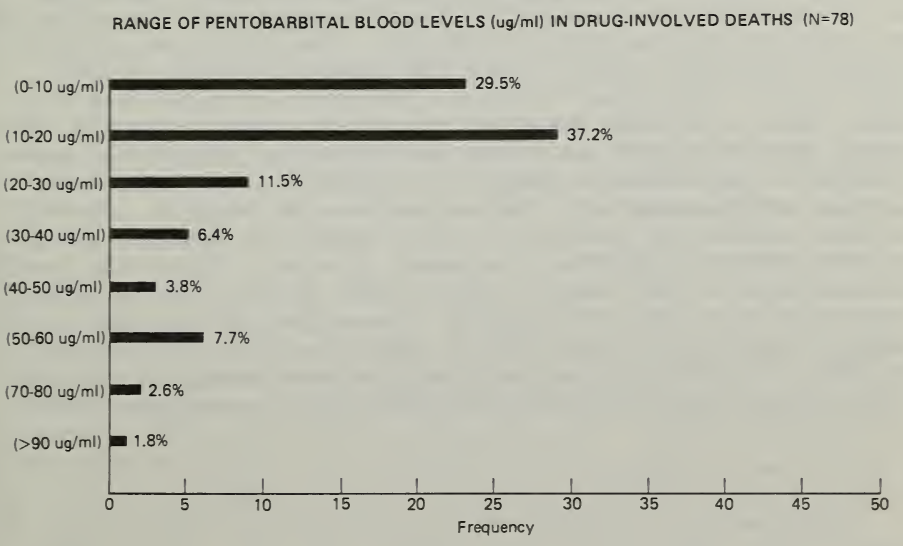
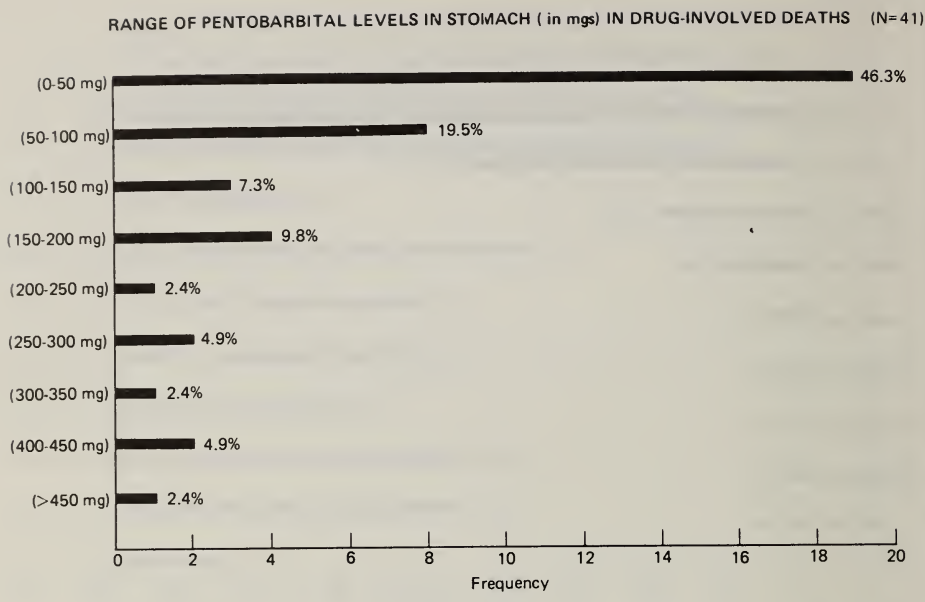


FIGURE 19



The next is a series of tables which will show you some of the results of our quality control and proficiency testing studies as reported by the toxicological laboratories of the medical examiners or coroners in the cooperating nine cities. Table 1, headed "Serum I Sample," was sent to the nine laboratories as a general unknown. It contained, as is indicated in the top row, 6.5 micrograms per milliliter (μg/ml) of secobarbital 6 μg/ml of pentobarbital and 400 μg/ml of salicylic acid. These were the amount that our own departmental Biochemistry Laboratory put into the serum samples sent to each toxicology laboratory. The bottom row on Table 1, labeled "Reference Assays," gives the amounts of these various drugs that an independent reference laboratory obtained from this serum sample. You will note that the amounts obtained by the reference laboratory (not one of the participating laboratories associated with the medical examiners or coroners) are not precisely the same as the amounts of drug we put into the samples. The differences between the reference assays and what we actually put into these unknowns indicates the amount of error variance that would be acceptable in such assays. What should be noted in the results reported by the nine participating laboratories is that some of the amounts found by them were close to what should have been found, but that, in several instances, the drugs were found but not quantitated and, in one instance (salicylic acid), no drug was found.

TABLE 1

SERUM I SAMPLE

Preliminary Compilation of Results - 9 Cities Reporting

DRUGS PRESENT - CONCENTRATION (in μgm/ml)		
Secobarbital	Pentobarbital	Salicylic Acid
6.5	6	400
RESULTS (in μgm/ml) REPORTED BY PARTICIPATING LABORATORIES		
4	3	340
6	6	360
5	3.8	--
7.17	5.41	402
——— 11.1 ———	*	567
7.5	7.1	390
+	--	+
6	6	380
——— 9.5 ———	*	360
REFERENCE ASSAYS		
6.2	4.9	410

This sample was sent as a general unknown.

* Secobarbital and pentobarbital were reported together by this laboratory.
(+) Drugs were found but not quantitated.
(-) Drugs were not found.

Table 2, headed "Urine IV Sample," shows on the top row the six substances which were in the unknown, namely, secobarbital, 6.5 μg/ml; methadone, 2.5 μg/ml; morphine, 3.5 μg/ml; methaqualone, 20 μg/ml; flurazepam, 30 μg/ml; and ethanol, 0.15%. In assaying the amounts of these drugs in this urine sample, the participating laboratories did not do so well. In many instances, the drug was detected but not quantified. In many other instances, the drug was not even detected. And, in many instances, though the drug was detected and quantitated, there was a sizable deviation from what was actually in the urine sample. On the other hand, at least two laboratories (see row 1 and row 5 under "Results Reported by Participating Laboratories") did quite well for the most part. These variations obtained from presumably good toxicological laboratories may be due to the different methods of extraction, or the different assay methods used by these laboratories, or to different instrumentation and human error. From the findings, you can see that social scien-

tists cannot safely assume that all biochemical measurements are automatically reliable and accurate.

The last point to notice in Table 2 is that one false positive was reported, namely, salicylic acid, in a sample in which it was, indeed, not present.

TABLE 2

URINE IV SAMPLE

Preliminary Compilation of Results - 9 Cities Reporting

DRUGS PRESENT - CONCENTRATION (in μgm/ml)						
Seco-barbital	Methadone	Morphine	Methaqua-lone	Flurazepam	Ethanol	False Positives
6.5	2.5	3.5	20	30	0.15%	
RESULTS (in ugm/ml) REPORTED BY PARTICIPATING LABORATORIES						
7.0	2.8	4.1	27.4	36	0.12	
4	10	3	-	-	0.12	
3.8	-	-	1.8	-	-	
7.0	-	+	+	+	0.12	
6	3	3	28, 25	42, 38	0.10	
5.2	-	+	5.1	-	-	
+	+	+	+	-	-	Salicylic acid
5	-	-	-	-	0.12	
+	+	+	+	-	-	
REFERENCE ASSAYS						
3.5, 5.6	2.1, 2.2	3.4, 2.7, 4.6	15, 6.7	+		

This sample was sent as a general unknown.

(+) Drug was found but not quantitated.
(-) Drug was not found.

Proceeding to Table 3, we see the results obtained by these laboratories (only seven cities reporting) on "Serum III Sample." Again, we see that a number of laboratories detected the drug but did not quantitate it, and in a number of instances the drugs were not even detected. Moreover, two laboratories located drugs which did not really exist in this serum sample. Such a range of errors might well increase a social scientist's skepticism about the accuracy of the medical diagnoses related to cause of death. On the other hand, some of these toxicological laboratories were consistently more accurate than others. This might mean that their epidemiological findings were more to be trusted than those reported by other medical examiners' and coroners' offices.

TABLE 3

SERUM III SAMPLE

Preliminary Compilation of Results - 7 Cities Reporting

DRUGS PRESENT - CONCENTRATION (in μgm/ml)						
Seco-barbital	Mepro-bamate	Chlorodia-zepoxide	Diazepam	Acetone	Ethanol	False Positives
6.5	25	20	25	0.05%	0.15%	
RESULTS (in μgm/ml) REPORTED BY PARTICIPATING LABORATORIES						
3.3,2.6	11	13	12	0.036	0.076	
3.8	-	10	20	0.043	0.088	
4.3	13.2	+	+	0.03	0.08	
-	34	-	-	0.048	0.085	
-	10, 10	-	-	0.031	0.075	Tyrosine
1.8	-	-	24.1	-	-	
-	-	-	3.6	0.034	0.029	Isopropanol 0.01%
REFERENCE ASSAYS						
5.8	30	10	8.6	0.04	0.09	

Drugs in this sample were identified as neutrals and volatiles.

(+) Drugs were found but not quantitated.
(-) Drugs were not found.

Proceeding to Table 4, headed "Urine II Sample," we again see one false positive and some instances in which the drugs were detected but not quantitated or instances where the drugs were not found when they should have been. On the other hand, as we continued sending these unknowns to the participating laboratories and giving feedback of their results in a confidential and coded fashion so that the nine laboratories could not identify which laboratories were very accurate and which ones were not, the overall accuracy of all the laboratories gradually began to improve.

TABLE 4

URINE II SAMPLE

Preliminary Compilation of Results - 9 Cities Reporting

DRUGS PRESENT - CONCENTRATION (in μgm/ml)							
Seco-barbital	Methaqua-lone	Diazepam	Amphet-amine	Morphine	Methadone	Codeine	False Positives
6.5	20	25	3.5	3.5	2.5	3.0	
RESULTS (in μgm/ml) REPORTED BY PARTICIPATING LABORATORIES							
2.5	10	4	0.8	3, 4	1.6	1.6	
5.8	18.3	22.2	3.5	3.2	2.4	2.9	
5.4	11.9	19.2	3.7	+	2.4	3.2	
5.2	33	+	+	5.5	8.3	-	Cocaine
5.3	15.7	24.2	4.1	5.8	0.7	3.7	
3.2	-	3.9	-	-	6.8	5.2	
6.7	17.7	2	3.8	1.3	7	2.1	
8	12	20	3	7.5	3	2.5	
4.9	27.0	37.0	5.0	11.0	2.5	8.0	
REFERENCE ASSAYS							
7	18	38	1.5				
5.3	12	22		1.9	1.7	2.3	

This sample was sent as a general unknown.

(+) Drugs were found but not quantitated.
(-) Drugs were not found.

Proceeding to Table 5, headed "Urine V Sample," we again see two false positives in an unknown in which the generic name of the drug was identified (at the time of pre-testing, the laboratories were told that a barbiturate was present, but not which one). Note that one laboratory reported 384 μg/ml. This was possibly an error in placing a decimal point, but, even so, an error that cannot be ignored.

TABLE 5

URINE V SAMPLE

Preliminary Compliation of Results - 8 Cities Reporting

DRUG PRESENT - Secobarbital

CONCENTRATION 6.5 μgm/ml

RESULTS (in μgm/ml) REPORTED BY PARTICIPATING LABORATORIES	FALSE POSITIVES
9.5	
4	
6	
4.4	Phenobarbital, 4.4
8.5, 6.4	
5.8	
5	Doriden, 3.6
384	

REFERENCE ASSAY

5.4

(Drug class was identified for this sample.)

The last, Table 6, headed "Time Study of Secobarbital Measurements by City," is also interesting from the viewpoint of quality control. Every one of the five unknowns, listed under the heading "Sample" in column 1, contained 6.5 μg/ml of secobarbital. We can see that some laboratories obtained results fairly close to the amount of secobarbital actually in each sample, and some laboratories showed considerable deviation.

TABLE 6

TIME STUDY OF SECOBARBITAL MEASUREMENTS, BY CITY

(Concentration - 6.5 μgm/ml)

SAMPLE	CITY 1	CITY 2	CITY 3	CITY 4	CITY 5	CITY 6	CITY 7	CITY 8	CITY 9
Serum I (February)	5.55*	4	4.75*	7.5	7.17	5.0	+	6.0*	6.0
Urine IV (March)	+	4	5.2	7.0	3.8	5	+	6	7
Serum III (May)	-	N/R	-	3.3	-	4.3	1.8	N/R	3.8
Urine II (June)	5.2	8	4.9	5.8	6.7	5.3	3.2	2.5	5.4
Urine V (August)	384	5	4.4	8.5, 6.4	6.0	4	9.5	N/R	5.8

* One-half of barbiturate (secobarbital and pentobarbital) concentration.

(N/R) No response.
(+) Drug was found but not quantitated.
(-) Drug was not found.

I think by viewing this series of slides from our quality control and proficiency studies of the toxicological laboratories associated with the determination of evidence for a drug-involved death, you should be reassured that some laboratories, on the one hand, get fairly respectable results. On the other hand, some laboratories show a consistently high error variance which might influence the accuracy with which they determine the final cause of death.

We are coming to a conclusion that the epidemiological data, using the expert opinion of forensic pathologists and medical diagnoses of the cause of death, are not equally accurate, at least from the point of view of one of the important sources of data on which medical and legal opinions are based, namely, the toxicological findings.

Indicators of Drug Abuse—Hepatitis

Michael Alexander, M.D.

I will briefly discuss the use of hepatitis as an indicator of incidence and prevalence of intravenous drug abuse. The way I would like to approach the discussion is to go into some of the limitations of this technique and, thereby, give you some idea why one has to be critical when looking at these kinds of data.

The first major problem in using hepatitis as an indicator is the difficulty in diagnosing a given case of hepatitis as being the kind of hepatitis associated with drug abuse. Hepatitis is a broad term, meaning inflammation of the liver. It is believed that most cases of hepatitis are caused by two viruses, hepatitis A and hepatitis B. However, hepatitis can also be caused by other viruses and by drugs and perhaps other sorts of infectious agents. Hepatitis A is infectious hepatitis; hepatitis B is serum hepatitis. It is hepatitis B that classically has been associated with drug abuse. Usually, a given case of hepatitis can be diagnosed as being either hepatitis A or hepatitis B by combining epidemiologic, clinical, and laboratory information. However, even when one is able to pinpoint the case as being typical of hepatitis B, the problem remains that hepatitis B can be contracted by routes other than intravenous drug abuse; for example, transfusions, personal contact, or perhaps as recent evidence would suggest, by sexual contact. Furthermore, even when a case seems typical of hepatitis A, the problem remains that hepatitis A, in addition to usually being contracted by non-intravenous routes, can also be spread by intravenous drug abuse. As you can see, separation of the two types of viral hepatitis becomes difficult and their mean-

ing becomes very confusing. In addition, while hepatitis B is the kind of hepatitis we are most interested in looking at as a measure of drug abuse, the vast majority of cases that are seen are probably hepatitis A. And as a result, there is a large background "noise" which may obscure changes in what we are trying to assess. As a result, interpretation of cases reported is further complicated.

Another problem area is the hepatitis reporting system. There are a number of nationwide hepatitis reporting systems which are channeled through public health agencies. Theoretically, all physicians in the United States are to report cases diagnosed as hepatitis to the local public health authorities. These in turn are reported by the local authorities to the state public health authorities and on a weekly basis, these reports are forwarded to the Center for Disease Control in Atlanta. The mechanisms within each state for doing this reporting are variable, so to understand what is going on in any given state, one has to examine that system individually. In the national weekly reporting system, cases are reported as being hepatitis A, or B, or unspecified, but the information on which that diagnosis is based is not given. As a result, one can never be certain of most diagnoses.

Most cases of hepatitis that are diagnosed are not reported. In fact, the Public Health Service has investigated major epidemics because they were interesting or important in terms of understanding the disease, yet, those same epidemics were never reported through the routine reporting system. A fraction of the cases that occur get reported and it is certainly possible that this nonreporting is not a random phenomenon. For example, in a ghetto area where the major source of medical care is a county hospital which is likely to have a university affiliation with faculty interested in public health or infectious diseases, the reporting system might be very good. In a suburban community, where reporting a person as having hepatitis B might imply drug abuse or similar social stigmata, there might be a bias against reporting cases. Analysis of the data does not show that this is occurring, but potential bias like this must be considered.

Another major problem area of hepatitis reporting is that there should be a lag period between an increased incidence of drug abuse and when we would see rising hepatitis case reports. This lag would be the result of a number of factors. First of all, the addict would have to go through a stage of "chipping" and would have to achieve a certain level of drug abuse before his exposure level got to a point where he would get hepatitis. Secondly, hepatitis B has an incubation period of two to six months. This means a delay of two to six months between the time that one becomes infected with the virus and when one has clinical evidence of hepatitis. In addition, there is the lag between getting the reports from the private physicians to the local level, to the state level, and to the federal level. I think all of these would combine to present difficulties in terms of using the hepatitis reporting system as an early indicator of changing incidence of drug

abuse. This is especially relevant if one is thinking of using indicators of increases in drug abuse as a clue to when to intervene.

Retrospectively, we can certainly show that at times when other epidemiologic data have suggested "epidemics" of drug abuse, the hepatitis data are in agreement. Whether or not they would have been very sensitive at the onset in terms of suggesting when and where to try to combat the epidemic is not clear.

What I planned to do next was to briefly describe some of the data which has accumulated at the federal level on this Hepatitis Surveillance Program and how that reflected on the epidemiology of drug abuse over the last 10 years. However, I think that Lee Minichiello has done an extensive evaluation of our data, much more extensive than we ever did. I think that I will defer to him in discussion and ask if he would like to raise some of those points.

AUTHOR

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DISCUSSION

Mr. Minichiello: I would like to comment on our study of the use of hepatitis as an indicator. As has been mentioned, our work has not always gotten outside the Government.

In 1972, we were asked by the Office of Science and Technology, which was then a part of the Government since abolished, to do a study on heroin data. In that study we conceptualized the measures that we liked and did some prototype analyses which over the last three years now have been completed and in one form or another have been recommended to the Government for implementation; to some extent they are being implemented. There are four such indicators: the treatment data base; federal purchases and seizures; hepatitis; and the DAWN system. Using the treatment data in 1972, we looked at what was happening in the heroin epidemic at that time based on age of incidence from admission data. We were able to show some peaking at that time; however, there was some discussion about whether it was zero or going up or down. In that study we also said the treatment data had some problems and one was the time lag between the start of using heroin and going into treatment and the other was the bias that Dr. Sells mentioned -- the data we looked at were mainly ghetto users in Eastern cities. Therefore, we said we would like to sample nationally and look at both the suburban and white young person. In addition, we said that we were interested in more than just incidence and prevalence -- we were interested in availability. I was very interested in Dr. Newmeyer's comments, for two reasons. One, availability actually influences incidence and prevalence -- for example, the East Coast heroin shortage. Then secondly, availability drives a lot of these indicators perhaps more than the absolute number of users. Drug deaths and overdoses is one example, urinalysis may be another, and there are probably others. That study was published in December 1972 and has not yet been distributed outside the Government.

The next study we did was to look at the DEA laboratory tapes, which are a record of all federal drug seizures. They are unique in that their strong point is that they are a good, definite, precise, chemical identification of the drug that was used. I have great confidence in that chemical data. From that, we developed an indicator of availability, using the price of heroin, the puri-

ty of it, and certain chemical characteristics. From this we have examined over the last three years the availability, the changes in the distribution system both at the retail and wholesale level, and the changes in the source -- the fraction of the country's heroin coming from different sources. Initial results were clear about six months ago. They are very similar for the nation to what Dr. Newmeyer said was happening in San Francisco. We saw a shortage develop from the East Coast of the United States which went all the way back to France and a decline in that source. We saw a corresponding increase in heroin coming across the Mexican border which had certain chemical characteristics that are unique to a process different from either the French or Southeast Asian; this began to represent a large supply of heroin that was more potent than the declining French. That has also been published and not disseminated, perhaps for good reason because, in terms of enforcement action, one does not want to get too open about the source signatures.

Then we extended the laboratory analyses to look at the other drugs. Here we have the concept of relative availability -- what happens to drug use when one drug becomes less available and another drug becomes more available? That appears to be very important if one wants to discuss polydrug use. This work is just being published and in it we have compared those data with the DAWN system using a grouping of the drugs that are compatible.* That, I think, may receive wider distribution than the earlier studies. In that study which covered the last three years -- 1971 through 1973, we saw an increase in marihuana that was quite pronounced. We saw a decline in heroin. We saw a relatively constant use of the other drugs, perhaps with some interesting changes within drug groups. For instance, you would see a decline in LSD but a corresponding increase in PCP, a decline in barbs, but an increase in non-barb sedatives.

Then I have done a study that is about to be published on trends in intravenous drug use, looking a hepatitis and DAWN.** I tried to tackle all the problems that Michael Alexander has mentioned in terms of using that system. Yet in spite of all those problems, hepatitis is perhaps the best indicator, so you can understand the problems of the others! I looked at it over eight years and we saw a great increase that is different in different parts of the country and different geographic and different demographic areas; you really have to do all these things -- geography and demography -- to compare these data bases. We saw an 8-fold increase from 1966 to 1972. And then in 1973, we saw a decline that was very preferential in time and geography to the area where the East Coast heroin shortage occurred and that was in New York City,

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*L. Minichiello, et al. "Supply, Distribution, and Usage Patterns of Drugs of Abuse." (DEA-STS-TR-13; IDA Paper P-1066), October, 1974.

**L. Minichiello. "Indicators of Intravenous Drug Use in the United States." (DEA-STS-TR-14; IDA Paper P-1068), March, 1975.

mid-Atlantic States to a lesser extent, and out of Chicago and down to the South Atlantic Census Division. We saw an increase in the number of whites in the last few years -- white cases grew and female cases grew. Then I compared the demographic and geographic distributions of those hepatitis reports with the DAWN mentions and found general gross agreement at the current time. The most recent thing that we are doing with DAWN now that it has a year and a half of reporting history, is to look at time trends. In the last six months, we see a turn upward in heroin mentions of about 30 percent, almost uniformly across the country in emergency rooms. We are trying to understand if this is related to some peculiarity within the DAWN system that is entirely unreal, to an increase in potency, or to a change in heroin usage.

Dr. Sells: In our data, as I reported earlier, between 1969 and 1972, we had an eight percent shift from the number of males to females and also an increase in the number of whites. Now, is it possible that what you are finding is merely reflection of the change in the population of users rather than a change in whatever is producing these cases?

Mr. Minichiello: I think the changes that I have observed are probably underlying patterns of drug abuse showing a real increase in the number of white male and female users in the United States since 1970.

Dr. Sells: I agree with that.

Mr. Minichiello: I base that on two independent systems. In other words, internally I looked at the non-drug related hepatitis to see if it was distributed right -- geography, race, and sex -- and it was, so it was hard to demonstrate obvious biases of the system. All this is very preliminary and we hope to get it out and let people tackle it and learn more as we go on. The Stride system is being implemented within the DEA and we are hoping that this kind of data will be routinely available to them. Stride is the data system in which the laboratory analysis of the federal seizures are reported.

Dr. Chambers: I have a comment concerning the use of hepatitis as an indicator. It is my experience that most drug use in this country is not intravenous. And if hepatitis is presented as one of the best measures of drug abuse, and plans are written and problem groups are defined, and strategies are made on the basis of hepatitis, I think it is misleading. I am not sure what I want to use as an indicator, but I do not want to use that one.

Mr. Minichiello: How about heroin? Most heroin use is intravenous.

Dr. Chambers: I am not sure that is true either.

Mr. Minichiello: We chose I.V. specifically with the rationale that it represented a form of serious use that people might con-

sider abuse and that it was one way of getting a look at a population which you could demonstrate was in some sense serious. We do not want to say any more than that. We do not say you should not look at non-I.V. use. I am just saying that I cannot do everything at once and here is one mode of insight into one form of the user population which we think captures many, if not most of the heroin users, and to a lesser extent the other drug users. We have documented the percent of I.V. use for each drug type within the DAWN system by each facility type to get an idea of the variance that you might see from those different things, like crisis centers *vs.* emergency rooms. What we are seeing is about 10 percent of the mentions being I.V. in DAWN and about 90 percent of the heroin mentions reported as I.V.

Dr. Chambers: What has been the correlation between the DAWN data obtained at crisis centers *vs.* the DAWN data obtained in emergency rooms?

Mr. Minichiello: They give essentially the same answers. There is not a good variance in these data with respect to I.V. use by facility type in DAWN. These give you the same answers.

Dr. Sells: What is the basis for your questioning the extent of I.V. use of heroin?

Dr. Chambers: We do not have any. Our contention is it has not been demonstrated. What we have is I.V. use as a predominant vehicle in the known or treated population.

Dr. Sells: In our data I would say we have maybe 30,000 cases involving heroin use of which 98 percent is I.V.

Dr. Chambers: I am not going to question that. But among the hidden users, and the recent users who are totally functional, the social or recreational users, I am not sure that 98 percent of them are I.V. users. That is all I am saying.

Indicators of Drug Abuse—Buys

John Newmeyer, Ph.D.

In regard to buys, we at the Haight-Ashbury Free Medical Clinic have been concerned mainly with heroin. Through a network we purchased samples of heroin from time to time on the street and submitted them to pharmacological labs for analysis.* These data are of special interest lately because of the detective work we are doing in regard to the second "wave" or "peak" of the incidence of heroin addiction in San Francisco. The latest estimates I have made show that the big peak of new addicts in the Bay Area in 1969-1970 was followed by a decline by about two-thirds in the incidence of new addicts as of 1972. Then in 1973 and 1974, there seems to have been a resurgence of new addiction to heroin, to a second peak. We sought to find out what was the character of this new wave of addicts. We got some demographic data from our own clientele and from the death-by-overdose roster, and it appears that this new wave of addicts was primarily of lower-class backgrounds. These individuals are not from the suburbs, nor from middle-class backgrounds, but more traditional addict-types, of black, Latino, and white working-class origins. Also, this time around, there is a higher proportion of females, more like 40 percent as opposed to the 15-20 percent of ten years ago. Also, there is a return to a lower proportion of whites. The age at which the typical San Franciscan now becomes involved with heroin is not significantly different from five years ago; if anything, it is very slightly higher than before.

*See Attachment.

My hunch in regard to the second "wave" is that it was due to the higher quality of heroin coming into the Bay Area and "taking hold" more easily than did the poorer-quality heroin that was available there during 1971-1972. I believe we were getting a higher quality of heroin at a lower price, due to San Francisco becoming a depot or entry-point for Southeast Asian or Mexican heroin. In other words, I perceive the new peak as resulting from more immediate, close access to the ultimate source of heroin, which now seems to be coming principally from Southeast Asia rather than from Turkey as it was five years ago. Hence, instead of having to come from the East Coast and all the way out to the West Coast through many layers of middlemen, our heroin market now has few layers of middlemen and hence fewer layers of price raising before it gets to the ultimate consumer. In summary, it appears as if -- in contrast to the first-wave epidemic of 1969-1970 which appeared to result from the germination and rapid spread of heroin use in the middle-class white youth subcultures which had already gone through several stages of abuse of other drugs (primarily amphetamines) -- the second-wave heroin epidemic was due to the introduction of good-quality heroin into traditional black and Latino communities of poverty.

We looked at the samples that we had picked up in various parts of the Bay Area. The way we did this was to pick our clients at random, asking them if they would bring in samples for analysis, for which we would pay them for any expense they had gone to. It is very hard to get any kind of random sampling of the heroin available on the streets. You can not use the Crawford-Hughes method of going out to a particular copping area, because that copping area is likely to be dominated by only one or two sources of heroin. About the best that you can do is to go to all the treatment centers in the city and ask a random sample of clients to go out to their connection and get you a sample of the heroin that is available.

I did summaries of these analyses, in which I developed one important statistic, the "price per pure gram of heroin" -- that is, how much does each buyer at a particular level of the distribution hierarchy have to pay for his heroin. The most current figure is a median of $835 for a pure gram of heroin at the lowest retail level, which is equivalent to $.83 per milligram of pure heroin. Traditionally, one thinks of a bag of heroin as containing 10 milligrams of pure heroin or $8 or $9 worth of heroin at the current retail price. (This study also necessitated discovering the weight of a Bay Area "bag" of heroin. To do this we asked several clients to tell us how much "Pream" equalled one bag, and this worked out to about .33 grams of powder. We assumed this was representative of the Bay Area; thus, with .33 grams, you only have to have three percent purity to get your traditional 10 milligrams of pure heroin.)

The interesting thing is that in the Bay Area, we have seen a resurgence from 1972 levels of roughly two or three percent purity of heroin to 1974 levels of five, six, or seven percent pure heroin in the typical bag at the lowest retail level. This suggests to

us that we are indeed seeing fewer levels of middlemen and fewer steppings-on of the high-purity heroin which is imported into the country. The other interesting thing about the recent samples is that there is a much higher incidence of the use of procaine as an adulterant. Of course, almost all samples have milk sugar, or lactose, or quinine as traditional diluents; but the procaine which we have seen in ever-increasing frequency suggests that perhaps there is use of procaine and other active drugs -- codeine, caffeine, amphetamines -- to deceive consumers who are much less sophisticated in the use of opiates than those of five or ten years ago. In other words, we believe that with the current wave of addicts, you can give them poor quality heroin and they will inject it and believe that they are getting high quality junk, because these active adulterants mimic the action of heroin, particularly the initial "rush." We are now examining some of the pharmacological aspects of the procaine rush as compared to the heroin rush. It does seem that there are now many people who can ingest substances which actually contain no heroin at all -- but which do contain a certain combination of procaine or codeine or other substances -- and remain convinced that they are using heroin. This pseudo-junkie phenomenon, this minority of people who believe themselves to be addicts but who in fact are not addicted to heroin, is something which we had not seen until three or four years ago, but which seems to be becoming ever more significant.

The wholesale samples -- the ounces and grams samples -- are fairly similar in median cost per pure gram and in the actual weight of the samples. The difference is that one is called an "ounce" and the other is called a "piece," and "pieces" tend to cost more and to contain a lower quantity of heroin.

The other interesting thing about this new second wave epidemic in the Bay Area is in regard to the expected correlates of heroin epidemic. Now, the overdose deaths that we would expect to result from the new wave of heroin addiction have indeed paralleled that pattern in 1974 -- i.e., deaths showed a sharp rise in 1974 over a low point in 1973. The property-crime figures, however, which were expected to parallel not so much the incidence of new addicts, but the prevalence of addicts who have to support their habits illegally, have shown a decline from those for 1971. We saw in 1970 and 1971 a peak incidence of property crimes in San Francisco, especially robberies and burglaries, and a decline since then. The year 1972 was a particularly low year, then there was a resurgence in 1973, and a further decline in 1974. Right now, in San Francisco, property crime is running about 30 percent below the 1970-71 peaks. It is almost as if both the incidence of new addicts and the incidence of property crimes are caused by a third factor, perhaps a kind of social anomie or some other kind of social phenomenon which is subject to periodic improvement and decline, such that we had a peak of poor conditions in the early 1970's which gave rise both to a high crime rate and a high incidence of new addiction; then there was a drop in both phenomena in 1972, followed by a resurgence in 1973, and now in 1974, the incidence of property crimes and *perhaps* also the incidence of new addiction

seems to be dropping off again. Clearer answers are yet to come; examining the quality of drugs on the street is certainly one path to those answers.

The cause of the current epidemic of heroin in San Francisco is still not clear to me. It does seem from our data that Mexican and Southeast Asian heroin are playing an increasing role in the heroin picture of San Francisco.

SUMMARY OF ANALYSES OF HEROIN SAMPLES - 1974 UPDATE

During the period from January to August, 1974, some forty-one samples of substances alleged to be heroin were analyzed by Pharm-Chem Laboratories in Palo Alto. A few of these samples were forwarded from the Haight-Ashbury Free Medical Clinic, which had obtained them from clients or friends, but other samples came from places as far distant as Vietnam. Twenty-nine of the samples were provided from the greater Bay Area, three samples came from other places in California, six were from other states in the U.S., and three came from abroad.

As with our earlier (1973) sample series, the "burn rate" was quite low. Only two samples (5%) contained no opiate at all, while a third had no heroin but did contain 13 percent methadone. The remaining samples all contained at least some heroin, but in varying level of dilution. The most frequent diluent was, of course, lactose, but procaine was seen in fully two out of every three samples, even among the "ounce-sized" samples. Evidently dealers are adulterating their heroin with an active substance which mimics some of the qualities of heroin and hence has the capacity to deceive some consumers -- and this adulteration is taking place at a fairly "high" stage in the local distribution hierarchy. Other adulterants included caffeine, glucose, quinine, mannitol, and phenobarbitol.

The "price per pure gram of heroin" is an important statistic, which was available for thirty-two of the samples. These samples came from both the wholesale and retail markets, as follows:

OUNCES: Eight samples

Range of stated prices: $750-$2800
Range of tested purity: 0%-74%
Range of cost per pure gram of heroin: $132-$930
(excludes one "burn")
Median: $353

GRAMS: Fifteen samples (including three "spoons" of approximately 1.33 grams)

Range of stated prices: $45-$200
Range of tested purity: 5.4%-84%
Range of cost per pure gram of heroin: $98-$910
Median: $335

BAGS: Nine samples

Range of stated prices: $5-$50
Range of tested purity: 2.6%-67%
Range of cost per pure gram of heroin: $135-$1560
(excludes one methadone "burn")
Median: $835

As compared to the 1973 samples, the wholesale (ounce, gram) buyer seems to be getting much less for his money, while the retail (bag) buyer seems to be getting rather more. The quality of some of the 1974 bags was extraordinarily high, compared to the 1973 bags; this may account for the high number of opiate overdose deaths noted in 1974.

AUTHOR

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Indicators of Drug Abuse—DAWN

Philip H. Person, Ph.D.

I am to speak about DAWN which has already come up in discussion today. DAWN is the acronym for the Drug Abuse Warning Network. It is a cooperative project between the Drug Enforcement Administration and the National Institute on Drug Abuse. The participants in the DAWN System consist of emergency rooms of general hospitals, medical examiners, in-patient hospital units, and crisis centers located in 23 cities. In three cities very large samples are drawn and in the remainder, an attempt is made to include all of the emergency rooms; the medical examiners are included in their entirety from all 23. There are six additional cities which are included on a partially sampled basis.

Someone this morning asked if it is true that anyone can get into te DAWN System by virtue of having been admitted to an emergency room with a drug emergency. The answer to that question is "Yes." You do not have to be a heroin addict or an addicted or dependent person before you can get reported into the system. The definition, as a matter of fact, of who to report goes as follows. Drug abuse admissions to emergency rooms include "the non-medical use of a substance for any of the following reasons: psychic effects, dependence, and self-destruction. Non-medical in this context means: a) the use of prescription drugs inconsistent with accepted medical practice; b) the use of over-the-counter drugs contrary to their instructions on the label; and c) the use of any other substances, for example, heroin, marihuana, peyote, glue, aerosols, etc." I am quoting here from the instructions that are given in the booklet to the reporting facilities.

The idea in obtaining reports of these various episodes is to identify drugs that bring people to the attention of emergency facilities, to identify the patterns of drug use and trends in these patterns with the idea of doing something about it if the problems become big enough, and to schedule drugs into one of the various DEA schedules.

The medical examiners report a different kind of episode from the emergency rooms. Drug-related death is defined to be "any death involving a drug overdose, where a toxic level is found or suspected. A drug-contributory death is one where a drug was only contributory and not said to be the cause of the death."

There are 836 emergency rooms, 613 of which are located in the 29 cities and 223 of which are located outside of these 29 cities. These other cities were sampled in such a way that when combined with the data from the 29 cities a national projection can be constructed which is valid for the entire United States. A similar kind of arrangement has been made for the medical examiner data such that they too can be projected to the entire United States. There is a total of 336 medical examiners reporting. The number of in-patient units is much smaller with 61; crisis centers data are reported from 71 places. To give you an idea about the volume of the system, in the nine months between July 1973 and March 1974, there were 70,000 episodes reported into the emergency room part of the system. Since each episode can report as much as six drugs, obviously, there are more drug mentions than there were episodes. In fact, there were a little more than 100,000 drug mentions. For the medical examiners, there were about 1,800 episodes reported within this nine-month period with 2,700 mentions. In the output from the system, the drugs are reported as to whether they were mentioned alone or in combination with something else. Somebody commented this morning about the difficulty in not being able to deal with episodes and drug patterns. Hopefully, we may be able to change some of the output and get into this type of analysis.

There was another remark this morning about comparing DAWN and one of the other reporting systems. Since DAWN is an indicator or is used as an indicator, and since other data reporting systems are also used as indicators, I think it behooves all of us to be careful as we compare the results of one indicator with the results of another. They are not necessarily comparable, and so to say that DAWN reports "x" number as mentions of a particular drug in a particular place and compare that with CODAP, which I think was the data system being used, one has to remember that the populations being described are quite different. CODAP is a description of patients coming into treatment and DAWN is a description of people experiencing emergencies with drugs, whether they are in treatment or not.

There is a report that has just been finished, called *DAWN 2 Analysis*, which is available on request.* I have another minute according to my watch; maybe Joe Murphy would like to comment. Joe is the DEA project officer and I am the project officer for NIDA -- so we are basically a dual team here.

Mr. Murphy: One other way that we use the particular system, of course, is we are attempting to use it as a measure of some activity within the DEA. There is a great concern now of measuring the effect of scheduling a particular drug, looking for indicators to give some indication of the effectiveness of the schedule. DAWN is one of the indicators. We at DEA use DAWN considerably, but we don't use it independently by itself. We are very conscious of its limitations and we are equally conscious of utilizing DAWN with other bits of intelligence information before any definitive action is made regarding a particular dangerous new substance.

**Drug Abuse Warning Network (DAWN) Phase II Report, July 1973-March 1974.* BNDD Contract No. 72-47. Drug Information Section. Special Programs Division. Drug Enforcement Administration, U.S. Department of Justice, Washington, D.C. 20537

AUTHOR

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Indicators of Drug Abuse—Urinalysis Screening*

Alan Berkowitz, M.D.

Urine testing for drugs of abuse is another one of those indicators that mystifies and makes problematic the findings of epidemiological research more than it illuminates the area. The results of urine testing in our commercial laboratories are so inaccurate as to all but preclude any attempt to determine if the tests are really useful to clients, therapists, administrators, funding agencies or researchers.

An example of these findings is the experience of the Army with almost one million urine tests done in their screening and identification program in 1971. Figure I represents a blind proficiency test of these contract laboratories. Accuracy rates range from 50 percent to 65 percent. This level of accuracy seems to be typical of the state of the art today when blind proficiency testing is done; that is, when the laboratory is unaware that it is being monitored and treats the urine sample as it routinely processes any sample it receives.

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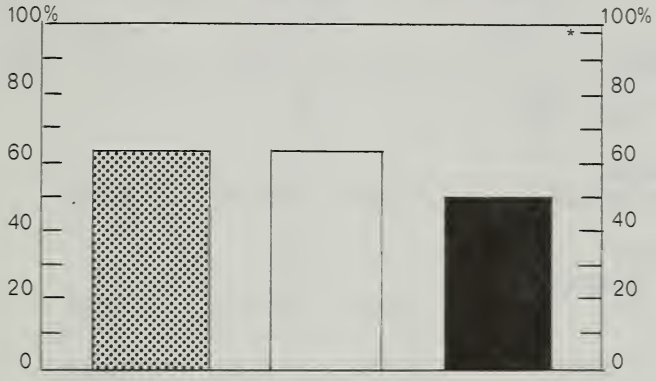
*SOURCE: System Sciences, Inc., *Cost Effectiveness of Urine Testing;* prepared for the National Institute on Drug Abuse, Department of Health, Education and Welfare, November 1974.

FIGURE 1

URINALYSIS ACCURACY RATES OF COMMERCIAL LABORATORIES PERFORMING UNDER ARMY CONTRACTS MONITORING PERIOD: NOV. 22, 1971-JAN. 3, 1972



*Accuracy or reliability required by the contract. . . .98 %

MODIFIED CONTRACTS REQUIRE THE CONTRACTOR TO CORRECTLY IDENTIFY A MINIMUM OF 90% OF THE QUALITY CONTROL SAMPLES

SOURCE: DEPARTMENT OF DEFENSE

United Medical Laboratory

Biochemical Procedures

Washington Reference Laboratory

Another example is the blind proficiency reported for Ultrachem Corporation which does more than 100,000 urine samples per year for the State of California Department of Corrections.* A 60 percent error rate was reported.

The results were no better in New York, which has had a proficiency testing program since 1970. Figure II** summarizes the results of three proficiency tests involving between 42 and 53 laboratories. Excluding reference laboratories, 68.5 percent of these laboratories did not receive passing grades.

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*Los Angeles Times, June 25, 1974.

**Sohn, David. "Analysis for Drugs of Abuse. The Validity of Reported Results in Relation to Performance Testing." *Int. J. Addict.*, 8(1),(1973):65-74.

FIGURE 2

Distribution of Overall Grades in
Drug Screening Performance Tests

Test	1	2	3	Total (all 3 tests)
Percent of labs with:				
Passing grades (all laboratories)	31.0%	43.2%	41.6%	39.0%
Passing grades (reference laboratories excluded)	23.9%	37.3%	32.3%	31.5%
Conditional grades	14.3%	9.9%	22.6%	15.8%
Failing grades	54.7%	46.9%	35.8%	45.2%
Participants	42	51	53	
Reference Laboratories	3	3	5	
Range of Scores	-45 to 100	-188 to 100	-140 to 100	

Outright failing scores (less than 75 percent) were achieved by 45.2 percent of all laboratories across the 3 tests. A total of 7 samples containing no drug at all were submitted to these laboratories; 49 or 14.2 percent of the 345 tests done on these samples resulted in the report of the presence of a drug of abuse -- in other words, false positives. The implications of this level of performance for epidemiology should be obvious, but what makes things worse is that there is a huge differential error rate. For example, of the 267 total false positives, 55 were methadone false positive reports, 24 phenothyazine, 42 barbiturates, 23 morphine, and so on.

The final example of a "blind" proficiency test is taken from a study of 24 laboratories made by the Center for Disease Control in Atlanta. Figure III* compares 24 laboratories that were given both open and blind proficiency tests. Only two of the 24 laboratories scored 80 percent or better on the blind proficiency test, whereas all but one passed the open proficiency test, the usual testing procedure at the Center for Disease Control.

*Supplied by Dr. Louis LaMotle, Director, Licensure and Proficiency Testing Division, Center for Disease Control, Atlanta, Georgia.

FIGURE 3

OPEN VS. BLIND PROFICIENCY TESTING

Lab Number	Grade Open PT	Grade Blind PT
1	100	38
2	90	100
3	71	6
4	100	57
5	100	57
6	100	71
7	100	62
8	100	48
9	90	46
10	100	43
11	100	42
12	81	52
13	90	90
14	100	43
15	100	33
16	100	0
17	100	0
18	100	71
19	100	57
20		62
21	100	57
22	100	Discarded Samples
23		29
24	100	71

SOURCE: CDC
04/16/74

Dr. Sells: I am curious about one thing. I spoke with Allen Green when he was in SAODAP about 5 years ago and he had figures very similar to yours at that time. What I am curious about is, why have these tests continued?

Dr. Berkowitz: That is a question I think we had best discuss later. I agree with you. They should not have continued in the form that they have.

The open proficiency test is the major test that CDC does and is supposed to represent the laboratory's best effort. The blind test, of course, represents their everyday performance. The specific kinds of false negative errors that these 24 laboratories made are instructive. There were 46 misses for amphetamines, but only 24 for methadone. Again, we see a differential error rate for false positives and false negatives.

Coming to the first graded survey that the CDC conducted in February 1973, we find that of 209 participating laboratories, 41 percent failed.

In Figure IV, Sample CT3-05 is instructive. It contained no drugs yet 60 laboratories (28.6%) reported false positives, mainly amphetamines (25 laboratories). Two samples contained only morphine (CT3-01 and CT3-10), both at a level of 1 mg/ml; 12 laboratories (6%) failed to identify the morphine in CT3-01 and 16 (8%) obtained false negative results on CT3-10. An additional 33 (15.9%) laboratories reported false positive results for CT3-01 and 31 laboratories (14.7%) had false positive results on CT3-10.

Dr. Sells: How does this compare with other kinds of tests that these laboratories do, for example, testing for sugar?

Dr. Berkowitz: When I was at the Center for Disease Control I saw the proficiency test results for blood sugar and most other laboratory tests. The results were comparable to those for urine testing for drugs of abuse.

These data are based on spiked urine samples. They are not metabolic urines. Recently CDC has submitted an addict urine as an open test containing only morphine at a concentration of 1.7 micrograms per ml, which is roughly three times the concentration they should be able to identify. Using thin layer chromatography, only 50 percent of the laboratories were able to identify the morphine, 43 percent could not use the EMIT system, and 6 percent did not identify the morphine. For the EMIT technique, by the way, there was an overall 12 percent false positive rate. EMIT is supposed to be one of the more accurate methods and is certainly one of the most widely used in jail screening and in epidemiological work.

In this last survey, reported in July 1974, 295 laboratories participated; 61 percent passed. The addict urine sample is shown in Figure V.

A total of 36 percent of the laboratories reporting did not find morphine to be present, 5.8 percent obtained false positive results. In that same test, there was a very interesting problem -- a sample with only Darvon in it presented the problem of cross reactivity. In the sample that had only Darvon and nothing else, 142 laboratories (48 percent of the total) had false positives; 61 laboratories identified the Darvon as cocaine, 53 as methadone, 14 as amphetamine, and so on.

The analysis of results by drug and methods shown in Figure V also shows some interesting results as well as those discussed for the addict urine. For amphetamines on gas chromatography, the 295 laboratories had a cumulative false positive rate of 8 percent.

For barbiturates, using EMIT, only 59 percent of 295 laboratories had a correct response, 41 percent reported false negative results.

FIGURE 4

DRUG ABUSE PROFICIENCY TESTING 1973

DISTRIBUTION OF RESPONSES BY 209* LABORATORIES

SAMPLE NUMBER	DRUG	TRUE POS.	TRUE NEG.	FALSE POS.	FALSE NEG.
CT3-01	Morphine	194 (94%)	---	---	12 (6%)
	Barbiturates	---	181 (87%)	27 (13%)	---
	Amphetamines	---	203 (99%)	1 (1%)	---
	Methadone	---	201 (100%)	0 (0%)	---
	Glutethimide	---	137 (98%)	3 (2%)	---
	Phenothiazine	---	123 (98%)	2 (2%)	---
CT3-02	Morphine	---	201 (98%)	5 (2%)	---
	Barbiturates	---	172 (83%)	36 (17%)	---
	Amphetamines	---	198 (97%)	6 (3%)	---
	Methadone	187 (93%)	---	---	14 (7%)
	Glutethimide	---	138 (99%)	2 (1%)	---
	Phenothiazine	---	124 (99%)	1 (1%)	---
CT3-03	Morphine	---	201 (98%)	5 (2%)	---
	Barbiturates	186 (89%)	---	---	22 (11%)
	Amphetamines	186 (91%)	---	---	18 (9%)
	Methadone	---	199 (99%)	2 (1%)	---
	Glutethimide	---	128 (91%)	12 (9%)	---
	Phenothiazine	---	119 (95%)	6 (5%)	---
CT3-04	Morphine	188 (91%)	---	---	18 (9%)
	Barbiturates	---	175 (84%)	33 (16%)	---
	Amphetamines	---	199 (98%)	5 (2%)	---
	Methadone	187 (93%)	---	---	14 (7%)
	Glutethimide	---	137 (98%)	3 (2%)	---
	Phenothiazine	---	120 (96%)	5 (4%)	---
CT3-05	Morphine	---	197 (96%)	9 (4%)	---
	Barbiturates	---	199 (96%)	9 (4%)	---
	Amphetamines	---	179 (88%)	25 (12%)	---
	Methadone	---	198 (98%)	3 (2%)	---
	Glutethimide	---	137 (98%)	3 (2%)	---
	Phenothiazine	---	114 (91%)	11 (9%)	---

FIGURE 4 (continued)

DRUG ABUSE PROFICIENCY TESTING 1973
DISTRIBUTION OF RESPONSES BY 209* LABORATORIES

SAMPLE NUMBER	DRUG	TRUE POS.	TRUE NEG.	FALSE POS.	FALSE NEG.
CT3-06	Morphine	---	201 (98%)	5 (2%)	---
	Barbiturates	195 (94%)	---	---	13 (6%)
	Amphetamines	---	202 (99%)	2 (1%)	---
	Methadone	---	198 (98%)	3 (2%)	---
	Glutethimide	---	128 (91%)	12 (9%)	---
	Phenothiazine	---	121 (97%)	4 (3%)	---
CT3-07	Morphine	200 (97%)	---	---	6 (3%)
	Barbiturates	189 (91%)	---	---	19 (9%)
	Amphetamines	---	202 (99%)	2 (1%)	---
	Methadone	---	199 (99%)	2 (1%)	---
	Glutethimide	---	136 (97%)	4 (3%)	---
	Phenothiazine	---	123 (98%)	2 (2%)	---
CT3-08	Morphine	---	203 (99%)	3 (1%)	---
	Barbiturates	---	200 (96%)	8 (4%)	---
	Amphetamines	192 (94%)	---	---	12 (6%)
	Methadone	---	200 (99%)	1 (1%)	---
	Glutethimide	---	137 (98%)	3 (2%)	---
	Phenothiazine	---	123 (98%)	2 (2%)	---
CT3-09	Morphine	---	200 (97%)	6 (3%)	---
	Barbiturates	195 (94%)	---	---	13 (6%)
	Amphetamines	175 (86%)	---	---	29 (14%)
	Methadone	---	199 (99%)	2 (1%)	---
	Glutethimide	---	133 (95%)	7 (5%)	---
	Phenothiazine	---	121 (97%)	4 (3%)	---
CT3-10	Morphine	190 (92%)	---	---	16 (8%)
	Barbiturates	---	186 (89%)	22 (11%)	---
	Amphetamines	---	203 (99%)	1 (1%)	---
	Methadone	---	199 (99%)	2 (1%)	---
	Glutethimide	---	135 (96%)	5 (4%)	---
	Phenothiazine	---	124 (99%)	1 (1%)	---

*Some of the respondents do not offer services for all of these drugs.

FIGURE 5

ANALYSIS OF RESULTS BY DRUG, METHOD AND CONCENTRATION

MORPHINE

	0.5 ug/ml		0.8 ug/ml		1.0 ug/ml		1.5 ug/ml		1.7 ug/ml		
	No. Correct [2]	No. F.N. [3]	No. Correct [2]	No. F.N. [3]	No. Correct [2]	No. F.N. [3]	No. Correct [2]	No. F.N. [3]	No. Correct [2]	No. F.N. [3]	No. F.P. [4]
TLC	186 (83%)	39 (17%)	216 (96%)	9 (4%)	215 (96%)	10 (4%)	219 (97%)	6 (3%)	129 (57%)	96 (43%)	38 (3%)
Gas Chromatography	15 (71%)	6 (29%)	19 (90%)	2 (10%)	20 (95%)	1 (5%)	20 (95%)	1 (5%)	10 (48%)	11 (52%)	4 (4%)
Fluorometric	22 (96%)	1 (4%)	23 (100%)	0 (0%)	23 (100%)	0 (0%)	23 (100%)	0 (0%)	18 (78%)	5 (22%)	0 (0%)
FRAT	10 (91%)	1 (9%)	10 (91%)	1 (9%)	11 (100%)	0 (0%)	10 (91%)	1 (9%)	8 (73%)	3 (27%)	2 (4%)
Radioimmunoassay	11 (92%)	1 (8%)	12 (100%)	0 (0%)	12 (100%)	0 (0%)	12 (100%)	0 (0%)	10 (83%)	2 (17%)	5 (8%)
Hemagglutination-Inhibition	17 (94%)	1 (6%)	17 (94%)	1 (6%)	18 (100%)	0 (0%)	18 (100%)	0 (0%)	17 (94%)	1 (6%)	7 (8%)
EMIT	44 (94%)	3 (6%)	47 (100%)	0 (0%)	47 (100%)	0 (0%)	47 (100%)	0 (0%)	44 (94%)	3 (6%)	29 (12%)

AMPHETAMINES

	1.5 ug/ml		2.0 ug/ml		2.6 ug/ml		4.0 ug/ml		5.0 ug/ml		
	No. Correct [2]	No. F.N. [3]	No. Correct [2]	No. F.N. [3]	No. Correct [2]	No. F.N. [3]	No. Correct [2]	No. F.N. [3]	No. Correct [2]	No. F.N. [3]	No. F.P. [4]
TLC	195 (86%)	33 (14%)	408 (89%)	48 (11%)	204 (89%)	24 (11%)	217 (95%)	11 (5%)	211 (93%)	17 (7%)	61 (7%)
Gas Chromatography	34 (79%)	9 (21%)	77 (90%)	9 (10%)	36 (84%)	7 (16%)	41 (95%)	2 (5%)	41 (95%)	2 (5%)	14 (8%)
EMIT	14 (34%)	27 (66%)	51 (62%)	31 (38%)	37 (90%)	4 (10%)	39 (95%)	2 (5%)	41 (100%)	0 (0%)	1 (1%)

Dr. McGlothlin: Do you have any data on the radio immunoassay technique?

Dr. Berkowitz: Yes I do. For morphine there was an 8 percent overall false positive rate for 295 laboratories.

Dr. McGlothlin: It is not supposed to give any false negatives essentially.

Dr. Berkowitz: For .5 micrograms, they had an 8 percent false negative rate, but for higher concentrations the false negative rate approached zero. That addict urine, which was a metabolic urine, was an exception with a 17 percent false negative rate.

Dr. Sells: How do the tests work under standard laboratory conditions compared to commercial laboratory conditions? What I am thinking about is very often, something works well in the laboratory where it is developed and tested under controlled expert conditions and then when it goes out in the hand of the practitioner, somehow or other, it deteriorates.

Dr. Berkowitz: There are some very fine laboratories, laboratories that consistently score 100 percent on both blind and on open proficiency tests. As a matter of fact, the Army's own laboratories did extremely well. In the same blind proficiency test in which Ultrachem Corporation had a 60 percent error rate, Brooks Army Hospital had only a 5 percent combined false positive/false negative error rate. It is not a function of the technique. There are plenty of problems with the technique, but major problems still lie elsewhere.

Dr. Kramer: What are these problems? What differentiates the laboratories that get the high positive and false negative rates from those that do not?

Dr. Berkowitz: I think you have to distinguish among methods. Thin layer chromatography, despite what many people think about it, is an extremely difficult procedure and correct interpretation of chromatographic spots takes quite a bit of experience. One of the problems with the Darvon false positives that I mentioned is that nicotine and caffeine spots look like Darvon. They can also look like other drugs. Without running proper standards and proper controls, thin layer chromatography degenerates into a guessing game. The major problem, however, seems to occur when, in order to be able to submit low contract bids, laboratories sacrifice excellence for cost.

Dr. Francke: What were you using for passing? What did you mean when you said a passing grade? Was that your own interpretation or CDC's?

Dr. Berkowitz: That was CDC's score of 80, which is not a percentage but is based on a point system. Errors are penalized, but they are penalized in a differential manner. False positives are

penalized more than false negatives. Errors on some drugs are penalized more than errors on others. It is a complicated scoring system.

Dr. Francke: I have been looking at CDC laboratory reports over a period of time and I have seen CDC reports come back zero to 100. So I know there is great variations there, but this sounds serious.

Dr. Berkowitz: CDC reports are very difficult to interpret because of the contradictions in the scoring system. For example, a laboratory that fails may make only three errors, while another that passes may make nine errors. It is not a clearcut thing.

AUTHOR

Alan Berkowitz is Research Psychiatrist, System Sciences, Inc., Bethesda, Maryland.

DISCUSSION

Dr. Gottschalk: I would like to endorse what Alan Berkowitz said in connection with the proficiency of laboratory analysis. We found that there are some medical examiners or coroners that have amazingly high levels of proficiency and some that have surprisingly low, as you might expect. Our thought was that you could grade the proficiency of different reporting bases and in that way temper the demographic and psychosocial findings from those areas. That is, you work in a correction factor regarding how seriously you take the conclusions.

Dr. Berkowitz: I do not know how you can do that. One thing comes to mind from your field: in 1970, Baden* found negative urine tests in one-third of the heroin overdose deaths and those were tested shortly after the deaths.

Dr. Gottschalk: That is not the kind of question and statement I made. I said if you take those nine cities, we can tell you which ones have consistently high or accurate reports on unknowns and those that had consistently low ones.

Dr. Berkowitz: But the type of error and the extent of error depends on what is in the sample.

Dr. Gottschalk: This is what I am saying is not true; we have found something over and above that. We have a variety of things in the unknowns. Varieties of substances are consistently correctly identified whether you have phenothiazines, barbiturates, narcotics or a mixture, by those toxicology labs that are amazingly good across all these drugs. It is true, there is a difference within drugs. For instance, in phenothiazines, there is no radio immunoassay method as yet developed, whereas, with narcotics, there is a method. But that is what I meant when I said you are involved with the state of the science and the art in assaying some of these drugs accurately and quantitatively. However, over and above that, some labs are just amazingly good considering the state of the science and art. I do not know whether they have a large budget or people who spend a lot more time at it or what.

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Dr. Berkowitz: I do not think there is much wrong with the technology.

Dr. Gottschalk: It is the technology in the sense that some medical examiners have a routine. If they find something present on the screen, they do or do not do other tests. They are living within a budget. They may have proficient people, but they have a routine where they figure they can just spend so much money on following this down to exactly what it is. That is something beyond the people.

Mr. Romm: Dr. Berkowitz was referring to data where there were verified positives by a second test.

Dr. Sells: I have recently seen a couple surveys about the accuracy of companies that score psychological tests by optical scanning and some other methods. The results come out very similar to those that you have seen. I daresay that if we take statistics turned out by different service bureaus, we might also find some glaring errors that would horrify editors that accept papers for publication. I wonder if what we are talking about, now that you have reassured us on the technology, is not simply the standards of workmanship that are accepted in our society.

Dr. Berkowitz: I would agree with that, and add the lack of control or power in CDC or other agencies of the federal government to regulate these things.

General Discussion

Dr. Gottschalk: I would like to propose that it is too narrow and parochial just to focus on narcotic use if you take the title of this conference seriously, "Epidemiology of Drug Abuse." I would like to propose that there should be surveys and ongoing studies not only of treatment areas that are labelled as treatment centers for drug abuse, but all general public and private medical clinics and psychiatric clinics where I know there are many instances of drug abuse covered up or not made available. They might well give somewhat different patterns, certainly of the non-narcotics. It is our impression from preliminary surveys we have done that 75 percent of the people coming into general medical clinics for the first time are already taking some drugs, not narcotics, but analgesics, combinations of drugs, drugs which do not have prescriptions, over-the-counter substances, or drugs which have been given to them by members of the family or just taken. By any conservative measures of drug abuse, they are abusing these drugs. I have not heard anything here about that broad area of everyday drug abuse. We are narrowing down, it seems to me, too much, although there certainly are interesting problems to be discussed regarding the narcotic drugs or other illegal drugs.

I would like to broaden the scope of this discussion to include all those and if we are not doing any surveys or any kind of "nose counting" in those area, we certainly should.

Dr. Robins: I would like to raise one issue that I think is terribly important. Most of the treatment data that we have heard this afternoon is not related to any kind of population data. Once you are given the fact that they have to be in treatment, if you have fewer male heroin addicts than earlier, then there have to be *more* women. It does not necessarily mean there are more women abusing drugs than there were before, or more polydrug users than before, but just that you have exhausted the group of heroin users. You are not working on a total population base but only on the base of people in treatment, so the less there are of men, the more there have to be of women. I think we get the peculiar idea that just because there are more women, there are more women drug abusers. It does not mean that, unless the number of the whole treatment group has gone up at the same time.

Dr. Sells: It would be nice to know what the prevalence rate is in the total population and then we could answer that question.

Dr. Robins: Exactly. When you do not have a population base, you cannot interpret that as meaning there are more polydrug users, or more women.

Dr. Sample: In the last year, June 1973 to June 1974, the trends we are talking about have not been reflected in the federal treatment structure, where the males have increased from 71.8 to 72.6 percent. The same applies for the blacks.

Dr. Sells: That could not be explained by error, you do not think?

Dr. Sample: It could be, but I do not think the error would be sufficient enough to reverse that trend. I doubt if the error is such that it would make it statistically significant in the other direction.

Dr. Kramer: There is a whole area of services that I have not heard mentioned here, namely, that if you look at a universe of psychiatric services defined by the state mental health hospitals, out-patient psychiatric services, and community health services, you get into another whole area of people who are coming in for treatment with (at least) a diagnosis of drug abuse, alcoholism, etc. I do not know how these fit into what you are talking about, but they account for some striking changes in the rates at which patients have been coming into state hospitals with the diagnosis of drug abuse. It seems to me that one ought to look at the other parts of the whole medical care system that may get involved with the persons who are given the label of drug abusers. When it comes to local planning and local use of information at the catchment area level, one has to look at these particular kinds of services. This brings up another point that I would like to emphasize and that is the need to put together the data that we have from these diverse kinds of treatment services into a unified picture.

Dr. Sells: Some of these clinics we are talking about *are* parts of community mental health centers. Are your data based only on hospitals?

Dr. Kramer: I have other data for hospitals, for community mental health centers, and psychiatric services in general hospitals, and other groups. I do not have trend data on that. The best trend data we have are for the state mental hospitals. There has been a tremendous change in the patterns of admissions into state mental hospitals where about 33 percent of the current admissions of males into state mental hospitals are for alcohol disorders and six percent are for drug abuse disorders; for females, it is now about four percent for alcohol disorders, and 2.5 percent for drug abuse disorders.

Dr. Sells: If you go back 15 years, most of the proliferation of clinics that treat drug abusers in the community did not exist and the only place that a person could go was to a state hospital.

Dr. Kramer: I know that. All I am trying to say is that, in looking at this overall problem, one has to in some way pull together these data in a meaningful way so that one can get measures of who is coming into a treatment service, by some definition of treatment service.

III. PROBLEMS RELATED TO APPLYING AND EXTRAPOLATING DATA

Problems Related to Survey Sampling

William Glenn, Ph.D.

My concern is with the quality of the kinds of estimates of the extent of drug abuse that you can get with a sample survey. I want to say, first of all, that in order to use the sample survey properly, you must have a probability sample. This is a sample in which every member of the population has a known non-zero probability of being selected. Quota sampling, frequently used in statewide drug-abuse surveys, is not probability sampling and therefore cannot yield estimates which are "good" from a statistical point of view. Reference to this point has been made by Dr. Cisin, and I am sure it is recognized by many others.

The quality of an attribute estimate (such as the percentage of heroin users in a stated population) may be measured by a statistic called the coefficient of variation (CV). CV may be estimated as

$$CV = \sqrt{\frac{p(1-p)}{n}} \Big/ p, \qquad (1)$$

where p is the estimated proportion of the population having the attribute and n is the sample size. CV is simply the standard error of the estimate expressed as a fraction of the estimate itself. The criterion for a good estimate, according to my colleagues who do a lot of sample survey work, is

$$CV \leq 0.30. \qquad (2)$$

This criterion is saying that for an estimate of reasonable statistical quality, the standard error should not exceed 30 percent of the estimate. The particular choice of 30 percent is arbitrary, of course, but it is a value commonly cited by statisticians specializing in sample surveys.

We may use the criterion cited above to estimate the sample size needed as a function of the proportion p. Combining (1) and (2) and solving for n, we have

$$n \geq 1\text{-}p/.09p. \qquad (3)$$

Suppose, for example, that p is on the order of 0.01 (about 1 percent of the population has the attribute of interest). The relationship (3) tells us that for a "good" estimate of p, our sample size should be at least 1100.

We have seen reports on statewide surveys which give the percentage of regular users of heroin as something less than one tenth of 1 percent. The report on a recent survey in North Carolina cites an estimated 2,760 regular users of heroin in a population of 3,731,520. This yields p = 0.00074 (i.e., we are being told that about 7 persons in every 10,000 in North Carolina are regular users of heroin). This small proportion could be encouraging to those who are concerned about heroin use in North Carolina, if they choose to believe it. But should anyone believe it? How good is the estimate? It was based on a sample of size 2,007, so, according to (1), it has CV = 82 percent, which far exceeds the maximum criterion value. It is, therefore, a very poor estimate. Add to this the fact that it was obtained with a quota sample, and you must conclude that you have nothing with which statistical credibility can be associated.

The estimate of 2,760 regular users of heroin in North Carolina proves, upon close examination, to be totally incredible from any point of view. (See Attachment.)

Suppose we had wished to estimate the percentage of regular users of heroin in North Carolina by means of a sample survey. Suppose further that the true percentage in the population is about one tenth of 1 percent. Our criterion tells us that we would need a sample size of at least 11,000. Such a survey based on a probability sample at today's prices could cost half a million dollars. Any reasonable person must conclude that spending that amount of money for that purpose makes no sense. There are, of course, those who would argue that if we can't afford a sample of 11,000, let's do the best we can with 2,000. This is fallacious reasoning because the low-quality estimate (based on 2,000 observations in this case) can give totally misleading information. The analogy that "we can't afford a Cadillac so we'll settle for a Chevrolet" can be pushed too far. The estimate with CV = 30 percent is a Chevrolet-class estimate. If I order something at a lower price, I may receive a beaten-up Volkswagen painted to look like a Chevrolet, and not know the difference until I look under the hood.

If I am the type who just rides off down the road before looking under the hood, I will never know the difference. That is how the analogy can be pushed too far; believe me, this is not an unfair description of what is happening.

In conclusion, I would like to emphasize the following points:

(1) Sample surveys aimed at estimating rare attributes can be very expensive if conducted properly. They can be so expensive that their cost/benefit ratio is too high for serious consideration.

(2) Sample surveys not conducted properly can be even more expensive to society through the imputed cost of wrong (and therefore misleading) information and the misuse of available resources.

(3) The presentation of any estimate based on a sample survey should give the reader/user a frank statement about the validity/reliability of the estimate.

(4) We may not know *a priori* the ballpark in which an estimate may fall. If we elect to do the best we can with the resources available, our report should include a fair statement of what the data tell us. If that turns out to be less than our client wants, we must live with the facts. To do otherwise with drug-abuse data is to do a potential disservice to society.

ABSTRACT:

Regular Users of Heroin Within the General Population of North Carolina

A report prepared for the North Carolina Drug Authority in February 1974 cites an estimate of 2,760 regular users of heroin within the general population of North Carolina (Table 100, p. 211). The report also provides the following information on the 2,760 regular users (Tables 101-105, pp. 212-216):

All 2,760 are in the age group 18-24,

1,320 are male
1,440 are female

1,320 are black
1,440 are white

All are in "Middle" socioeconomic status

1,320 are in "Lower Middle" social position
1,440 are in "Upper/Upper Middle" social position

1,320 are employed in the category "Skilled/Semi-Skilled"
1,440 are employed in the category "White Collar/Other Clerical"

1,320 use heroin "at home"
1,440 use heroin "at a social gathering"

1,320 also use non-controlled narcotics/prescription non-narcotic analgesics
1,440 also use barbiturates, non-barbiturate sedative-hypnotics, controlled narcotics (non-heroin), LSD, and cocaine.

All 2,760 also use marihuana/hashish, psychotogens other than LSD, and methedrine/methamphetamine.

1,320 are moderate drinkers of alcohol.
1,440 are heavy drinkers of alcohol

All 2,760 are "moderate or light" smokers of tobacco

All 2,760 are regular users of prescription drugs.

The report also breaks this information down by geographic regions within the state, as follows:

Region 1: *Western* (32 counties)
Regular users of heroin: none (Table 198, p. 353)

Region 2: *North Central* (16 counties, including Durham and Orange)
Regular users of heroin: 1,440 (Table 268, p. 424)

All 1,440 are white, female, and in the age group 18-24 (Table 269, p. 425)

All 1,440 use heroin "at a social gathering" (Table 270, p. 426)

Region 3: *South Central* (20 counties, including Wake)
Regular users of heroin: 1,320 (Table 339, p. 495)

All 1,320 are black, male, and in the age group 18-24 (Table 339, p. 496)

All 1,320 use heroin "at home" (Table 340, p. 497)

Region 4: *Eastern* (32 counties)
Regular users of heroin: none (Table 406, p. 564)

Thus the estimated 2,760 regular users of heroin within the general population of North Carolina consist of the 1,440 white females in the north central region who use heroin at social gatherings and the 1,320 black males in the south central region who use heroin at home.

In a report dated March 1974, were present "implications and recommendations" pertaining to the findings in the February 1974 report.

Those implications which pertain to the regular users of heroin in the general population are the following:

100.0% are in the age group 18-24 (Table 19, p. 95)
47.8% are males; 52.2% are females (Table 29, p. 105)
100.0% of those in Region 2 are females (Table 33, p. 109)
100.0% of those in Region 3 are males (Table 35, p. 111)
47.8% are black; 52.2% are white (Table 39, p. 115)
100.0% of those in Region 2 are white (Table 43, p. 119)
100.0% of those in Region 3 are black (Table 45, p. 121)
100.0% are employed persons (Table 49, p. 125)
47.8% are employed males; 52.2% are employed females (Table 51, p. 127)
100.0% are non-students (Table 53, p. 129)

These implications follow, of course, from the data cited in the February 1974 report. If we are to accept them as credible, we must agree, among other things, that:

1. Regular users of heroin in the general population of North Carolina are found only in the two central regions of the state (in 36 of the 100 counties).

2. In one of those regions all of the regular users are white and female; in the other, all of them are black and male.

3. There are no white males or black females who are regular users of heroin in the general population of North Carolina.

4. No students in the general population of North Carolina are regular users of heroin.

5. No persons over age 24 in the general population of North Carolina are regular users of heroin.

It is unlikely that anyone familiar with the current "drug scene" will accept all of these statements as credible.

It is clear from a few quick calculations that all of the "conclusions" cited above are based on two observations. One was the white female in Region 2, the other the black male in Region 3, both of whom admitted to being regular users of heroin. These were "projected" into 1,440 and 1,320 respectively, and the rest was simple arithmetic. However, arithmetic cannot erase the very tenuous nature of the conclusions. The only valid conclusion is that in the general population of North Carolina, two people were willing to admit to the regular use of heroin. One was a white female in Region 2; the other was a black male in Region 3. What does this tell us about regular users of heroin in the state? Very little, except that out of 2,007 people interviewed, two were willing to admit being regular users. Apparently one person in 1,000 interviewed is willing to admit to regular use of heroin. The number who actually are regular users and their sociodemographic characteristics remain as unknown as they were before the survey.

AUTHOR

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DISCUSSION

Dr. Robins: One difficult problem is that you do not know the "P" more or less until after you have done the study.

Dr. Glenn: Correct, we do not know the "P" until after we have done the study. Then let us tell the people what the C.V. is; we know that after we have done the study. If the estimate is poor, let us say so; admit it. I am not suggesting that you must know the "P" before. If you did, there would be no point in doing the survey. I am saying that when the "P" comes out to be 130 percent instead of 30 percent, that we should tell our readers that.

Dr. Sells: I think there is a methodological point to be considered. There is a great deal of pressure to edit the kind of information that the surveys are addressing. There is a real conflict that one encounters between attempting to do something that might be productive without violating the rules of inference too greatly. I wonder whether a possibility here is that of attempting to survey the high risk segment of the population where the "P" would be higher but the cost of the survey in relation to a usual estimate would be more reasonable. In other words, use that as an outside or upper limit figure for the purpose of estimation.

Dr. Glenn: I believe that Lee Robins mentioned something very much like this yesterday -- working with homogeneous sub-populations. Yes, that would certainly be a suitable solution.

Dr. Sells: The reason I wanted to mention it is I think that sometimes in this kind of discussion, sentiment swings one way or another. Of course, this is a very sophisticated group, but I do not want this to turn into a general condemnation of the survey method.

Dr. Glenn: I hope you do not think I am condemning the survey method. I am merely saying let us try it properly.

Dr. Cisin: I do not think anyone is condemning the survey method. I do not think we can do without the survey method and no one dares condemn it without something better to suggest for the kinds of purposes that we are dealing with. I have not heard a better suggestion. As surveyors, we are groping with some extremely difficult problems that we have to solve and, as usual in science, we

are trying hard, but the message that is coming through does not deal with survey method as method. What I hear Bill Glenn saying is: these darn point estimates, that lead us to the 9 million alcoholics or the 2700 heroin users, or whatever they are. These point estimates should be recognized as the terribly fallible pieces of data that they are. We all grew up on interval estimates and this coefficient of variation that Dr. Glenn suggests is a short cut to reminding us that we are dealing with an interval estimate.

Our interval estimates can be pretty good but our point estimates are rotten. We are embarrassed, in some cases, by the fact that the interval estimate is very broad. All we really know in some cases is that the proportion we are estimating -- the attribute we are estimating -- is either characterized with some reasonable confidence, somewhere between zero or delta, and 5 percent. For certain political purposes that may not be enough, but for scientific purposes, that is about all we have.

DEA Register

Joseph A. Greenwood, Ph.D.

I will describe the DEA Register very briefly. The register contains narcotics abusers, cocaine and marihuana abusers. Police everywhere are supposed to send in DEA form #34 which has on it about a dozen elements of data, e.g., sex, date of birth, residence, how long addicted, the drug used, source of supply, and so on. Sometimes they do not do that. They may send in their own rap sheet which has at least that much information on it. At any rate, it eventually gets into our files.

I am supposed to say something about the problem of the register. The problems pertain only to what we are going to do with it, of course. This is a voluntary reporting system, and consequently, it has elements of being sporadic. There are efforts to improve this. The best of these would appear to be to take advantage of the FBI arrestee reporting system wherein identification is unequivocal through the fingerprint system. That would eliminate the present problems of sporadic reporting to DEA, and identifying numbers of unique persons being reported, and how many times each was reported. Another possibility would be to have someone make copies of the rap sheets in most of the largest cities and send to DEA to process.

The arresting policeman decides whether or not to label an arrestee as an addict. We think that he does a pretty good job at making the correct decision. The police are concerned about the drug problem and do see many abusers in the course of duty and

have several symptoms they look for besides questioning the arrestee. Of course no one can get around the ambiguity of definition of an addict, but basically they are turning in to DEA what we now call abusers. These include addicts, of course.

Now to discuss some of the uses of the DEA register data, actual and potential. When an investigator has access to certain kinds of data, he usually must devise or find procedures and analyses specific to these data if he hopes to get his questions answered. The DEA data sampled the entire U.S., which gave it a drug coverage of narcotic abusers greater than any other extant system. However, being of a voluntary nature, it was subject to sporadicity of reporting, the effect of which we cannot be sure of. In spite of this defect of the data, my adaptation* of a method in order to estimate prevalence of U.S. narcotics abuse seems to be rather robust for large coverages, and I believe has an averaging effect which accounts for its robustness.

It appears quite possible to do other things with the DEA register. For one thing I envisage the following reasonable attempts to estimate the fraction of the DEA-obtained narcotics abusers who are addicts in the sense that a treatment facility (TF) patient may be termed an addict. Obtain random samples of TF patients and of DEA arrestees. For each sample make a histogram or graph of frequency versus average daily dose. Superpose the graphs so the abscissae coincide and shrink (uniform reduction factor for all ordinates) the TF curve until it is essentially all just under the DEA sample curve. Then the ratio of area under TF curve to area under DEA curve is an estimate of the desired fraction.

Another possibility is obtaining U.S. relative incidence of narcotics abuse using a procedure** developed for and successfully applied to treatment facility data. The appropriate data are already being collected in the DEA system.

As a final use I have in mind for the DEA register, I need it as a testing ground for another method of estimating prevalence. This is based on the Poisson distribution which, in an initial try, gave a prevalence agreeing with 5 percent of the corresponding value obtained by the present capture-recapture method. If it should work out reasonably well in another try on the DEA data, I would feel inclined to apply the method to estimate prevalence of dangerous drug abusers in a large city. The basic data would be abusers entering emergency rooms.

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*Joseph Albert Greenwood. "Estimating Number of Narcotic Addicts." SCID-TR-3, Oct. 1971, Bureau of Narcotics and Dangerous Drugs, Justice Department, Washington, D.C.

**Joseph Albert Greenwood. "Estimating Relative Incidence of Narcotic Addiction Using Treatment Facility Admissions." RTP-TR-11, June 1974, Bureau of Narcotics and Dangerous Drugs, Justice Department, Washington, D.C.

AUTHOR

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DISCUSSION

Dr. Rootman: I would like to say a few words about the Bureau of Dangerous Drugs, Narcotics Registry in Ottawa, Canada because compared with what Dr. Greenwood has said, the Registry in Ottawa is somewhat different. I think it is a reasonably good source of information in that it draws not only from police reports, but also from pharmacy reports and from treatment centers. We have been doing a number of studies based on this narcotics registry, and I would like just to mention a few of the studies that we are involved in. One of the studies is a look at some of the trends in incidence and prevalence over the past 10 or 15 years; we have almost completed that and hope to send it in for publication shortly. We are also doing a study based on 1973 cases that have come to the attention of the Register. In other words, we are going to look in great detail at the incidence for 1973. We plan to do this on an on-going basis every year. This will allow us to get tabulations by particular locations in the country, which has never been done before. Another study that we are thinking of doing involves taking a close look at cases dropped from the Registry. In contrast to the DEA Register, cases are maintained for 10 years, rather than 5 years, so that it gives us a little different perspective. We are also planning to look at the spread of reported heroin use in Canada using the Registry as a basis. As I mentioned earlier, we found that in the interior of British Columbia, there was a relatively high incidence of narcotics cases reported in 1973. We are going to follow-up these cases with other sources of information to find out whether or not they originated from the area or came in from other areas. We are also planning to do a number of validity studies of the Register comparing the Register with other sources of data, such as death data. That describes some of the things we are hoping to do with the Bureau of Dangerous Drugs Registry. The point that I would like to make is that, at least in Canada and I assume in the States as well, it is a very good source of information on reported incidence and prevalence. It certainly has a lot of problems with it, but it is satisfactory for some purposes.

Research Uses of the NYC Register Data

Irving Lukoff, Ph.D.

I have been asked to talk to you about the research potential of the Narcotics Register in New York City. I may not be the happiest choice, at least from the perspective of the staff of the Narcotics Register, since my two efforts to obtain cooperation from the staff of the organization ultimately drew blanks each time. One of these efforts, which is discussed in one of the reports prepared by the Register, was an expensive and time-consuming experience. I will briefly discuss them because they bear upon important public issues. I will also attempt briefly to reconstruct the way in which they assemble their data and report on some of the uses to which information on the files of the Register have been used.

The concept of a Register for a particular disease or disability or social category is not a recent invention. A Narcotics Register has to cope with problems that arise in most such registers where there are almost insuperable difficulties in assuring an accurate flow of information from a multitude of sources and where the very status of the individuals may be dynamic, undergoing significant alterations through time. The oldest and most successful activity concerns the accumulation of information on death, although the identification of the "causes" of death is still troublesome. At least the requirement that individuals be buried has ensured reasonable compliance with the demand for death certificates so that a cumulation of reasonably accurate data is ensured. Births, it should be noted, until a few decades ago were only collected in a limited area even in an advanced country like the United States because of the unreliability of reporting sources. The compila-

tion of morbidity statistics raises all the issues that arise in a Narcotics Register and there is an accumulation of experience in working with such data sets that could be helpful in improving the uses to which such information can be applied, although this has not been done.

In an earlier research reincarnation I had to work with a register for the blind where all the issues of identification of who might be legally blind, the problem of cooperation of multitudes of referral sources, the alterations in the status of blind people, and the culling out of deceased or recovered blind persons raised the same issues as arise in the documents prepared by the Narcotics Register. And though our heightened sensitivity to confidentiality, as well as the perhaps more profound stigma, at least the legal aspects, associated with addiction certainly add additional complications to a Narcotics Register, these are only ones of degree and not qualitatively dissimilar.

But this accumulated experience seems to never have been brought to bear on the Narcotics Register. As one reads their own materials one has the feeling that the wheel is being rediscovered once again. Yet it would be conceivable that various administrative devices, as well as special investigations, could provide reasonably accurate estimates of many of the errors, some unavoidable, in order to facilitate adjustment of the data. At the least, it would be possible to modulate one's interpretations of the findings with some clear information on the direction that errors might take.

The basic sources of information for the Narcotics Register are the police and treatment programs. The latter have increased in prominence as the treatment system rapidly expanded after 1969. In the beginning, police reports predominated, coming to a peak in 1972. In the latter half of that year and subsequently, treatment programs have become the major source of information. This, of course, reflects the emergence of a large number of programs although the decline in police reports is subject to many interpretations. There have been policy changes throughout this period that shifted police emphasis from street-level addicts to persons involved in the sale and distribution of narcotics so that the decline in the absolute number of arrested addicts is at least partly the result of policy changes by the police department.

It is noted in a recent report by NR (September 1973) that the police are more likely to report cases previously unknown to the register. We also know that the treatment population is likely to be from 7 to 10 years older than recently recruited addicts. All of this raises serious questions on the credibility of the reports that "incidence" is declining since the interpretation of successive cohorts of new cases are, in a very real sense, noncomparable. Police activity, as is well known, is a variable and cannot be presumed to be constant over time. This is particularly the case in a volatile area like the arrests of addicts.

These estimates are further confounded since the Board of Education does not act as a referral source and juvenile arrest data is severely limited in New York City. Consequently, the limited sources of referral for a crucial age-range for estimating incidence and the decline of police activity create almost insurmountable problems. Although there could conceivably be efforts at estimation of these various sources of error, they have not been carried out.

There are clear legal rulings that support the requirement that programs and physicians report to the Register. Despite this clear legal back-up many private physicians, as well as treatment programs, do not systematically report to the Register. Nor does any concerted effort seem to be operative to encourage compliance. Further, although only minimal information is requested, the forms often arrive without significant identifying data, some of this reported in earlier reports put out by the Register. Identification of addicts is troublesome under most circumstances and where reliance is often on judgments of police and court staffs, this adds even further ambiguity to the compilation of the data. An index of the magnitude of some of these issues may be gauged by a follow-up study I carried out on patients who dropped out of one program. The Register as a matter of policy will not assist programs in tracking down patients or in learning whether they may be simultaneously enrolled in other programs. This is a troublesome problem for methadone programs that want to avoid multiple registrations. However, at the Rockefeller Institute they do keep records on all programs related to the Beth Israel group of programs and on any other programs willing to cooperate. This file is also one of the major sources of information for the Narcotics Register. Yet, fully half of the individuals I was seeking could not be located on that file. This proportion may only reflect the administrative vagaries of one particular treatment program; however, it also provides some indication of the way in which even addicts in publicly funded treatment programs may also escape the referral net of the NR.

Although at one time the Register culled out individuals who were not reported to the Register for five years, this policy has been rescinded so that even the prevalence figures raise further questions. Not only are non-narcotic users commingled with those who use opiates, but many persons not addicted at all are presently retained on the files for estimating prevalence.

Other registers, notably DEA, cull people out on whom reports do not appear for five years. In Toronto a ten-year span is used. This at least contributes to the (1) elimination of many possibly erroneous reports; and (2) allows for individuals who make the transition to a more conventional life-style to be removed. Obviously, some addicts will also be eliminated but this at least serves as a counterbalance to the inevitable "false positives" to some unknown extent. The prevalence estimates prepared by NR do not make any effort to account for mortality, long-term jail sentences, migration or successful treatment. Instead, in a recent

document the assertion is made that "It is also imperative that individuals never be dropped from an addict registry," although no rationale is even suggested.

Where precise estimates are made NR does adjust for the likely rate of recovery (and mortality), but on a foundation that is clearly inadequate. It is a method that is extraordinarily sensitive to the problems we previously described so that it exaggerates the types of errors that influence the referral patterns over time. These include the growth of the treatment system, on the one hand, and the shifts in the operations of the police department.

Many of these problems are a result of the particular structure of programs and legal restrictions concerning juveniles in New York City and are probably not altogether surmountable. Although improvements could be made in working with various referral sources and in ensuring more complete information, this also has its limits. The only solution would be one that is directed toward providing more realistic estimates of various sources of error so that meaningful adjustments could be made. Early efforts were made to identify some of these sources of error; however, this activity is no longer carried out.

Confidentiality is an essential feature for a narcotics register if cooperation is to be achieved from physicians and treatment programs. Dr. Newman has been fervid and courageous, even having gone to court for his refusal to cooperate with the district attorney to provide information on an addict. In a recent monograph he states categorically, "Demands for identifying information must be refused with firm consistency whether made by the FBI, the police department or homocide squad, the United States Secret Service, Federal Narcotics Agency, researchers, employees, friends, relatives, or even the reported individual himself...". Two issues deserve comment. First, what appears to be a civil-libertarian stand has in fact engendered conflict with one of the main thrusts of civil liberties groups; namely, an individual's right to access to information that might be harmful to him. There are serious questions concerning the quality of the reporting sources that may result in stored information that is not only harmful to the individual but may be erroneous. Civil liberties is a two-edged sword and here it appears to have been invoked somewhat haphazardly.

Researchers also have an obligation to protect their sources of information and there is abundant activity presently to elaborate the procedures to ensure protection of human subjects. Serious questions arise, however, when a tax-funded organization not only compiles data with only very limited utility, but also refuses, as a matter of policy, to provide access for legitimate research and program uses. There are many devices to ensure anonymity that would protect the individuals on the register from harmful consequences. It is legitimate to attempt to prevent multiple registration of methadone patients. The refusal to provide this service -- one that might have as a trade-off improved cooperation --

forces the programs to establish a costly independent register. Thus, individuals in programs have multiple files and the assumption that an individual's records are unassailable is simply not the case for most methadone patients.

There have been cooperative research efforts, the major activity associated with the early research conducted by Dr. Francis Gearing at MMTP. Other studies were carried on by staff members and outside researchers, but this activity seems to have been curtailed. A recent report refers to a study initiated with the predecessors of the present administration but which was aborted unilaterally after a large investment of money and time. Here is what is stated about this research activity in its entirety:

> "Other requests (for cooperation) are superficially more reasonable and seemingly offer no threat whatever to those whose records are sought. Researchers, for instance, have asked for the identity of a random sample of persons known to the Register who could serve as a matched control group against which to measure the outcome of a particular treatment approach."

There was indeed no possibility to identify any individuals in the design of this study. It would have provided, however, one of the rare opportunities to gain insight on (1) selective factors within a community on who seek treatment; and (2) provide a matched untreated group of addicts in order to contrast with patients in treatment. Neither of these activities has ever been carried out in the assessment of drug programs and the arbitrary discontinuance of this activity, which was nearing completion, prevented a rare research option in evaluation from being completed.

The emphasis on protecting confidentiality prevents activities that could improve the quality of the data, provide important services to treatment programs, or assist legitimate research activities. While rigorous strictures are indeed necessary, it appears that these terminate in a no-win policy: poor data difficult to assess, and little or no contribution to the ongoing treatment and research scene.

It should also be noted that at present there are no experienced epidemiologists or bio-statisticians, or persons with related experience on the staff. It would appear that the Register, if it is to serve its purposes, must be separated administratively from its present location within a city bureaucracy which perforce has its own commitments, and these are often at loggerheads with the total treatment and research scene.

AUTHOR

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DISCUSSION

Q: Who is it funded by?

Dr. Lukoff: Originally by NIMH, now by DAAC.

Dr. Richman: The case Register has been involved in collaborative studies with researchers in the past, one of whom is myself. We had a very satisfactory relationship in which the researcher input was provided to the data and worked out to three studies. Currently we have an understanding with Dr. Newman that he will collaborate in providing tabulations on the basis of not violating confidentiality. This is somewhat of a different perspective than what Irving has talked about. I want the numbers and not the names.

Dr. Lukoff: The protocol we worked out with the predecessor of Dr. Newman is one in which we would have access to no names. We trained their people to get the data from the Register and to get the police records and to perform certain tabulations for us. We would never see the police records and never see the names. Their staff was trained to code the material. We would get aggregate outputs from them. There was no possibility of violation of confidentiality in my case.

Ecological Studies of Narcotic Addiction

Alex Richman, M.D., MPH

This research is concerned with the ecology of narcotic addiction within selected areas of New York City. The objective of this research is to increase knowledge of the social factors associated with the onset and prevalence, distribution and spread of heroin addiction in the community, and is intended to:

- improve our epidemiologic knowledge of the natural history of heroin addiction (causes and course)
- assess social factors associated with the extent and distribution of heroin addicts in New York City, and their entry into treatment
- provide better data for use with statistical models of the distribution of spread or diffusion of heroin addiction.

The approach is ecological and is therefore related in concept and method to medical geography, statistical geography and social area analysis. Ecological studies of mental disorders suffer from many difficulties. Difficulties in ecological research include scanty data; lack of age-sex standardization; transiency of the denominator and relative mobility of the numerator; the "ecological fallacy" of applying areal correlations to individuals. In addition there are problems in assessing to what extent ascertainment is affected by social area differentials in "visibility"; or whether differential treatment results in area differences in morbidity. Research is also hampered by unsatisfactory clinical methods for

characterizing and classifying the type, severity, or prognosis of the disorders being studied; and problems in defining the onset of the disorder and determining the time relations of exposure to the social factors being considered (Ross; Dunham).

These specific problems are illustrated in Figure I which shows the geographic distribution of "narcotics" arrests in Fresno, California during 1970, 1971 and 1972. In addition to the number of arrests increasing from 415 to 1,186 to 1,320, there is a progressive extension of the areas in which the arrests occurred; areas in which arrests for prostitution and assault also increased. Although the concentration and spread of arrests are striking, it is not possible to assess to what extent the changes represent extension of the disorder or changes in police activity; or whether the increasing number of events (arrests) represent increasing number of individuals. The clinical characteristics of the arrestees are unknown; a population base is not presented from which to determine rates; year of first arrest or onset of "addiction" is unknown; the apparent correlation with arrests for other social problems such as prostitution and violence is not straightforward. (See Figure I -- a, b, c.)

The epidemiologic perspective of the research to be outlined in this paper is concerned with the relation between social factors and the distribution of heroin addiction; is clinically substantiable and current; is population based (permitting comparison with the general population); considers demographic and geographic characteristics and relations between person, place and time; is standardized for sex, age and ethnic factors; and distinguishes events and individuals as well as prevalence, incidence and admission rates.

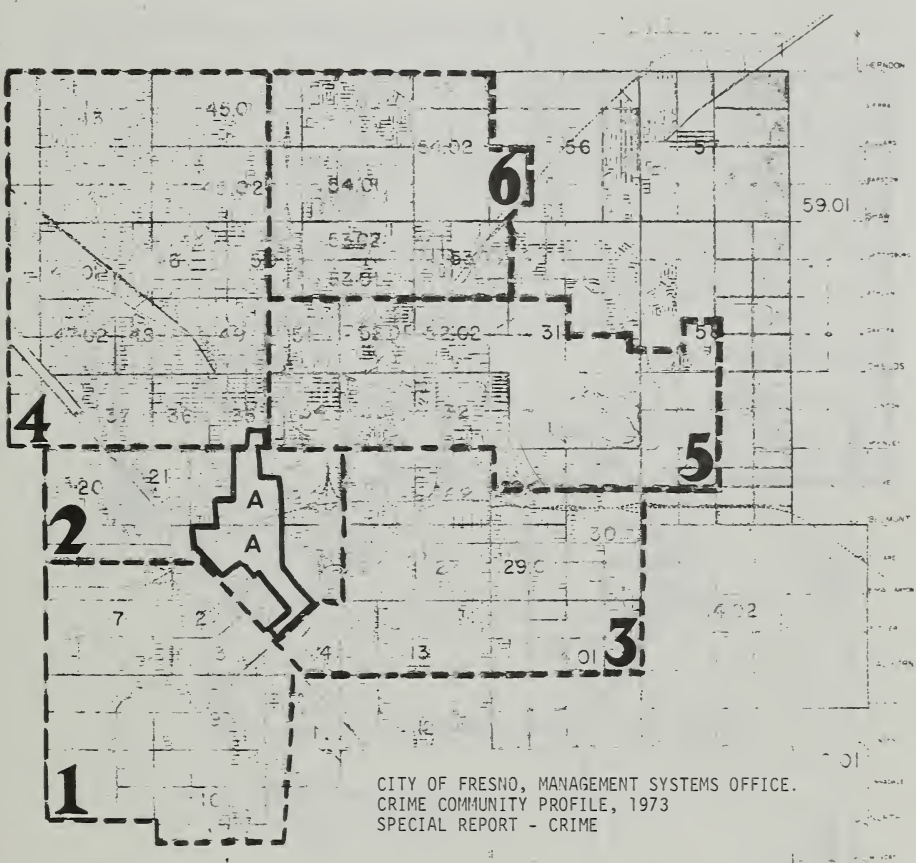
BACKGROUND

Faris and Dunham (1939) reported the ecological distribution of 772 drug addicts without psychoses admitted to institutions for the insane in Chicago during 1922-1934. Since 50 percent of the cases were in hobo or rooming house areas at the center of the city, the authors concluded that drug addicts came mainly from the zone of transition where "...it is, of course, easier to obtain an in-group solidarity and maintain contacts with other addicts and dope peddlers," and that "...drug addicts tend to select the more mobile areas of the city where their habits and activities are less likely to be scrutinized."

Bingham Dai (1937) widened Faris and Dunham's study by collating data from law enforcement sources, as well as, from Cook County Psychopathic Hospital for 2,619 individuals seen during 1928-1934. Dai concluded that the areas with the highest rates of addiction were characterized by "...indications of a considerable degree of physical deterioration, as well as, the lack of what may be called the neighborhood spirit, and that most of the occupants of the areas were unattached men who were constantly moving from one

FIGURE 1A

NARCOTICS ARRESTS, 1970
FRESNO, CALIFORNIA



Number of Arrests - 1970 (Total = 415)

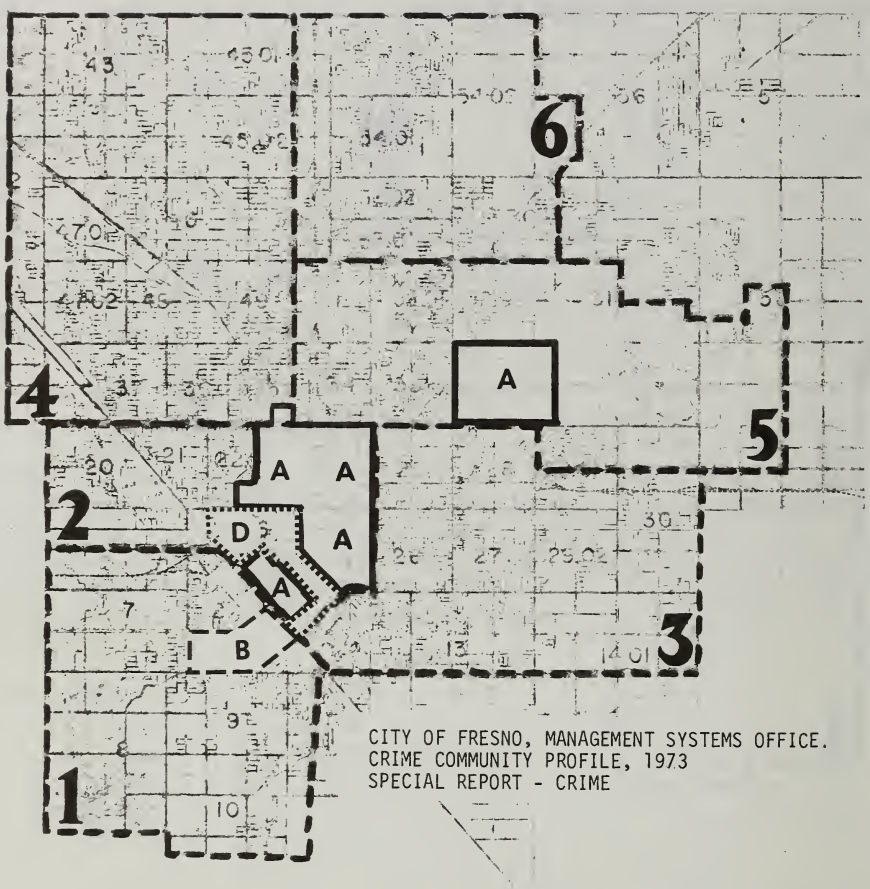
D	C	B	A	
185+-149	148-112	111-75	74-38	37-0

CONDITIONS WORSE THAN AVERAGE

CONDITION BETTER THAN AVERAGE

FIGURE 1B

NARCOTICS ARRESTS - 1971
FRESNO, CALIFORNIA



CITY OF FRESNO, MANAGEMENT SYSTEMS OFFICE.
CRIME COMMUNITY PROFILE, 1973
SPECIAL REPORT - CRIME

Number of Arrests - 1971 (Total = 1186)

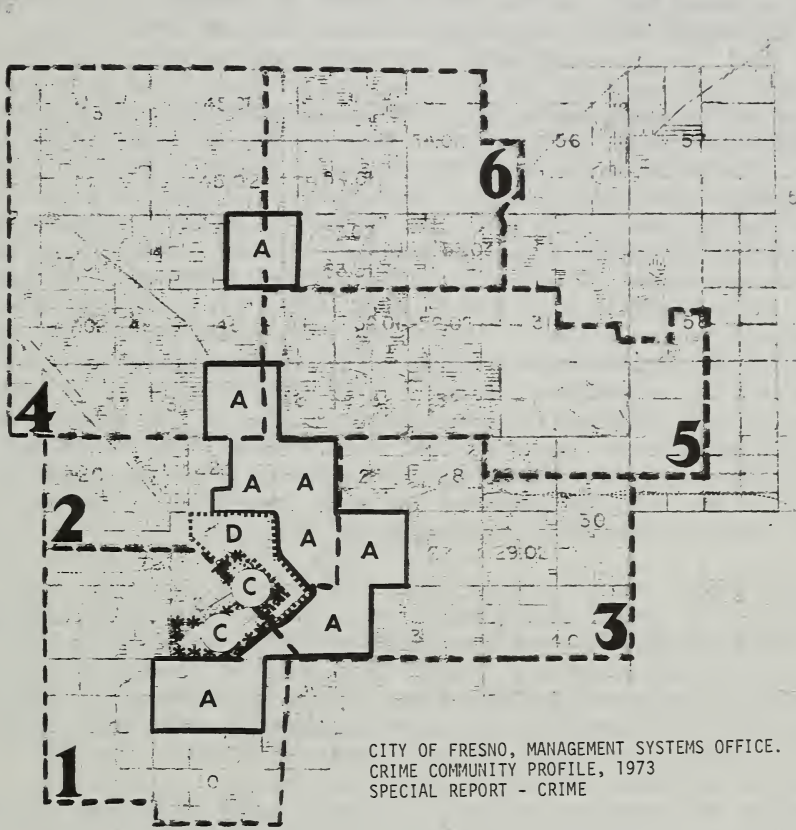
D	C	B	A	
185+-149	148-112	111-75	74-38	37-0

CONDITIONS WORSE THAN AVERAGE

CONDITIONS BETTER THAN AVERAGE

FIGURE 1C

NARCOTICS ARRESTS - 1972
FRESNO, CALIFORNIA



Number of Arrests - 1972 (Total - 1320)

D	C	B	A	
185+-149	148-112	111-75	74-38	37-0

CONDITIONS WORSE THAN AVERAGE

CONDITIONS BETTER THAN AVERAGE

place to the other; and that the environment of the area was most conducive to personal disorganization."

Chein et al. (1964) studied the distribution of 3,457 boys aged 16-20 reported from courts and hospitals as having been involved with narcotics in New York City between 1949 and 1955. They found that the three variables that stood out most in all three boroughs in accounting for the variance in drug rates were percentage of Negroes, percentage of low-income units, and percentage of males in "lower" occupations. In the case of some of the variables, the correlations with drug rates were quite different, even in ordinal terms, in the three boroughs. They concluded that there was an association in "some fashion" between adolescent addiction and living in areas of economic squalor. "As far as the social environment is concerned, the vulnerability of teen-aged males in New York City to the lure of narcotics is in the main associated in some fashion with living in areas of economic squalor, but other unwholesome aspects of the social environment also contribute in substantial measure. That is, conditions of economic squalor dominate the picture but virtually the entire complex of unwholesome factors plays a contributory role. We had hoped, but not really expected, to discover one or two clear-cut factors that could account for the lure of narcotics; but, as usual, social causation is a complex affair."

Clausen (1961) concluded that disorganized neighborhoods with high rates of social and economic problems produce concentrations of narcotic problems. Koval (1960) who analyzed New York City Narcotics Register data, reported that areas with high rates of reported opiate use were also areas with high delinquency rates, high rates of financial assistance and unemployment, low family income, large percentages of out-of-wedlock births, and high rates of health problems, including venereal disease, tuberculosis, hepatitis and infant mortality. However, areas with high rates of problems were not always the areas with the highest rates of opiate use.

Nurco (1972) analyzed the ecological distribution of 833 persons classified as narcotic addicts by the Baltimore City Police Department 1966-1968. He concluded that addicts (from this source) were often found in those areas where there was extreme deprivation, crime, and juvenile delinquency; but that narcotic addiction did not follow as closely the same pattern of "residence" as did being a poor person, a criminal or a juvenile delinquent.

Hughes and Crawford (1972) described the identification of 11 macro-epidemics in Chicago between 1967 and 1971, the largest outbreaks continuing "...to occur in underprivileged neighborhoods." Their data "suggest that the majority of addicts are produced by macro-epidemics that occur in a relatively small number of neighborhoods."

The strong relationship between social environment and geographic concentrations of opiate addicts has been emphasized in all the

above studies. It is not clear whether certain areas "produce" addicts; whether addicts are more "visible" in certain addresses or whether addicts choose certain parts of the city in which to live for anonymity and convenience; whether these concentrations represent the consequences of selective contact with reporting agencies, or whether these relationships can be attributed to various statistical or methodological problems.

The ecological correlations by Faris and Dunham, Dai, Chein et al., Koval and Nurco all deal with the *prevalence* of cases found during a period of time without regard to the onset of the disorder. Koval, in her analysis of Narcotics Register Data, distinguishes persons first reported to the Narcotic Register during the study period (first admissions). Hughes' Chicago studies differentiate prevalence and incidence and consider the year of first heroin use reported by addicts from specified areas; but, since they do not deal with a denominator, population-based rates of onset are missing. The distinction between incidence and prevalence (and their relation to first admission rates) has been discussed in full by Kramer (1957). While Hollingshead and Redlich (1958) found that the prevalence of treated mental illnesses was related significantly to social class, "incidence" rates did *not* vary inversely with social class. In contrast to other mental disorders, year of onset of narcotic addiction can be more readily assessed and the crucial index of incidence determined.

Secondly, in addition to the type of morbidity index, there is the problem of major demographic differences in the distribution of narcotic addiction. *Standardization* for sex, age, color and ethnicity is essential. As well, appropriate attention must be paid to the possible effect of geographic differences in mobility of the numerator and denominator.

Thirdly, the method and process of *ascertainment* must be considered. To what extent can areal differences in morbidity result from differentials in identification, contact with treatment agencies, or interval between onset and contact. As the sources of ascertainment are widened, there is a parallel decrease in the information upon which the clinical diagnosis of narcotic addiction must be based. This fourth factor of *clinical confirmation* of narcotic addiction is crucial for studies based on data derived from non-clinical settings. In addition, since narcotic addiction is not homogeneous in characteristics, severity or course, it is necessary to compare the clinical characteristics of addicts from areas of high and low morbidity.

Finally a major problem in ecological research has been the attribution of social characteristics of the environment (which are found to be correlated with morbidity) to the individual with the disorder (the "ecological fallacy"). It is therefore necessary to determine whether such social characteristics are also more frequent among the individuals with the disorder being studied.

The research outlined in this paper attempts to reduce these meth-

odological problems, differentiates prevalence and incidence, standardizes morbidity rates by demographic characteristics; assesses areal variation in ascertainment; recognizes clinical variations and considers the social characteristics of individual narcotic addicts, as well as, their environment.

SETTING

The study population is derived from the experience of the M.J. Bernstein Institute of the Beth Israel Medical Center, New York, which has had a large scale treatment program for narcotic addiction since 1961. The M.J. Bernstein Institute (MJBI) has had minimum screening for suitability or selection of patients by psychological, social or geographic considerations. Although located in Manhattan's Lower East Side, over 45,000 individuals have been admitted from all parts of New York's five boroughs, and involved in a variety of treatment approaches including detoxification, methadone maintenance and therapeutic communities.

The numerical relation of MJBI patients to those reported to the Narcotics Register from other sources in New York City has been considered in a collaborative study (Richman et al., 1971). Subsequent reports were analyzed for a random sample of 155 persons first reported to the Narcotics Register in 1967. Fifty-five percent of this sample has reports *only* from law enforcement sources up to the end of 1968; the criteria used by arresting officers in defining an addict are not necessarily medical or clinical criteria. By the end of 1968, 25 percent of the original sample had been admitted to MJBI; and among the persons who had been reported from sources other than law enforcement, 61 percent had been in contact with MJBI. Contact with MJBI was highest among non-white addicts reported to the Narcotics Register.

The extent of MJBI contact with New York City heroin addicts is also shown by the proportion of former MJBI patients found among narcotic-related deaths reported by the City Medical Examiner during 1966, 1968 and 1970 (Jackson and Richman, 1971). Although the number of deaths increased from over 300 in 1966 to over 600 in 1968 and over 1,200 in 1970, former patients of the Bernstein Institute made up a similar proportion -- 18-20 percent of the total deaths reported in each of these years. Among the addicts who died in New York City during 1970 who had been reported to the Narcotics Register before death, one-half had been previously hospitalized at MJBI.

FIGURE 2

BERNSTEIN INSTITUTE CONTACT AMONG SAMPLE OF 155 NARCOTIC USERS FIRST REPORTED TO NARCOTIC REGISTER, 1967

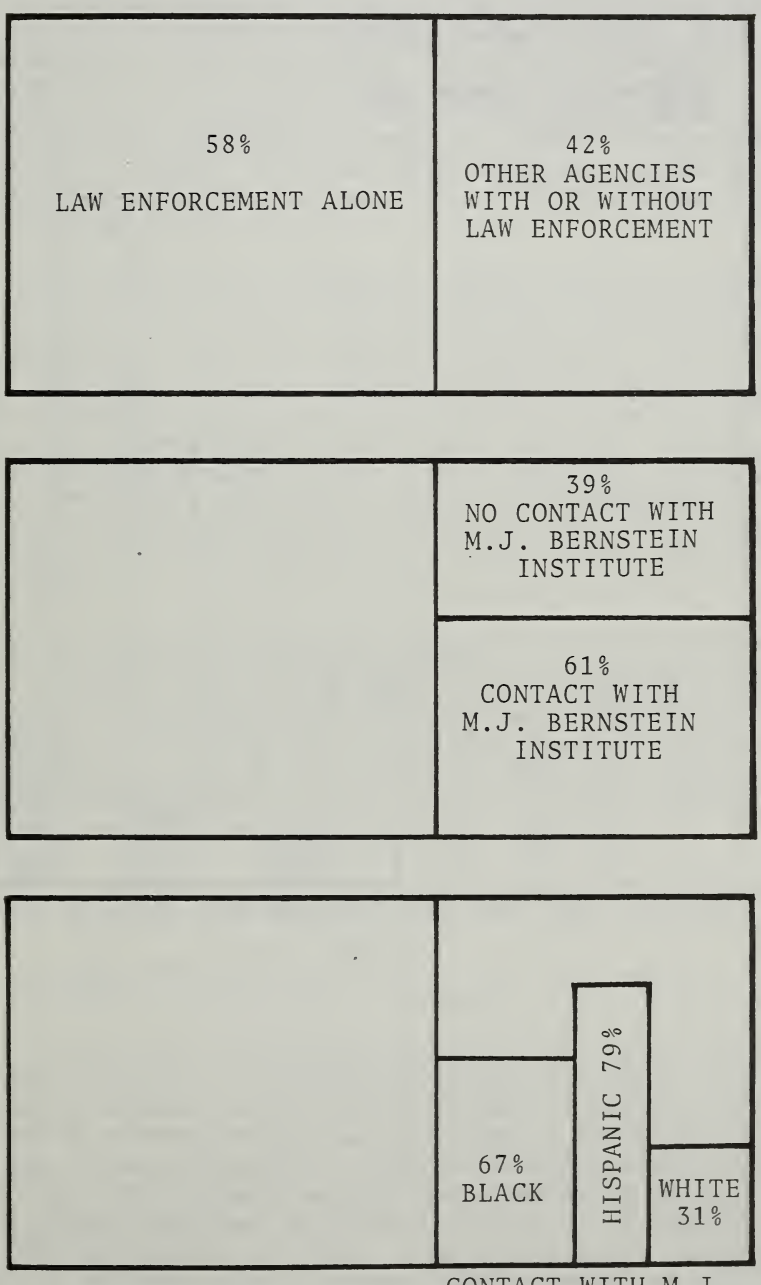
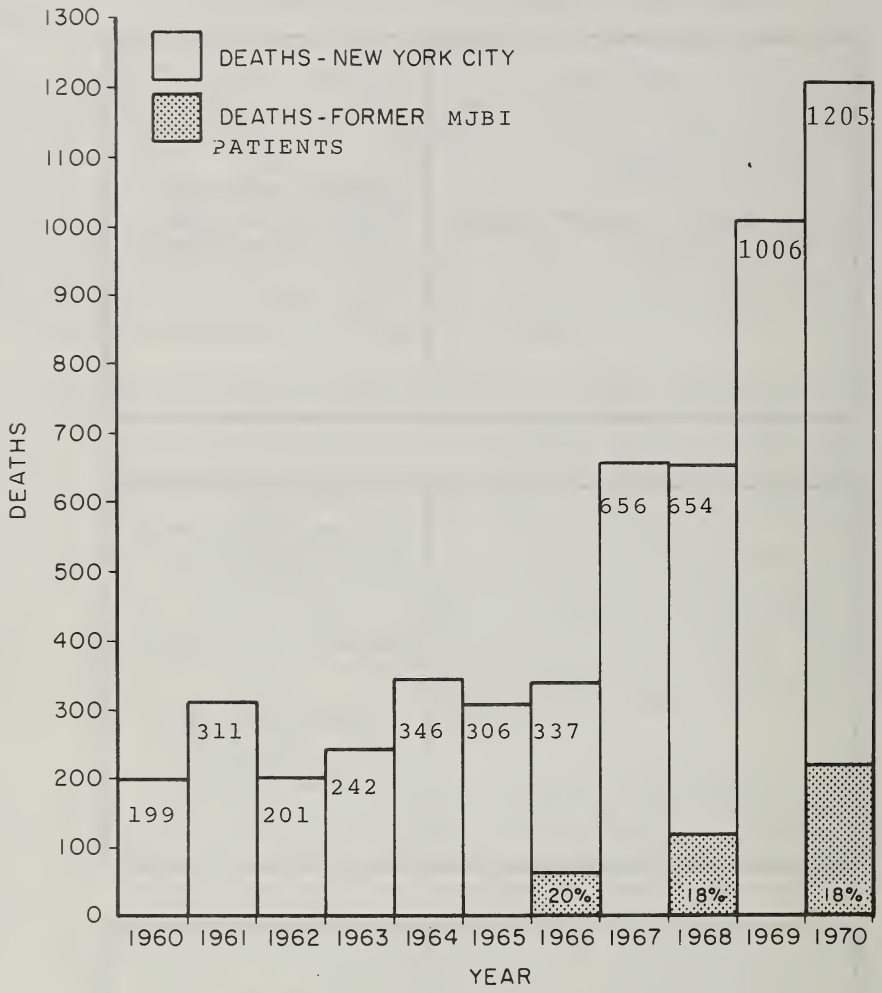


FIGURE 3

NARCOTICS-RELATED DEATHS, NEW YORK CITY, 1960-1970, AND DEATHS AMONG FORMER MJBI PATIENTS FOR 1966, 1968 AND 1970.



Although this research is based on New York City narcotic addicts who have been defined by contact with a single treatment facility, this source represents a numerically significant portion of the known population of narcotic addicts -- either in terms of drug abusers reported to the Narcotics Register from sources other than law enforcement, or in relation to those persons whose deaths have been associated with narcotic use. It is not assumed that the findings from this group of MJBI patients can be extended to *all* heroin users; on the other hand, epidemiologic perspectives of

this clinically defined group can provide essential information on a large segment of the known population of narcotic addicts.

METHOD

Considerable effort has been directed to developing an integrated data system for enhancing the scope and quality of epidemiologic information from patients (Richman and Moore, 1971). Since 1969, an adaptation of the 1970 Federal Census questionnaire has been completed by all persons entering the MJBI Admissions Unit. This includes persons applying for detoxification or entering the Methadone Maintenance Treatment Program, referred to a Phoenix House Therapeutic Community Induction Unit, or those referred from the community for assessment of addiction status by the New York City Department of Social Services.

The Census Questionnaire wording is derived from the 1970 Federal Census, and is self-administered, as was the Federal Census. This procedure has worked well, with few refusals and little difficulty in completion of the questionnaire. The items are shown on Figure III. Residence, a crucial item for social area analysis, was verified during the early years of the study by letters mailed (Figure IV) to the address given by the patient. Use of a self-administered Census Questionnaire in such a way is feasible, provides detailed demographic data which is specifically intended for epidemiologic research, and allows us to consider the characteristics of those who applied for detoxification, but were not later admitted (Figure V).

Patients were required to substantiate their identification with documents such as birth certificate, draft cards, Medicaid cards, etc., so that a single chart could be maintained for the various contacts or readmissions of a patient. These Census Questionnaires were collated for individual heroin users seen during the six months spanning April 1, 1970 (Federal Census Day) for individuals who had given an address within the Lower East Side Health District (Figure V). The study population comprised:

1. 350 admissions to MJBI treatment programs (detoxification and methadone maintenance)

2. 312 applicants for detoxification not later admitted for assessment of their addiction status

3. 70 heroin users referred by Department of Social Service for assessment of their addiction status.

After cross-tabulating age, sex, color and health area of residence for the 732 individuals, we calculated the age-sex-color specific ratios of addicts/population for the entire Health District. These Health District age-sex-color specific ratios were then applied to the 1970 Census for each of the 13 health areas to determine the number of patients expected if the health area had the same fre-

FIGURE 4

BETH ISRAEL MEDICAL CENTER
MORRIS J. BERNSTEIN INSTITUTE

CENSUS INFORMATION

6 CONSECUTIVE NUMBER 12

SERVICE 3 5 DAS MMP MMD MED DSS PEC PSY SPL IDO INC (DEF) BIH GHSP

PLEASE ANSWER ALL QUESTIONS. DO NOT HURRY. REPLY TO EVERY QUESTION. PLEASE PRINT.

(1) DATE: 13 MO. DAY. YR. 18

(2) NAME: 19 LAST FIRST 35

(3) TELEPHONE: 36 42

(4) SEX: 44 ☐ MALE ☐ FEMALE FILL IN ONE SQUARE.

(5) COLOR OR RACE 45 WHITE W ☐ AMER. INDIAN A ☐ JAPANESE J ☐ OTHER O ☐
NEGRO N ☐ CHINESE C ☐ HAWAIIAN H ☐ (SPECIFY)_____

(6) AGE: LAST BIRTHDAY 46 47

DATE OF BIRTH: 48 MO. DAY YR. 53

(7) MARITAL STATUS: 54 M = NOW MARRIED ☐ D = DIVORCED ☐
S = NEVER MARRIED ☐ W = WIDOWED ☐
X = SEPARATED ☐ R = REMARRIED ☐

(8) RELIGION: PROTESTANT ☐ CATHOLIC ☐ JEWISH ☐
OTHER ☐ _____ SPECIFY

55 COUNTY 58 HEALTH AREA DISTRICT 61

(9) YOUR PRESENT ADDRESS: _____
NUMBER STREET APT. NUMBER BOROUGH STATE ZIP CODE

(10) WHEN DID YOU MOVE TO YOUR CURRENT ADDRESS?_____

(11) WITH WHOM DO YOU LIVE? LIVE ALONE ☐ WITH MOTHER ☐ WITH CHILDREN ☐ OTHER ☐ INSTITUTIONAL ☐
MARK ALL WHICH APPLY WITH SPOUSE ☐ WITH FATHER ☐ WITH BROTHERS OR SISTERS ☐ _____ SPECIFY

(12) WHERE WERE YOU BORN? U.S.A. ☐ YES IF YES NAME STATE _____
☐ NO IF NO NAME THE COUNTRY _____

(15) WHAT WAS YOUR MOTHER'S NAME: _____ FIRST _____ MAIDEN

(13) IN WHAT COUNTRY WAS YOUR FATHER BORN?_____

(16) WHERE DID YOU LIVE ON APRIL 1965?
☐ U.S.A. STATE_____
☐ OTHER _____ NAME OF COUNTRY

(14) IN WHAT COUNTRY WAS YOUR MOTHER BORN?_____

(17) WHAT IS THE HIGHEST GRADE OF SCHOOL YOU EVER ATTENDED?
NURSERY SCHOOL ☐
KINDERGARTEN ☐
NEVER ATTENDED SCHOOL ☐
1 2 3 4 5 6 7 8 9 10 11 12
COLLEGE ☐

HOW MANY YEARS HAVE YOU LIVED IN NEW YORK CITY?_____

(18) HAVE YOU ATTENDED REGULAR SCHOOL SINCE FEBRUARY 1969? ☐ YES IF YES: PUBLIC SCHOOL ☐
☐ NO PAROCHIAL SCHOOL ☐ OTHER ☐

(19) WHAT IS THE NAME OF THE LAST SCHOOL YOU ATTENDED?
_____

(20) WHAT LANGUAGE OTHER THAN ENGLISH WAS USUALLY SPOKEN AT HOME WHEN YOU WERE A CHILD?
ENGLISH ONLY ☐ SPANISH ☐ FRENCH ☐ OTHER ☐ _____ SPECIFY

(21) HAVE YOU EVER SERVED IN THE U.S. ARMY, NAVY OR ARMED FORCES: YES ☐ VIETNAM ☐ KOREA ☐
NO ☐ W.W. II ☐ W.W. I ☐ OTHER ☐ _____ SPECIFY

(22) MEDICAID NUMBER: _____

(23) WHERE ELSE HAVE YOU APPLIED FOR TREATMENT FOR ADDICTION: _____ WHEN? _____

(24) WHERE HAVE YOU BEEN TREATED FOR DRUG ADDICTION? LEXINGTON ☐ BERNSTEIN INSTITUTE ☐ PHOENIX HOUSE ☐
NOWHERE ☐ STATE HOSPITAL ☐ INTERFAITH ☐ RIVERSIDE ☐ METHADONE PROGRAM ☐ NACC ☐ (STATE PROGRAM)
METROPOLITAN ☐ OTHER ☐ _____ SPECIFY

(25) DID YOU DO ORDINARY WORK FOR PAY AT ANY TIME LAST WEEK? YES ☐ NO ☐

(26) WHEN DID YOU LAST WORK, EVEN FOR A FEW DAYS? _____ YEAR WHERE _____

(27) TO WHAT MAILING ADDRESS CAN LETTERS BE SENT TO YOU ABOUT COMING IN EARLIER?
_____
IN CARE OF NUMBER STREET APT. NO. BOROUGH TELEPHONE NUMBER

YOUR SIGNATURE _____ THANK YOU

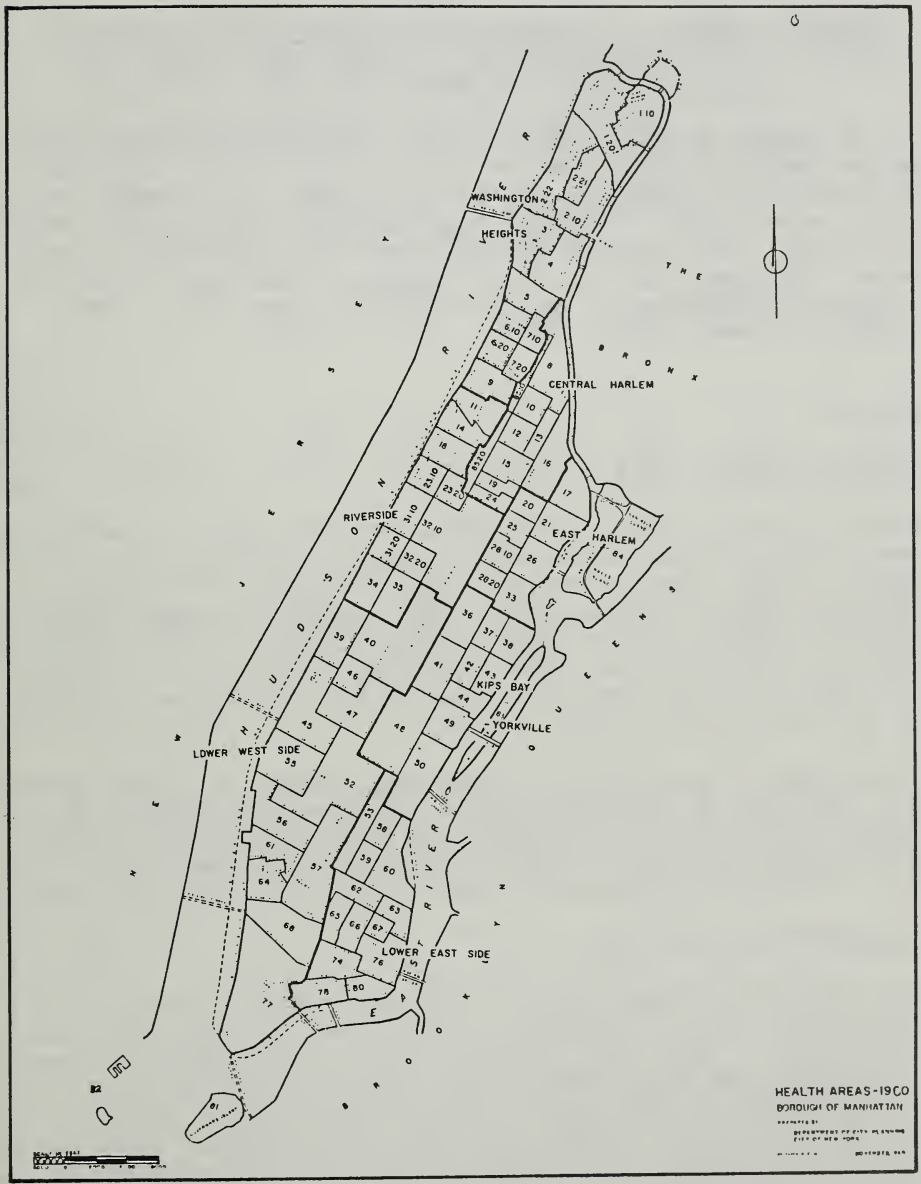
62 UNIT NUMBER 67 68 SOCIAL SECURITY NUMBER 76

G.A.D. _____ GIVEN BY _____ SOUNDEX _____
INTERVIEWER

YEAR FIRST ADMITTED _____ YEAR LAST ADMITTED _____ PRIORITY LAST DISCH. _____
NUMBER OF DISCHARGES ____ NUMBER OF AMA'S _____ NO. OF DISCHARGES _____

DRAFT 11/25/70
BIMC 3-76 (OLD #217)
ABG 65420

FIGURE 5


WASHINGTON HEIGHTS
CENTRAL HARLEM
RIVERSIDE
EAST HARLEM
KIPS BAY
YORKVILLE
LOWER WEST SIDE
LOWER EAST SIDE
THE BRONX
HARLEM RIVER
HUDSON RIVER
EAST RIVER
NEW YORK BAY
HEALTH AREAS-1960
BOROUGH OF MANHATTAN

quency of addiction as the Health District.

Standardized Treated Addiction Ratios for each health area were calculated from the ratio:

$$\frac{\text{Number of addicts with addresses in the health area (observed)}}{\text{Number of addicts (expected) if the health area had the same rates as the Health District}}$$

If the number of heroin addicts from a health area equalled the expected number computed from the Health District rates, the Standardized Treated Addiction Ratio would be 100. The Standardized ratio would be 200 if a health area had double the expected number of addicts. The Standardized Addiction Ratios calculated in this manner represent total treatment prevalence since they include patients with addiction of any duration, first applicants, admissions and readmissions to MJBI.

RESULTS

Population Based Rates of Contact

Based on the 1970 Census, the population based rates of contact were calculated for the six month interval centering around April 1, 1970. These rates were higher for blacks than whites, men than women and decreased with age. Among those aged 15-24, about two percent of the men and one-half percent of the women were in contact with our agency during a six month period. The six month rates of contact are used for the social area correlates to be described later in this paper.

Demographic differentials are shown more clearly in a later analysis. During a 25 month time period, there were about three thousand individual narcotic addicts from the Lower East Side Health District; rates of contact for the age group 15-24 amounted to 5.7 percent of the male whites and 11.9 percent of the male blacks.

Geographic Differentials

The Standardized Treated Addiction Ratios within the thirteen Health Areas for the six month period ranged from two percent to 206 percent. (Over the 25 month period, the differential between the highest and lowest Health Areas was less marked -- about a 25 fold difference). Geographic differentials persisted after standardization for age, sex and ethnicity. Within the six month study period, opportunity for transiency of patients and selective mobility of the denominator was minimized and cannot account for the wide geographic differential.

TABLE 1

M.J. BERNSTEIN INSTITUTE
BETH ISRAEL MEDICAL CENTER, NEW YORK
CONTACT WITH INDIVIDUAL NARCOTIC ADDICTS FROM LOWER EAST SIDE HEALTH DISTRICT, MANHATTAN
TOTAL TREATED PREVALENCE, JUNE 1, 1970 - JUNE 30, 1972

RATE PER 100 POPULATION (1970 CENSUS) BY SEX, AGE GROUP AND COLOR

	MALE			FEMALE		
	15-24	25-34	35-44	15-24	25-34	35-44
WHITE						
Patients	860	572	134	323	132	26
Census Population	15,212	16,165	11,891	17,619	16,311	11,185
Ratio	5.7%	3.5%	1.1%	1.8%	0.8%	0.2%
BLACK						
Patients	282	295	95	102	60	23
Census Population	2,378	2,270	1,482	2,541	1,921	1,520
Ratio	11.9%	13.2%	6.4%	4.0%	3.1%	1.5%

Ecological Correlates

The correlation of the six-month Standardized Treated Addiction Ratio with various socio-economic characteristics was calculated for the 13 Health Areas. The socio-economic characteristics were those derived from the 1970 Federal Census by the NIMH Mental Health Demographic Profile System for U.S. catchment areas (Goldsmith and Unger). Pearson product moment correlations were calculated for 103 socio-economic characteristics and found to be significant at less than the 0.01 level for 29 characteristics, between 0.01 and 0.05 for another 28 characteristics, and over 0.05 for 46 characteristics. Further multi-variate analyses are required since many of the social characteristics are inter-related, e.g., the highest correlations were found for female headed households with own children; the next for children living with both parents; then, working mothers with children; and the fourth highest correlation for female headed families with children in poverty.

DISCUSSION

The prevalence of treated addiction has been shown to be significantly related to a variety of socio-economic characteristics for the 13 Health Areas within the Lower East Side Health District,

CORRELATIONS FOR SELECTED CHARACTERISTICS
WITH SIX - MONTH STANDARDIZED TREATED ADDICTION RATIOS

CHARACTERISTIC (Number refers to order in Unger and Goldsmith, Table 8)	RANGE WITHIN 13 HEALTH AREAS	PEARSON PRODUCT MOMENT CORRELATION	
		F	Correlation Coefficient
(77) Female headed households with own children	9-37%	90.3	.94 **
(108) Poverty children	2-49%	51.9	.91 **
(16) Labor force unemployed	2-12%	18.7	.79 **
(104) Large families with low income	-70%	16.2	.77 **
(25) Low occupational status, white male	9-47%	13.9	.75 **
(107) Disabled population unable to work	2-8%	13.7	.74 **
(50) Sex ratio-males per 100 females (in households)	68-111	10.0	.69 **
(14) Rent-non-Negro	$51-$176	9.8	.69 **
(39) High-school completion, white	32-76%	8.9	.67 *
(95) Teen-agers not in school	8-22%	6.9	.62 *
(81) Overcrowded housing	4-22%	4.3	.53
(92) Recent movers, Negro	4-58%	3.6	.50
(87) Large apartment structures	47-98%	2.8	.45
(68) Rooming house and related population	... -10%	1.2	.31
(94) Migrants (different county than in 1965)	8-37%	.5	.22
(93) Mobile persons (different house than in 1965)	27-58%	.2	.13

... less than 1%
** $p < 0.01$
* $0.01 < P < 0.05$

Manhattan. As emphasized previously, this analysis includes both first and readmissions with any duration of addiction.

Further analyses in this research project will consider:

1. Incidence within a circumscribed time period; and relation of the time of onset to residence within the study area. This focus upon time and place of onset is crucial for assessing the relations of onset and spread of narcotic addiction with the characteristics of the social environment.

2. Standardization for ethnicity. Further standardization of rates will consider Hispanics, who form a significant portion of the Lower East Side population.

3. Ascertainment. This research is based upon persons coming to a specific clinical facility. Although the relation of this source of ascertainment to other agencies has been assessed, it is necessary to determine to what extent areal differentials result from variation in the interval between onset and contact, whether contact with clinical facilities varies from place to place. Such differentials in duration of disorder preceding first contact can be assessed from the distribution of ascertainment intervals (Richman, 1974, 1975).

4. Clinical characteristics. Psychological symptoms, multiple drug abuse and demographic characteristics are inter-correlated (Richman, 1975). The characteristics and concomitants of various clinical types of narcotic addiction must be considered in assessing social factors in onset and spread.

5. Individual significance of ecological correlations. Since many of the socio-economic characteristics being studied are known for the individual, it will be possible to consider whether environmental characteristics related to morbidity are also significant in the individual patient. To determine the generalizability of social correlation to a variety of regions, the expanded project includes four other geographic areas in Manhattan and Queens.

6. Spread. A crucial question in the epidemiology of narcotic addiction is the definition and assessment of spread and delineation of the social factors correlated with diffusion of the disorder from place to place and person to person, as well as the individual factors involved with transmission and perpetuation of narcotic addiction.

SUMMARY

Narcotic addiction is concentrated in certain places and among particular social groups. Heretofore it has not been possible to assess which environmental factors are critical for the initiation of narcotic use, its transition to narcotic addiction; and subsequently, entry into treatment or whether there are environments which select those particularly prone to social deviancy while others migrate out. There are few systematic descriptions of the social and environmental distribution of narcotic addicts. This paper describes the initial results of assessing the characteristics of the social environment (1970 Federal Census) of 3,000 individual narcotic addicts from the Lower East Side of Manhattan who were treated at the Beth Israel Medical Center.

Pearson correlations between treated addiction ratios and 1970 Census characteristics were calculated for 13 health areas. Out of 103 socio-economic characteristics from the NIMH Mental Health Demographic Profile System, 29 were statistically significant at $p < 0.01$ and 28 at $0.01 < p < 0.05$. Highly significant correlations were found for households with children, female headed ($r = .94$, $F = 90.3$) and population in poverty ($r = .89$, $F = 41.7$). Correlations with density (over 1.01 persons per room) or population mobility (residing in different house five years ago) were not statistically significant.

This paper has outlined some of the methodological and theoretical problems of ecological studies of narcotic addiction, and emphasized the need for concentration on the socio-dynamics of diffusion. Methodological problems include consideration of the time and place of onset; demographic standardization; ascertainment differentials; clinical characterization; and assessment within individuals of ecological correlations.

AUTHOR

Alex Richman is Professor of Psychiatry, Department of Psychiatry, Beth Israel Medical Center and Mount Sinai School of Medicine, New York, N.Y. The assistance of T. Dubrow, N. Beauchamp Palmer, E. Gonzalez, P. Zappel, and M. Dunlap is acknowledged. The cooperation of B. Rosen, Biometry Branch, Office Program Planning and Evaluation, NIMH in supplying the Small Area Profile data is very much appreciated. This research is supported by Research Grant Number 00666-02 from the National Institute on Drug Abuse.

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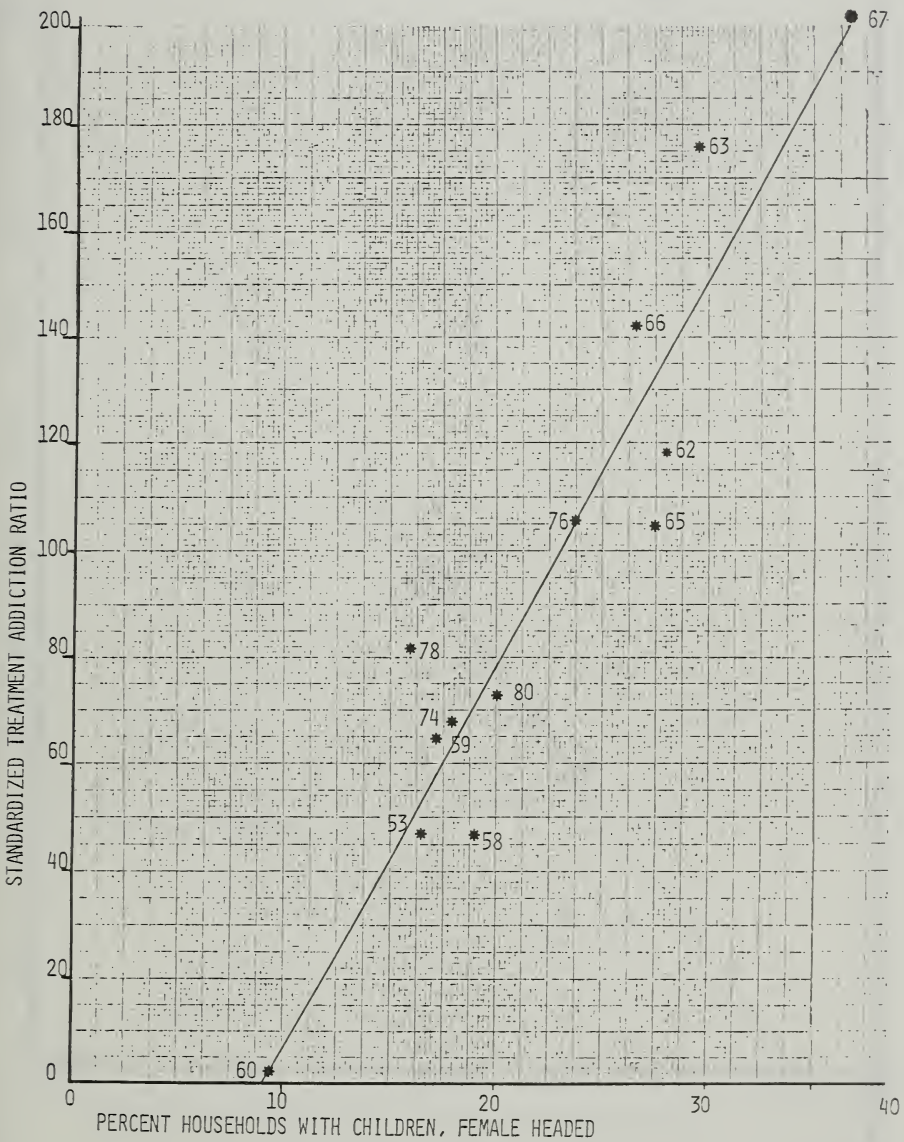
FIGURE 6


STANDARDIZED TREATMENT ADDICTION RATIO
200
180
160
140
120
100
80
60
40
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20
30
40
PERCENT HOUSEHOLDS WITH CHILDREN, FEMALE HEADED
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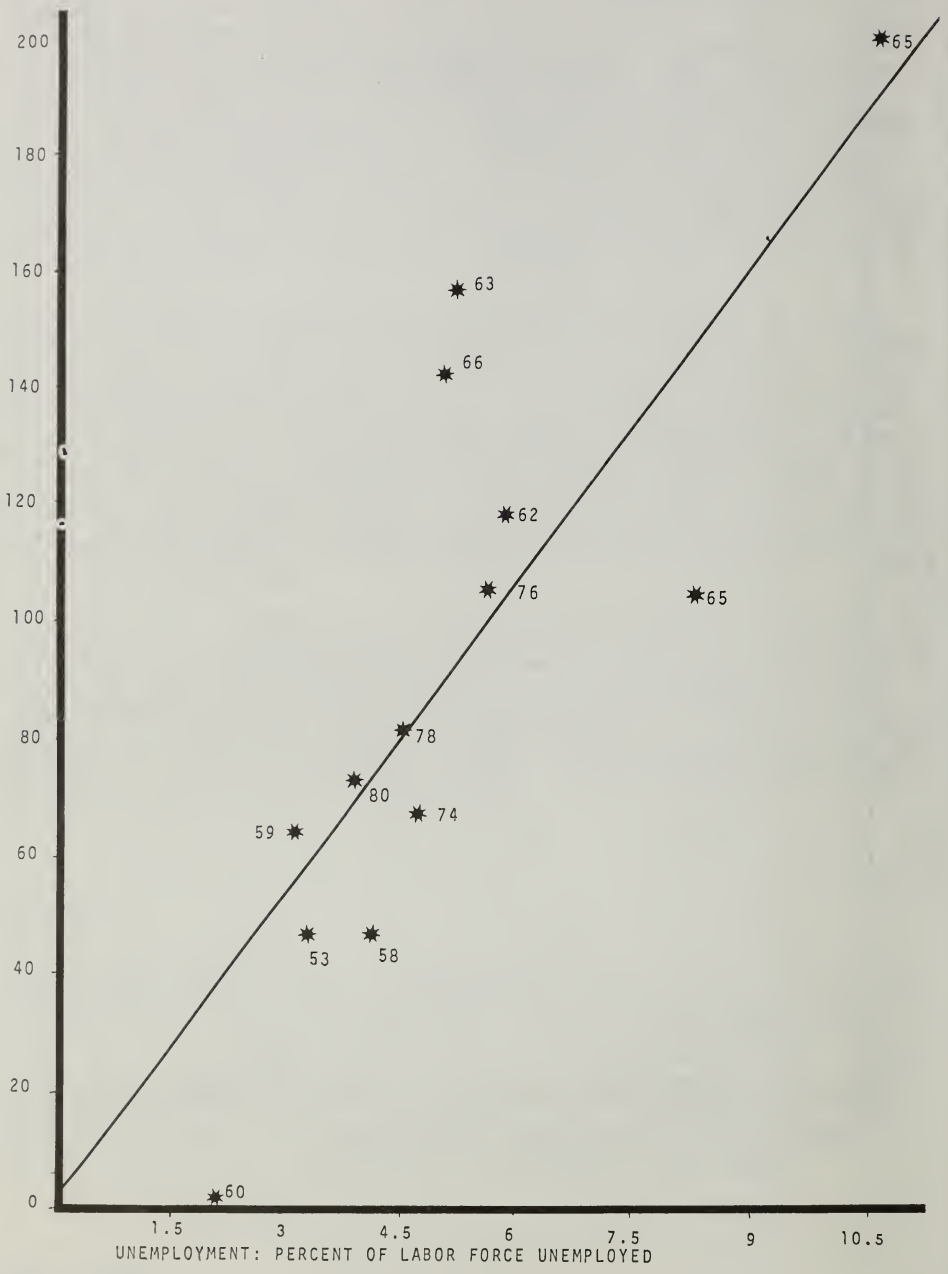
FIGURE 7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UNEMPLOYMENT: PERCENT OF LABOR FORCE UNEMPLOYED
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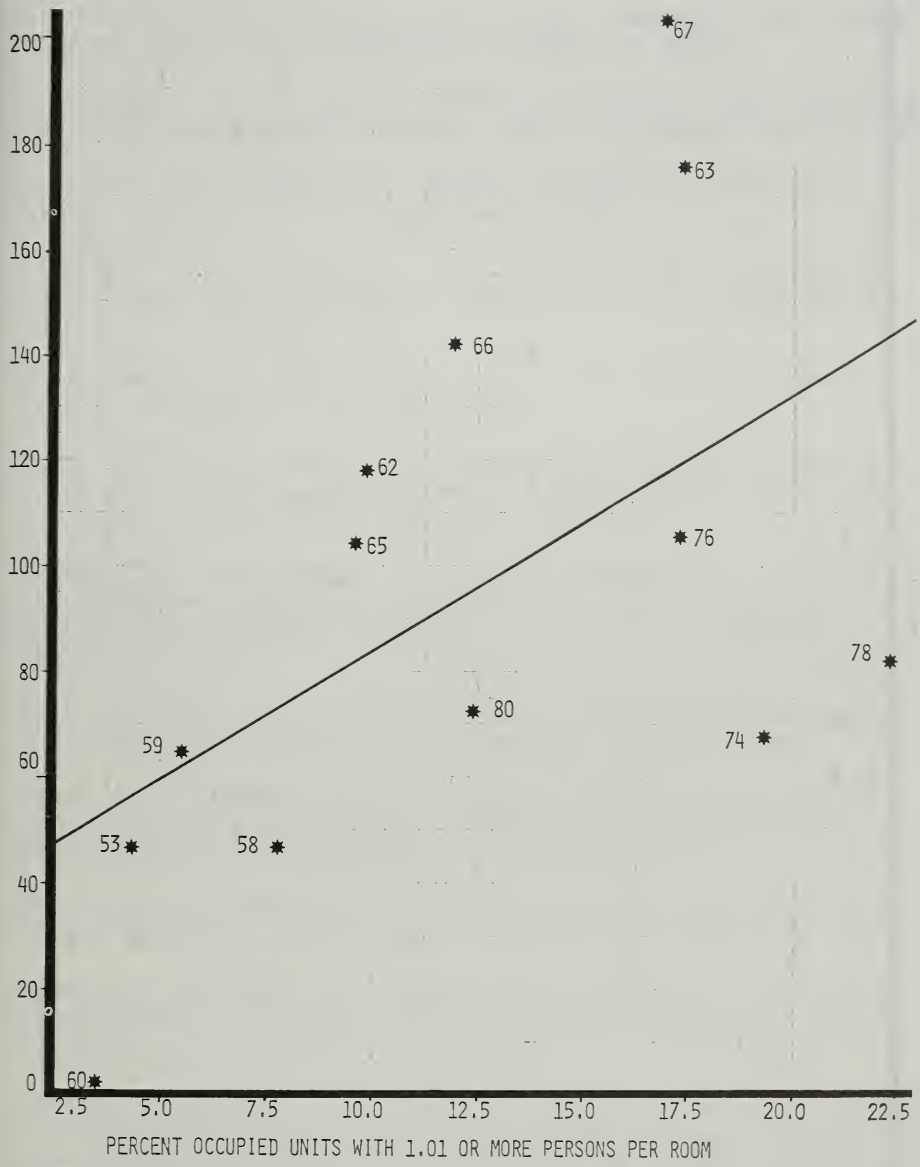
FIGURE 8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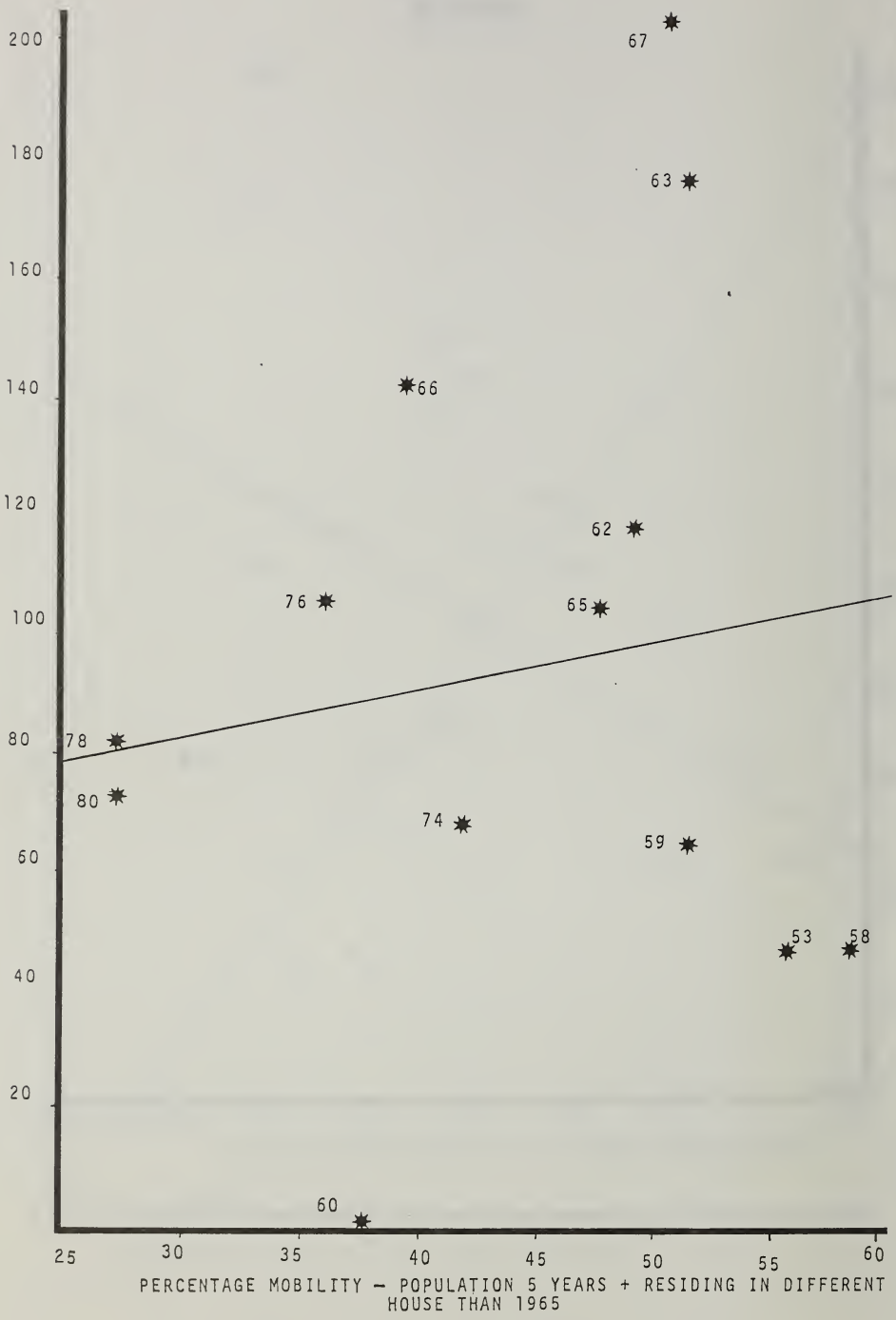
PERCENT OCCUPIED UNITS WITH 1.01 OR MORE PERSONS PER ROOM

FIGURE 9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45
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60
PERCENTAGE MOBILITY – POPULATION 5 YEARS + RESIDING IN DIFFERENT HOUSE THAN 1965

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Institutional Data—CODAP

C. James Sample, Ph.D.

The presentations under the topic of data systems have focused on the ability of the various recurring reporting systems to contribute information on the epidemiology of drug use or abuse. The Client Oriented Data Acquisition Process (CODAP) represents another, and possibly the most useful, of these various recurring reporting systems. This system, as revised in November 1974,* is a required reporting system for all drug abuse treatment units sponsored by NIDA, VA and BOP.** Reports are received monthly from approximately 1,800 treatment units. The coverage is extensive and the compliance in reporting is improving significantly because of pressure on the treatment units by the funding agencies.

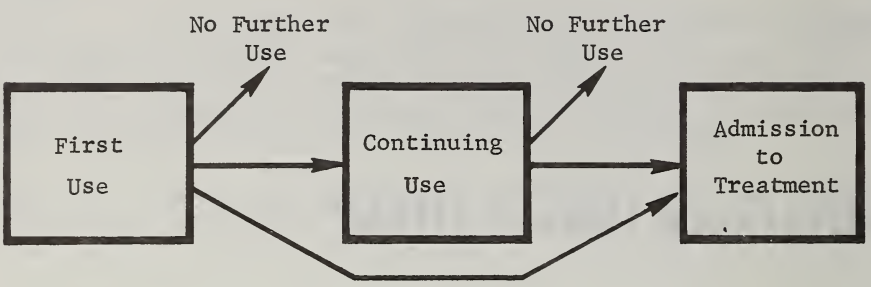
CODAP was not designed for extrapolation nor for the study of the incidence and prevalence of drug abuse. The system does, however, have the ability to offer some insights into these issues, principally by providing the information for onset analysis. It is not my intention to discuss the contribution of CODAP to onset analysis or any other particular approach. However, I will use this example to demonstrate the major weakness of epidemiological extrapolations from CODAP. Data generated at the treatment unit level is based on a particular population, namely clients who, for any reasons, find it necessary or advantageous to enter treatment for a drug problem. Consequently, utilizing CODAP information

*CODAP system was revised again in September 1976.

**The five input forms that comprise the system are attached.

for extrapolations to the drug using population requires that a number of assumptions be made.

FIGURE 1



The chain of events shown in Figure 1 demonstrate our major problem in this area. The expressed logical chain of events from first use of a drug, through continued use and finally to treatment provides the basis for the onset analysis. What is missed, of course, by gathering this information at the treatment level is the information on individuals who initiate drug use but never enter treatment. Like DAWN and DEA arrest data, CODAP requires an institutional capture point. Only survey methodologies, which have their own problems, avoid this particular problem.

Use of this example should not be interpreted as a criticism of onset analysis from this data. Quite to the contrary, onset analysis, as I am sure will be discussed by Dr. Mark Greene who is our next discussant, has provided extremely useful insights into the problem. My particular concern is to emphasize that data from any self-reporting, recurring data system is based on a very particular population. In this case this population consists of drug users who enter treatment.

The revised CODAP system was designed first as a drug abuse treatment management tool and secondly as a research tool. These two objectives are complementary as they should be. Here we are focusing on the research potentials of the system. The self-reporting, recurring nature of this system offers an alternative as well as an additional method to gather information. Once operational, the self-reporting nature of CODAP, as compared with survey methods, is relatively inexpensive to maintain and provides extremely current information. Reports are produced 45 days after each report month (i.e., reports on the treatment activity in January are published by March 15). This enables rapid identification of changes in factors effecting the treatment population. Because of the coverage of the system, sensitivity to drug abuse trends in small areas is possible. This is a factor so often stressed in analysis of drug use and abuse trends in the United States. We cannot generalize from one part of the country to another. CODAP's coverage is sufficiently extensive that these generalizations are not necessary.

These are some of the extremely useful points of the system. However, the problems mentioned by Dr. Sells yesterday with reference to the DARP reporting system are also present in CODAP. The same problems of reliability and consistency are always present in a self-reporting system. This leads to difficulty in interpretations of trends which is a major focus of this conference. It is compounded with the present reporting system by a number of factors, e.g., the fact that it is only a federal reporting system. The question then becomes what percent of the total universe is accounted for by the federal clinics -- and there does not seem to be a sound current estimate. Secondly, less than 100 percent of the reporting units are actually reporting, although as mentioned earlier, the reporting rate is improving. Our best estimate now is that we have increased reporting from about 60 percent of those expected to somewhere between 80 percent and 90 percent. The difficulty in obtaining a sound estimate of the percent of clinics actually reporting is a function of accurately establishing the denominator -- which has been a problem. We are still carrying information in our files on many programs such as LEAA and HUD Model Cities Programs which report infrequently and have not been pressured by their funding sources to report. So we have somewhat less than 100 percent of the expected. Thirdly, there is a discontinuity in reporting. A clinic that reports this month may not report next month. There has been some fluctuation. However, over the last three or four months, this has changed significantly because of NIDA's increased insistence upon reporting by their clinics and actually linking it to the funding criteria.

The fourth factor, and possibly the most disturbing influence affecting the ability to extrapolate from CODAP data, is the reliability of the data as a result of NIDA's insistence on treating particular drugs of abuse. NIDA has strongly communicated the policy decision that clinics are not in business to treat alcoholics, marihuana users and primary prevention clients. This policy decision is reasonable from NIDA's viewpoint. The result of this policy is that clients admitted to treatment that fall into one of these three classifications are under-reported and the relative magnitude of heroin, barbiturates, polydrug use, etc., is over-reported. The funding decisions are, in these cases, affecting the distribution of drugs reported and the extent of this bias is not known.

In order to make a reporting system effective and provide reliable and consistent data, a couple of things have to be present. First of all, I think the system has to reflect reality. The revised CODAP system does this. At least that is the reaction we have had from the treatment programs over the last two and a half months when we have been training clinic level personnel. Secondly, we have to develop, I think, a better feedback mechanism to the local programs. Unless we are able to provide useful data to the programs, the reliability of the data that we receive will be less than what we would like. The present rate of reports accounts for approximately 100,000 clients in treatment at any one time. So the volume of reports is substantial; in terms of numbers to extrapolate, there are certainly sufficient numbers there. I have attempt-

ed to cover the major problems in extrapolating CODAP data. The major issue of the hidden population of drug users (i.e., those that never enter treatment) cannot be solved by any self-reporting, recurring data system.

Most of you are familiar with the CODAP system as implemented in May of 1973. I urge you to become familiar with the new system. I am sure that it will have data that all of you can use and profit from. This is particularly important because the research and epidemiology data items have been expanded and refined. The reporting rate is increasing and our feedback loops are better, which should also increase the reliability and consistency of the data. However, as Dr. Sells said yesterday, there is not a speech for all occasions. This is also true with reporting systems.

AUTHOR

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NATIONAL INSTITUTE ON DRUG ABUSE

FORM APPROVED
OMB NO. 68 R1442

CODAP ACTIVITY REPORT

REPORT IDENTIFICATION

CARD 1

1. Clinic Identifier: [ ][ ][ ][ ][ ][ ][ ][ ] Col. 10-17

2. Report Month: Month [ ][ ] Year [ ][ ] Col. 18-21

3. Date Form Completed: Month [ ][ ] Day [ ][ ] Year [ ][ ] Col. 22-27

SERVICES PROVIDED – DURING REPORT MONTH

CARD 2

			NO. OF SERVICES SESSIONS CONDUCTED	Col.	NO. OF PERSONS RECEIVING SERVICES	Col.
4. Applicants During Intake:	a. Interviews			10-12		40-42
	b. Medical Examinations			13-15		43-45
5. Clients During Treatment:	a. Therapeutic Counseling:	Group		16-18		46-48
		Individual		19-21		49-51
		Family		22-24		52-54
	b. Physician Contacts			25-27		55-57
	c. Vocational and Job Development:	Group Training		28-30		58-60
		Individual Counseling		31-33		61-63
		Placements		34-36		64-66
6. Former Clients Post Treatment:	Follow-up Sessions			37-39		67-69

CLIENT ACTIVITY DURING MONTH FOR THOSE IN TREATMENT ON LAST DAY OF MONTH

CARD 3

			NUMBER OF CLIENTS	
7. Employed:	Part-Time			10-12
	Full-Time			13-15
8. In Education Programs				16-18
9. In Skill Development Programs				19-21
10. Urinalysis:	a. Total Clients Tested			22-24
	b. Total Number of Clients with Positive Urines			25-27
	c. Number of Clients with Positives For:	Non-RX Opiates		28-30
		Non-RX Stimulants		31-33
		Non-RX Depressants		34-36
11. Alcohol Abuse Problem				37-39
12. Total Clients Arrested and Charged				40-42

FUNDING SOURCE

13. Funding Sources Ordered By Greatest To Least Amount Of Funding Col.

1 [ ][ ] 43-44
2 [ ][ ] 45-46
3 [ ][ ] 47-48
4 [ ][ ] 49-50
5 [ ][ ] 51-52

FUNDING CODES:

10 - NIDA	60 - Other Federal
20 - BOP	70 - State
30 - VA	80 - Local
40 - LEAA	90 - Private
50 - HUD	

REMARKS

CARD 1

14. Coded:

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47
21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40
48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67

15. Written:

ADM 427-5 (Rev. 9-74)

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DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
ALCOHOL, DRUG ABUSE, AND MENTAL HEALTH ADMINISTRATION
NATIONAL INSTITUTE ON DRUG ABUSE

FORM APPROVED
OMB NO. 68-R1442

CODAP CLIENT PROGRESS REPORT

REPORT IDENTIFICATION

CARD 1

1. Clinic Identifier: [ ][ ][ ][ ][ ][ ][ ][ ] Col. 10-17

2. Client Number: [ ][ ][ ][ ][ ][ ][ ][ ][ ] Col. 18-27

3. Date Form Completed: Month [ ][ ] Day [ ][ ] Year [ ][ ] Col. 28-33

4. Reporting Quarter: Month [ ][ ] Day [ ][ ] Year [ ][ ] TO Month [ ][ ] Day [ ][ ] Year [ ][ ] Col. 34-45

CURRENT TREATMENT STATUS

5. Modality At End Of Quarter — Col. 46
   1 = Detoxification
   2 = Maintenance
   3 = Drug Free
   9 = Other *(Specify in Remarks)*

6. Environment At End Of Quarter — Col. 47
   1 = Prison
   2 = Hospital
   3 = Residential
   4 = Day Care
   5 = Outpatient

7. Medication(s) Prescribed At End Of Quarter — Col. 48-49, 50-51
   00 = None
   01 = Methadone
   02 = LAAM
   03 = Propoxyphene-N
   04 = Naloxone
   05 = Cyclazocine
   06 = Disulfiram
   07 = Other Antagonist
   99 = Other *(Specify in Remarks)*

8. Legal Status — Col. 52-53
   01 = Voluntary
   02 = NARA I
   03 = BOP-NARA II
   04 = NARA III
   05 = BOP IPDDR
   * 06 = BOP Study
   07 = BOP Probationer
   08 = Other BOP
   09 = Federal TASC
   10 = VA ASMRO
   20 = State Non-Voluntary
   30 = Local Non-Voluntary

* *Do not submit Progress Reports on Study Clients*

PROGRESS DURING QUARTER

MEASURE (CARD 2)			MONTHS IN QUARTER 1	Col.	2	Col.	3	Col.
9. Employment	a.	Part-time *(Yes/No)*		10		24		38
	b.	Full-time *(Yes/No)*		11		25		39
	c.	Total Gross Income	$	12-15	$	26-29	$	40-43
	d.	If Working, Employed in Skill Area *(Yes/No)*		16		30		44
10. In Education Program *(Yes/No)*				17		31		45
11. In Skill Development Program *(Yes/No)*				18		32		46
12. Urinalysis Positive For		Non-RX Opiates *(Yes/No)* *(If not tested, leave blank)*		19		33		47
		Non-RX Stimulants *(Yes/No)* *(If not tested, leave blank)*		20		34		48
		Non-RX Depressants *(Yes/No)* *(If not tested, leave blank)*		21		35		49
13. Alcohol Abuse Problem *(Yes/No)*				22		36		50
14. Arrested and Charged *(Yes/No)*				23		37		51

REMARKS

15. Coded — [1][2][3][4][5][6][7][8][9][10][11][12][13][14][15][16][17][18][19][20][21][22][23][24][25][26]

16. Written — [52][53][54][55][56][57][58][59][60][61][62][63][64][65][66][67][68][69][70][71][72][73][74][75][76][77]

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Rev. 9-74

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NATIONAL INSTITUTE ON DRUG ABUSE

FORM APPROVED
OMB NO. 68-R1442

CODAP CLIENT FLOW SUMMARY

CLINIC IDENTIFICATION

CARD 1

1. Clinic Identifier [ ] [ ] [ ] [ ] [ ] [ ] [ ] [ ] 10-17

Name of Clinic:

Address: *Number and Street*

City, State, Zip Code

Telephone: *Area Code and Number*

Name of Program

2. Program Identifier [ ] [ ] [ ] [ ] [ ] [ ] [ ] [ ] Col. 18-25

3. Date Form Completed — Month [ ] [ ] Day [ ] [ ] Year [ ] [ ] 26-31

4. Report Month — Month [ ] [ ] Year [ ] [ ] 32-35

5. Approved: (Signature) _____ (Title) _____

6. Check Box If Any Of The Information In Item 1 Is New Since Last Report [ ] 36

INTAKE SUMMARY

7. Admitted To Treatment at This Clinic [ ] [ ] [ ] 37-39
8. Referred To Treatment Within This Program + [ ] [ ] [ ] 40-42
9. Referred To Another Drug Treatment Program + [ ] [ ] [ ] 43-45
10. Rejected + [ ] [ ] [ ] 46-48
11. Placed On Waiting List This Month and Pending + [ ] [ ] [ ] 49-51
12. TOTAL APPLICANTS SCREENED THIS MONTH = [ ] [ ] [ ] [ ] 52-55

CLIENTS IN TREATMENT

13. Total Clients in Treatment on Last Day of Previous Month *(Item 18 on previous report)* [ ] [ ] [ ] [ ] 56-59
14. Clients Admitted During Report Month + *(Equals number of admission reports enclosed)* [ ] [ ] [ ] 60-62
15. Clients Admitted During Previous Months + *(Equals number of late admission reports enclosed)* [ ] [ ] [ ] 63-65
16. Clients Discharged During Report Month — *(Equals number of discharge reports enclosed)* [ ] [ ] [ ] 66-68
17. Clients Discharged During Previous Months — *(Equals number of late discharge reports enclosed)* [ ] [ ] [ ] 69-71
18. TOTAL CLIENTS IN TREATMENT ON LAST DAY OF REPORT MONTH = [ ] [ ] [ ] [ ] 72-75

19. CLIENT CENSUS – LAST DAY OF MONTH

CARD 2

ENVIRONMENT	MODALITY: DETOXIFICATION TOTAL CLIENTS	Col.	MAINTENANCE TOTAL CLIENTS	Col.	DRUG FREE TOTAL CLIENTS	Col.	OTHER TOTAL CLIENTS	Col.	GRAND TOTAL CLIENT CENSUS
Prison		10-12		25-27		40-42		55-57	
Hospital		13-15		28-30		43-45		58-60	
Residential		16-18		31-33		46-48		61-63	Col. 70-73
Day Care		19-21		34-36		49-51		64-66	
Outpatient		22-24		37-39		52-54		67-69	

CURRENT WAITING LIST

CARD 3

20. Total Number of Applicants on Active Waiting List on Last Day of Report Month [ ] [ ] [ ] 10-12

OTHER SUBMISSIONS

21. Number of BOP Client Progress Reports [ ] [ ] [ ] 13-15
22. Number of Corrected Copies of Previously Submitted Reports [ ] [ ] [ ] 16-18

REMARKS

23. Coded — 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26
24. Written — 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39 40 41 42 43 44

ADM 427-4 (Rev. 9-74)

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DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
ALCOHOL, DRUG ABUSE, AND MENTAL HEALTH ADMINISTRATION
NATIONAL INSTITUTE ON DRUG ABUSE

FORM APPROVED
OMB NO 68 R1442

CODAP ADMISSION REPORT

REPORT IDENTIFICATION — CARD 1

1. Clinic Identifier: (Col 10-17)
2. Date Form Completed: Month Day Year (Col. 18-23)

ADMISSION STATUS

3. Client Number: (24-33)
4. Date of Admission: Month Day Year (34-39)
5. Admission Type (40)
   1 = First Admission *(to any Clinic within Program)*
   2 = Readmission *(to any Clinic within Program)*
   3 = Transfer Admission *(from another CODAP Clinic within Program)*
   4 = Transfer Admission *(from a non-CODAP Clinic within Program)*
6. Modality Admitted To (41)
   1 = Detoxification; 2 = Maintenance; 3 = Drug Free; 9 = Other *(Specify in Remarks)*
7. Environment Admitted To (42)
   1 = Prison; 2 = Hospital; 3 = Residential; 4 = Day Care; 5 = Outpatient
8. Medication(s) Prescribed (43-44, 45-46)
   00 = None; 01 = Methadone; 02 = LAAM; 03 = Propoxyphene-N; 04 = Naloxone; 05 = Cyclazocine; 06 = Disulfiram; 07 = Other Antagonist; 99 = Other *(Specify in Remarks)*
9. Legal Status (47-48)
   01 = Voluntary; 02 = NARA I; 03 = BOP-NARA II; 04 = NARA III; 05 = BOP IPDDR; 06 = BOP Study; 07 = BOP Probationer; 08 = Other BOP; 09 = Federal TASC; 10 = VA ASMRO; 20 = State Non-Voluntary; 30 = Local Non-Voluntary

CLIENT CHARACTERISTICS

10. Sex (49) — 1 = Male; 2 = Female
11. Year Of Birth 19 (50-51)
12. Race Or Ethnic (52-53)
    01 = White; 02 = Black; 03 = American Indian; 04 = Japanese; 05 = Chinese; 06 = Other Asian; 07 = Puerto Rican; 08 = Mexican American; 09 = Cuban; 99 = Other *(Specify in Remarks)*
13. Employment Status (54)
    0 = Unemployed; 1 = Part-time *(less than 30 hours per week)*; 2 = Full-time *(30 or more hours per week)*

EDUCATION STATUS

14. Last Formal School Year Completed (00-20) (55-56)
15. Currently In Education Program (57) — 1 = Yes; 2 = No
16. Currently In Skill Development Program (58) — 1 = Yes; 2 = No

PRIOR TREATMENT

17. Number Of Prior Treatment Experiences *(in any drug treatment Program)* (59-60)
18. Months Since Last Treatment Experience (61-62)

DRUG PROBLEMS

CARD 2	PRIMARY PROBLEM	Col.	SECOND PROBLEM	Col	THIRD PROBLEM	Col
19. Drug Type(s)		10-11		19-20		28-29
20. Frequency of Use At Admission		12		21		30
21. Year of First Use	19	13-14	19	22-23	19	31-32
22. Year of First Continuing Use	19	15-16	19	24-25	19	33-34
23. Year of Last Continuing Use	19	17-18	19	26-27	19	35-36

24. Has Problem With More Than 3 Drugs (37) — 1 = Yes; 2 = No

DRUG TYPES

00 = None; 01 = Heroin; 02 = Illegal Methadone; 03 = Other Opiates & Synthetics *(with morphine-like effects)*; 04 = Alcohol Abuse; 05 = Barbiturates; 06 = Other Sedatives, Hypnotics or Tranquilizers; 07 = Amphetamines; 08 = Cocaine; 09 = Marijuana/Hashish; 10 = Hallucinogens; 11 = Inhalants; 12 = Over-the-counter; 99 = Other *(Specify in Remarks)*

FREQUENCY OF USE AT ADMISSION

0 = No present use; 1 = Less than once per month; 2 = Less than once per week; 3 = Once per week; 4 = Several times per week; 5 = Daily

REMARKS

25. Coded:
26. Written:

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31
38 39 40 41 42 43 44 45 46 47 48 49 50 51 52 53 54 55 56 57 58 59 60 61 62 63 64 65 66 67 68

ADM 427 1 (Formerly MH-427-1)
Rev 9-74

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DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
ALCOHOL, DRUG ABUSE, AND MENTAL HEALTH ADMINISTRATION
NATIONAL INSTITUTE ON DRUG ABUSE

FORM APPROVED
OMB NO 68 R1442

CODAP DISCHARGE REPORT

REPORT IDENTIFICATION

CARD 1

1. Clinic Identifier: [ ][ ][ ][ ][ ][ ][ ][ ] Col. 10-17

2. Date Form Completed: Month [ ][ ] Day [ ][ ] Year [ ][ ] Col. 18-23

DISCHARGE STATUS

3. Client Number: [ ][ ][ ][ ][ ][ ][ ][ ][ ][ ] 24-33

4. Date of Discharge: Month [ ][ ] Day [ ][ ] Year [ ][ ] 34-39

5. Date of Admission to This Clinic: Month [ ][ ] Day [ ][ ] Year [ ][ ] 40-45

6. Reason for Discharge [ ] 46
   1 = Completed Treatment
   2 = Transfer to another CODAP Clinic within Program
   3 = Transfer to a non-CODAP Clinic within Program
   4 = Referred outside Program
   5 = Program decision to discharge Client for non-compliance to Program rules
   6 = Client left before completing treatment
   7 = Incarcerated
   8 = Death

7. Modality at Time of Discharge [ ] 47
   1 = Detoxification
   2 = Maintenance
   3 = Drug Free
   9 = Other *(Specify in Remarks)*

8. Environment at Time of Discharge [ ] 48
   1 = Prison
   2 = Hospital
   3 = Residential
   4 = Day Care
   5 = Outpatient

9. Medication(s) Prescribed [ ][ ] 49-50 [ ][ ] 51-52
   00 = None
   01 = Methadone
   02 = LAAM
   03 = Propoxyphene-N
   04 = Naloxone
   05 = Cyclazocine
   06 = Disulfiram
   07 = Other Antagonist
   99 = Other *(Specify in Remarks)*

TIME IN TREATMENT

10. Number of Months and Weeks of Uninterrupted Treatment Prior to This Discharge *(In any and all Clinics in this Program)*
    Months [ ][ ] 53-54
    &
    Weeks [ ] 55

CLIENT CHARACTERISTICS

11. Sex [ ] 56
    1 = Male
    2 = Female

12. Year of Birth 19[ ][ ] 57-58

13. Race or Ethnic [ ][ ] 59-60
    01 = White
    02 = Black
    03 = American Indian
    04 = Japanese
    05 = Chinese
    06 = Other Asian
    07 = Puerto Rican
    08 = Mexican American
    09 = Cuban
    99 = Other *(Specify in Remarks)*

14. Employment Status at Time of Discharge [ ] 61
    0 = Unemployed
    1 = Part-time *(less than 30 hours per week)*
    2 = Full-time *(30 or more hours per week)*

EDUCATION STATUS AT TIME OF DISCHARGE

15. Last Formal School Year Completed [ ][ ] 62-63
    (00-20)

16. Currently in Education Program [ ] 64
    1 = Yes
    2 = No

17. Currently in Skill Development Program [ ] 65
    1 = Yes
    2 = No

DRUG USE STATUS AT TIME OF DISCHARGE

18. Identify Drugs Used, if any 66 [ ] 67 [ ] 68 [ ] 69 [ ] 70 [ ] 71 [ ]
    00 = None
    01 = Heroin
    02 = Illegal Methadone
    03 = Other Opiates & Synthetics *(with morphine-like effects)*
    04 = Alcohol Abuse
    05 = Barbiturates
    06 = Other Sedatives, Hypnotics or Tranquilizers
    07 = Amphetamines
    08 = Cocaine
    09 = Marijuana/ Hashish
    10 = Hallucinogens
    11 = Inhalants
    12 = Over-the-counter
    99 = Other *(Specify in Remarks)*

REMARKS

19. Coded

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35

20. Written

ADM 427-3
Rev. 9-74

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Applications of Indicator Data (Epidemics)

Mark H. Greene, M.D.

I have a topic that might strike many of you as bizzare, having heard the list of problems and difficulties in interpreting the data obtained from various sources of what we call indicator data. My topic implies that these data can, in fact, be used; many of you may be skeptical about that. I think, though, we are really not faced with the choice that Ira Cisin mentioned, that is, probability samples on one hand and bits and pieces that don't fit together on the other. My own personal feeling is, and I suppose it is not surprising since it is what I spend most of my time doing, that there is a middle ground and if judiciously applied, many of the bits and pieces do in fact fit together. And you can learn a great deal from trying to put the bits and pieces together if you are careful about how you collect the bits and pieces. Our experience began in the District of Columbia and has since expanded to field visit in 14 cities around the country. I would like to share with you, not so much the data, as some of the difficulties we encountered in trying to use the various sources of information.

Before I go into the specific indicators that we have tried to evaluate, all of which really have been discussed previously in yesterday's and today's sessions, I would like to mention just a few guidelines that we have used in this enterprise. In the first place, it has never been our claim, and I do not think we really feel, that our approach to the problem enables us to describe reality with absolute perfection. This is an attempt at trying to understand what is going on in the community; it is an attempt

to approximate reality as best we can. I think it is clear from the discussions that have preceded my comments, however, that there are many problems in using the various sources of data and this can only be regarded as a rough approximation of reality. But I think that, again as Ira Cisin mentioned, there may be more important information to gather than precise head counts. Rough approximations may in fact give you very valuable information for planning and intervention purposes. Our experience has been based on heroin use as a model, but I think with minor modifications the sources of data that have been used can be applied for non-parenteral and non-opiate drug use as well.

Our approach has been to try and use a set of indicators, rather than rely on any one, and my answer to Dr. Richard's seventh question "What is the best indicator of drug use?", is I do not think there is a best indicator. If you rely on only one, you are in very dangerous territory. It is difficult enough relying on a package of them, but I think when you put together a series of indicators, if there is consistency in the kind of data that you collect, you can feel more comfortable about the conclusions that you draw. Therefore we rely on a set of indicators and not just one. Our data have been collected using data as they are available at the local level. We have not undertaken special studies in an attempt to assess the problem.

I think mentioning the local level leads to another major point and that is, in my personal opinion, that there are enormous hazards involved in trying to make broad generalizations about trends at the national level based on the kinds of data sources that we have available right now. Dr. DuPont and I argue at great length about this sort of thing. My own personal feeling is the best we can do is to make a reasonable assessment of what is going on locally, at the community level; that is feasible. To then extrapolate from there to what is going on nationally is a very, very difficult process and I think vividly illustrated by what happened when people over-generalized from what was going on in the East Coast over the past couple of years, suggesting that it was going on everywhere when, in fact, it was not going on everywhere. It was a regional phenomenon. And here is one of the major problems I think in using CODAP for example, as a source of data. Take, for example, the year-of-first-use distribution, which I really believe properly handled can be a very powerful tool at the local level. To collect data from hundreds of programs around the country and then display the year-of-first-use distribution for the entire country and try to draw inferences about national trends in heroin use is really stretching the data inappropriately. I do not think it should be done. We focused at the micro level, at the community level, and not at the national level.

Finally -- and this may be difficult for those of you who are accustomed to at least trying to deal with more precise measures, with good sampling techniques and extrapolations from your sample to the general population -- our approach has been to look more at the relative trends in use rather than absolute trends. I think

the preoccupation with trying to find out whether there are 568,000 or 569,000 heroin users in the country is a waste of time. It does not give you information that is useful from a planning point of view; really what you need to know is something that is fairly crude -- is the problem getting better or worse and in what parts of the community is the problem getting better or worse and what segments of the population are at greatest risk? So we focus on relative trends and not absolute trends and try not to get dragged into the debate of how many users of "x" there are in the community today.

With that as sort of a background, we have looked at the indicators that have previously been discussed. Primarily these include drug-related deaths, drug-related hepatitis, law enforcement data of two types (property crimes and drug-related arrests), treatment data -- primarily year of first use with information on age, race, and sex of those admitted to treatment, data from emergency rooms where it has been feasible to obtain them, and information regarding buys and seizures.

That leads me to make one comment on the point that Lee Minichiello has made and Carl Chambers has mentioned too; that is, the availability of drugs is really a key issue in the studies that all of us are involved in. I think insufficient attention has been paid to that issue in our deliberations here today. My own personal experiences over the last couple of years have led me to believe that it may well be *the* major factor in determining use patterns; people back home will not be happy to hear me say that, but that is the way I really feel. The information that you can obtain from buys and seizures can be very helpful in assessing availability, particularly of heroin, but also of other drugs as well. John Newmeyer did not mention the source of information that most people have fairly ready access to in this regard and that is law enforcement buys and seizures which circumvent the need for exposing both the client and the clinic to the risk of making purchases in the illicit market. In fact, local, state and federal law enforcement agents are in the process of making undercover buys on a regular basis and these data can be obtained. The purity of heroin at the street level has to be carefully defined because where you are buying in the distribution system makes a very big difference as to what the purity and cost will be. However, I think it is a reliable indicator of how available the drug is. Changes in that purity have been associated with other indicators to suggest that heroin is more widely or less widely available. In our travels around the country, we have found, much to our distress, that many localities in fact do not bother to analyze for the purity of the drug, and that is a tremendous loss of data. They simply do a qualitative analysis to find out whether opiates are or are not present in the sample they have submitted and do not do a quantitative analysis to determine what the heroin purity is. This would be one of the recommendations I would make to whoever makes policy at this level; this is a very valuable source of information. Currently, state and local seizures and buys in general are not analyzed because the DEA does not have the laboratory facilities to

undertake that; some way ought to be explored to see how they might be more fully utilized.

Q: How often do you have to make buys to keep track of trends?

Dr. Greene: I don't know the answer to that, to be honest. Most of my experiences are from the District where buys are being made on a regular daily basis. There may be an average of anywhere from 30-50 buys per month in the District and we collect the data on a monthly basis. In fact, this is a lower number now than it used to be. It used to be in the range of 100-200 buys a month when the heroin market was wide open in the city. You get into problems in terms of the statistical significance of observed changes when you are dealing with relatively small samples there. So we usually end up aggregating the data on a quarterly basis to try and get around some of that. In general though, at the street level, the buys fall into a fairly narrow range, and when the range changes, it usually means that it is because the market structure has changed -- so that kind of variability has implications. The other law enforcement data we have looked at have been attempts to draw some connection between property crime and addiction in heroin use in the community. We have had a very difficult time making any sense out of this at all. I think this is so primarily because we have approached it in a crude fashion and the kind of association that probably does exist is much more complicated than we have been able to demonstrate. In fact, when we compare our incidence curves to the property crime curves in the cities we have visited, in some places it seems to fit, and in others it does not seem to fit at all. I think we are just not clever enough to understand exactly what the relationship is. I hope that Dr. Savitz's work offers the opportunity to elucidate the relationship a lot more clearly than we have been able to do so far.

On the other hand, drug-related arrests, particularly in our surveys -- i.e., heroin arrests -- have been a fairly valuable source of information. If you take into account local law enforcement policy, the changes in the numbers of arrests and in the demography of the arrestees have seemed to move in conjunction with what has been seen at other observation points in the system -- in treatment, among those who are dying, and among those who show up in emergency facilities. So in our experience, property crime data as they exist are not of much help, but heroin-related arrests are. You have to be careful when you are dealing with these kinds of data for all the reasons that Dr. Savitz mentioned and also, because what constitutes a drug arrest varies a lot from place to place. In some places all drugs are lumped together; in some places "narcotics arrests" so-called also include marihuana and in other places it does not; in some places it is "opiates," and includes cocaine in addition to heroin and the narcotics and in other places it does not. You have to be quite careful on how you collect these kinds of data.

We have had a lot of discussion about the hepatitis indicator. My own personal feeling is that, again, if you can get data in which

drug-related cases of antigen positive serum hepatitis are reported, you have a very valuable indicator of parenteral drug abuse. Most communities, however, do not collect data in this way. In fact, we are in the midst of very dramatic and rapidly changing developments in the hepatitis field, such that it is now possible to very specifically make the diagnosis of hepatitis B where it was not five years ago. Many physicians have never had any interest in the entity of hepatitis B. So that situation is changing rapidly. At the moment, most communities do not collect the data in the form that is usable for this purpose. It is a shame, but that is the way it is.

The emergency rooms, again, in a few instances have provided very valuable information; but repeatedly in places where we have evidence of wide spread of the drug, the people in the emergency rooms claim that they are not seeing any problems related to heroin. They are not seeing stupor or coma in young people, let along anything they diagnose specifically as heroin overdose. They are not using the drugs that are standards in the treatment of opiate overdose and in view of the evidence that we have that there has been opiate use in these communities at a fairly extensive level, these observations raise serious questions about what is going on in the health care systems in these communities. These cases must be occurring and they must be being called something else. This is another area which I think merits further exploration.

Q: What sort of hypothesis do you have about the problem of the non-appearance of the expected cases in emergency rooms?

Dr. Greene: There is a variety of explanations. I guess it could relate to the sophistication of the user; if the drug is tested by administering a small dose before taking the whole dose, you can avoid some of the problems. If the drug is not taken in conjunction with other drugs, you may not get into trouble. The quality of the heroin that is available in the community probably also has an effect. It is possible that people are using relatively low potency heroin and not getting into actual pharmacological overdose situations, although when the potency changes they can get into trouble, particularly people who do not have opiate tolerance.

Dr. Gottschalk: I think another reason is that friends do not like to bring those cases into the hospital. They will bring barbiturate cases, but they try to give them milk and other good things to cure them at home -- so they end up in the coroner's office.

Dr. Greene: The point that I was about to make is that this is a major social problem, I think, in that the policy of many emergency rooms around the country is to report people who are seen for drug-related episodes to the police. The local clients who might potentially use this source of health services are aware of this and, therefore, go to any lengths they can to avoid showing up there. We have not looked at enough places to be able to draw that conclusion on a hard and fast basis. On the anecdotal basis, it ap-

pears that that is the case.

In Pensacola, Florida, for example, which is one of the communities where this kind of policy does exist, we visited all three hospital emergency rooms and they claimed they had never seen a case; they have never used naloxone. The treatment program has treated hundreds of people over the past couple of years for heroin use, but the emergency room had never seen an overdose. There were deaths, too, that we were able to glean out of their medical examiners' records by going through and picking out the fairly characteristic ones.

Dr. Khalil: Isn't it almost impossible to get an overdose with heroin, to cause heroin death itself? When you consider that the package may contain about eight milligrams or so of heroin, it would require the person to be independently wealthy to afford to kill himself with heroin.

Dr. Greene: Yes, I think that relates to what I said before. Potency of what is available in the street certainly influences that and in times of higher availability, there is a greater degree of variability as to what the individual buys in the streets. The chances of encountering an unexpected high dose package of heroin is much greater at a time of good availability than at a time of restricted availability. Over the past year (1973), for example, in Washington, when the heroin purity in the street was running about two percent, and the average number of milligrams in the street level bag was perhaps three or four milligrams -- barely enough to get you and me high -- we have five heroin deaths during the entire year as opposed to 40 or more deaths in the years before that. This year the purity has gone up to four percent over the past six months and already we have 12 deaths through the first 10 months of the year, over twice what we had last year. I think potency clearly has a major imput.

Dr. Sells: How many communities do you monitor? Do you have a standard program or do you try to trouble shoot where you get information that your services or your attention are needed?

Dr. Greene: The only community that I personally have been involved with monitoring on a regular basis is the District of Columbia. The other 14 cities that we have visited have been one-shot situations, part of an effort to understand the utility of the various sources of data and also to assess some of the questions about whether heroin use was spreading into smaller communities. They are not being done regularly.

Dr. Sells: In effect, you are operating what might be called a demonstrative program.

Dr. Greene: Yes, I think so. That brings me to another sort of general point and that is, we are not talking about predicting problems. What we are talking about is monitoring a community in a way that allows you to detect problems at as early a stage as

possible and is feasible; prediction is another goal entirely. I think monitoring -- surveillance, if you want to use the epidemiologic term -- of drug use trends is possible on an on-going basis and the kinds of data sources we have used have been very sensitive to local changes in drug using behavior. We have written and published a good bit on how this has helped us understand the problem of methadone abuse in the city, and the problem of amphetamine abuse. All of these things show up in the kind of monitoring system that combines treatment data, emergency room data, overdose data, law enforcement information, and hepatitis data, if you can get it. I think a fundamental consideration is that you realize that each of these sources of information has very serious limitations and that you are on tentative ground all the way.

You have to use common sense, restraint and good judgment in how you interpret the data and not jump off the deep end in predicting what is going on. We usually wait anywhere from three to six months before taking any change in the indicators seriously. But at least with the heroin situation, they all move together. This is the sort of thing that is hard to swallow. As a scientist, it makes me very uncomfortable to be dealing with such soft sources of information. But it seems to work. We have been able to document very significant changes in use both up and down, with multiple indicators simultaneously, not only in our community, but in other communities. John Newmeyer's experience in San Francisco has been very similar. Let me make just one last comment and then throw it open to further questions.

It is my own personal feeling that the year-of-first-use distribution obtained from a sample of addicts, whether it is treatment based or law enforcement based, when properly used can be one of the most powerful tools in analyzing local trends and incidence. I have to double underline "when properly used." Alex Richman has written a great deal about the many problems involved in interpreting this kind of data. There are variations and confounding that can be introduced by a wide variety of variables, from the standard demographic shifts to changes in the use of other drugs and changes in the kinds of services that are available. At least in the District of Columbia, we have been able to take almost all of these into account and have gotten repeatedly the same kind of results. In our experience in the other communities in the 14 cities that we visited over the past six months, we have found that the variations observed in the year-of-first-use distribution almost always have an explanation. There appear to be very distinct problems at a micro level within each community, such that you can demonstrate, I think, separate spread within a white community and a black community and within different race and sex specific groups within a community as well. Residence specific incidence plays a role, so that when you have a year-of-first-use distribution that assumes an unusual configuration, there is almost always a good explanation and an explanation that says something important about what is going on in the community. But, of course, you do have to take into account the many possible confounding variables that can influence this sort of thing.

AUTHOR

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DISCUSSION

Dr. Newmeyer: We found it useful to take all the 1974 year-of-first-use data that came in the fall of 1974, look at it carefully demographically and so forth, and compare that with similar 1971 year-of-first-use data that we saw in the fall of '71. I think you get a true early warning system on trends.

Dr. Greene: This has been one area where there has been some confusion depending on how you slice the data. I think probably the most effective way to look at this is to use onset cohorts -- people who have begun use in the same year -- rather than admission cohorts. We originally began to do this and subsequently shifted because the admission cohort is a much more heterogeneous group of people. The people who were admitted in 1973 included people who had been using for 40 years as well as people who had begun six months ago. There can be very striking differences within this group. The other thing I did not mention was that in using year-of-first-use data, we tried to take into account one of the major problems in using that information from a treatment source -- that is, the problem previously referred to regarding lag between onset of use and entry into treatment. Only a relatively small proportion of any group of people who begin heroin use in a particular year will enter treatment during that same year. For example, if one looks at our admissions now for people who began use in 1974, one will find relatively few in number, unless something very unusual is happening. What we have tried to do is develop a correction factor to plug into the year-of-first-use distribution based on how onset cohorts have behaved in the past. I know you have done the same kind of thing -- that is, taking all people who began use at a certain point and seeing how long it takes them to come into treatment. We have done this for about a dozen programs that have been in existence long enough to give them enough data to make this kind of determination and we have developed a correction factor that has its own problems, but we are trying to take into account the factor that there is this delay of several years between beginning and entry into treatment that can affect year-of-first-use as an index.

Dr. Sells: The year-of-first-use is an interesting idea. I assume this is the first use of any illicit drug?

Dr. Greene: It is heroin particularly, not any drug; but it could be done for any drug.

Dr. Sells: In our files, we have both data items available but by inference. We infer it because we have date of birth and age at first use, and are therefore able to compute it. I am not sure the results would have been the same if we had asked them in what year they first used. Apart from this however, I want to make the observation that the onset statistic is probably one of the fairly large number of variables that are very actively inferred from age. If you have someone who has been using for 30 years, the chances are he is older than someone whose year of first use was this year. Generally speaking, it is possible to make much more use of that age as a variable than is commonly realized. I think that is important. When you are dealing with problems of this kind, you may run across data sets that would be helpful in which the specific item is not available but can be inferred on the basis of the correlate years.

Dr. Greene: Yes, we have used the trends in the age of first admission among the treatment clientele as one of the indicators. Given what we know about onset of use, you can infer what the trend should look like if incidence were high or low. In fact, that fits with the other data we have used. Another one is simply the proportion of people who are below a certain age -- below age 18 or below age 21 -- depending on the program admission criteria, of course. Large proportions of young people are more suggestive of a current problem.

Dr. Chambers: I have two questions. The first one deal with age at first use *vs.* age of first continuous use. What does that do to the data when you cut it both ways, instead of the first? My intuition would lead me to believe that age of first continuous use would be better, considering how some segments of the country go through experimentation stages.

The other question involves the concept of community. I and a number of people have gotten into trouble in the past projecting national trends from rather diverse data pools from around the country. The concept needs to be locally-defined. I am a little concerned about what local really means.

Dr. Greene: As far as your first question is concerned, we have not run our data by continuous use, so I cannot tell you how it looks. Clearly, time elapses between one and another. I think conceptually though, first use, at least as an incidence indicator, makes better sense from a logical point of view. In the first place, it avoids the problem of having to decide what continuous means. In the second place, to the extent that the communicable disease analog holds, we are really talking about exposure to the opportunity to take heroin, so that in terms of an incidence measure, what you really want to know is, when did that happen for the first time? Looking at the natural history of the disease is a different question. I think first continuous use data can give

you helpful information, but as an incidence indicator, I think year of first use makes more sense -- it is cleaner. The first time someone uses heroin is a fairly memorable occasion; you do not have to get into the problem of when were they strung out, when were they addicted (and what does addicted mean?), how many times do they have to use it for it to be continuous and that sort of thing. So it is just a cleaner piece of information, I think.

We have not really explored the various problems of how to define community. I think there is no question even within the smaller communities we have visited -- the cities in the size range of 50,000 or 150,000 -- that there are variations even within the community, from one neighborhood to another and from one ethnic community to another, that make it difficult to see the whole thing as a homogeneous unit. In fact, I think one of the major criticisms of the study that we have done in these 14 communities is that we have been too simple-minded regarding how we have defined a given metropolitan area. We have, for example, neglected the influence of nearby large cities that may give a city with relatively small population many of the characteristics of a community of much larger population density. I am not very experienced at this and it is clearly something that has to be looked at.

Dr. Richman: I am delighted to hear Dr. Greene describe or emphasize the need to look at onset cohorts rather than admission cohorts because it is quite fundamental to the whole concept of year-of-first-use. On the other hand, looking at a cohort of people who have come into treatment, in order to get some assessment of what is happening in the community, namely incidence, is not only a critical but fatal error. I think it can be underscored that what is being dealt with there is something that requires a term other than incidence. Jack Elinson points out quite appropriately that my standardized addiction ratio is really a standardized treatment addiction ratio. Fine, that makes a better acronym -- S.T.A.R. not S.A.R.! Similarly, year-of-first-use does not deal with incidence within the community. It might be called inferential hypothesis about what might be happening out there -- if that makes an acronym -- but it should not be called incidence, even for planners.

Dr. Greene: A lot depends on where you stand as to how you use these kinds of terms. When I talk about incidence strictly defined epidemiologically, I usually call it an incidence rate. However incidence in general tends to be used in a fairly loose fashion. When you are talking about cases per 100,000 per unit population over a given period of time, you are talking about an incidence rate and I have never claimed that our methods give you any insight into rates at all. We are talking about relative trends and a measure of onset. It is an indicator, it is not a direct measure.

Dr. Lettieri: I have one question and one commentary amplification. With some of the indicators Dr. Greene mentioned, particularly the

one of drug-related deaths, or overdose deaths, the use of potency to explain that phenomenon belies the issue. I do not think anyone will argue that pharmacologic overdoses can occur. I have seen one that was a clear-cut pharmacologic overdose -- the case of Janis Joplin who wound up taking a 16 percent solution rather than her typical five percent, unknowingly. That, I think, constituted a clear overdose. However, the concept of overdose is not so much how strong the drug is. I think the issue is really the clinical judgment, the looseness, if you will, of the medical examiner in determining in fact what is an overdose. That regrettably has to be determined clinically by how tolerant he thinks that individual was. Often that data is rather loose, aside from bodily measures. So it is not so much the fact of taking four percent or two percent, but rather how tolerant that individual is; and tolerance regrettably is not well defined -- it is a judgment. Therefore, the measure of potency, I think, belies this issue.

Dr. Greene: I am not sure whether or not your comment is intended to raise the question of the physiology of death in narcotics victims. Are you saying that most of them are not dose-related pharmacologic overdose deaths?

Dr. Lettieri: No they are not. In New York, Baden would argue that most of the cases he sees do not seem to be clear pharmacologic overdoses and yet the only cause he can point to seems to be heroin. There is one resolution to that and although it is a debatable issue, it is the only conceptual explanation I have ever seen that I could half-way buy. That is that one can lose one's tolerance for heroin very quickly. In other words, having been off heroin for as short as three days, taking a standard dose might in fact be equivalent to an overdose. That may help us explain why Halpern and Baden have said overdose death victims do not seem to have taken more than they usually take. What we need to look for then is, how long has it been since the last time they took it.

Dr. Greene: I am not sure that that is correct. I would say that the New York experience is subject to question. It has never been systematically analyzed. They did not have toxicologic verification of presence or absence of the drug, at least during the period when they were writing most of their papers. Many of those cases were never autopsied. The overdose was designated as the cause of death on presumptive and circumstantial evidence in many cases. They are just beginning to systematically look at their case material, so there are real questions about what their experience means. People who have recently studied cases of individuals who have died with no other determined cause of death, and have performed careful toxicologic analysis including blood morphine levels, which is a relatively new technique that has only been available on a general basis for perhaps four or five years, have found a very strong correlation between blood morphine levels and those cases that are called heroin overdose deaths. There have been several epidemiologic studies including our own that have shown a very strong correlation between heroin overdose deaths and the potency of the heroin preparations that are available in the com-

munity. That has been observed in Washington, Atlanta, and in Texas, and there are several studies that make correlations betweeen blood morphine levels and the diagnosis.

Dr. Lettieri: What level is used? Is there an index? That is what I am trying to get at -- what level constitutes an overdose?

Dr. Greene: It is not a clear-cut situation and my own personal feeling is that somebody dies in conjunction with taking heroin for a whole variety of reasons. It is a multifactorial etiology. The kind of things that weigh into whether a given person dies or not include his level of opiate tolerance, the root of administration, the dose, the presence or absence of other drugs, the rate at which the medication is administered; if it is given intravenously, perhaps the diluents that are also present in the material, and the underlying metabolic status of the individual himself -- whether he is a slow metabolizer of certain drugs or not, whether he has underlying hepatic pathology or not. All of these things, in some way that is difficult to quantify, weigh in to determine whether any given individual is going to die or not when he takes a particular dose of heroin.

Dr. Lettieri: Has any medical examiner been able to get all that data? All I am arguing is that often, most of that data are not available, and the element of confusion is more in the judgmental process.

Dr. Greene: I think the studies from the District of Columbia and Atlanta go into pretty good detail on circumstances surrounding death, and reliable toxicology on other drugs. I may be contradicted on this, but my impression from everything I know is that the cases are very well investigated. In fact, we have been able to show that at least 55 percent of the people who die have histories that strongly document abstinence from opiates in the period prior to death, either because of incarceration, hospitalization, or voluntary attempt at abstinence or withdrawal. Therefore, I think a substantial number of them probably are lacking opiate tolerance at the time they die, maybe many more than we are able to document.

Dr. Lettieri: It would be very useful, I should think, if Dr. Luther in the Washington office would put some of his guidelines for other medical examiners, such as levels, in writing. As far as I know, and I have been very interested in the problem, it has never been documented. And there is so much variance.

Dr. Greene: I think the question as to the precise blood level that may be required to cause death is really not the issue. The diagnosis is a clinical diagnosis and it requires experience and judgment. To simply say that if you find someone who has 20 micrograms percent of morphine in his blood means he is an overdose victim is far too simplistic, unfortunately. I think you really need someone who is skilled in medical-legal death investigations, who has an interest in the problem and is careful enough to rule

out the other causes of death before he makes the diagnosis of an overdose.

Dr. Lettieri: It would be good if someone would make it a bit more explicit.

Dr. Greene: It is in the literature. We published two papers on it.* The people in Texas, from Dallas, and the people in Atlanta have also published their research experiences. In all of these our system is described.

Dr. Lettieri: I am not medically trained and I have listened to Dr. Michael Alexander from CDC talk about hepatitis and then I have heard Dr. Greene's comments about it and I am not really clear now. Can we clearly identify serum type B hepatitis? If I understood Dr. Alexander, he was saying "No, there is a lot of noise in the system" and you are saying "Yes, we have a new technique."

Dr. Greene: There are now specific laboratory tests for making the diagnosis of hepatitis B. It is called the hepatitis B associated antigen. It is something that was a laboratory procedure until about two years ago and is now available on a mass basis from the clinical laboratories all over the country. As far as we can tell, it's specific for this kind of hepatitis -- serum hepatitis, long incubation hepatitis, the type traditionally associated with parenteral drug abuse. The problem is in the reporting system, in that the morbidity and mortality weekly reporting system that Dr. Alexander described only receives from the states a list of how many cases of type A, type B, and type unspecified have been reported; they do not know whether the type B cases were verified through the use of this test or not. There are many cases that are called type A, for example, in which the test was never done; they might, in fact, be type B and misclassified. So there is a lot of confusion in the reporting system because this test is something that has only recently been introduced on a wide scale and there is no analogous test for type A. Type A cannot be diagnosed specifically in the laboratory.

Dr. Lettieri: Would it be fair to say that only when we start using this rather specific test, that is, only when this new test is widely used, will we be able to have a bit more confidence in using hepatitis as an indicator and that everything in the past

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*M. Greene, J.L. Luke, and R.L. Dupont. "Opiate Overdose Death in the District of Columbia -- I: Heroin-Related Fatalities." *Medical Annals of the District of Columbia* 43, (1974):175-181.

M. Greene, J.L. Luke, and R.L. Dupont. "Opiate Overdose Death in the District of Columbia -- II: Methadone-Related Fatalities." *Journal of Forensic Sciences* 19, (1974):575-584.

is essentially debatable?

Dr. Greene: There may be a little bit of a middle ground. I think clearly the use of the test will enhance the reliability of the data, but the two forms of hepatitis are epidemiologically fairly distinct. The major problem is one of improving reporting and getting the reporting physicians to pay attention to these characteristics that would enable them to say this is A or B. There are places, however, where that is now done, so there are some states in which a more detailed reporting form, for example, is used. In fact, there is a second reporting system at the CDC that looks at a portion of the total MMWR sample in which information on antigen possitivity, history of parenteral drug abuse, and history of exposure of other causes of hepatitis are listed. So in some places the data are already in a form that make them reliable. The problem, as with many of these indicators, comes when you try to extrapolate on a national basis from these kinds of data.

Dr. Gottschalk: I just want to endorse what Dr. Greene is saying about the use of toxicological data. You cannot just find the level that will tell you exactly what happened and what the causes were. You have to have all kinds of other data from other systems. I would like to add that in these determinations, it is also important in toxicological examinations to find out what the blood level of morphine is in the urine, because in the cases of deceased individuals, the time of finding the body is terribly important. In cases that have died, it is quite possible that there is very little found in the blood but a fair amount of time has passed and the morphine levels in the bile or in the urine are quite high. As a result, it gets to be a complicated matter and takes somebody that is well versed in the use and interpretation of all the data available to make the kinds of determinations Dr. Greene is talking about.

In our experience with both psychiatric emergency admitting rooms where there is intake of 800 patients a month, and general medical emergency rooms where there is intake of about 1500 per month, in addition to coroners' and medical examiners' data, it does not seem to matter whether or not that state or county or city has a law that it is necessary to report that the person has had narcotics. I think it is something in the culture or subculture of the narcotic users and addicts that they try to take care of their problems themselves. It does not seem to make much difference if there is a law or not, they are still very anxious about going to any authorities.

Lastly, when you speak about year-of-first-use, naturally nobody thinks of the year-of-first-use in terms of whether or not they were active or passive participants, that is, whether the individual might be *in utero*. But if you look at the broad rubric of the epidemiology of drug abuse, you do have to consider whether or not the fetus was exposed to sex hormones, tobacco, or other psychoactive drugs. The data are now beginning to accumulate that if a fetus is exposed to stilbisterol or other substances, it might

have a higher possibility of having a cancer. Or as you know, in some of the data, if a mother smokes, a child is liable to have a lower birth weight and there may be other complications. These are, in a sense, ramifications of drug abuse. We do not know, and this should be researched, what are the long-term effects, not the short-term effects, of the use by the mother of various types of psychoactive drugs. If one takes any kind of medication, say a barbiturate, that influences and activates the development of various liver enzymes which break down other substances faster, within weeks after taking such a drug, you might have much more tolerance. That is because the enzymes have been activated. Liver enzymes work on these metabolites and break them down faster, so that one can take a large dose of phenothiazine or some other related drug and not have a very high blood level. Blood levels, as it is known now, of those given standardized doses per kilogram of body weight, vary extremely widely. Evidence is more and more solid that the effects of those drugs and hence any propensity or proclivity to continue taking those drugs, is related to those blood levels. This is a research question. What are the long-term effects which are not known yet -- the memory, if you will, the enzyme level of somebody's reactability to various psychoactive drugs -- if they were exposed to these drugs when they were a fetus? It is strange in a way, but it is turning out to be very important with various drugs and their long-term effects and hence, their adverse long-term effects. The dose factors are going to have to be explored more and more.

Mr. Romm: It seems to me that this problem relates to non-illicit drugs as well; it is not limited only to psychoactive drugs.

Dr. Gottschalk: Absolutely. In fact, I think of drug abuse not in terms of the illegal ones, but in terms of the drugs that are extant, some of them which are very useful.

Dr. Kramer: However, if you look at genetic effects too, and there is evidence that some of these psychoactive drugs may in some way affect chromosomes, you may indeed get involved with the genetic effects of the use of drugs, which again affect the fetus, which again may have some relation to production of certain enzymes, etc.

Dr. Gottschalk: Year-of-first-use may go back pretty far!

Dr. Newmeyer: Regarding property crime statistics, I found that one is able to set a maximum figure on the number of addict thieves that must be out there based, not upon the police reports of crimes, but upon the recent data we have on the number of crimes a sample of the general public reports having happened to them. By making a string of assumptions we can get a figure that tends to set an upper limit on the number of thieves there might be. This is available in a report.*

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*John Newmeyer and Gregory L. Johnson. "Estimating Opiate Use Prevalence in San Francisco: Feasibility Studies." SAODAP, Executive Office of the President, Washington, D.C. 1973.

Q: Are most of these thieves addicts?

Dr. Newmeyer: At most 50 percent.

Dr. Chambers: I have a lot of difficulty using official crime statistics to describe, predict or to do anything else with. When you look at the rates of victimization, and you look at the rates of self-reported criminal involvement of addicts, neither one of them looks anything like the official statistics. The use of official statistics to describe what is happening seems highly questionable. I have never been able to use official crime statistics for anything other than to generate hypotheses.

Dr. Savitz: You have to be very cautious in the use of LEAA data on victimization. They have not as yet indicated the methodologic limitations of the data, in spite of attempts to secure that information. But the data are being used in many cities for political purposes. There are limitations to crimes known to the police, crimes known to the victims and crimes known to the criminal -- that is, self-reported types. There are advantages in official statistics; they are the smallest. There are too many doubts in people's minds to use victimization statistics. There is also enormous doubt concerning the validity of the self-reported statements made by people when they are reporting about events that could put them into jeopardy should they become known to other people.

Dr. Josephson: Having chaired one conference on the epidemiology of drug use and now having attended this one, I am a little bit more puzzled than ever as to just what epidemiology means in this context. I thought that epidemiology had to do with the research discipline intended to help us understand and prevent disease and illness and help with problems. I think that what has not been said here and the implications of the kind of research being discussed here, whether surveys of households, or people in treatment, or monitoring of police and other records, if that all assume a kind of medical model which is naturally taken from the traditional epidemiological discipline in public health. The implication, of course, is that drug use is a disease. We have not talked about that part, however. The implications of the moral and political considerations are very serious. Dr. Kramer just spoke of possible chromosomal damage. There is no question that some forms of drug use lead to disease; some diseases lead to drug use. But, drug use *per se* is not a disease and probably most people who use drugs, since that includes most people in this society, are not diseased for doing so. I think we have to be careful about the application of these models for thinking about such problems. It carries over, for example, into all our discussions about contagion, about epidemics of drug use -- all of these adapted almost unquestioningly from traditional epidemiological consideration of infectious diseases in particular. There is, of course, a kind of infection going on, but it is a social contagion rather than some other kind.

I think we tend to forget that not only is man a drug using animal, he is also a drug inventing animal. Karl Marx said that religion is the opium of the people; some people have corrected him by saying the opium is the the opium of the people. What this really adds up to is that the discussions tend to get very technical and we tend to concentrate on technical questions of data collection and interpretation, but I think what we really are talking about is not a health problem so much as a moral issue. This is a truism by now; we would not be here if it were not a moral and therefore a political issue. The real question for epidemiologists and anybody else who is concerned about drug use and how any society might or should or can respond to it, is to search for ways in which we can better accommodate both the society at large and also those who are using drugs, some harmfully and some not so harmfully, i.e., two different patterns of drug use. I suppose here the model that comes to mind is the attempt of various societies to accommodate to the use of alcohol. Various policies have been adopted to control, to limit, to prevent, to treat; but nevertheless, in many societies, not all, accommodation has been made in the sense of accepting certain levels of this particular form of drug use, and this, I think should be considered in some of the implications of the research we are doing. I think we should also remind ourselves that the danger of the sort of medical model in the epidemiology of drug use is the implication -- again, we come back to the moral question -- that we must and can do something about it. I am only suggesting that there are many issues in which we not only have difficulty in pointing out the extent and dimensions of the problem, but for which we also have difficulty in trying to determine just what and if anything can or should be done about it. I will end by saying that one of the things that can be done about it is nothing. It is not the only thing but it is one of the things to be done about the drug problem. Some elements in society have, indeed, done little or nothing about it. I am not recommending that, but I think we need to remind ourselves of some of these dimensions and alternatives.

Dr. Kramer: The paramedical model for epidemiology really responds to the public health model, where you are manipulating variables that deal with the characteristics of the host, agent, and environment. Epidemiology has been successfully applied to non-infectious diseases as you know -- cancer, cardio-vascular diseases, etc. It has also been applied to accidents. So I think a lot depends on getting some clarification on some of these terms and what we are trying to get at.

Dr. Josephson: My only point was that we have only mentioned the diseases. Drug use is not a disease.

Dr. Kramer: Accidents are not a disease either. It may be a disease of society if you want to call it that, but it is not a disease. It is a disability, a defect, a characteristic and you are trying to study its distribution on the population in some way.

Dr. Cisin: Exactly -- you are talking about a condition or an event which the society decides is undesirable. Certain diseases

that we now consider undesirable have in the past or in other societies been considered desirable conditions, for example, epilepsy among the Greeks. All I want is a definition of what a society will consider desirable and what a society will consider undesirable.

Dr. Kramer: That is another issue. It has nothing to do with the study of the condition.

Dr. Cisin: Absolutely -- the societal decision as to what is desirable and undesirable is irrelevant to the techniques that are used to study how to stamp out what we in our stupidity or wisdom decide is desirable and undesirable in the society.

Dr. Richards: I chose the word "epidemiology" for this Conference with a distinct knowledge of its shortcomings. In fact, I have written a few words on the subject.* I think it has real limitations; it implies a medical model which should not be used for behavior in the usual sense. There are other models, such as models of technological change or consumer behavior; there are a lot of other models from the social sciences that have not been explored. Therefore, any time we use the word epidemiology, I think we have to do so with tongue-in-cheek.

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*"The Measurement of Prevalence, Incidence and Patterns of Drug Use in the United States" -- CENTO Seminar on the Epidemiology of Non-medical Drug Use, Turkey, October 6-11, 1974.

IV. CURRENT ESTIMATES, RANGES, AND TRENDS

Trends and Projections

Carl D. Chambers, Ph.D.

I am in kind of a bind. We sat here all day yesterday and said we do not know how to describe what is currently going on or what has gone on in the past. Dr. Cisin has told us about the problems in getting the kind of data that I typically gather, and yet I have been asked to give you projections on what is going to happen tomorrow or the next day. In spite of that, I am going to do it anyway.

I fully agree with everything that Dr. Cisin said and I would like to reinforce some of the points he made. It's getting more difficult to do survey research, but I do not think it is impossible to do it. I think it is necessary we continue to do our survey research, it being one of the best data pools available for long range intervention planning. However, I do think consumers go astray with survey research results in that they attach too much "hardness" to the numbers that we give them. Instead of using them as "problem indicators," they want to talk about those projections as if they were an actual census of real people. If your survey shows up 7,291 people from a projected base, they put that into their planning books as 7,291 people who do something. Unfortunately, the consumer extracts the numbers without including the qualifiers which are contained in the text. As survey researchers we are going to have to train those who receive our results how to utilize those results better. I think we have been a little remiss in the past.

Our survey research group has now done roughly 30,000 general population household interviews. There are 17 separate studies each one done with the same basic instrument and approximate methodology throughout. In addition, we have a data base of approximately 3,000 interviews collected over the last two years with active "involved" users -- we call them high frequency users -- on the street and we have approximately 1,000 interviews with "skid row" residents. We recently completed two family studies -- one involving 50 families, in a small rural town with a population base of 2,500 -- roughly 270 families -- and another family study of approximately 280 farm families -- rural residents living on farms in the midwest.

It is from this data base I would share what I believe the trends are and what I think we have to look forward to. (The data is available for secondary analysis either through me or through the clients we've had. The Institute of Defense Analysis has just completed such a supplemental study of our surveys.) In the area of prevalence and current use I will start out by simply sharing with you what, in the area of illicit drugs, we found in the District of Columbia, our most recent survey. We completed the survey 30 days ago and delivered the product two weeks ago. More than anything I have seen in the past three or four years, these survey data support an earlier statement about availability influencing prevalence and incidence. For example in the category of "ever used marihuana," we get a prevalence figure of 27 percent in the population 15 years old and above. For "current use," roughly 14 percent of the base population has smoked marihuana in the last 30 days. That compares with the reported figure of around five percent.

In order to provide you with a comparative base, let me share with you data obtained from Iowa with the District of Columbia data. Both surveys were done with the same basic methodology and instrument.

	(Washington, D.C. Interviews)		(Iowa Interviews)	
	Ever Used	Last 30 Days	Ever Used	Last 30 Days
Marihuana	27%	14%	8%	3%
L.S.D.	4%	.6%	1%	.1%
Amphetamines	5%	.8%	2%	.4%
Cocaine	7%	2%	2%	.3%
Heroin	3%	.4%	.3%	.1%

Our survey data suggest that some 15.9 percent of the population residing within the boundaries of the District of Columbia (age 14 and above) is currently using one or more of the illicit drugs ... translated to a projected number of people, this would be some 90,000 persons. The *vast* majority of these people are

users of marihuana. These findings highlight one of the more significant trends in drug abuse. *There exists an ever expanding group of citizens who smoke marihuana on a fairly regular basis but who do not use any other illegal drugs.* Although these marihuana smokers may have experimented with other drugs at some time in the past, most appear to use only marihuana and alcohol in any long term pattern of social or recreational use. For example, in the District of Columbia we found 60 percent of all current and regular users of marihuana did not use any other illegal drug. Not unexpectedly, we found the majority of marihuana smokers were *not* students. By and large they are people who began experimenting during their earlier years and have continued to do so as they grew older. They do pose some interesting planning problems for those who write our laws and those who are called upon to enforce them.

Combining our general population surveys and our attempts at penetrating the subcultures, let me give you what I think is the drug problem, if we indeed have a drug problem, in proportion to what heroin use is in the country. Using heroin numbers as your comparative base: We believe that there is as much LSD use as there is heroin use. We believe there is approximately twice as much use of amphetamines -- methamphetamine, speed, whatever you want to call the stimulants other than cocaine -- as there is heroin use. We believe there is four times as much cocaine use as heroin use. And finally, we believe there is approximately 40 times as much regular marihuana use as there is heroin use.

Most of the surveys we have done are done for planning purposes and for clients other than the federal government. People responsible for planning want to know how to "rank" their drug problems and where they should make their interventions.

One way to discuss intervention is to discuss which of the drugs used has the highest progression risk, i.e., once you have begun experimentation, the risk of continuing on to regular social/recreational use, and then continuing on to dysfunctional use. If you compare "ever use" figures with people who continue that use, you find that marihuana has the highest progression risk of any of the drugs, at least to the level of regular social/recreational use. To interpret data for planning purposes, however, is very touchy. For example, if you apply the same technique with alcohol, it would probably show up as the drug with the highest progression risk. But nevertheless, I think it is fruitful to at least rank the drugs by progression risk. Our data would suggest that you can rank marihuana as first, amphetamines as second, cocaine as third, heroin as fourth, the sedatives as fifth, and the psychedelics as sixth. There is very little evidence of such progression with the solvents and inhalants. However, we may miss this progression as we rarely go below age 14 in our surveys, missing the younger group who is most at risk for currently using inhalants or solvents.

As most of you know, our surveys probe for the use of all psychoactive substances rather than focusing only on the illegal drugs.

These include the prescription drugs, the over-the-counter drugs, alcohol and illegal drugs. If I were to project what our *next* major drug problem will be, I would suggest it will be adolescent abuse of alcohol. In fact, our data indicate "the problem" has already arrived.

Applying Cahalan's index to our data, we find adolescents in the age group 14-17 are at least proportionately represented among heavy daily drinkers and in some geographic areas are already overrepresented. As epidemiologists we have become concerned with a correlate trend ... the mixing of alcohol and other sedatives among these younger drinkers.

One of the reasons we pursued our family studies was to try to look at correlations between parental use patterns and adolescent patterns. We found a direct correlation between parental drinking and the amount of drinking, and adolescent drinking and the amount of drinking. We did *not* find a correlation between parental use of psychoactive substances other than alcohol and adolescent experimentation of psychoactive substances. I don't know how that matches with Denise Kandel's study population of New York City adolescents.

Dr. Kandel: That fits in very well. We find there is a very slight correlation between parental use of psychoactive drugs and kids use of illicit drugs. Parental use of alcohol is much more important. But we found a slightly curvilinear relationship. I do not know if you found this also: at the very highest levels of parental alcohol use (those parents who say they use it daily), there seems to be a slightly lower proportion of kids who are using alcohol themselves, but that proportion is still higher than in the families in which their parents don't drink alcohol at all.

Dr. Chambers: Let me indicate some of our other correlations. We did find a positive correlation between adolescent use of cigarettes and subsequent use of illicit drugs. We found *no* correlation between adolescent drinking and adolescent use of other psychoactive substitutes. Almost everyone drinks, whether they use illicit drugs or not.

Q: Are you doing anything at all about partitioning your samples according to demographic and other breakdowns in order to determine the patterns of variation in the population?

Dr. Chambers: Yes, we normally group data by social class as measured by observation, and social position as measured by family income, parental education, etc., as well as by various labor force categories and of course sex, age and race.

Q: Can you say anything about those variations?

Dr. Chambers: There are variations. People who misuse legal psychoactive drugs are more likely to be in the middle and upper classes; part of that is artifact because they are more likely to be the ones who seek out physicians. However, the same amount of

chemical coping occurs in the lower classes, but I think it is occurring with alcohol and, to a certain extent, with the OTC drugs.

Dr. Sells: There is one point I would like to check. In our data, we have been very much impressed with the significance of the heavy use of wine as a correlate to deviance, etc. I wonder if you have anything to add to that?

Dr. Chambers: The only thing that we have is that the pop wines have essentially changed the patterns of drinking for people under 21. In the past, when you could not drink legally until 21, whoever looked the oldest in a group would go in and buy a half pint of Four Roses and four people would hide in a car and get drunk on it. Now a kid can drink at 18, so 14 and 15 year olds have full access to alcohol and in most places, they can even buy it at grocery stores, etc. The pop wines are the major consumption for the age group; they move from that to beer. They drink pop wines because they taste good. It is not like when I had the experiment with alcohol at 14 and 15, and the darn stuff didn't taste good. I really had to pretend I liked it. I think with the pop wines, you do not have to do that any more. They do taste good and people soon learn you can use them in a variety of ways. The most popular being in combination with sedatives. Such combining is not entirely new. For example, we knew 10-15 years ago that you could take marihuana and heated wine and alleviate heroin withdrawal distress by simply staying knocked out.

Let me move back to the question of availability. We have studies everywhere except the far West Coast and we have found heroin, cocaine, the psychedelics, and illicit amphetamines to be available everywhere. Even the smallest rural communities in Iowa, Arkansas, Mississippi, etc. can have heroin and cocaine if they want it. The sources are there and we don't know what forces there are or what buffers an individual has on whether or not he can openly experiment with those drugs as he does in our more populated areas.

If you look at the recent data from SAODAP and the Drug Council, there is evidence that heroin is showing up in the smaller cities. I think the smaller cities are running "behind" in a variety of drug abuse patterns, not only heroin use. I felt for a long time that we had a real decrease in LSD use but recently it appears to be coming back. I think we are finding a new group of people who are experimenting with LSD and this may not represent an increase in use but may only be a different group. The reason it looks like an increase is because we are looking at different places now than we looked in the past. I think if we looked nationwide or looked at some central four or so points across the country, we would find the LSD incidence pattern has been fairly stable. I think *most* of the drug use patterns are fairly stable; I think that the rate of experimentation and the incidence of progression to the social use of drugs probably will be found to be fairly stable.

We detected one pattern of abuse which I earlier predicted to be one of the next abuse patterns -- that is, the abuse of anti-depressants. The truckers and the kids were abusing them in the mid-west and the south. And I said, once we don't have amphetamines any more everybody is going to get on to Elavil and have a good time. I am not sure if that is occurring or not occurring. Once I picked it up and began to ask about the pattern, I got too many people telling me "Say, that sounds like a good idea." I felt a little reluctant about participating in the spreading of a drug pattern around the country. I felt we were actually showing people a new drug they could abuse, and so I have quit asking about it. Similarly, I quit asking if there were any thefts of veterinary supplies, etc., in rural areas because people replied "Gee, I didn't know that vets had those kinds of drugs around."

In the area of misuse of the legal prescription psychoactive drugs, we find as many as 20 percent to 40 percent of all regular users *misuse* these drugs, e.g., don't have a prescription or do not take them as prescribed. As has everyone else, we find women about the age of 25 to be the primary consumers and the primary misusers of these drugs. Divorcees and widows, regardless of employment status, are most at risk for this pattern of abuse.

We have found considerable misuse of over-the-counter stimulants, tranquilizers and sedatives. I think this abuse pattern is going to be a major health issue in the near future. Primary consumers of these drugs appear to be people on fixed incomes. As the economy worsens -- and I cannot help but feel that it is going to -- I think people who have been going to physicians and using prescription drugs will be switching to the OTC drugs, and they are not being as "careful" with these drugs as they were with their prescription counterparts. If you will look at DAWN's emergency room statistics (and our Miami metropolitan area statistics correspond) the major *increases* in overdoses have involved the over-the-counter drugs. I do not know if such has been occurring across the country although I suspect it has. We have a very large fixed-income population in the Miami area, and we have always had a high consumption of the OTC drugs which may bias my perception. However, I do think we are going to find OTC abuse becoming a major problem.

One of the final things I would like to share with you are the "problems" we pursue when we interview; for example, has the respondent ever had an adverse reaction to drugs -- physical or psychological; have there been any problems in the family (self-defined by the respondent) -- trouble with the police or on the job, etc. It is relatively astonishing to me the number of people in our population who have already had problems in the family, problems with the police and on the job as a result of drugs or alcohol use. For example, in one statewide population study which is representative of all our studies, we found the following:

	From Using Drugs	From Drinking Alcohol
Have had trouble in the family	6%	11%
Have had trouble with the police	2%	5%
Have had trouble on the job	2%	5%

I share the concern of our Canadian colleague, Dr. Rootman, as to whether or not we are actually going to address the problems that drugs cause or the drugs themselves; I am more inclined to say we ought to begin to think about the people who are having problems with drugs and not be terribly concerned about which drug it is.

One final note that I would address to those people who have policy making and planning powers. In the subcultures we have been in, and we have been in a variety of them across the country, about one-fourth of our active drug users *do not know of any treatment program in their community*. Outreach is not terribly effective in most places in the country. Yet outreach does work and I would demonstrate the ease with which it does by sharing with you one of the problems I had as a researcher involving a study in Detroit.

I was trying to do a longitudinal study of active users in the subcultures. I would identify an individual and then relocate and reinterview him in 60 days, relocate and reinterview him in another 60 days to see what life style and drug consumption changes had occurred. The people I hired as guides or field workers to assist in the project happened to have been outreach workers who wanted to do this as a means of supplementing their income. They were very effective on the street. The problem was that within the first 30 days, everybody I had identified they had recruited for their program making it impossible to do longitudinal work. I finally fired some very effective interviewers, and found people who would not recruit all of my respondents into treatment programs. Outreach is possible and I begin to think about how some of our epidemiological data point to those things. In addition to the 25 percent who knew of no programs, an additional 25 percent of our respondents had so many negative feelings about the current treatment delivery system that they would not go into treatment. We are, I believe, losing one-half of the people who would accept treatment if treatment were available and acceptable to them.

AUTHOR

Carl D. Chambers is Executive Vice President, Resource Planning Corporation, Miami, Florida.

DISCUSSION

Dr. Rootman: I would like to support Carl Chambers' point about the rural areas. In 1971 I was involved in a large study in rural Alberta (Canada). We found that essentially, the patterns of use were no different in rural Alberta than in metropolitan Toronto and metropolitan Montreal at approximately the same time, except that in Alberta, there was much higher alcohol use, glue and solvent use at the same period.

We found in our studies based on the Bureau of Dangerous Drugs and Narcotics Registry, that the highest incidence rates for narcotic abuse or recognized narcotic abuse in Canada, in 1973, were not in metropolitan Vancouver, but in some of the smaller communities in the interior of British Columbia. It would suggest perhaps a spreading out into the rural areas, not only the case in the United States but in Canada.

Dr. Sells: You mentioned the aversion of going into treatment. I have the impression from the DARP file that an overwhelming proportion of the patients are in treatment under coercion rather than as volunteers.

Dr. Chambers: I have felt for a long time that as long as a person is able to support his habit and the police are off his back and his source remains fairly constant, why go into treatment? It is only when his family begins to hassle him or he losses his source or the police have identified him, that he comes into treatment. The thing that concerns me, in spite of this, is that the bulk of our treatment delivery system is stereo typical. It is narcotic-oriented, and I think the whole narcotic picture is shifting. I think we are getting a pattern of social/recreational use of heroin much greater than we had in the past, and therefore, new data is showing a resurgence of heroin use. In the early '60's the dominant philosophy was that you could not "chip" around with heroin. I have become convinced, however, that most people can "chip" around with heroin and indeed they do so. To take that individual and place him into a treatment delivery system based upon hard-core narcotic addiction is inappropriate for him and he knows it. If you ask him what he wants instead, he can not tell you.

There is the example of Moffet's study in Philadelphia where recently initiated addicts were detoxified with methadone, as part of the standard detox program. On follow-up, a significant number

of them had adopted methadone as their drug of choice, because it was simply better than what they had before. There is always that risk of giving somebody something better than he has had before.

Dr. Elinson: I would like to point out that one of the simplest concepts, namely "current use" has been defined as "a period of 30 days" and as "the lack of intention never to do it again." I just want to remind the group that this is where we are with respect to defining what appears to be even the most simple terms in area research.

Dr. Abelson: I would like to add to that by pointing out a concept which gave us equal difficulty, and which we by no means feel we have solved, and that is the communication of the notion of non-medical use of prescription drugs to a household population. The term non-medical by itself is certainly a precise one in ordinary survey communication. The operational definitions which we concluded go part of the way -- it is all right to ask people if they have tried any of these drugs just to see how they feel. But there comes a time when this is really quite illogical, because making oneself feel good can easily be interpreted as also related to need. I am just starting to get around the edges of this. At any rate, I would like to point out that the notion of non-medical use is by no means an easy one or one that we think everyone will accept or respond to in the same way.

There are two or three other points I would like to make. One is that in getting through a catalog of drugs with the household population, we really have a big trade-off. The trade-off is in what all of us who are craftsmen in this field believe is some of the higher parts of our art -- that is, the development of the interview experience through the initial acceptance by the respondent of the interviewer, to the point of increasing interest, to a point of being able to ask questions of respondents which truly might have been "turn-offs" at the start, but no longer are at this stage in the interview. In other words, the development of the interview as an art form and its distinction from what the PTA might do if it wanted to poll its members is something in some part that has to be abandoned when there is this necessarily repetitious kind of catalog of experience that we are looking for.

I would like to tell you a little about an experience that we had with the first study that we did for the Marihuana Commission in which, as a matter of curiosity (it was not part of our charge) we included some questions on the hard drugs. We then pulled the 55 questionnaires where there was reported heroin usage and tried to examine those questionnaires clinically. Incidentally, this is one of the things that I would recommend -- that is, to identify quantitatively segments of the consuming drug public and then examine samples of those questionnaires in a clinical fashion, as though one is looking at one protocol at a time to see what can be learned from it, even though the questions are fairly superficial. Starting with a kind of *a priori* semi-intuitive notion of what else

should have been said in this interview of 55 heroin users to indicate their heroin experience, we succeeded in getting that list down to 12 people whom we were certain of and the remainder whom we were not at all certain of. In turn, this led us to a form of analysis which we have already used and intend to examine for its use again in this new study, simply because there is so much repetitive data. That is, using a set of techniques developed by Sam Messick about 10 or 15 years ago in connection with test development to score each question for each respondent and develop a set score -- that is, the likelihood that the response was part of a pattern -- as compared with the content score, and then examine the relative contribution to the total response of the set and content components. By themselves, I would not recommend that one go with just these two indicators for each question, but with other data, they worked out fairly well for us.

Lastly, I think one of the larger contributions to total variance and to error variance may be the over-reactive way in which we communicate to respondents that the information we are looking for is so sensitive that we surround it with an enormous number of privacy type devices to insure the respondent that in no way will that questionnaire become public. The fact is that we do and it is true, but I wonder what the impact of that is on the interview.

V. CURRENT EPIDEMIOLOGY PROGRAM AND RECOMMENDATIONS

Current Program Dimensions

Richard Myrick, Ph.D.

I have passed out two pieces of paper and I will deal first with the one with the date November 1974* in the upper right hand corner. This is a matrix which shows what grants we have. The problem with classifying grants is you can either classify them by drugs -- but as soon as you do, you discover that somebody is dealing with a whole lot of different aspects of one drug -- or you can think about them as by topics -- and you discover that somebody has got one topic but he is dealing with a whole series of drugs under that one topic. This is what has led us to make a matrix which shows topics down the left side and drugs across the top. This particular matrix gives you the number of grants funded with FY 74 funds; FY 74 is the 12 month period that ended June 30, 1974, and it shows how many grants there are in each of these different topics and each of these different drugs. One of the advantages of making a matrix is that it shows you in some areas we have a great many projects going on. For example, under Mechanisms of Action where it meets the Opiates column, you will see that there are 10 grants there. On the other hand, you can see just to the left of that under Mechanisms of Action and Hallucinogens, there are no grants. Hence, this matrix leads people who are planning our program to look at both empty cells and very crowded cells to see whether indeed we should be having as many

*See Attachment 1.

projects as we are, if the cell is crowded; or if the cell is empty, or has a small number, maybe we should have more emphasis in that area. You can see by looking in line 1, Epidemiology and Surveys, in the next to last column on the right, that we have nine projects total, as of June 30, 1974. That means that there are nine Epidemiology grants funded with FY funds. For your general interest, the dollar value of those is $1.35 million. You can calculate the percentage of that if I tell you that is out of a total of 204 projects and a budget of $17.6 million; so you can see that Epidemiology is getting a little under a tenth of the grant funds here. This does not show you contracts. We also have as of June 30, 1974, six contracts dealing with Epidemiology and they are valued at $350,000. That is because only one-half of them drew at FY 1974 funds, but this gives us a total of $1.7 million in Epidemiology. By way of contrast, you can look at the bottom line at the extreme right hand column and see what the program was a year and a half earlier, as of January 1973. You can see, for example, that there were only five grants in the Epidemiology field then, and only a total of 142 grants in all. You can also see by comparing the two bottom lines in the two right hand columns, how much the program has shifted. Generally speaking, the major change that has occurred in the program was that a year and a half ago, we were somewhat under fire for putting too much money into marihuana and not enough into opiates, so there was a big push to put money into opiate research then. Now, the wind has shifted and we are busy getting more money into the marihuana field.

It is complicated to try to put together our dollar amounts and our program amounts. For example, in FY 1974, although we had 204 projects funded with FY 74 funds, we had 166 additional ones which were not funded with FY 74 funds. They were either carried forward from a previous year, or they may have expired, or some may still be going on. Just to give you a rough feeling for budgets, in FY 73, the grant program was $16 million and in FY 74, it was $17 million; and at the moment, for FY 75, it is scheduled to be nearly $21 million.

Macro Systems and Jack Mendelson have prepared two books for us which describe our program.* One of them has a list of all drug research that is funded by the federal government. It is listed by researcher and the principal investigator.

Q: Does that include alcohol research as well, or is alcohol not a drug for that purpose?

A: It is not a drug in this case.

**Drug Research Directory FY 74;* and *Federal Supported Drug Research,* 2 vols. SAODAP, Washington, D.C.

Q: Is there a similar list for the alcohol projects?

A: No.

Volume 2 of the Macro-Mendelson publication also has all kinds of pie charts, showing how the money was spent one year and how it is going to be spent in another year, organized by agencies and by drugs. I really just wanted to flash it by you so you would know that it does exist -- if you want to get information about specific studies, you get one of these volumes and if you want the whole federal effort, particularly in dollars and number of projects, you get the other.

Now lastly, there is a second page of the handout which is labelled *A Trellis....** Over the weekend there was a story in the newspaper that the people who operate one of the radio telescopes had beamed a message out to a distant star. It said that it was going to take 24,000 years for the message to get there and if the fellows out there who get it can figure out what we are saying and send a message back, we will have an answer in 48,000 years. Anyway, this is the way I sometimes feel when I watch the money that is being poured into drug abuse research in terms of getting an answer back. We see lots of research happening, and we are told now that heroin is on the increase. But it should not be on the increase after everything we have been doing! It should be more on the decrease. Maybe it is that the 48,000 years have not yet passed, and maybe by then it will be decreasing. In any event this trellis is a device for keeping track of our program and some of the program analysis efforts that we make. If you begin at the lower left hand corner, you will see the word "List" and it says Report No. 201. That is an IBM printout that you can get that lists all our projects for all the years. The purpose of that is to answer general inquiries and generally give the administrators of the program some idea of what is going on.

Moving to the second column, you can see it says "List" again. This is because the list that we use for Categorization is not the same list, or at least it does not contain the same kind of data. It is drawing on the same kind of projects, but it may in one case be all parent projects and in another case, all projects funded in just one fiscal year. We have a person named Nancy Schwartz who puts them in categories. The categories, generally speaking, agree with the topical area and the drug area that I showed you earlier.

Then we have still another kind of analysis which is in column 3 and that says "Mendelsohn," under the word "Fiscal," that refers to these green documents that I showed you. Again, he is using all of the federal efforts so his list is different from the other lists. He also has a different category scheme, but he does tell you how the program breaks out in fiscal categories.

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*See Attachment 2.

As you get towards columns 4 and 5, you begin to get out of the administrative aspect of this effort and into the scientific aspect where Nancy Schwartz provides summaries of projects and we respond to requests from people who want to know what it is we have going. Then periodically, our staff pulls together what is going on. That is column 5 where we attempt to assess the progress of our projects. With regard to column 6, Macro Systems held a conference to identify gaps and overkills. They came up with long lists of things where they thought there was too much effort and other things where they thought there was not enough effort.

Columns 7 and 8 address the social utility of drug abuse research. These were pipe dreams on my part. We have yet to really determine the relevance of research to the real world of drug abuse. We have in some of our clinical projects. But in the last part, the idea of examining social indices and getting the final payoff of our program -- this is something that has the least amount of our efforts. We spend most of our efforts making lists of our projects, way back in column 1. I guess I should have put the 48,000 years across the bottom of this to show the time lapse before we ever begin getting the payoff.

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November 11, 1974

NATIONAL INSTITUTE ON DRUG ABUSE
Division of Research
ANALYSIS OF THE GRANTS PROGRAM

SUBJECT AREA	DRUGS: Marihuana	Amphetamines	Barbiturates	Hallucinogens	Opiates	Cocaine	Replacement	Antagonists	Undefined or Multiple	Total as of 6/30/74	Total as of 1/73
1. Epidemiology and Surveys					2				7	9	5
2. Psychosocial					3		1		25	29	17
3. Chemistry and Metabolism	8	1		1	6	1	2	6	18	43	22
4. Mechanisms of Action	5	2	2		10	1	2		26	48	39
5. Treatment Related			1		1		2	4	4	12	7
6. Adverse Effects	9	2		1	1		1	2	7	23	9
7. Methodological Studies	3				2			2	8	15	15
8. Behavioral	4				2			1	14	21	25
9. Multiple Effects	1				1				2	4	3
Totals of of 6/30/74	30	5	3	2	28	2	8	15	111	204	
Totals as of 1/73	45	8	1	4	31	1	7	6	39		142

ATTACHMENT 1

A Trellis Demonstrating Different Levels of

Program Analysis

							Examine social indicies of drug abuse
						Determine relevance to real world of drug abuse	Determine relevance
					Identify gaps & overkills	Identify gaps & overkills	Identify gaps & overkills
		Fiscal (Mendelsohn)	Summarize Projects (Nancy Schwartz)	Assess progress of projects	Assess progress (Carol Anglin)	Assess progress	Assess progress
	Categorization (Nancy Schwartz)	Categorization	Categorization	Categorization	Categorization	Categorization	Categorization
List (Report #201)	List	List	List	List	List	List	List
1.	2.	3.	4.	5.	6.	7.	8.
Purpose: Answer general inquiries	Provide finer Description	Show resource allocation	Inform scientific community	Aid scientific monitoring	Plan scientific effort	Application	Payoff

ADMINISTRATIVE ASPECT _____ SCIENTIFIC ASPECT _____ SOCIAL UTILITY

ATTACHMENT 2

Interpretations and Policy Implications

Helen Nowlis, Ph.D.

I have some problems. In addition to the topic announced, Louise Richards asked me if I would try to summarize. I look at my watch and realize we still have two important speakers and topics to go, so I think my summary will be very brief.

I always have a great deal of difficulty with these conferences. Sometimes I describe it as being in an adolescent identity crisis, and other times I describe it as being schizophrenic, because my whole background has been in research and it is only in the last three years that I have attempted to get involved in policy and program implementation. So I have difficulty in keeping these two things separate.

I would like to talk first from my research point of view. We have been hearing at least three things in the last two days. One is that this conference has been organized around a methodology and it is quite clear that everyone here does not mean exactly the same thing by that methodology. We have heard about it from the practitioner's point of view. We have heard about it from the theoretical point of view. We have heard about it from a mathematical or a statistical point of view, and we have heard about it from a pragmatic point of view. I think the point I would like to emphasize, and it is not too different from what Eric Josephson said a little while ago, is that any methodology is only as good as the skill with which it is applied to an appropriate problem. I think we have raised considerable issues about the skill with which we have applied the methodology and I trust that Dr. Kramer will

talk about this some more; and I think there have been important issues raised about the appropriateness of its use for certain purposes. We have heard about the dangers of extrapolating from inadequate data. We have heard about the fact that any statistic is only as good as the validity and the reliability of both the numerator and the denominator. We have been cautioned about the various terms and techniques that are used in epidemiological research, and cautioned to make distinctions among prevalence and incidence, point incidence, point prevalence, interval, etc. We sit here and discuss this and I think most of us understand what we are discussing, but the thing that most people look at is the end product. I would like to suggest that among the consumers, there are very, very few people who understand what it is that goes into the projection or extrapolation. Somehow or other, maybe because of all the work that has been done -- the touting of political polls, surveys, household surveys and the great accuracy with which we can predict things -- the great majority of people have far greater faith in the result than any of us do or than they should. Almost never does anyone who is going to use the materials, outside of the methodologically sophisticated, look at the basis from which the figures came. I think we have gotten into some very difficult positions as a result of that.

After all, in epidemiology, we are dealing primarily with statistics. But in the real world, we are dealing with people. I think, as Lloyd Johnston and Gail Crawford and Lee Robins have pointed out in a sort of indirect way, that it is possible to use these data *vis à vis* real people, but not in terms of national surveys, probability samples, or extrapolations. I have often wondered exactly why it is, as the question has been raised several times, that we have this desperate need to get national figures and national trends. They are no help to me as I try to design programs and relate to individuals, to individual school systems, to teachers, to students, to community programs -- no help at all. I often wonder if we are not trying to deal with the whole problem of drug abuse as if it were a disease, an infectious disease, and not a social problem. Because once we say social problem and once we say human behavior, we must take into account the characteristics of all human behavior -- a) it is motivated, b) it is variable, and c) it is multiply determined. I can't figure out how national trends and extrapolations are going to help me in dealing with that kind of thing. I can begin to look at the implications of some of the studies either that take homogeneous samples -- because that tells me something about a group of people -- or that deal with correlates, whether they be antecedent, simultaneous or subsequent.

There are some cases in which we have a technique in search of a problem and other cases in which we have a problem in search of a technique or a methodology. I think we have to be very, very careful how we balance those two out. This whole thing gets, as these last two days have shown, very confused by the lack of clarity of definition, whether it is in terms of our methodology or whether it is in terms of the phenomenon we are concerned with. It bothers

me terribly to throw all marihuana use, cocaine use, heroin use, stimulant use, and sedative use into one bag as if they were various forms of influenza or something like that, because it is not helpful. Different people are using different drugs, at different frequencies, under different conditions, for different reasons. And if I am going to try to address those problems or in any way intervene, whether by prevention, intervention, treatment, or rehabilitation, I have to deal with people and not with statistical extrapolations. I often wonder why we have this desperate need, as someone put it yesterday, to count noses. I would conclude that it was more in an effort to justify legal, social, and moral positions, then it is to intervene in behavior which is at least potentially destructive. The other things, which Dr. Rootman also mentioned, is why are we not more concerned with the destructive effects of use of any substance rather than with nose counting of users?

After I talk like this, people ask "How can you stand to be in this business, don't you get discouraged?" I don't get discouraged. I think if we do a good job at whatever we do and make it very clear, as we certainly have done today, that there are limitations of whatever methodology we use, that we can contribute to a total picture. I long ago gave up being terribly concerned about whether this particular substance was up or down, whether alcohol was moving in or heroin was moving out, or that marihuana was going up. If you are dealing with behavior, which involves the use of substances to modify mood, feeling, and behavior, you don't have to get involved with specific substances except as use reaches the point where there are medical complications. At the level of prevention, nobody has ever used drugs. Or if they have, they have only experimented with it. We must deal with it as a social problem, not as a medical or a disease problem. That does not mean that we can not learn something from the behavior of other conditions. But I find very little help in planning programs, implementing programs, forming specific programs from these models. We have reached the stage where we deal with what we call prescriptive action. That is, for any given community, what is the problem, what are the resources, and what -- from a smorgasbord of strategies, techniques, methodologies -- is most appropriate for solving that problem involving those people in that community?

Now this is a long, long way from national trends. One thing that bothers me is that I do not find out about these trends until the year after they have occurred and, by that time, they may have changed. It is a pretty shaky ground on which to plan. I want you all to keep doing studies. I want to be aware of them, but in terms of their helpfulness in dealing with the potentially destructive behavior involved in the use of a great variety of substances, it does not help me. I am delighted that you have had this conference. I am delighted to be here. All I am saying is that it may be fine for social decision-making, but not as the basis for intervention.

AUTHOR

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Relationships Between Mental Health and Drug Abuse Epidemiology

Morton Kramer, Sc.D.

Before discussing the topic assigned to me -- the relationship between mental health and drug abuse epidemiology -- I should like to comment on the essential role of epidemiologic research in our efforts to prevent and control disease, disability and major social problems. These comments were stimulated by the remarks of the prior speaker, who seemed to have doubts about the importance of such research.

Epidemiological profiles of diseases and disabling conditions for the U.S. as a whole -- and their variations among regions, States, counties, cities and other geographic subdivisions serve many important purposes. Data on the size of these problems, their costs to the Nation and their social impact are essential to the process of getting the support of Congress, State legislatures and local governments to establish programs to prevent and control these problems and provide the funds and manpower needed to implement them. Planners need such data to develop strategies for attacking these problems. Evaluators need baseline and other types of data to measure the effectiveness of efforts to combat these problems. These include incidence, duration and severity of disability, mortality rates and related indices. The uses to which such epidemiologic data are put have been admirably summarized by Morris.*

* Morris, J.N. *Uses of Epidemiology* (Second edition). Baltimore, Maryland: William and Wilkins Company, 1964.

1. To assess changes over time in incidence and prevalence of diseases, the disability they cause and their mortality.

2. To diagnose the health of the community and the conditions of the people, to measure the dimensions and distribution of the problems of disease and disability, to define health problems for community action and their relative importance and priority for action, and to identify vulnerable groups needing special protection.

3. To study and assess the health services with a view to their improvement.

4. To estimate individual risks, on the average, of acquiring various diseases and conditions.

5. To identify syndromes.

6. To complete the clinical picture and describe the natural history of chronic disease.

7. To provide clues to causes.

These uses are relevant not only to efforts to combat disease and disability, but also to parallel efforts to eradicate the many social problems that play a major role in creating the conditions in which disease and disability thrive and hamper our efforts to prevent and control them. Indeed, it is a vicious circle because disease and disability -- inappropriately attended and controlled -- perpetuate and exacerbate social problems and vice versa.

A careful discussion of the relationship between mental health and drug abuse epidemiology requires more time than I have available to me in the closing minutes of this conference. Basic to such a discussion are operational definitions of drug abuse and mental health. These are needed to specify types of relationships, study designs for elucidating the relationships, and data collection instruments and procedures for acquiring the data needed to compute the various indices. Since the types of relationships to be considered have not been specified, I will suggest a few that might be considered.

For this illustration, I will assume the following: (a) the operational definition of drug abuse has been agreed upon; (b) a subgroup of drug abusers are persons who would be classified as drug dependent; (c) the definition of drug dependence is that given in the American Psychiatric Association's (APA) manual on Mental Disorders (DSM-II) which includes dependence on such substances as opium, opium alkaloids, synthetic analgesics, barbiturates, cocaine, cannabis sativa, amphetamines and other psycho-stimulants; (d) the mental disorders other than "drug abuse" are those defined

in the APA Manual (DSM-II).* The problem before us then is to investigate certain relationships between the occurrence of drug dependence and mental disorders other than drug dependence in a defined population of persons. Let us specify that we have a catchment area of population size N and we wish to classify its residents on two major axes: (1) Presence or absence of mental disorders other than drug dependence; and (2) presence or absence of drug dependence as in the following four-fold table:

Mental Disorders (Excluding drug dependence)	Drug Dependence		TOTAL
	Present	Absent	
Present			N_1
Absent			N_2
TOTAL	N_3	N_4	N

To proceed further, it is necessary to specify the types of questions to be answered from this table. If we are merely after a point prevalence count, i.e., the number of persons as of a specified date (given point in time) who have a specified characteristic, then we would need case finding procedures for determining which of the residents fall in each of the cells of the above four-fold table. It would then be possible to determine the proportion of the population of the catchment area as of a given date (e.g., July 1, 1974) that are: (a) both drug dependent and have another mental disorder; (b) drug dependent only; (c) with other mental disorder only; or (d) neither drug dependent nor with other mental disorder. The marginal totals provide the data needed to provide the point prevalence rates of drug dependence and other mental disorders. This is only one type of prevalence count. Other types of prevalence questions might be asked. For example, how many residents of the catchment area have had an episode of drug dependence and/or mental disorder in the past 12 months? Answers to such a question provide the interval prevalence rate; i.e., the number of persons who have experienced episode(s) of illness(es) any time during the past 12 months. This is called an interval prevalence rate, i.e., the proportion of the population who have experienced an episode of illness at any time during an interval of time.

The above prevalence measures (point or interval) can be computed specific for age, sex, race, type of mental disorder, type of

*American Psychiatric Association. *DSM-II Diagnostic and Statistical Manual* (2nd edition). 1700 Eighteenth Street, N.W., Washington, D.C., 1968.

substance on which the person is dependent, marital status, living arrangements, etc.

Referring again to the four-fold table, we can ask other questions. What is the prevalence of drug dependence among persons known to be: (a) schizophrenic; or (b) to be suffering from a major affective disorder, i.e., manic depressive disorder (bipolar depression), manic disorder only, or depressive disorder only (unipolar depression)?

Or, given persons known to be drug dependent, what is the prevalence of organic brain syndromes among them?

Still other questions can be asked, such as: given persons who have a major affective disorder but are not drug dependent as of the survey date, what is the incidence rate of drug dependence within some defined period of time, e.g., one year following that date. That is, how many persons with a specified mental disorder become drug dependent for the first time during a defined interval of time?

We can also raise questions about the extent to which persons in the cells of the above four-fold table are receiving services either for their drug dependence, their other mental disorder, or both. Are these services being rendered by personnel in a single facility? Are these services being coordinated? Or, how many persons receive services for drug problems in one facility and services for schizophrenia in another facility? Again, are these services being coordinated?

Many questions can be asked relating to the incidence and/or prevalence of various physical illnesses; e.g., cancer, cardiovascular diseases, tuberculosis, nutritional deficiencies, accidents, suicide among persons who are classified as drug dependent. Or, vice versa, i.e., how persons with a specified illness became drug dependent.

Since the interrelationships that can be investigated are many, I would urge that, if we are serious about exploring them, and I hope we are, then a committee should be established to specify the important questions to be answered about the reciprocal relationships between drug dependence and other types of mental disorders, the types of services required by these persons, the extent to which they are receiving them, and their effectiveness.

Persons doing field surveys and evaluative studies related to mental disorders have developed case finding techniques, designs for evaluation of effectiveness of services, etc. Persons working in the drug abuse field have developed techniques for similar purposes. A mechanism is needed to assure that there is a frequent interchange of information among these investigators. To illustrate, the Division of Biometry has developed a demographic profile system which provides for each of the 1,500 mental health center catchment areas (established in the U.S. under the community

mental health centers regulations) a series of 107 social indicators. These indicators define types of high risk population for mental health, other health and social problems. Certainly, such data should be of value to persons planning drug abuse services. In addition these population data provide denominators for a whole series of morbidity rates. What we lack are the numerators. Indeed, a major problem that must be resolved to carry out the types of research specified above revolves around the development of reliable, valid, sensitive and specific case finding procedures for drug dependent persons and for persons who have mental disorders (e.g., schizophrenia, affective disorders, psychoneuroses, organic brain syndromes, etc.). Another illustration of work going on in the mental health area is a study which the Biometry Division is funding jointly with NIDA in New York State. The Director of Statistical Services for the NYS Department of Mental Hygiene is studying the patterns of use of services for mental health, alcoholism and drug abuse in a selected number of catchment areas in the State ranging from very rural to highly urbanized populations and relating the use patterns to demographic variables and diagnosis and to service networks available in each area.

I would urge that we pool our knowledge and experience to develop a series of studies that would be mutually beneficial. From what I have heard today, you have acquired extensive experience in community surveys. I would hope that, in the future, those of you conducting such surveys might find it possible to add to your instruments questions that would be of value to those of us who are concerned with the epidemiology of the other mental disorders and planning programs for the delivery of services to prevent and control disability associated with these disorders and evaluating their effectiveness.

In the mental health field, we have developed a body of techniques for collecting and analyzing data for monitoring the delivery of mental health services. Those of you actively involved in delivering services to drug abusers and drug dependent persons have developed similar techniques for monitoring and evaluating these services. I would urge that we pool our knowledge. This can only be done by developing collaborative arrangements for exchange of information and mechanisms for planning studies that will investigate the many inter-relationships between mental disorders and drug dependence and the delivery of services to the persons suffering from these disorders.

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Program Gaps and Future Direction

William McGlothlin, Ph.D.

The assignment that Louise Richards gave me was to talk about Gaps and Future Direction. I think we all have been talking about gaps in the field of epidemiology the last couple of days and I am not presumptuous enough to try to provide a super list. Instead, I direct my comments more in the context of what I see as the major focus of this conference on epidemiology -- that is, the measures of incidence, prevalence and trends -- rather than some of the broader aspects of epidemiology such as etiology, natural history and the characteristics of the higher risk groups.

One thing that seems to me to have largely been ignored is the benefit of looking at the epidemiology of alcohol use. It has always seemed to me illogical to separate drug use and alcohol use. Beginning at the NIH level there is a tendency to discourage rather than encourage what could be valuable interactions and insights we could obtain from the much better established, longer existing programs in the study of alcohol. I look at the Canadian Addiction Research Foundation which merges these two in a way that I think is quite beneficial. I do not have any particular familiarity with the alcohol research literature, but to the extent that I have looked into it, I am impressed with the way that one can get insights about the other drug epidemiological issues by looking at work in this area. One example I will point out concerns epidemiological surveys in alcohol use. We would not pay much attention to an alcohol study which only dealt in frequency of alcohol use -- which lumped all daily users of alcohol together, for example. Yet, we do this routinely with regard to drug use.

We seldom attempt to collect data on the amount of use. A few years ago we talked in terms of ever-used *vs.* not ever having used. Now we talk about frequency of use and we talk about recreational, casual users and daily users. I think this rather crude approach has partly grown out of our considering the non-socially approved drugs as a moral issue, and, therefore, we tend to think of those who are and those who are not.

It is interesting to note that in other countries, not nearly as advanced as we are in methodological techniques, the little literature there is frequently reports the amount of use. Opium is almost always referred to in number of grams per day, and similar data are often presented for cannabis use. I think if we are going to describe drug use quantitatively rather than qualitatively, we will certainly have to extend our measures to include amount used and not just frequency.

There is a similar problem with regard to defining opium dependence. We talk about the number of opiate-dependent individuals as though it is continuous, rather than the intermittent phenomenon which is more frequently observed in this culture. Another thing I think we need to do is look at larger models of the factors affecting drug use. I particularly agree with the fact that we have neglected to give sufficient emphasis to the role of availability of drugs in determining use. This is rather peculiar inasmuch as we have adopted the epidemiological model, namely, the host and the agent; yet we are only concentrating on the host and ignoring the agent. I just mentioned the fact that opiate use is often, in this culture, intermittent rather than continuous and apparently this is due to an interaction with availability and other restrictions imposed by prevention attempts. In countries where opiates are used under conditions of acceptance, once dependence occurs, it does usually seem to be a continuous rather than an intermittent phenomenon, but in this culture, the low habits that we observe are explained by the availability of the drug; the on and off use also seems to be related to availability. Similarly, the maturing-out phenomenon is not typically observed in other cultures where the drug is freely available and used under conditions of acceptance. This is one example of the need to look at drug use in a larger context, at least in terms of both the host and the agent.

Incidentally, availability seems to influence the recent patterns of cocaine use as well. I have wondered why some of the older literature describes cocaine as being a very highly reinforcing drug; one which is hard to give up and one which quickly produces a strong psychological dependence. What I have heard about the more recent use of cocaine seems to show a much more casual pattern of use. I have talked to a number of people who use cocaine in this fashion, and they have reported that, indeed, if they have a supply of cocaine, it is difficult to stop until one has used up the supply. But, because it is so expensive, intermittent patterns are pretty much imposed by the expense and unavailability of the drug. So here is another apparent case where one can not really

look at the demand and use patterns without also considering the question of availability.

I was interested to note what Mark Greene had to say about using the data on incidence of use among treatment populations on a local rather than national basis, because I was under the impression from the last conference that I attended that this peaking of incidence in the late 60's or early 70's was a nationwide phenomenon. While I could understand the development of the middle class drug epidemic in general -- it spilling over into heroin use among the white youth -- I was unable to use a similar interpretation among lower socio-economic minority groups which seemed to display a similar peaking of incidence at this time. Apparently, from what has been said in the last couple of days, this is a more complicated phenomenon -- more tied to the examination of local data.

I just want to add a couple comments on future directions. From what I have heard here, the trend seems to be more towards collecting recurring trend indicators rather than the one-shot prevalence-incidence measures -- and I think this is all to the good. I have always found the little post card size survey that is annually reported on San Mateo high schools to be one of the most interesting pieces of data available -- mainly because it has been conducted now for seven years and it can make some useful observations of what is occurring over time. I think probably, on a dollar for dollar basis, NIDA got more for its money with this project than perhaps any other that it has funded. I think it started out around $5,000 or $6,000 a year. So I just want to emphasize, from my point of view, that measures of trends, collected in a standard fashion and reported on a regular basis, are one of the more important aspects of drug epidemiology. I also think that there is a need to gear these studies very much to evaluation of public policy and the planning of treatment programs. I think that probably with the reduced funding that is likely to come about in the next few years, this aspect will receive a stronger emphasis, because I think it is pretty clear that the fund giving agencies have in mind to use these types of epidemiological data primarily for evaluation and planning purposes.

Lee Robins mentioned one other aspect about the future of doing this type of work -- the confidentiality problems. Sources of data for doing good surveys are drying up because of some very restrictive confidentiality requirements which have exceeded reason in many instances. Usually nothing at all is really reported on the individual. It is group data, but the restrictions placed on the data collection are very serious in terms of limiting future epidemiological research in the drug area. Perhaps one of the outputs of this conference should be a plea for a unified stand on some recommendations for confidentiality regulations which more adequately balance the risks and benefits.

One personal thing. I have been frustrated that much of the data that has been described by Mark Greene and others is not more widely available, at least to people working in epidemiology. I

do not feel I am able to provide constructive criticism without having a broader understanding of what the data are. I do not know exactly how to bring it about, but I would like to see things like CODAP and DAWN data, and hepatitis data, and drug-related death data provided in a form that more of us could use.* Otherwise we really do not have a basis to evaluate some of the things that are being reported and to evaluate some of the conflicting conclusions we have heard at this conference.

One final thing. I have felt for some time, as a lot of you have, that for broader epidemiological work -- other than measures of incidence, prevalence, and trends -- the longitudinal study is essential, and I think that from all appearances the funding agencies have also recognized this. I think we are going in the right direction in having a number of longitudinal studies in the drug area.

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*CODAP and DAWN data are now available on a quarterly basis from NIDA.

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CONCLUSION

Louise G. Richards, Ph.D.

I have come to the realization that a better title for this conference would have been "The Measurement of the Extent and Nature of Non-Medical Drug Use." This would have avoided the problems inherent in the concept of Epidemiology and the fear of "nose counting" with nothing more illuminating involved. We have however, addressed ourselves to the broader terms not supplied in the title. I would not want to say that the size of the problem is the only consideration or the most important aspect. The purpose of gathering numbers is not merely to have data to take to Congress but to allow everyone to examine the conceptualizations and signify the order of this phenomenon compared with other phenomena in our lives. We have produced an awful lot of caveats here these two days, and I am delighted because this is what we wanted and expected. This is the best group I can think of who are capable of living with those caveats and striving for even greater sophistication. Thank you.

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