ASSESSMENT OF PRETRIAL URINE-TESTING IN THE DISTRICT OF COLUMBIA

MONOGRAPH No. 5

PERIODIC URINE TESTING AS A SIGNALING DEVICE FOR PRETRIAL RELEASE RISK

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A NOTE ON THE MONOGRAPH SERIES

Beginning in March 1984, a comprehensive pretrial urine-testing program was implemented in the criminal justice system of the District of Columbia, with funds awarded by the National Institute of Justice (NIJ). The testing program is operated by the DC Pretrial Services Agency (PSA), an independent agency of the DC Government that is charged by law with the responsibility for (1) interviewing all arrestees to determine their eligibility for pretrial release; (2) making recommendations to the court as to appropriate terms and conditions for pretrial release in all criminal cases; and (3) monitoring compliance with pretrial release conditions for all defendants, except those released on surety bond.

Unless they are charged with federal offenses or relatively minor crimes, arrestees in Washington, DC are brought to the DC Superior Court lockup. PSA tests virtually all adult arrestees coming through the DC Superior Court lockup for the presence of selected drugs in their urine at the time of arrest; these drugs are opiates (primarily heroin), cocaine, phencyclidine (PCP), amphetamines and methadone. Test results are made available that same day to PSA's in-court representatives, who are present at the bail-setting hearing to make pretrial release recommendations to the court.

Before PSA's urine-testing program began, the only release option specifically tailored to the needs of drug users had been referral to treatment. With the advent of the drug testing program, however, a new release alternative became available for drug-using defendants, namely, placement in PSA's program of periodic urine-testing before trial. Continued drug use by a defendant, as shown by the urine-test results, is considered a violation of pretrial release conditions and is reported by PSA to the court, which may impose sanctions for the violation. Because of the increased likelihood that sanctions would be imposed for such a violation of release conditions, placement in this program was considered likely to encourage defendants to forego drug use during the pretrial period. This in turn was considered likely to reduce defendants' pretrial criminality, given the findings from prior research that drug use and crime are often related.

PSA's urine-testing program has been evaluated by Toborg Associates, Inc., under a separate, parallel NIJ grant, distinct from PSA's grant for program operations. The findings from that study are the subject of a series of six monographs. Each is briefly described below, so that interested readers can quickly identify the individual monographs of greatest utility to them.

Background and Description of the Urine-Testing Program (Monograph No. 1) presents background information on drug-crime relationships generally and, in particular, in the District

of Columbia; on the workings of the DC criminal justice system; and on the overall organization and mission of PSA. Additionally, it provides a detailed description of the operations of PSA's urine-testing program, including discussions of the various components of the program and of the way in which the program was implemented.

Analysis of Potential Legal Issues (Monograph No. 2) discusses a number of areas where legal challenges conceivably could arise, stemming either from Constitutional provisions or from established doctrines in American criminal procedure. The Constitutional issues pertain to the right to be free from (1) illegal searches and seizures; (2) self-incrimination; and (3) excessive bail; as well as the rights to be accorded due process of law and equal protection of the law. These various rights stem from the Fourth, Fifth, Eighth and Fourteenth Amendments to the Constitution. Possible challenges under criminal procedure law include the adequacy of chain-of-custody procedures for handling urine specimens; the accuracy of the urine-testing technology used; and the right of the defendant to confront and rebut government witnesses and to be accorded an administrative hearing in the face of reported violations of a court order.

The Views of Judicial Officers (Monograph No. 3) presents the findings from interviews conducted approximately one year after the start of PSA's urine-testing program with 25 DC Superior Court hearing commissioners and trial judges who had recently heard criminal cases. Topics covered include the ways in which judges use PSA's urine-testing information, their views about how the current drug testing program compares with the situation that existed before PSA's program began, and their opinions about the program's impact and about the nature of the drug-crime problems in the District of Columbia.

Analysis of Drug Use among Arrestees (Monograph No. 4) presents major findings from PSA's urine-testing of arrestees brought through the DC Superior Court lockup. The monograph discusses the rates and types of drug use found; the characteristics of users of various types of drugs, as compared with non-users of drugs; how urine-test results compared with defendants' self-reports of drug use; and the pretrial release rates of users of various types of drugs.

Periodic Urine-Testing As a Signaling Device for Pretrial Release Risk (Monograph No. 5) presents a statistical analysis of the relationship between the behavior of defendants ordered by the court into PSA's pretrial urine-testing program and subsequent observation of pretrial misconduct, that is, pretrial rearrest or failure-to-appear for court. In particular, the monograph considers whether the relative success of defendants while in the urine-testing program is associated with different rates of pretrial misconduct and whether the urine-testing program

can be viewed as a "signaling device" by which defendants identify themselves--after they have been released to await trial--as posing either high or low pretrial release risks.

The Efficacy of Using Urine-Test Results in Risk Classification of Arrestees (Monograph No. 6) considers the extent to which the initial urine-test results from the lockup testing can help to classify defendants as to differences in expected pretrial misconduct (pretrial rearrest and failure-to-appear for court). The monograph presents a statistical analysis of this issue and uses a technique which takes into account the "selection bias" caused by the facts that (1) some arrestees were not tested; (2) some arrestees were not released before trial, so no pretrial misconduct could be directly observed for them; and (3) some released defendants had conditions imposed on them that may have affected their underlying propensities to engage in pretrial misconduct. The results of the analysis show the additional exlanatory power in predicting misconduct stemming from information on drug use, as determined by the initial lockup urine-test.

SUMMARY

Background

One component of the pretrial urine-testing program operated by the DC Pretrial Services Agency (PSA) consists of periodic urinalysis of selected drug-using defendants who are released to await trial. This monograph presents the results of statistical analyses of the relationship between participation in PSA's urine-testing program as a condition of pretrial release and pretrial rearrest, failure-to-appear and overall pretrial misconduct (defined as pretrial rearrest and/or failure-to-appear).

The analysis covers defendants arrested during an eight-month period (June 1984--January 1985) shortly after PSA's urine-testing program began. During that period certain drug-using defendants released to await trial were randomly assigned to three groups: one was placed in the program of periodic urine-testing before trial; a second was referred for treatment to the city-wide drug abuse treatment agency (an established practice which pre-dated initiation of the PSA urine-testing program); and the third was a control group, released with neither urine testing nor referral to treatment. Altogether, 1,874 defendants were placed in these three groups during the eight-month experimental period.

The analysis discussed in this monograph is limited to those defendants who were part of the experiment during this eight-month period; it does not apply to all arrested defendants or even to all released defendants during that time. Rather, it deals with defendants who (1) tested positive--at the lockup test, shortly after arrest--for drugs and/or who admitted drug use; (2) were not already in treatment and did not request referral to treatment; and (3) were released pretrial on non-financial conditions (i.e., other than money bail).

Two fundamental research questions are addressed:

- Was the relative success of the defendants in PSA's urine-testing program associated with different rates of pretrial misconduct?
- Did initial assignment to urine testing rather than to the treatment referral or control groups result in a lower expected rate of pretrial misconduct?

These questions stem from the role of urine-testing (and other pretrial release conditions) in changing the incentives that defendants face and, hence, their behavior. There are two types of defendants' responses that are of particular interest. A direct incentive effect occurs when defendants placed in urine testing lower their drug use and/or their level of pretrial

misconduct because they fear the consequences of failure in the pretrial urine-testing program. The direct incentive effect operates by reducing the chances for the defendant to engage in pretrial misconduct without being detected (the "detection" effect) and/or by increasing the penalties facing the defendant if misconduct is discovered (the "punishment" effect).

Besides the direct incentive effect, a urine-testing condition of pretrial release may provide a mechanism for communicating information through "signaling," that is, defendants may show--or "signal"--that they are good release risks by complying satisfactorily with the pretrial urine-testing condition. The effectiveness of a signaling device depends on its ability to separate defendants who are less likely to engage in pretrial misconduct from those who are more likely to do so. Consequently, a successful signal must be based on a behavior of defendants which is comparatively more difficult to achieve for individuals who have the greatest tendency to engage in misconduct. For example, a requirement to call in periodically during the pretrial release period would probably not serve as an effective signal of law-abiding behavior because defendants engaged in illegal activities could produce the signal (i.e., report daily by telephone) as well as anyone else without lowering the benefits of engaging in crime at the same time. A good signaling mechanism must also permit screening to be done reliably at acceptable cost.

Periodic urine testing appears to have both elements of an effective post-arrest signal for pretrial releasees. Modern technology makes screening for drug use through urinalysis both relatively precise and relatively inexpensive. It also appears likely that defendants who are more disposed to engage in pretrial misconduct will have greater difficulty eliminating or substantially reducing drug use than will those who have a lower likelihood of pretrial misconduct. This hypothesis seems reasonable because the same qualities of discipline that promote the elimination of illegal drug use should lower pretrial misconduct, and because more extreme drug dependence is itself likely to be associated with deeper involvement in crime.

One of the interesting features of pretrial urine-testing as a signaling mechanism is that it is a post-arrest signal, i.e., defendants signal their level of pretrial release risk by actions they take after pretrial release. The signaling mechanism does not depend--as many risk classification systems do--on pre-arrest variables, such as residence, employment, prior record, and so on. Although pre-arrest signaling mechanisms (or classification systems) have been widely used to separate high- from low-release-risk defendants, few post-arrest signaling mechanisms now exist at the pretrial stage.

Note that this argument that the continued use (or absence of use) of drugs serves as a post-arrest signal does not rest on the controversy over whether drug use is a cause or a correlate

of crime. Rather, the efficiency of drug testing as a signaling device rests on the hypothesis that, among arrested defendants who test positive for drugs, those who are less likely to engage in pretrial misconduct are also those who will find it easier voluntarily to reduce drug use. It may be that drug use, once reduced, will also lower the need or desire for pretrial misconduct, but it is not necessary to prove this to show that urine testing is a good signaling device.

Major Findings

Statistical analyses were performed by estimating pretrial rearrest, failure-to-appear and overall pretrial misconduct (i.e., pretrial rearrest and/or failure-to-appear) equations, including available information on the personal characteristics, criminal history, current charge and lockup test results of the defendants arrested during the eight-month study period. In these analyses defendants' "successful participation" in the PSA program of pretrial urine testing (defined as appearing as scheduled for at least four tests) versus "non-participation" (defined as failing to report at all or dropping out before the fourth test) separated defendants into two groups with large differences in expected pretrial rearrest rates, failure-to-appear rates and overall pretrial misconduct rates. These differences were large and statistically significant, indicating that successful participation in urine testing was serving as a signal of defendants' comparative pretrial release risks.

Table 1 shows that the defendants who participated in the PSA urine-testing program performed markedly better than other defendants, while those who dropped out did notably worse. Rates of pretrial rearrest, failure-to-appear and overall pretrial misconduct for defendants who participated in urine testing were about one-half the rates for defendants who dropped out of the urine-testing program. Altogether, approximately two-thirds of all defendants referred to the urine-testing program participated in it--again, defined as appearing for at least four tests. Defendants who participated in urine testing also performed better than persons referred to treatment or those placed in the control group.

The differences in rates of pretrial misconduct between defendants who participated and those who did not participate in urine testing as assigned were very large in percentage terms and most significant. This is the type of separation which is associated with signaling processes. By continuing to appear for urine testing, defendants signal that they pose low risks of pretrial misconduct. Screening is easily achieved because a simple criterion of appearing for at least four tests was sufficient to attain the large and significant level of separation found.

TABLE 1

PRETRIAL REARREST, FAILURE-TO-APPEAR AND MISCONDUCT RATES BY URINE-TESTING STATUS

Urine-Testing Status	Pretrial Rearrest Rate	Failure-To-Appear Rate	Pretrial Mis-Conduct Rate
Participated in Urine Testing	16.4%	16.9%	29.0%
Dropped Out of Urine Testing	33.1	33.4	52.6
Referred to Treatment	20.4	19.7	35.7
Placed in Control Group	20.7	18.6	34.7

Although the results of the analysis of the signaling effect seem very clear, findings from the analysis of the direct incentive effect are less clear. This is because the available data are incomplete regarding the extent to which sanctions available to the court were in fact imposed on defendants who violated the urine-testing conditions of their pretrial release. According to the data available from PSA, failure in urine testing did not result in significant adverse action for many defendants, as shown by the number of "show cause" hearings recorded as being convened and by the actions taken at those hearings.

However, DC Superior Court judges reportedly often handled violations of pretrial urine-testing conditions as "add-ons" to regularly scheduled hearings on other matters in the case, rather than in specially scheduled show-cause hearings. Typically, PSA representatives were not present at such ad hoc hearings (though they usually were present at show-cause hearings on drug-testing condition violations), and no routine reporting procedures existed at the court to assure that PSA was informed of all sanctions imposed on defendants for violations of pretrial urine-testing requirements. Hence, the data base used for the present analysis doubtless does not reflect all the sanctions that were imposed on defendants. Unfortunately, we have no way to determine the precise extent to which sanctions not reflected in the data base were in fact imposed or the effects of those sanctions. (See Monograph No. 3 in this series for a discussion of the ways in which judges reported that they imposed sanctions on defendants who violated urine-testing conditions of pretrial release.)

As discussed previously, the probability of pretrial rearrest, failure-to-appear and overall pretrial misconduct varied significantly between defendants who appeared for urine testing and those who dropped out. However, these probabilities did not vary significantly by the initial assignment to urine-testing, treatment referral or control groups. This is understandable, given the structure of the experiment, which

permitted defendants who were initially assigned to the urine-testing group subsequently to seek treatment.

Moreover, after the initial assignments of defendants to the three groups had been made by PSA for purposes of the experiment, some judges ordered defendants from the treatment referral and control groups into urine testing. This was possible because the pretrial period in Washington, DC often spans many months, with defendants making multiple court appearances during that time, prior to final case adjudication. As knowledge of the urine-testing program spread, judges began ordering defendants into it --at a point after the initial release decision but before the final disposition of the case. Ironically, such actions by the judges showed their high regard for the urine-testing program--which is a type of outcome measure for the program as a whole--but they greatly confounded the original analysis plan and may have obscured real differences in outcomes among the three initially established groups. (Appendix A to this monograph discusses the experimental procedures, as originally designed and as actually implemented.)

Conclusions and Policy Implications

In conclusion, the results of the pretrial urine-testing program operated by the DC Pretrial Services Agency suggest that such a program operates as an effective signaling mechanism. Defendants who as a group pose greater-than-average release risks, as shown by the fact that they are active drug users, can nevertheless often be safely released before trial. If such release is conditioned on periodic reporting for urinalysis, the Washington, DC experience indicates that they will soon sort themselves into two subgroups: (1) those who comply with the release conditions, by appearing as required for urine testing; and (2) those who do not comply, either by failing to appear for testing at all or by dropping out after only a few tests. Moreover, those defendants who do comply with the urine-testing requirements will have sharply lower rates of pretrial rearrest, failure-to-appear and overall pretrial misconduct (i.e., pretrial rearrest and/or failure-to-appear) than those who fail to comply.

Other implications for public policy stem from these findings. One is the need to develop additional mechanisms that can serve as risk signaling devices based on pretrial defendants' post-release behavior. Selected pilot programs are underway in various communities that could be viewed as such efforts. For example, Indianapolis, IN is experimenting with the use of electronic monitoring devices as a way to reduce jail crowding by placing pretrial defendants who would otherwise be detained until trial in the county jail, because of inability to post money bond, under electronically supervised house arrest. Compliance with the electronic monitoring requirements for a short period of time, such as 90 days, may serve as an effective signal that the

defendant could be safely released under less restrictive, non-financial conditions (e.g., third party custody).

Another example comes from Washington, DC, where certain defendants who have been unable to make bail are granted release --first to a residential halfway house, and later to the community under restrictive conditions of supervision, including urine testing. In this case good behavior in the halfway house serves as a signal that the defendant is a good candidate for supervised pretrial release in the community.

These and other approaches may eventually identify a range of post-arrest signaling devices that can be used to separate high- from low-release-risk pretrial defendants. Under such circumstances, pretrial release policies and practices could focus more on monitoring the signals provided by defendants, so that persons who identified themselves as high risks could be placed under greater restrictions, while those who identified themselves as low risks could either remain under current supervision levels or have those levels reduced. In this way a better tailoring of risk level to pretrial supervision could occur--one that would be based on defendants' <u>demonstrated actions after release</u>, rather than solely on risk predictions made at the time of arrest, based on background data about the defendants.

I. INTRODUCTION

This monograph, which is the fifth of six monographs reporting on the pretrial urine-testing project for adult defendants in Washington, DC presents a statistical analysis of the relation between behavior of defendants in the pretrial drug-testing program and subsequent observation of pretrial misconduct, that is, pretrial rearrest and/or failure-to-appear for court. The data used for the analysis were collected in connection with the adult pretrial urine-testing program conducted by the Washington, DC Pretrial Services Agency (PSA).

The Toborg Associates' research project was designed to determine the effectiveness of drug testing in a pretrial release program. PSA conducted lockup tests shortly after arrest. At the pretrial release hearing, some defendants were held or had bail set. For those defendants released on recognizance, some were selected for participation in the urine-testing experiment; these will be termed the experimental group. Selection for the experimental group was usually based on a positive lockup drug test result, although defendants who admitted drug use but tested negative for drugs were also included.

Participants in the experiment were divided randomly into three groups. One group was subjected to a urine-testing condition of pretrial release, which involved continued pretrial drug testing until case disposition. A second group was referred to the local DC Government's citywide drug abuse treatment program; and the third group was a control, released with neither urine-testing nor referral to treatment. (See Appendix A for more information on the design and implementation of this experiment.)

The sample actually subjected to a urine-testing condition of pretrial release was drawn from those (1) testing positive for drugs or admitting drug use, (2) who were not already in treatment or did not request referral to treatment, and (3) for whom release on non-financial conditions was granted. This is not a random sample of all those arrested; rather, it is conditional on a positive lockup test result, on non-financial release, and on final assignment to the experimental group for defendants who were not in treatment when arrested.

The data provide a number of outstanding opportunities to examine the relationship among drug use, pretrial misconduct (both pretrial rearrest and failure-to-appear), and pretrial urine testing. This monograph concentrates on the following two fundamental research questions:

- Was the relative success of the accused in urine testing associated with different rates of pretrial misconduct?

- Did assignment to urine testing rather than treatment referral or control result in a lower expected rate of pretrial misconduct?

These two questions arise from possible responses which theory suggests might characterize the reaction of defendants with a substance abuse problem to imposition of a urine-testing condition. This condition creates new incentives for the defendant. If these incentives are considered in terms of the basic economics of crime model, they imply that specific behavorial responses may take place in response to the urine-testing condition. The first, and easiest behavorial change to detect, is the "signaling" response in which defendants show or "signal" that they are good risks by satisfactory performance in urine testing. The second, or direct incentive effect, occurs when defendants placed in urine testing lower their drug use and their level of misconduct because they fear the consequences of failure in urine testing. Statistical tests can be performed to determine if the predicted behavorial responses were actually observed in the experimental data.

An additional important question concerning the use of initial lockup test results to classify defendants based on differences in expected pretrial misconduct is analyzed in the sixth monograph stemming from this study. It might appear that analysis of the classification possibilities of the lockup test would be relatively straightforward and should be accomplished before considering the efficacy of the urine-testing release condition. Actually, the degree of complexity is just the reverse because the use of initial test results in classification requires that inferences be made about the relative misconduct of defendants who use drugs compared to those who do not. Essentially, these inferences must be made using the entire sample of arrested defendants.

Of course, there are big differences in the ways in which defendants are handled in the pretrial release system; and these differences, which relate to expectations of misconduct, must be considered. For example, drug users are generally given more restrictive release conditions than are non-users, and these differences in release conditions--rather than drug use itself--may explain differences in misconduct. Consider what would happen if all drug users were held without bail. Then, simple statistical analysis might infer that persons testing positive for drugs engaged in lower levels of misconduct precisely because they were not free. In statistical jargon, analysis of lockup test results as a predictor of misconduct requires that unconditional estimates be made for the entire population of defendants. Such unconditional estimates involve complicated statistical techniques, which are discussed in Monograph No. 6 of this series.

The two questions concerning the relation between setting a drug urine-testing condition, such as that in PSA's adult drug-testing program, and the subsequent level of pretrial

misconduct were analyzed using the data on participants in the experiment. As noted in the discussion of classification problems, there is a potential sample selection issue involving inferences about behavior in urine testing. Participants in the experiment are not a random sample of all defendants, and results developed here are conditional on the selection process generating the sample sent to the experimental groups. No adjustment for sample selection is made in this monograph because inferences are developed explicitly for the types of defendants found in the experiment, that is, drug-using defendants who were granted non-financial pretrial release in the Superior Court of the District of Columbia. Certainly, this is the group of greatest practical interest, and there would be little point in studying effects of urine testing on defendants who did not use drugs. It is possible that behaviorial results for drug users detained until trial or released on bail could be fundamentally different, but these groups are relatively small in the District of Columbia so that overall results for drug users would be dominated by those on personal recognizance. Extension of inferences to populations with different drug use or criminal justice system characteristics is more problematic.

The Toborg Associates' research project focused on an eight-month period (June 1984--January 1985) shortly after PSA's urine-testing program began. Monograph No. 4 in this series provides additional information about the characteristics of all defendants tested during this time period and about drug use trends before, during and after this period. The present monograph--as noted previously--focuses on a subset of drug-using defendants tested during the eight-month study period, namely, those who participated in a controlled experiment involving random assignment to urine-testing, treatment referral or control groups during the pretrial release period. The following chapters of this monograph present key findings from the analysis of outcomes for those defendants, with particular attention given to the defendants who participated in PSA's pretrial urine-testing program.

II. INCENTIVE EFFECTS OF THE URINE-TESTING CONDITION ON PRETRIAL MISCONDUCT

Pretrial misconduct is defined as pretrial rearrest and/or failure-to-appear (FTA) which results in issuance of a bench warrant during the pretrial release period. The first step in statistical analysis of the urine-testing/misconduct relationship is to model the incentives created for the accused by choices of alternate release conditions. This requires consideration of alternate arguments concerning the role of release conditions in influencing pretrial misconduct. Most release conditions are designed to provide incentives for the accused to avoid pretrial misconduct. Certainly, this is the classic and probably most common understanding of the role of these conditions. The release condition produces a direct incentive effect which operates by reducing the chances for the accused to engage in misconduct without being detected (hereafter this is termed the detection effect) and/or increasing the penalties facing the accused if misconduct is discovered (hereafter this is termed the punishment effect).

The analysis of direct incentive effects follows the "economics of crime" literature, including the early work of Becker (1974), Erlich (1974), and Block and Heineke (1975). The economics of crime model assumes that criminal behavior arises from rational self-interest on the part of the criminal who seeks to maximize utility. Criminal activity increases when the individual has few alternative sources of earnings; rewards of crime are large; the probability of punishment is small; and/or the magnitude of expected punishment is small. A small number of papers, including work by Landes (1974), Manski (1978), Witte (1983), and Myers (1981) deal with this topic. Of these works, only Landes (1974) and Myers (1981) have dealt with the pretrial release period. These works provide general support for the direct incentive effects of release conditions hypothesized here, although the issue of urine-testing conditions has not been studied in the economics of crime literature. The second fundamental research question stated in the Introduction (Chapter I) of this monograph concerns the size of the direct incentive effect generated by the urine-testing condition.

In addition to the direct effect on incentives, well-established economic theory which is commonly used to explain labor market behavior suggests another role of release conditions, namely, providing a mechanism for communicating information through signaling. This is the basis of the first fundamental research question presented in the Introduction of this monograph. First developed in the classic work by Spence (1974), market signaling has been shown to characterize decisions about individuals which are made based on behavorial traits which cannot be observed directly. In the process of hiring, for example, individuals are screened based on indicators of their future performance because work habits and productivity cannot be observed directly. Such decisions are based on records of past

educational achievement and work history, which are assembled with some care by the applicant, who wishes by submitting these to the prospective employer for consideration to signal future performance. Obviously, education has some direct effect on productivity, but signaling models predict that education is also used by individuals to signal their potential and intent to be good employees. Tests of the extent to which higher education serves as a "filter" have been made by Arrow (1981), Albrecht (1981), and Riley (1979). They suggest that employers behave as if education were being used to provide extra information about applications for positions where objective measures of productivity are difficult to make.

Future behavior is difficult to predict and measure. Often the screening is based on key signal variables which applicants can produce to indicate how eager they are to receive a favorable evaluation. To the extent that these signals serve to differentiate individuals based on future performance, they form the basis for a useful screening procedure. The success of a signal is based on the ease with which it can be produced by persons who have the desired behavorial traits which cannot be measured directly.

Screening is particularly important in the criminal justice system in pretrial release or parole decisions. Information on good behavior in prison is used in parole release decision-making, and this forms a post-incarceration signal which is transmitted by the convicted (and confined) individual. Pretrial release conditions such as urine-testing may be designed to allow accused individuals to produce signals which indicate their likelihood of misconduct during the pretrial period. However, few such pretrial signaling opportunities now exist.[1]

In some cases, release conditions may operate in all the ways noted above. They may produce direct incentive effects through both detection and punishment and also provide opportunities for signaling good behavior. Certainly, this is the case with urine testing. However, these effects are not usually of equal importance, and one of the aspects of the effect on expected misconduct is usually dominant. The exercise of working through the rationale for and function of a release condition is profitable and should probably be conducted periodically for all release conditions.

A. Direct Incentive Effects And The Economics Of Crime

Direct incentive effects of release conditions are analyzed using the economics of crime literature pioneered by Becker (1974) et al. and more recently applied to pretrial misconduct by Myers (1981). These papers argue that the likelihood of participating in misconduct increases with the expected return from illegal activity, compared to the return available from legal sources. The net gain from misconduct is the difference of whatever positive benefits the accused expects to receive from

the activity less the costs expected. These costs are based on the product of the probability of punishment and the penalty expected in case of punishment. The expected costs of misconduct to the accused may be expressed as the product of the probability of punishment, PR(p), and the expected level of the penalty given that one is imposed, p*. Thus the expected cost of misconduct is given by: E(c)=PR(p)p*. The detection effect discourages misconduct by raising costs, and hence lowering net gain, through an increase in the probability of punishment, i.e., by raising PR(p). The punishment effect discourages misconduct by raising the costs of misconduct through an increase in the expected penalty, p*.

Consider the following examples of restrictive release conditions designed to operate on PR(p) and on p*: Reporting to the pretrial services agency, adhering to a curfew, living at a certain residence and similar requirements placed on the released defendant lessen opportunities for misconduct and particularly for avoiding detection. Thus, they raise PR(p). In contrast, a "no rearrest" condition has no effect on PR(p) but it would raise p* if the accused knew that there would be no release if any pretrial misconduct were detected. These are separate and specific instances of the way in which restrictive release conditions operate to raise the expected cost of misconduct and to deter misconduct. The quantitative effects on misconduct, of course, are fundamentally an empirical issue. The theory allows one to analyze what mechanism the condition is attempting to employ, but the quantitative significance depends on how the accused perceives the increase in cost and on sensitivity to such cost increases.

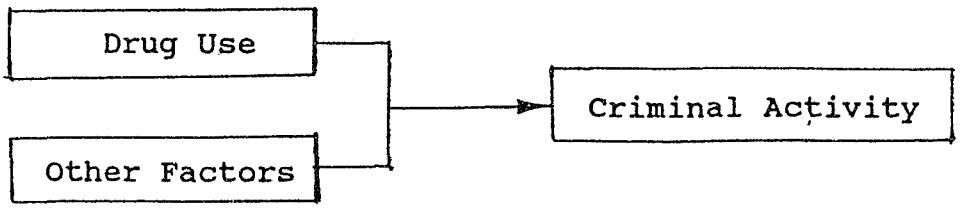
The urine testing performed under PSA's adult drug-testing program could be interpreted as a release condition designed to have direct incentive effects. Analysis of the way in which the detection effect can cause defendants to lower drug use can clearly be developed based on the economics of crime model. To the extent that defendants believe that evidence obtained from the urine testing raises the probability of punishment, PR(p), they will tend to lower their misconduct levels. For example, in the PSA program, defendants released with urine-testing conditions may have believed that failure in urine testing would increase the probability that the terms of their continued pretrial release would be modified, and, in some cases, that they could be detained during all or part of the pretrial period. Thus, drug testing raises the probability that failure to remain drug-free would be detected and release conditions would be tightened and thus made less desirable. The extent to which failure in urine testing was actually used to modify release conditions is an empirical question. If defendants did not perceive adverse consequences arising from failure in the program, then the incentive effect should be small.

Urine testing could have a punishment effect which lowered illegal drug use if defendants testing positive or refusing testing faced harsher outcomes, or if a failure in urine testing

could be used as an argument for more severe punishment in the future (i.e., sentencing enhancement). In the District of Columbia, performance in urine testing was not to be used in determining guilt or innocence on the underlying charges, including drug charges. Such use would have raised legal challenges of self-incrimination through participation in testing. However, continued positive urine tests while on pretrial release could result in the imposition of sanctions by judges for violating pretrial release conditions. Moreover, judges reported in interviews that they often considered an individual's record of compliance with all pretrial release conditions--including urine testing as well as such other conditions as curfews, residence requirements, etc.--when making post-conviction decisions regarding appropriate sentences. Indeed, some judges observed that an individual's compliance with release conditions during the period after conviction and before sentencing was an excellent indicator of the person's likely success on probation and, consequently, was factored into sentencing decisions. (See Monograph No. 3 in this series for more information on this point.) Thus, there are several potential ways in which urine testing could have a punishment effect. Unfortunately, as discussed subsequently in this monograph, the data available for analysis of this effect are incomplete and, hence, a comprehensive analysis of this particular point could not be undertaken.

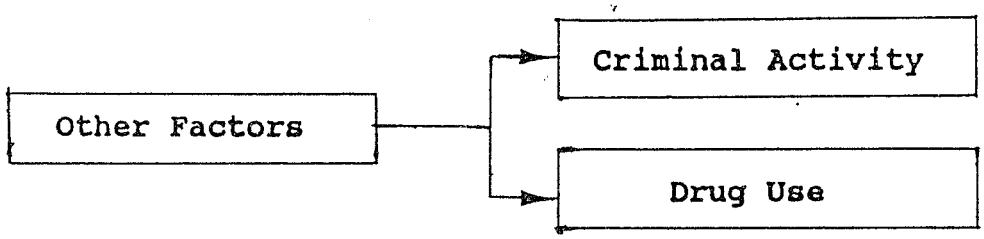
Another, more general, argument is often made for the potential effect of urine testing on misconduct. This rests on the possibility that there is a particular causal relationship between drug use and certain income-generating property crimes, such as robbery, burglary, fraud, theft, etc. A urine-testing condition which reduces illegal drug use through the detection or prevention effects discussed above could lower other forms of misconduct--both pretrial crimes of various types and failure-to-appear--if there is a causal link between illegal drug usage and misconduct. To make such an argument, one would need to establish a close causal relationship, such as that which some observers suggest holds between property crimes and substance abuse. The argument may be expressed as shown in Figure 1. Some would argue that this type of causal relationship characterizes drug-crime interaction. Drug addiction, by lowering the ability to do legal work, introducing the addict to opportunities for criminal activity and/or raising the need for immediate income, could directly cause an increase of criminal activity.

FIGURE 1
ONE VIEW OF THE RELATIONSHIP BETWEEN DRUG USE AND CRIME



Unfortunately, causal relationships are difficult to distinguish from situations of joint causation, such as that in Figure 2, following. Arguments for joint causation of crime and drug use could be based on personality or environmental factors that cause persons inclined to criminal activity to use drugs also. For example, the argument made above for a drug use/job loss/crime sequential relationship could be restated in the context of the joint causation in Figure 2. The same personality factors which cause poor performance at work or in school may also cause individuals to ignore the consequences of substance abuse. One such personality factor that has been identified is a high degree of "present orientation" or a tendency to discount consequences of dysfunctional behavior which occur sometime in the future. To the extent that Figure 2 rather than Figure 1 accurately describes the situation, urine testing will not have a large behavior modification effect on criminality because the causes of criminal activity would not be monitored. Sorting out the differences between causation, Figure 1, and mere correation, Figure 2, is always most difficult. While urine testing may function through direct behavior modification to lower misconduct in the form of illegal drug use, the effects of urine testing on general misconduct are an empirical issue, with theory from the economics of crime literature yielding few strong conclusions.

FIGURE 2
ANOTHER VIEW OF THE RELATIONSHIP BETWEEN DRUG USE AND CRIME



B. Urine-Testing Conditions And Signaling By Accused Persons

The urine-testing condition of pretrial release provides defendants with an opportunity to indicate that they are good risks through signaling. By succeeding in urine testing, the defendant demonstrates a willingness and ability to report as scheduled for tests and to avoid substance abuse. Defendants may regard this as an opportunity to demonstrate that, in spite of poor prior performance, they are worthy of nonfinancial pretrial release, i.e., they use urine testing as a signaling opportunity. Pretrial release agencies and magistrates, in turn, may find the urine-testing condition provides one of the few screening devices available to them. In observing compliance with the urine-testing condition, they can screen good from poor risks based on post-arrest but pretrial behavior. Currently, there are few effective, inexpensive screening mechanisms available for use during the pretrial period.

In the market signaling model developed by Spence (1974), the effectiveness of a screening device depends on its ability to separate defendants who are less likely to engage in misconduct from those who are more likely. Success in separating defendants based on likelihood of misconduct depends on the relation between the signal which is observed in the screening process and the misconduct decision. A successful signal is based on a behavior of defendants which is more difficult for individuals who have the greatest tendency to engage in misconduct. For example, a requirement to call in periodically during the pretrial release period is probably so trivial that it would not serve as a signal of law-abiding behavior because defendants engaged in illegal activities could produce the signal (i.e., report daily by telephone) without lowering the benefits of engaging in crime. Indeed, compliance with this condition might even be higher among those engaged in crime, in order to avoid drawing attention to themselves. Clearly, screening based on such a trivial reporting requirement would not be expected to be effective in separating high- from low-crime-risk defendants.

In order for a screening procedure to work effectively, the difficulty of producing the signal has to be greater for the people engaging in misconduct, and the signal itself must be detected and screening performed reliably at acceptable cost. Tests of the signaling hypothesis by Ripley (1979) indicate that education is used as a signal by employers, as discussed previously. Workers who are brighter and more industrious complete school with less difficulty than those who are less intelligent, organized, and/or motivated. One reason that people invest in education is to signal that they are intelligent, organized, and motivated workers. As long as education level is easily evaluated (by knowing the school and the grade level reached), education serves as an effective signal, and employers screen applicants based on education. Education and/or current employment status may also be used as the basis for setting differential release conditions for the accused. Education and employment records are <u>pre-arrest signals</u>, that is, the education or employment record was assembled before arrest. It is most important to distinguish such pre-arrest signals, which could be used as part of a formal statistical model of the determinants of pretrial misconduct, from <u>post-arrest</u> signals, which would serve as the basis for release conditions and allow the accused to produce information on likely conduct <u>after pretrial release.</u> Clearly, there are many variables that could serve as pre-arrest signals, including most of those used in labor markets to judge the reliability of employees. However, there are relatively few good opportunities for <u>post-arrest</u> signal variables in the pretrial release process.

As noted above, signaling is important in the corrections system. Consideration of good behavior as a mitigating factor in the parole release decision can be interpreted as allowing the sentenced and incarcerated individual to signal that future misconduct is unlikely. The "time off for good behavior" rule provides an effective post-sentence signal to the extent that

good behavior is easily observed and the difficulty of maintaining a record of good behavior is greater for those most likely to engage in post-release crime. The use of a clean prison record as a signal is probably based on the notion that inmates who are less likely to commit post-release crime find it easier to adjust to prison life and avoid problem behavior while confined in prison.

If a pretrial release condition is to be used as a post-arrest signal, then it should follow that compliance with the condition is easily determined (screening should be relatively cheap and accurate) and that the difficulty of producing the signal should be greater for defendants who are more likely to engage in misconduct. In the case of telephone reporting, screening cost is low but major differences in compliance difficulty for low- and high-risks do not appear to exist. It might be argued that release conditions which require some form of participation in community service could function as post-arrest signals. This would follow if persons engaging in pretrial misconduct experienced more difficulty or got smaller satisfaction from such work than defendants who were free of misconduct. However, such a relationship would be difficult to demonstrate. Perhaps a release condition based on continuing to work or securing regular employment could function as a post-arrest signal, again based on the notion that defendants involved in pretrial misconduct would experience smaller rewards from employment or find the costs higher. But continued employment is based largely on pre-arrest conditions, and it may be difficult to secure a new regular position while awaiting trial.

Periodic urine testing appears to have both elements of an effective post-arrest signal for pretrial releasees. Modern technology makes screening both precise and relatively inexpensive. It also appears likely that defendants more disposed to engage in pretrial misconduct have more problems eliminating or substantially reducing drug dependence than those who have lower likelihood of pretrial misconduct. This hypothesis seems reasonable because the same qualities of discipline that promote the elimination of drug use should lower pretrial misconduct and because more extreme drug dependence is likely to be associated with deeper involvement in crime.

Note that this argument for use of drugs as a post-arrest signal does not rest on the controversies over drug use as a <u>cause</u> versus <u>correlate</u> of crime. Rather, the efficiency of drug testing as a screening device and drug abstinence as a signaling device rests on the hypothesis that, among arrested persons who test positive for drugs, those who are less likely to engage in pretrial misconduct are also those who will find it easier to reduce drug dependence. It may be that drug dependence, once reduced, will also lower the need or desire for misconduct--but this is a prevention effect, and it is not necessary to prove this to argue for testing as a screen. Thus, urine testing may provide the accused with an opportunity for

post-arrest/pretrial signaling regardless of whether the drug-crime relationship in Figure 1 or Figure 2, above, is correct.

These differences between tests for efficacy of urine testing in a signaling versus a behavior modification context can be seen by examining the causal str :ture of the arguments for direct behavior modification versus post-arrest signaling. The behavior modification argument assumes that the probability of misconduct depends directly on the costs of misconduct as expressed in the product $E(c) = PR(p)p^*$ discussed above. In order for a release condition to lower misconduct through direct behavior modification, one must show that the condition causes a significant increase in either $PR(p)$ or p^* or both terms. Such causation is difficult to demonstrate. In contrast, showing that a release condition can serve effectively as a post-arrest signal merely requires demonstrating that the condition is correlated with variables which measure either the costs or the benefits to the defendant from misconduct. This is a much weaker condition. The signaling variable does not have to play a direct role in $PR(p)$ or p^*. Rather, it may merely be correlated with a variable that enters these expressions or with a variable that enters the calculus of crime on the side of gain to the accused. For example, lack of ability to succeed in drug testing may be associated with factors that generate criminality, such as present-orientation, or the preference for short-run pleasure even if the consequences are painful in the long run.

In summary, restrictive release conditions can function through the detection or punishment effects to lower incentives for pretrial misconduct, particularly that directly related to substance abuse, or as a post-arrest signal of future behavior while on pretrial release. Urine testing could possibly be used in all three ways. However, in order to test the use of urine testing in a punishment mode, it would be necessary to provide harsh sanctions for defendants who tested positive or failed to appear for testing. It is difficult to determine from the data available from PSA and court records the frequency with which significant sanctions were imposed.[2] Thus, it is difficult to test the hypothesis that urine testing altered misconduct significantly through the punishment effect (although this may have happened). However, the structure of the PSA drug testing program experiment seems ideal for testing hypotheses about the use of urine testing in a post-arrest/pretrial signaling context.

C. Sample Structure And Inferences About Urine Testing And Pretrial Misconduct

In using the PSA adult drug-testing program data (or any other data produced by the criminal justice system), it is important to consider the nature of the sampling process that produced the sample being analyzed. In the criminal justice system--especially at the pretrial stage--regard for the rights of the accused often prohibits the types of controlled

experiments possible in other contexts. The PSA drug-testing program is no exception to this rule. While initial drug testing in the lockup is performed on most arrested persons, the subsequent flow of persons through the pretrial release process is based on choices made by both the judges and the defendants. The group of accused persons selected to participate in the experimental part of the PSA drug-testing program (i.e., the group randomly assigned to urine testing, treatment referral, or control) during the eight-month study period was not representative of all arrested persons in the District of Columbia during this period of time.

A first issue, and one that is usually overlooked in studies of the pretrial release system, is whether the inferences are being made for a random sample of all arrest cases or for a random sample of all arrested persons. The sampling implications for these two situations differ in the pretrial release system because, over any given period of time, some persons are arrested more than once. Thus, a sample of all arrests during a three-month period, for example, will include a number of instances in which there are several separately papered arrests for a single individual. If the goal of the study is to make inferences about the average characteristics of an arrest that leads to prosecution, then such multiple arrests create no problem for sampling. The average number of pretrial rearrests per arrest case, for example, would be the total number of pretrial rearrests observed for the sample of arrest cases divided by the number of arrest cases.

However, if the study is to make inferences about arrested persons, then problems are created by multiple papered arrests of a single individual because such persons appear several times in the sample. In computing the average number of pretrial rearrests per arrested person, defendants with multiple papered arrests should only be counted once--presumably the first time they are arrested. Thus, a person-based as opposed to a case-based sample should be used to make inferences about pretrial rearrests by arrested persons. In the person-based sample, subsequent papered arrests for pretrial crime by arrested persons would be dropped from the initial sample of persons--and considered only as subsequent arrests for that person--so that each defendant appeared in the sample only when first arrested. While this may appear to be a small difference in sampling procedure, the difference between the average number of pretrial rearrests per arrest case and the average number of pretrial rearrests per defendant may be significant, because some persons may be rearrested 10 or more times during a six month period. Such individuals would appear 10 times in case-based sampling but only once in defendant-based sampling. (Toborg and Kirby, 1984.)

Another problem in making inferences is that the handling of accused persons is often based on expectations of their future misconduct. For example, at one extreme, defendants are released on recognizance without conditions based on the expectation that they are unlikely to engage in pretrial misconduct; at the other

extreme, defendants are held in pretrial detention or have high money bond set. Other groups of defendants fall between these extremes. Care must be taken in comparing the subsequent reaction of these groups to the criminal justice system because they have already been selected based on expected future misconduct. For example, comparing the average rate of misconduct for those released with conditions with that of those released on recognizance without conditions could not serve as the basis for an evaluation of the effects of release conditions on misconduct. Persons with high expected misconduct rates are systematically, rather than randomly, given more restrictive release conditions. If they subsequently engage in misconduct at a higher rate, this does not necessarily demonstrate that the release conditions were ineffective in deterring such behavior.

Such sample selection problems are inevitable in the criminal justice system, which does not use random assignment procedures to determine pretrial release conditions. It is possible to make general inferences about the effects of release conditions using such selected samples, but special attention must be given to both the statistical techniques used and to the way in which statistical results are applied. In presenting statistical results below, special attention has been given to these sampling issues and to limitations on the inferences or conclusions that may be made. These limitations should be acknowledged before applying the results in a policy context. As noted above, it is generally easier to make inferences about the effectiveness of urine testing as a signaling device than as a direct instrument of behavior modification through either the detection or punishment effects.

III. EMPIRICAL FINDINGS ON URINE TESTING AND PRETRIAL MISCONDUCT

The results presented here are stated in terms of a series of findings which involve successive levels of complexity in either the effects of urine testing or the statistical procedures used to estimate these effects. Some care will be taken to identify the sample or subsample of accused persons who were the object of each statistical test and to the range of defendants for whom a particular generalization has been demonstrated. Of course, all results are generated for accused persons in the Washington, DC pretrial release system. Various institutional aspects of that system which could have a bearing on the results are reviewed elsewhere. (See Monograph No. 1 in this series.)

The eight-month experiment conducted by the PSA adult drug testing program provides a unique opportunity to evaluate a number of hypotheses that bear on previous literature on the relationship between crime and drug usage. Of course, the ultimate goal of the statistical analysis is to assess the role of urine testing in direct deterrence and signaling applications designed to reduce pretrial misconduct.

A. Assignment to Urine Testing, Treatment Referral or Control Groups Was Random

Analysis of the characteristics of defendants assigned initially to the three groups in the experiment indicates a random assignment. While there are very significant differences in demographic and criminal career characteristics of arrested persons in the sample as a whole, these differences are not significantly related to the initial group assignment.

B. Type and Combinations of Drugs Detected by the Lockup Test Were Significantly Related to Differences in the Probability of Pretrial Misconduct

This result was developed only for participants in the eight-month experiment and, hence, refers to relative degrees of pretrial misconduct among individuals already testing positive for at least one drug and assigned to the experimental group subject to urine testing, treatment referral, or control. (Unconditional estimates of misconduct for all arrested persons are needed if lockup test results are to be used as part of a general release classification scheme; this is the topic of Monograph No. 6 on classification.)

First, consider the partial relationship between lockup test results and the probability of pretrial rearrest. This was determined using estimates of a pretrial rearrest equation which are presented in full in Appendix B and are summarized in Table 1, below. The numbers in the table under "size of

deviation" refer to the increase in probability of pretrial rearrest associated with different drug test results over the probability associated with the reference drug test outcome. In the case of Table 1, this reference drug test finding is positive for cocaine and negative for all four other drugs.

TABLE 1

DIFFERENCES IN PRETRIAL REARREST PROBABILITY ASSOCIATED WITH LOCKUP TEST RESULTS

Note: Mean Probability of Pretrial Rearrest Is 0.204; Results Are Presented As Partial Effects Of Deviations From This Mean For Each Lockup Test Result

Tested Positive For	Size Of Deviation	Standard Error
Cocaine Only (Reference Group)	0.000	0.000
Amphetamines Only	-0.052	0.101
Methadone Only	0.142	0.165
Opiates Only	0.045	0.041
PCP Only	-0.048*	0.031
Opiates & Cocaine Only	0.035	0.037
PCP & Cocaine Only	0.041	0.037
Opiates & PCP Only	0.017	0.042
Combinations Other Than Above	0.026	0.077

* indicates significantly different than reference group at 20% level

Generally, as shown in Table 1, differences in the probability of pretrial rearrest associated with the particular drug combinations found in the lockup testing were not statistically significant. For example, the coefficient estimate for opiates only indicates that those testing positive for opiates and negative for all other drugs had a probability of pretrial rearrest which was 4.5 percentage points higher than the reference group, i.e., defendants testing positive for cocaine only. However, this difference was not statistically significant. The -0.048* entry for "PCP Only" shows that those testing positive for PCP only have expected pretrial rearrest rates 4.8 percentage points lower than the cocaine only reference group and that this result was significant. While 4.8 percentage points may not appear large, this is almost 25% of the mean pretrial rearrest rate of 20.4%. Thus, in terms of the frequency of pretrial rearrest, the differences in expected rates associated with some lockup test results were quite consequential. These results were developed for participants in the experiment and, hence, are conditional on testing positive for some drug initially. In Table 1, that reference drug combination is cocaine only. Note that pretrial rearrest rates

for cocaine only users are relatively low but not significantly different than other drug combinations.

Second, consider the partial relation of lockup test results with failure-to-appear which are displayed in Table 2. Again, these results are comparisons among accused persons who participated in the experiment and, hence, who all tested positive for one or more drugs. The differential effects of type-of-drug-test-result on probability of failure-to-appear are far more consequential than those found above for pretrial rearrest. The reference drug test group in Table 2 again is defendants testing positive for cocaine and negative for the other four drugs. In interpreting the results in Table 2, note that the mean probability of failure to appear is 0.197 or 19.7%. Thus, the size of the change in estimated probability of failure-to-appear attributable to differential lockup test results is relatively large for all of the cases in which statistically significant differentials were observed. For example, the result for "PCP Only" of -0.119*** indicates that the expected probability of pretrial failure-to-appear was 11.9 percentage points lower than that for those testing positive only for cocaine. This is a very large differential effect: 11.9 percentage points is about 60% of the mean of the dependent variable, 19.7%. Three of the groups testing positive for a single drug--amphetamines, methadone, and PCP only--and the PCP/opiates combination have much lower expected probability of failure-to-appear than the "cocaine only" test group.

TABLE 2

DIFFERENCES IN FAILURE-TO-APPEAR PROBABILITY
ASSOCIATED WITH LOCKUP TEST RESULTS

Note: Probability Of Failure-To-Appear Is 0.197;
Results Are Presented As Partial Effects Of Deviations
From This Mean Probability For Each Lockup Test Result

Tested Positive For	Size of Deviation	Standard Error
Cocaine Only (Reference Group)	0.000	0.000
Amphetamines Only	-0.197**	0.099
Methadone Only	-0.247*	0.162
Opiates Only	-0.036	0.040
PCP Only	-0.119***	0.031
Opiates and Cocaine Only	0.006	0.036
PCP and Cocaine Only	-0.042	0.036
Opiates and PCP Only	-0.122***	0.041
Combinations Other Than Above	-0.045	0.074

* indicates significantly different than reference group at 20% level
** significance at 10% level
*** significance at 5% level

A general analysis of the partial relation between lockup test results and probability of pretrial misconduct, holding personal characteristics and criminal history constant, is indicated in Table 3, below. The reference group is defendants in the experiment who tested positive for cocaine only. Given that pretrial misconduct is based on either or both the occurrence of pretrial rearrest or failure-to-appear, the results in Table 3 are not the simple sum of the previous two tables. There are three categories of test results which are associated with significantly lower probability of pretrial misconduct than the cocaine only group. These are amphetamines only, PCP only, and, to a lesser extent, those testing positive for the combination of PCP and opiates only. All other test results are associated with levels of pretrial misconduct which do not differ significantly from those of the cocaine only group.

TABLE 3

DIFFERENCES IN PROBABILITY OF OVERALL PRETRIAL MISCONDUCT BASED ON LOCKUP TEST RESULTS

Note: Mean Probability of Pretrial Misconduct Is 0.389; Results Are Presented As Partial Effects of Deviations From This Mean Probability Associated With Each Lockup Test Result

Tested Positive For	Size of Deviation	Standard Error
Cocaine Only (Reference Group)	0.000	0.000
Amphetamines Only	-0.204*	0.119
Methadone Only	-0.064	0.195
Opiates Only	-0.022	0.048
PCP Only	-0.160***	0.037
Opiates and Cocaine Only	0.021	0.043
PCP and Cocaine Only	-0.014	0.044
Opiates and PCP Only	-0.093*	0.050
Combination Other Than Above	-0.044	0.090

* indicates significantly different than reference group at 10% level
*** indicates significantly different than reference group at 1% level

Overall, these results indicate significant differences in pretrial misconduct associated with drug use as indicated in the lockup test. These differences are more pronounced for failure-to-appear than for pretrial rearrest. Overall, those accused persons in the experiment who tested positive for PCP only, PCP in combination with opiates, or amphetamines only had lower expected probabilities of pretrial misconduct.

These results should not be used as a general comment on the ability of pretrial drug testing to differentiate all accused persons based on probabilities of pretrial misconduct. This is so because they were developed for the selected sample of defendants participating in the eight-month experiment during which certain drug users were placed in PSA's pretrial urine-testing program, referred to treatment or served as a control group.

C. Continued Appearance for Urine Testing Was Associated with Lower Probability of Pretrial Misconduct

Defendants initally assigned to urine testing--and certain other defendants whose initial assignment was to treatment referral or control but who were subsequently ordered by the court into the urine-testing program--were given regular drug testing. For both groups, those in drug testing because their initial assignment was urine testing and those in drug testing after initial assignment to treatment referral or control, the partial effect of continued appearance at tests on the probability of pretrial misconduct was negative. For those in urine testing this negative effect was large and statistically significant indicating that success in urine testing was being used as a signal. Continued appearance was defined as appearance at four or more tests. Of course, subsequent rearrest could have an effect on continued appearance; however, this result persisted even after adjustment for rearrest.

The criterion used to define "continued appearance" is four or more appearances for testing. Of the 455 defendants under urine testing who were analyzed, 299 or about two-thirds qualified under this criterion. The chances that appearance failure in urine testing was due to interruption by a speedy trial were eliminated by considering only cases which took more than 30 days to reach disposition.

Statistical tests were performed by estimating pretrial rearrest, failure-to-appear, and overall pretrial misconduct equations including available information on the personal characteristics, criminal history, current charge, and lockup test results of the accused. The full results are presented in Appendix B. The summary numbers in Table 4 indicate the decrease in the expected probability of pretrial rearrest compared to the reference group, that is, defendants assigned to urine testing who did not meet the appearance criterion of three tests and are labeled "dropped out." For example, the coefficient of -0.167*** for the group labeled "Urine-Testing/Appeared" means that the expected probability of pretrial rearrest for those assigned to urine testing who appeared for four or more tests was 16.7 percentage points lower than for the reference group, i.e., those assigned to urine testing who appeared for fewer than four tests. This difference of almost 17 percentage points associated with appearance for testing is approximately 80% of the mean expected

probability of pretrial rearrest of 20.4%. Thus, participation in urine testing separates defendants into two groups with large differences in expected pretrial rearrest rates.

TABLE 4

DIFFERENCES IN PROBABILITY OF PRETRIAL REARREST, FAILURE-TO-APPEAR, AND OVERALL PRETRIAL MISCONDUCT BASED ON APPEARANCE FOR URINE TESTING

Note: Results Are Presented As Decreases In Expected Probability Compared To Those Assigned To Urine Testing Who Dropped Out By Failing To Appear For Tests

Urine-Testing Status/Appearance	Increase in Probability	Standard Error
Results For Pretrial Rearrest, Mean Probability = 0.204		
Urine-Testing/Dropped Out (Ref. Group)	0.000	0.000
Urine-Testing/Appeared	-0.167***	0.040
Treatment Referral Group	-0.127***	0.035
Control Group	-0.123**	0.041
Results For Failure-To-Appear, Mean Probability = 0.197		
Urine-Testing/Dropped Out (Ref. Group)	0.000	0.000
Urine-Testing/Appeared	-0.165***	0.039
Treatment Referral Group	-0.137***	0.034
Control Group	-0.148***	0.040
Results For Overall Pretrial Misconduct, Mean Probability = 0.357		
Urine-Testing/Dropped Out (Ref. Group)	0.000	0.000
Urine-Testing/Appeared	-0.236***	0.047
Treatment Referral Group	-0.169***	0.041
Control Group	-0.179***	0.047

*** indicates significantly different than reference group at 1% level

Note that both the defendants referred to treatment and those in the control group had expected pretrial rearrest rates about 12.5 percentage points below that for those assigned to urine testing who did not appear for four or more tests. The difference in expected pretrial rearrest rate between those appearing for tests and defendants referred to treatment or in the control is about 4 percentage points (16.7-12.5). While this may not appear large, 4 percentage points is 20% of the 20.4% mean for the entire sample.

For both pretrial rearrest and failure-to-appear, individuals in urine testing who appeared for testing had probabilities between 16 and 17 percentage points lower than

urine-testing dropouts. For overall pretrial misconduct (pretrial rearrest and/or failure-to-appear), the individuals appearing in urine testing had expected probabilities over 23% lower than the urine-testing dropouts. This difference is approximately two-thirds of the mean pretrial misconduct rate.

The results in Table 4 can be transformed into simple rates of pretrial rearrest, failure-to-appear and pretrial misconduct, as shown in Table 5. For all three outcome measures, the defendants who appeared for urine testing--defined as appearing for four or more tests--performed markedly better than other defendants, while those who dropped out did notably worse. Rates of pretrial rearrest, failure-to-appear and pretrial misconduct for defendants who appeared for urine testing were approximately one-half the rates for defendants who dropped out of the urine-testing program. Defendants who appeared for urine testing also performed better than persons referred to treatment or those placed in the control group.

TABLE 5

PRETRIAL REARREST, FAILURE-TO-APPEAR AND MISCONDUCT RATES BY URINE-TESTING STATUS

Urine-Testing Status	Pretrial Rearrest Rate	Failure To-Appear Rate	Pretrial Mis-Conduct Rate
Appeared for Urine Testing	16.4%	16.9%	29.0%
Dropped Out of Urine Testing	33.1	33.4	52.6
Referred to Treatment	20.4	19.7	35.7
Placed in Control Group	20.7	18.6	34.7

Some of the individuals initially assigned to treatment referral or control did participate in urine testing. There were only 119 defendants fitting this category, and a variety of circumstances could be responsible for their appearance in urine testing. Some of these defendants were court-ordered into urine testing. Thus, they could have been transferred into the urine-testing program based on pretrial misbehavior of some sort, and a strict interpretation of the effects of participation in urine testing on those not initially assigned to urine testing cannot be made. Nevertheless, it is possible to estimate the partial effect on pretrial misconduct of appearance at urine testing for those in treatment referral and control who were subsequently assigned to urine testing. In all cases, appearance in urine testing was associated with a lower rate of pretrial misconduct. However, the effect was non-significant for failure-to-appear and overall pretrial misconduct. For pretrial rearrest, appearance in urine testing lowered the predicted rate by 8.7 percentage points, which was significant at the 15% level.

Taken together, the evidence clearly points to continued appearance in urine testing as an indication of lower rates of overall pretrial misconduct. The differences in predicted rates of misconduct between the two groups--those appearing and those not appearing for urine testing as assigned--are very large in percentage terms and most significant. This is the type of separation which is associated with signaling processes. By continuing to appear for urine testing, the defendants signal that they pose low risks of pretrial misconduct. Screening is easily achieved because a simple criterion of more than three tests was sufficient to achieve the large and significant level of separation observed here.

D. For Those Appearing As Scheduled for Urine Testing, Negative Tests Lowered Pretrial Misconduct

Those defendants assigned to urine testing who appeared for more than three tests can be further divided into two groups based on the ratio of positive tests. In order to secure a more or less equal division, the standard of less than 25 percent positive tests for any drug was used to characterize the defendants appearing in urine testing who are termed "clean," with those exceeding the 25 percent standard termed "dirty." There is no special significance in the 25 percent standard except that, by using it, the number of dirty defendants was almost equated to the number of clean defendants.

The partial effect of falling in the group of defendants in urine testing who appeared and were clean was judged by estimating equations for the various types of pretrial misconduct and examining the coefficient of the clean dummy variable. This indicates the additional fall in the probability of pretrial misconduct associated with testing clean as opposed to dirty. Not surprisingly, the estimated coefficient of the clean variable is always negative, but it is nonsignificant in failure-to-appear and pretrial rearrest equations and only marginally significant in the overall pretrial misconduct equation, although the magnitude of its effect on overall pretrial misconduct, 7.4 percentage points lower than those with dirty test results, is quite consequential.

While the statistical significance of the effects of having a high ratio of negative tests on pretrial misconduct is rather low, these results are consistent with signaling behavior. Defendants signal their low risk status by moderating or eliminating drug use, so that the proportion of negative tests is high.

The results could also be interpreted as reflecting an underlying connection between pretrial misconduct and drug involvement. Those defendants having the lowest level of addiction to drugs may be best able to avoid positive tests.

Similarly, the lack of strong drug addiction may reduce the level of criminal behavior to the extent that there is a causal connection between drug use and crime. The nonsignificant estimated coefficient of the clean variable in the pretrial rearrest equation is not consistent with the hypothesis of a close causal relation of drug usage and crime for this group of defendants.

Overall, the results in Sections B-D, above, provide strong support for the hypothesis advanced in the first research question that urine-testing conditions evoke a signaling response in defendants and that they can be used as a post-arrest screening device in a pretrial release system.

E. Failure in Urine Testing Did Not Result in Significant Adverse Action, As Identified in the Data Available for Analysis

The direct incentive effect operates through the punishment and detection effects which are based on the perception by defendants that failure to comply with the release conditions will result in direct adverse action. There were limitations placed on the use of urine-testing results in the pretrial release decision and in subsequent court action involving the defendant. First, individuals initially assigned to urine testing were allowed to choose a treatment referral alternative without prejudice against them. This introduced an element of voluntarism into the urine-testing population that should have affected the incentive effects of the condition.

There is also an empirical question concerning the consequences of failure in urine testing for the defendant which is examined here. Failure in urine testing could take several forms: failure to appear for an initial test, failure to continue to appear, or failure to produce clean tests. One initial measure of the consequences of failure in urine testing is observation of the number of "show cause" hearings held. This is shown in Table 6. One important limitation on the results in this section is that the system for collecting data on subsequent holding of "show cause" hearings was not perfect. It is likely that a number of these hearings were missed. However, these omissions should occur at a uniform rate across categories in Table 6, for example. Thus, all percentages in the final column are likely to be reduced proportionally below their true values. Hence, it seems reasonable to conclude that, for most categories of failure in urine testing except when the accused switched to treatment referral, a hearing was more likely to be held than in cases where a failure was recorded. However, the availability of the treatment option may have substantially reduced the direct incentive effects of the urine-testing condition.

TABLE 6

FREQUENCY WITH WHICH "SHOW CAUSE" HEARINGS WERE HELD BASED ON COMPLIANCE WITH URINE-TESTING

Release Status of Defendant	Total Number	No. of Hearings	Frequency of Hearings
Initially Referred to Treatment	959	14	1.46%
Initially in Control	345	5	1.45%
Succeeding in Urine Testing	155	1	0.65%
Failed Regular & Intensive Urine Testing	75	11	14.70%
Failed To Report For Testing	143	20	13.95%
Failed Regular Urine Testing & Referred to Treatment	78	0	0.00%

A second indication of the direct incentive effects associated with the urine-testing condition is the nature of the hearing outcomes for the cases presented above. Unfortunately, the final outcome from a significant percentage of the hearings was not recorded or simply did not fit any of PSA's pre-established categories for capturing this information. Nevertheless, the data that are available on the outcomes of show cause hearings reinforce the general impression formed by Table 6 that failure in urine testing did not carry sufficient negative consequences to evoke a significant punishment effect, at least based on the available data regarding show cause hearings and their outcomes. This raises the strong possibility that the direct incentive effect, which is the object of the second fundamental research question presented in the Introduction to this monograph, is not large for the urine-testing condition as implemented. However, as stated previously, this conclusion is limited by the incomplete nature of the data available for analysis of this point. (For more information on this data problem, see the discussion in Chapter II, Section B, regarding the imposition of sanctions for defendants who failed in urine testing.)

F. The Probability of Pretrial Misconduct Did Not Vary by Initial Group Assignment

There is no direct measure of pretrial crime and in this study, as in previous studies, pretrial rearrest is used as a proxy for pretrial crime. Failure-to-appear is indicated by issuance of a bench warrant in response to failure-to-appear for a scheduled court date. Overall pretrial misconduct is observation of either pretrial rearrest or failure-to-appear or both.

The hypothesis that there was an association between initial group assignment and subsequent pretrial misconduct was tested by

estimating equations which relate the observation of overall pretrial misconduct, failure-to-appear, and pretrial rearrest to characteristics of the accused, including demographic information and prior criminal history. The effect of group assignment is captured by inserting dummy variables for assignment to treatment referral or control. Full tables of estimation results are given in Appendix B. However, the final results are easily summarized. In no case--overall pretrial misconduct, failure-to-appear, or pretrial rearrest--was the partial relationship between initial group assignment to treatment referral or control statistically significant. This indicates that the fact of initial assignment to one of these groups, holding constant the personal and criminal characteristics of the accused, was not associated with statistically significant differences in the probability of pretrial misconduct. These results were developed for the defendants selected for the eight-month experiment.

Such results suggest that assignment to urine testing did not, of itself, have a large direct incentive effect. This is understandable, given the structure of the experiment which allowed defendants who were initially assigned to the urine-testing group subsequently to seek treatment and the evidence presented under Section E, above, that such switches did occur and that show cause hearings were apparently avoided by making them. If the consequences of failure in urine testing had been different (or if better data had been available about those consequences), a significant direct incentive effect might have been found. Of course, these results are conditional on the choice process used to select individuals for participation in the experiment and should not be extended to others, such as defendants for whom money bond was set, who were excluded.

G. Concluding Remarks

The results of the pretrial urine-testing program operated by the DC Pretrial Services Agency suggest that such a program operates as an effective signaling mechanism. Defendants who as a group pose greater-than-average release risks, as shown by the fact that they are drug users, can nevertheless often be safely released before trial. If such release is conditioned on periodic reporting for urinalysis, the Washington, DC experience indicates that they will soon sort themselves into two subgroups: (1) those who comply with the release conditions, by appearing as required for urine testing; and (2) those who do not comply, either by failing to appear for testing at all or by dropping out after only a few tests. Moreover, those defendants who do comply with the urine-testing requirements have sharply lower rates of pretrial rearrest, failure-to-appear and overall pretrial misconduct (i.e., pretrial rearrest and/or failure-to-appear) than those who fail to comply.

Other implications for public policy stem from these findings. One is the need to develop additional mechanisms that can serve as risk signaling devices based on defendants'

post-release behavior. Selected pilot programs are underway in various communities that could be viewed as such efforts. For example, Indianapolis, IN is experimenting with the use of electronic monitoring devices as a way to reduce jail crowding by placing defendants who would otherwise be detained in the county jail, due to inability to post money bond, under electronically supervised house arrest. Compliance with the electronic monitoring requirements for a short period of time, such as 90 days, may serve as an effective signal that the defendant could be safely released under less restrictive non-financial conditions (e.g., third party custody).

Another example comes from Washington, DC, where certain defendants who have been unable to make bail are granted release--first to a residential halfway house, and later to the community under restrictive conditions of supervision, including urine testing. In this case good behavior in the halfway house serves as a signal that the defendant is a good candidate for supervised release in the community.

These and other approaches may eventually identify a range of post-arrest signaling devices that can be used to separate high- from low-release-risk defendants. At that time, pretrial release policies and practices could focus more on monitoring the signals provided by defendants, so that persons who identified themselves as high risks could be placed under greater restrictions, while those who identified themselves as low risks could either remain under current supervision levels or have those levels reduced. In this way a better tailoring of risk level to supervision could occur--one that would be based on defendants' <u>demonstrated actions after release</u>, rather than solely on risk predictions made at the time of arrest, based on background data about the defendants.

FOOTNOTES

1. Such programs as intensive supervision and electronic monitoring, which are now being tested in certain jurisdictions, may also function as signaling mechanisms.

2. Judges reportedly often handled violations of pretrial urine-testing conditions as part of hearings on other matters, rather than in specially scheduled "show-cause" hearings. Typically, PSA representatives were not present at such hearings (though they usually <u>were</u> present at show-cause hearings), and no routine reporting procedures existed to assure that PSA was informed of all sanctions imposed on defendants for violations of pretrial urine-testing requirements. Although PSA attempted to obtain this information through informal channels (e.g., by asking judges and their law clerks to notify the Agency when such sanctions were ordered), the resulting data were considered incomplete. Hence, the data base used for the present analysis does not reflect all the sanctions that were imposed. Unfortunately, we have no way to determine the extent to which sanctions <u>not</u> reflected in the data base were <u>in fact</u> imposed.

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APPENDIX A

DESCRIPTION OF THE EXPERIMENTAL COMPONENT OF THE DC PRETRIAL SERVICES AGENCY'S URINE-TESTING PROGRAM FOR DEFENDANTS AWAITING TRIAL

In March 1984 the DC Pretrial Services Agency (PSA) initiated a comprehensive program of urine testing for defendants awaiting trial in the District of Columbia. One component of this program was an experiment, designed to test the efficacy of periodic urine testing before trial--as compared to (1) referral to treatment and (2) no drug-related intervention--in reducing pretrial rearrest and failure-to-appear rates for defendants released before trial on non-financial conditions. As originally designed, the urine-testing program would operate as follows:

- All arrestees processed through the lockup of the DC Superior Court would be tested for the presence of five drugs in their urine: opiates, cocaine, phencyclidine (PCP), amphetamines and methadone.

- Defendants would be eligible for participation in the experiment if they (1) tested positive for any of these drugs or admitted drug abuse when interviewed by PSA staff; (2) were not already in treatment; (3) did not request referral to treatment at the time of the PSA interview; and (4) were released by the court on non-financial conditions, subject to reporting to PSA for appropriate assignment of specific drug-related release conditions.

- Eligible defendants would be randomly assigned by PSA to one of three groups: one group would participate in a new program, to be run by PSA, of periodic urine testing before trial; a second group would be referred to drug abuse treatment, usually at the citywide treatment agency; and the third group would be a control group for whom no special release conditions related to drug abuse would be imposed.

- Defendants who violated their conditions of release--e.g., by continuing to use drugs and/or by failing to report as scheduled for testing or treatment--would have those violations reported by PSA to the court, which could then impose a variety of sanctions, ranging from a warning with re-release to jail sentences for contempt of court or outright revocation of release.

The randomization procedure based assignments to the three groups on the last digit of the Police Department Identification (PDID) number. This is a unique identifier assigned by the DC Metropolitan Police Department when an individual is first arrested; it is used for that person throughout all subsequent

arrests. Because these numbers are given to arrestees before PSA has any contact with them, PSA's staff could not manipulate the random assignment to the three experimental groups. Overall, approximately 30 percent of the defendants who met the eligibility criteria for the experiment would be assigned to PSA's new urine-testing program; 50 percent would be referred to treatment; and 20 percent would serve as the control group.

Two important concerns during the design of the experiment centered around developing appropriate criteria for failure in the urine-testing program and determining appropriate actions to take in response. These issues were discussed extensively by PSA staff, other local criminal justice practitioners, and the Toborg Associates research team; their resolution required that trade-offs be made between program needs and research needs. The final solution adopted was to have the urine-testing program consist of two phases. Defendants were first placed in "regular urine surveillance," which provided for once-a-week urinalysis. Those persons who failed this phase of the program--by testing positive for drugs, or by failing to appear for a drug test, either twice in a row or three times over three months--entered a second phase: they were given the option of either entering "intensive urine surveillance," which provided for twice-a-week urinalysis, or being referred to treatment. A violation was reported to the court only for failure in intensive urine testing or treatment, not for failure in the first-phase (regular urine surveillance) program.

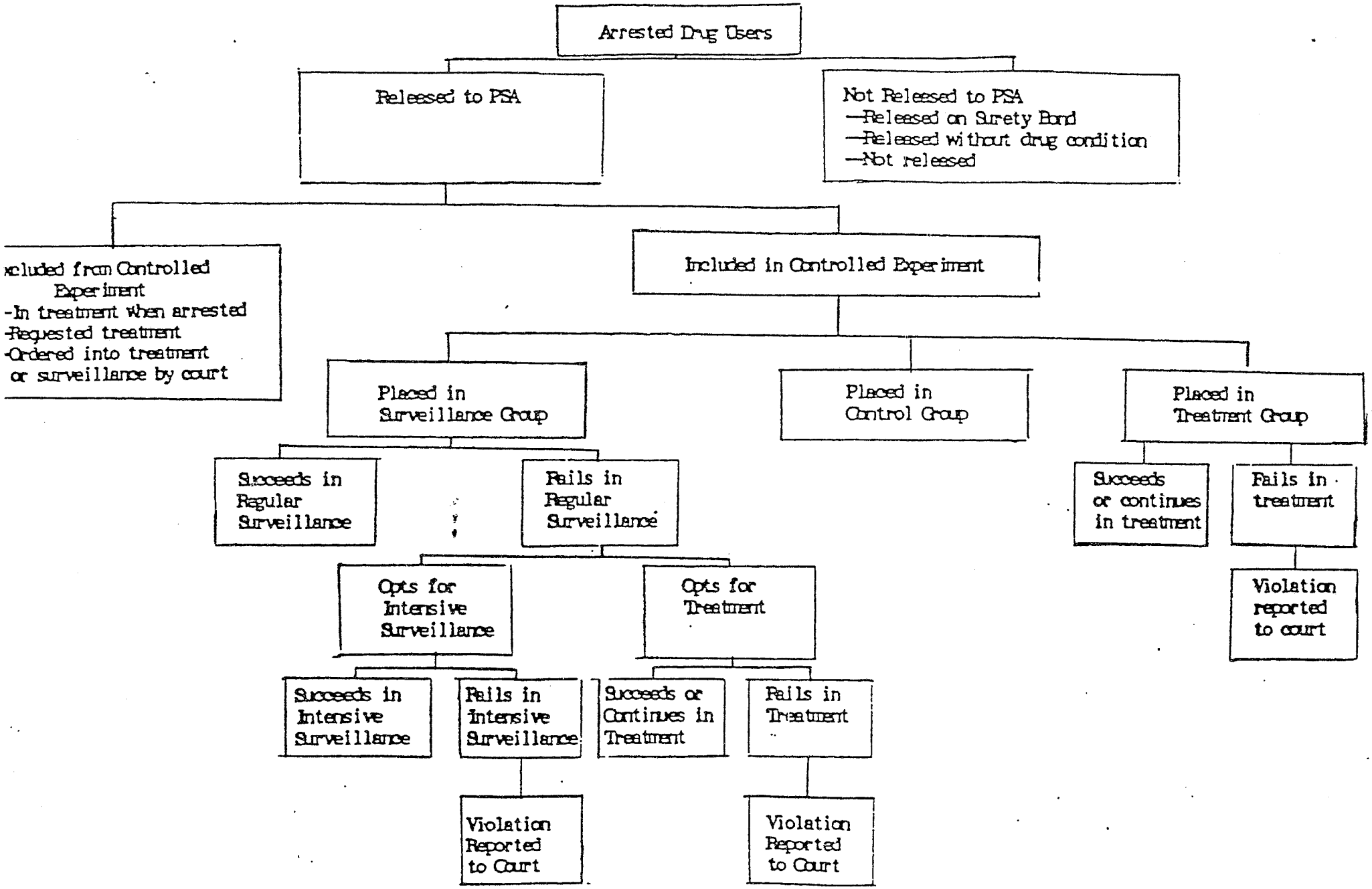
This approach was taken for two reasons. First, PSA had a history of offering defendants a "second chance" before reporting release condition violations, and it did not want to change this policy. Second, there was some concern by PSA that its reports of urine-testing violations would lack credibility with the court, if those defendants who were reported to have "failed" had never been offered the option of treatment.

Figure 1 provides a flow diagram of this process of selecting defendants for the experiment and monitoring their progress. It shows the ways in which defendants could be excluded from eligibility for participation in the experiment as well as the different paths that defendants might follow, once selected.

Although PSA's urine-testing program began in March 1984, it took several months for the various procedures to stabilize. Consequently, the first few months of operations were excluded from the analysis of the experimental results. That analysis was based on arrests during the eight-month period from June 1, 1984, through January 31, 1985. During that time 1,874 defendants were assigned to the three experimental groups, as follows:

- urine-testing group, 570 defendants (30.4 percent of all defendants in the experiment);

Flow Diagram for Arrested Drug Users in the District of Columbia (June 1984-January 1985)



Another problem in maintaining group comparability arose because some defendants in both the urine-testing and control groups opted to enter treatment during the course of the pretrial period. As stated previously, any defendant who wanted to enter treatment at the time of the initial arrest--which was also the time of PSA's interview and initial, "lock-up," urine test--was permitted to do so; such defendants were excluded from eligibility for the experiment. This procedure seemed fair and reasonable, given that Washington, DC has a citywide drug abuse treatment program (with both outpatient and residential components) that any DC resident can seek to enter at any time. To deny any defendants the opportunity to enter treatment would have deprived them of a service available to all other District of Columbia residents--and of a service that would have been available to them, but for the experiment--and, hence, was rejected as an option.

Although it was anticipated that defendants who wanted to enter treatment would indicate this at the time of initial arrest --and many did so--other defendants decided to seek treatment later in the pretrial period. Some of these defendants had initially been assigned to the experiment's urine-testing or control groups. Again, this problem arose in part because the pretrial period is often a long one in the District of Columbia. Also, as discussed previously, the urine-testing program itself was designed to give defendants who failed the first stage of it the option of seeking treatment before a violation was reported.

Finally, some problems with maintaining group comparability arose because many defendants had multiple arrests during the experimental study period. Although PSA attempted to keep rearrested defendants in their originally assigned groups (i.e., urine testing, treatment referral or control), the releasing magistrates in new cases would sometimes order them into <u>different</u> groups. Thus, a defendant who was originally assigned to urine testing could have been ordered by the court at the time of rearrest to enter treatment. Similarly, a rearrested defendant originally assigned to the treatment referral group could have been ordered by the court into urine testing.

Thus, a variety of events occurred <u>after the initial random assignment</u> of defendants but <u>before case disposition</u> that affected the comparability of the three experimental groups. As a result, straightforward comparisons of pretrial rearrest rates and failure-to-appear rates across the three groups--a key feature of the original analysis plan--were inconclusive. Those comparisons showed no significant differences across groups, but it is impossible to determine whether that is (1) because there was no difference in the impact of urine testing, treatment referral or no intervention; or (2) because the comparability of the three groups was not maintained throughout the pretrial period and, if it <u>had</u> been maintained, important differences in outcomes might have been found.

Although this question could not be resolved in the context of this experiment, there are a number of important findings that stem from the analysis of the data collected in connection with the experiment. These are discussed in the body of the monograph and have to do in particular with the way in which participation in the pretrial urine-testing program served as a "signaling" device that separated defendants according to levels of release risk.

APPENDIX B

ECONOMETRIC RESULTS SUPPORTING CONCLUSIONS ON SIGNALING

The material in this appendix provides compelete estimation results for the pretrial misconduct equations discussed in Chapter III of the monograph on signaling behavior by participants in urine testing. All results are for 1,494 persons who were part of the Washington, DC adult pretrial urine-testing experiment and whose cases did not reach disposition within 30 days. As explained in the body of the monograph, this is a person-based sample rather than a case-based sample so that there is only one observation per person even if the individual was rearrested several times during the observation period. Missing data were treated by using casewise deletion, i.e., eliminating an observation in which there was any missing data. This was particularly important for cases in which lockup drug test results were not present.

The results reported in this appendix are estimates of the determinants of various types of misconduct, failure-to-appear and pretrial rearrest. The explanatory variables include personal characteristics, the past criminal record, the most serious charge under the current arrest, lockup drug test results, and performance in pretrial urine-testing. The estimation technique employed to generate the results in the tables in this appendix was ordinary least squares which allows easy interpretation of the estimated coefficients as partial effects on the probability of misconduct. Separate equations were estimated for failure-to-appear, pretrial rearrest, and pretrial misconduct. These are reported in Table B-1, below, which follows the glossary of variable names.

GLOSSARY OF VARIABLES

PERSONAL CHARACTERISTICS OF THE DEFENDANT

AGE = age of the defendant in years
AGESQ = age in years squared
EMPLYD = dummy variable equal to unity if defendant employed, zero otherwise
MALE = dummy variable equal to unity for male, zero otherwise

CRIMINAL JUSTICE RECORD OF THE DEFENDANT

EXCON = number of prior convictions
PAROLL = dummy variable equal to unity if defendant on parole, zero otherwise
PENDCASE = number of cases pending against the defendant
PROBATION = dummy variable equal to unity if defendant on probation, zero otherwise

MOST SERIOUS CHARGE AT ARREST (ALL 0-1 DUMMY VARIABLES)

RAPE = 1 if arrested for rape, 0 otherwise
BURGLE = 1 if arrested for burglary, 0 otherwise
DRUGS = 1 if arrested for drug possession or distribution, 0 otherwise
FLIGHT = 1 if arrested for flight to avoid prosecution, 0 otherwise
FORGERY = 1 if arrested for forgery, 0 otherwise
FRAUD = 1 if arrested for fraud, 0 otherwise
KIDNAP = 1 if arrested for kidnapping, 0 otherwise
LARCENY = 1 if arrested for larceny, 0 otherwise
ROBBERY = 1 if arrested for robbery, 0 otherwise
PROSTI = 1 if arrested for prostitution, 0 otherwise
STOLCAR = 1 if arrested for auto theft, 0 otherwise
STOLPTY = 1 if arrested for possession of stolen property, 0 otherwise
WEAPONS = 1 if arrested for illegal possession of weapons, 0 otherwise
PSESCRM = 1 if arrested for possession of criminal tools, 0 otherwise
DESTPTY = 1 if arrested for destruction of property, 0 otherwise

(Continued)

LOCKUP TEST RESULTS (ALL 0-1 DUMMY VARIABLES)

AMPHAM = 1 if test positive for amphetamines and negative for other drugs, 0 otherwise
METHDO = 1 if test positive for methadone and negative for other drugs, 0 otherwise
OPIATE = 1 if test positive for opiates and negative for other drugs, 0 otherwise
PCP = 1 if test positive for PCP and negative for other drugs, 0 otherwise
OPICOC = 1 if test positive for opiates and cocaine and negative for others, 0 otherwise
PCPCOC = 1 if test positive for PCP and cocaine and negative for other drugs, 0 otherwise
OPIPCP = 1 if test positive for opiates and PCP and negative for other drugs, 0 otherwise
TWODRG = 1 if test positive for two or more drugs but not for any of the three drug combinations shown above, 0 otherwise

URINE-TESTING PERFORMANCE AND GROUP INDICATORS (ALL 0-1 DUMMY VARIABLES)

TREAT = 1 if defendant assigned to treatment referral, 0 otherwise
CONTROL = 1 if defendant assigned to control group, 0 otherwise
ORDER = 1 if defendant subsequently court-ordered into urine testing, 0 otherwise
APPEAR = 1 if in urine-testing and appear for 3 or more tests, 0 otherwise
CLEAN = 1 if APPEAR=1 and less than 25% of drug test results positive for each of the five drug types tested, 0 otherwise

PRETRIAL MISCONDUCT VARIABLES (ALL 0-1 DUMMY VARIABLES)

FTA = 1 if bench warrant issued on defendant for failure-to-appear before disposition of case or end of observation period, 0 otherwise
PTCRIM = 1 if defendant arrested before disposition of case or end of observation period, 0 otherwise
MISCON = 1 if either FTA = 1 or PTCRIM = 1, 0 otherwise

TABLE B-1

RELATION BETWEEN DEFENDANT CHARACTERISTICS,
INCLUDING LOCKUP TEST RESULTS,
AND PRETRIAL MISCONDUCT USED TO FORM TABLES 1,2, & 3

INDEPENDENT VARIABLE	DEPENDENT VARIABLE PARREST	FTA	MISCON
CONSTANT	0.496*	0.446*	0.791*
	(3.26)	(3.00)	(4.23)
AGE	-0.020*	-0.014*	-0.026*
	(-2.27)	(-1.61)	(-2.50)
AGESQ	0.0002	0.0002	0.0003*
	(1.58)	(1.46)	(1.80)
MALE	0.070*	0.004	0.061*
	(2.30)	(0.13)	(1.69)
EMPLYD	-0.026*	0.003	-0.027
	(-1.22)	(0.14)	(-1.06)
RAPE	0.002	0.122	0.139
	(0.01)	(1.06)	(1.00)
BURGLE	0.066	0.127*	0.134*
	(1.11)	(2.18)	(1.91)
DRUGS	0.018	0.034	0.044
	(0.04)	(0.88)	(0.96)
FLIGHT	-0.214	0.349	0.183
	(-0.74)	(1.25)	(0.54)
FORGERY	0.077	0.175*	0.181
	(0.80)	(1.85)	(1.59)
FRAUD	0.441	0.375	0.328
	(1.54)	(1.34)	(0.98)
KIDNAP	-0.153	0.901*	0.760*
	(-0.38)	(2.31)	(1.63)
LARCENY	0.022	0.136*	0.139*
	(0.38)	(2.38)	(2.02)
ROBBERY	-0.267	0.055	0.041
	(-0.45)	(0.96)	(0.59)
PROSTI	-0.002	0.364*	0.328*
	(-0.02)	(5.26)	(3.93)
STOLCAR	0.062	0.110*	0.123*
	(1.11)	(2.02)	(1.87)
STOLPTY	-0.015	0.245*	0.187*
	(-0.22)	(3.76)	(3.39)
WEAPONS	-0.029	-0.031	-0.025
	(-0.48)	(-0.49)	(-0.34)
PSESCRM	0.043	-0.157	-0.050
	(0.25)	(-0.96)	(-0.26)
DESTPTY	-0.023	-0.007	-0.029
	(-0.30)	(-0.98)	(-0.32)
EXCON	0.032*	0.008	0.033*
	(4.76)	(1.31)	(4.23)
PF[illegible]DCASE	0.099*	0.036	0.091
	(1.99)	(0.70)	(1.48)
PAROLL	-0.003	0.004	-0.019
	(-0.08)	(0.09)	(-0.36)

INDEPENDENT VARIABLE	DEPENDENT VARIABLE PARREST	FTA	MISCON
PROBTN	-0.009	-0.040	-0.059
	(-0.28)	(-1.21)	(-1.52)
ADMIT	0.016	-0.010	0.005
	(0.72)	(-0.45)	(0.19)
AMPHAM	-0.052	-0.196*	-0.204*
	(-0.51)	(-1.98)	(-1.70)
METHDO	0.142	-0.247	-0.064
	(0.86)	(-1.53)	(-0.33)
OPIATE	0.045	-0.036	-0.022
	(1.10)	(-0.89)	(-0.45)
PCP	-0.048	-0.120*	-0.160*
	(-1.58)	(-3.89)	(-4.33)
OPICOC	0.036	0.006	0.021
	(0.97)	(0.16)	(0.48)
PCPCOC	0.041	-0.042	-0.014
	(1.11)	(-1.17)	(-0.32)
OPIPCP	0.017	-0.122*	-0.093*
	(0.41)	(-2.96)	(-1.86)
TWODRG	0.027	-0.045	-0.043
	(0.35)	(-0.60)	(-0.48)
ORDER	-0.039	0.045	0.017
	(-1.29)	(1.53)	(0.48)
TREAT	-0.019	-0.031	-0.018
	(-0.80)	(-1.34)	(-0.62)
CONTROL	-0.013	-0.040	-0.024
	(-0.42)	(-1.32)	(-0.64)
F(35,1458)	2.1	3.0	3.0

"t-ratios" shown in ( ) under estimated coefficients.
* Indicates statistical significance at 10% level.

TABLE B-2

RELATION BETWEEN DEFENDANT CHARACTERISTICS,
INCLUDING LOCKUP TEST RESULTS,
AND PRETRIAL MISCONDUCT USED AS THE BASIS FOR TABLE 4

INDEPENDENT VARIABLE	DEPENDENT VARIABLE PTARREST	FTA	MISCON
CONSTANT	0.610*	0.560*	0.956*
	(3.97)	(3.75)	(5.31)
AGE	-0.020*	-0.014	-0.026*
	(-2.27)	(-1.61)	(-2.50)
AGESQ	0.0002	0.00019	0.00029*
	(1.58)	(1.46)	(1.81)
MALE	0.070*	0.004	0.061*
	(2.32)	(0.14)	(1.72)
EMPLYD	-0.029	0.001	-0.030
	(-1.35)	(0.01)	(-1.21)
RAPE	-0.018	0.10	0.110
	(-0.16)	(0.89)	(0.81)
BURGLE	0.054	0.115*	0.116*
	(0.90)	(1.97)	(1.65)
DRUGS	0.013	0.028	0.035
	(0.33)	(0.72)	(0.76)
FLIGHT	-0.271	0.292	0.101
	(-0.95)	(1.05)	(0.30)
FORGERY	0.058	1.54*	0.151*
	(0.60)	(1.69)	(1.33)
FRAUD	0.378	0.310	0.237
	(1.33)	(1.21)	(0.71)
KIDNAP	-0.164	0.889*	0.743
	(-0.41)	(2.29)	(1.59)
LARCENY	0.008	0.122*	0.118*
	(1.38)	(2.14)	(1.73)
ROBBERY	-0.038	0.044	0.024
	(-0.64)	(0.76)	(0.34)
PROSTI	-0.027	0.338*	0.291*
	(-0.38)	(4.90)	(3.51)
STOLCAR	0.058	0.105*	0.116*
	(1.05)	(1.95)	(1.78)
STOLPTY	-0.030	0.229*	0.164*
	(-0.45)	(3.52)	(2.10)
WEAPONS	-0.028	-0.031	-0.026
	(-0.44)	(-0.50)	(-0.35)
PSESCRM	0.033	-0.164	-0.059
	(0.20	(-1.00)	(-0.32)
DESTPTY	-0.041	-0.024	-0.053
	(-0.53)	(-0.32)	(-0.58)
EXCON	0.030*	0.008	0.032*
	(4.61)	(1.13)	(4.01)
PENDCASE	0.095*	0.031	0.083
	(1.82)	(0.60)	(1.36)
PAROLL	0.002	0.010	-0.012
	(0.05)	(0.22)	(-0.49)

INDEPENDENT VARIABLE	DEPENDENT VARIABLE PTARREST	FTA	MISCON
PROBTN	-0.026*	-0.144	-0.067*
	-(0.48)	(-1.34)	(-1.74)
ADMIT	0.015	-0.012	0.003
	(0.65)	(-0.53)	(0.09)
AMPHAM	-0.058	-0.205*	-0.217*
	(-0.56)	(-2.07)	(-1.86)
METHDO	0.152	-0.240	-0.052
	(0.92)	(-1.49)	(-0.28)
OPIATE	0.044	-0.037	-0.024
	(1.088)	(-0.93)	(-0.59)
PCP	-0.043	-0.117*	-0.156*
	(-1.38)	(-3.80)	(-4.26)
OPICOC	0.026	-0.004	0.006
	(0.70)	(-0.12)	(0.41)
PCPCOC	0.043	-0.041	-0.012
	(1.18)	(-1.14)	(-0.29)
OPIPCP	0.022	-0.119*	-0.088*
	(0.051)	(-2.88)	(-1.78)
TWODRG	0.028	-0.045	-0.043
	(0.36)	(-0.61)	(-0.48)
APPEAR	-0.157*	-0.143*	-0.202*
	(3.44)	(-3.19)	(-3.75)
CLEAN	-0.020	-0.143*	-0.074
	(-0.44)	(-2.05)	(-1.36)
TREAT	-0.12*	-0.137*	-0.168*
	(-3.57)	(-3.98)	(-4.08)
CONTROL	-0.122	-0.148*	-0.178*
	(-3.01)	(-3.71)	(-3.72)
F(35,1458)	2.52*	3.46*	3.67*

"t-ratios" in ( ) under estimated coefficients
* Indicates statistical significance at the 10% level

- treatment referral group, 959 defendants (51.2 percent of all defendants in the experiment); and

- control group, 345 defendants (18.4 percent of all defendants in the experiment).

Analyses showed that these three groups had indeed been assigned randomly. While there were very significant differences in the demographic, prior criminal record and other background characteristics of defendants in the experiment as a whole, these differences were not significantly related to the initial assignments to the three groups.

Although the initial random assignment process worked as planned, problems subsequently arose in maintaining the comparability of the three groups throughout the pretrial period. These problems developed in part because of the length of the pretrial period in the District of Columbia. It is not unusual for a case to take six to eight months--or even longer--to reach disposition, and during this time a defendant may be required to appear in court on several occasions on a variety of matters related to the same case. After PSA's urine-testing program began, and judges became more knowledgeable about it, a number of judges started ordering defendants who had not been initially assigned to the urine-testing program to enter it and comply with its requirements. These judicial orders often occurred during court proceedings that were held several months after the defendant's initial release to await trial and, thus, several months after the initial assignments to the experimental groups had been made.

Ironically, this action by the judges--which confounded the initial experimental design--shows the high value they placed on the urine-testing program. Another component of the reseach study was intended to deal with exactly that topic, namely, how judges and other criminal justice officials respond when a new pretrial urine-testing program is implemented in a given jurisdiction. When PSA's urine-testing program was in the design stages, there had been concern that judges would ignore the urine-test results. This concern stemmed from earlier PSA experiences when violations of other pretrial release conditions had been reported to the court. In those instances, judges frequently took no action with regard to the reported violations. This apparently happened because DC judges, facing the ever-growing criminal case backlog in Superior Court, did not want to allocate any substantial amount of time to violations hearings except in unusual circumstances, such as instances where the defendant had committed a serious new offense. Thus, it was something of a surprise when judges not only began holding "show-cause" hearings on violations of pretrial urine-testing conditions but also, on their own initiative, began to order defendants into the urine-testing program.