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Restoring accountability in pretrial release: the Philadelphia pretrial release supervision experiments

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Abstract. As drug arrests and jail overcrowding added pressure to increase pretrial release in localities during the 1980s and 1990s, the need to manage a larger and higher-risk pretrial population of defendants awaiting adjudication in the community became a high priority for justice agencies. In the late 1990s Philadelphia officials sought to discover the ingredients of a successful supervision strategy through four interlinked field experiments to provide an empirical basis for a major reform of the pretrial release system. The results of the linked randomized experiments question common assumptions about "supervision," its impact and effectiveness, about the underlying nature of the noncompliant defendant, and deterrence implications. The study emphasizes the importance of interpreting the findings in the context of implementation of the policy reform. Findings suggest that facilitative notification strategies wield little influence on defendant behavior and that deterrent aims are undermined by the system's failure to deliver consequences for defendant noncompliance during pretrial release. The most significant contribution of the article is its illustration of a major evidence-based policy reform undertaken by a major court system.

Key words: field experiment, pretrial misconduct, pretrial release, supervision

Introduction: the need for effective pretrial release strategies

The pretrial release decision represents the highest volume decision stage in the judicial process. Its high-volume – and often perfunctory – nature belies its importance for defendants, the public and the justice system. The justice system must be confident that arrested persons will attend court proceedings to complete adjudication of their criminal charges. At the same time, the public relies on the judicial process to protect the community from defendants who may pose a danger to individuals or the community.

For the accused, nothing less than personal liberty is at stake in the pretrial release decision. Either defendants will gain release – under a promise to appear or some condition(s) restricting activities – or the defendant will be confined, through direct denial of release or, in many jurisdictions still, through the assignment of unaffordable cash bail amounts.[1] The disadvantages to the accused who is detained

in defending the charges, in maintaining employment and family responsibilities have been well documented over many decades.[2]

Because of its location at the threshold of the criminal process and because of the volume of incoming cases, the pretrial release decision is also the judicial stage at which the largest numbers of persons processed into the criminal justice system are placed at risk of or actually experience incarceration.[3] The often overlooked concomitant of this fact – and the focus of the research discussed in this article – is that at any given moment the large majority of defendants undergoing judicial processing in the United States of America are on some form of release in the community. It is highly problematic, certainly, that the numbers who nevertheless remain in confinement during the adjudication process in many jurisdictions still easily outstrip available jail confinement capacity. Successive generations of initiatives in many local jurisdictions have sought to respond to jail overcrowding pressures by trying to cull the lowest risks from the population of jailed defendants, with the result that the remaining inmates represent a more concentrated population of higher risk, less releasable defendants than has previously been the case. Yet, because of unrelentingly crowded conditions, defendants released from these populations under emergency procedures pending adjudication of criminal charges pose notably higher risks of flight and crime to the community. The new reality is that a great many defendants are released to the community who, in earlier times, would have been held on cash bail.

The policy challenge for Philadelphia and other jurisdictions in similar circumstances is to devise methods of reducing and preventing excessive crowding in overburdened jail facilities, while somehow maintaining accountability over released defendants so that they do not pose an extra threat to public safety or to the integrity of the judicial process. This article describes a series of linked, sequenced field experiments conducted in the late 1990s designed to generate evidence to inform Philadelphia's emerging pretrial release supervision strategy. The multi-part study formed part of a process of system reform that drew heavily on research evidence, policy development and evaluation to examine and "repair" the pretrial release process in Philadelphia.

Managing higher-risk defendants in the community: policy challenges and theoretical implications

This article considers the experience of one major urban American jurisdiction, Philadelphia, as it was compelled to confront the problems posed by the need to release and supervise greater numbers of higher-risk defendants safely in response to a crisis of severe jail overcrowding and system dysfunction.[4] The challenges faced by the Philadelphia justice system were not at all unique but were common during the 1980s and 1990s in many American localities. The policy problem faced by the Philadelphia court system – like that faced by many other systems – was how to respond to an extreme need to reduce the levels of confinement in the city's overcrowded and aging local correctional facilities, while, at the same time, not exposing the community to increased risk of crime or the courts to higher rates of

nonappearance of defendants. This article describes a multi-stage experimental approach to developing an evidence-based intervention strategy to resolve these competing demands by testing assumptions both about the nature of the non-compliant defendant (offender) tied to criminological theory and about the nature of the supervision intervention and its relevant legal (due process) and deterrence underpinnings.

Focusing first on the noncompliant defendant to begin the process of constructing a pretrial supervision strategy, the Philadelphia task force officials debated the relevance in designing a corrective strategy of two explanations (two competing "conventional wisdoms") of noncompliant defendant behavior, each with their counterparts in criminological theory: (a) the "intentional criminal" defendant, and/or (b) the "disorganized (unintentional) defendant." The "intentional" noncompliant defendant was viewed by officials as a system-wise, experienced individual who was quite probably guilty and was determined to defy, avoid, evade or ignore the requirements of the judicial process in the hopes of escaping criminal penalties whenever possible. This conceptualization of non-compliant behavior would credit the defendant with purposeful, choice-making attributes associated with deterrence or rational choice perspectives.[5]

In marked contrast, the second type of noncompliant defendant was viewed instead as a hapless individual who was generally disorganized and dysfunctional in life. Resembling the low self-control offender of Gottfredson and Hirschi's (1990) general theory of crime, this defendant would be easily confused by the judicial process and its requirements, highly distractable (often because of drugs and alcohol) and would often act irresponsibly. These defendants would miss court because they comprehended the process poorly, had difficulty keeping dates, times and places straight (e.g., drug addicts are not punctual), and/or they suffered from educational, learning, cultural, language and transportation difficulties.

The supervision strategy finally adopted by the Philadelphia officials sought to take both kinds of behavioral explanations into account and organized implementation of pretrial supervision in a way that would permit testing of three utilitarian intervention rationales. To address the problem posed by the "intentional" noncomplier, the supervision strategy sought to deploy deterrent and incapacitative release conditions. To respond to the "disorganized" defendant, the strategy adopted supportive, rehabilitative procedures to foster compliant behavior.[6]

The pretrial release guidelines intervention called on deterrence, incapacitation theory and the principle of least restrictive intervention from substantive due process in pretrial release law. There were supportive functions that would educate the defendant about the nature of the process, its expectations and the consequences of noncompliance in a deterrence mode (the communication of the threat and use of sanction in deterrence theory), including use of arrest and confinement as sanction. There were incapacitative aims in the strict supervision and/or confinement of targeted categories of medium-to-high risk defendants. Finally, there were related themes from substantive due process that release of the accused should be favored and presumed, and that conditions of release (supervision) should include elements that involved the least restrictive necessary

interference with a person's liberty required to address the risk of noncompliance (crime or flight) posed.[7]

Court officials characterized the situation in Philadelphia relating to the performance of pretrial release at the time of the study as one governed by a "culture of no consequences." Through adoption of various court-ordered emergency release procedures over time, many types of defendants soon learned that, though they could be arrested, they simply could not be placed in jail under many categories of crimes. They discovered that the threat of deterrent sanction implicit in the judicial pretrial release orders could not be enforced. There was simply, as one judge phrased it, no "hammer" (option to use jail) to enforce compliant behavior. It became common knowledge "on the street" that, because of the crowding emergency procedures, it was unlikely that many of even the most noncompliant defendants would go to jail for failing to attend court or follow judicial orders, unless charged with very serious offenses. As a result the idea of releasing greater numbers of higher-risk defendants to reduce the inmate population seemed guaranteed to produce a greater volume of noncompliant defendants in the community. Policy makers were presented the paradoxical need for both greater and lesser use of jail at the same time to (a) use jail more effectively as the ultimate enforcement sanction (as "the hammer" in judicial deterrence language) to ensure compliance among released defendants and (b) avoid, and even reduce, the use of confinement because of the crowding crisis. It was in this context that policymakers and researchers turned to the idea of introducing a new role for supervision at the pretrial stage to target higher-risk defendants usually held in detention and basing the approach on experimental evidence of impact.

Background

Absence of research evidence on pretrial (or other) supervision

The effort to shape a contemporary pretrial supervision mechanism for the Philadelphia court system found little help in the empirical or professional literature. At the time of the initiation of the Philadelphia supervision experiments, neither the American Bar Association (1968, 1978) nor the National Association of Pretrial Services Agencies (1978) addressed supervision in their standards relating to pretrial release.[8] The Administrative Office of the U.S. Courts (Probation and Pretrial Services Division, Administrative Office of the U.S. Courts 1994) had published an agency guide to pretrial services supervision for Federal jurisdictions, but it included no discussion of the relative effectiveness of elements of supervision in minimizing pretrial flight or crime.

Over the past quarter century only a handful of studies addressed aspects of supervision in pretrial release and, among those, findings likely to be helpful in constructing an effective supervision approach in Philadelphia were rare. No study had examined the overall impact of pretrial supervision or of the relative impact of

the ingredients that might constitute an effective supervision approach. In the earliest study found, the District of Columbia Bail Agency (1978) compared the performance of defendants assigned to three levels of supervision in an agency report employing non-experimental comparisons. Methodological issues aside, the study reported that neither rearrest nor failure-to-appear rates varied by pretrial supervision level. Compliance with supervision conditions did vary by supervision level, however, with greater compliance found among defendants placed on the most restrictive conditions of release. Five years later, Austin and Krisberg (1983) published a study of supervised pretrial release in three jurisdictions (Miami, Milwaukee, and Portland), using random assignment of selected defendants to two groups of supervision conditions (supervision versus supervision supplemented by other services). Austin and Krisberg (1983) found that, among these defendants, the addition of treatment and other services to supervision did not improve defendants' attendance in court or lower rearrest when compared to those supervised without additional services. Their study did not address the question of whether supervision offered improved outcomes compared to no supervision. (No other study had addressed that basic question.) Still other studies contributed findings tangentially relevant to the development of a pretrial supervision approach. For example, Yezer et al. (1987) and Visher (1990) found that defendants failing to perform initial urine testing upon release were more likely to subsequently fail to attend court than those who complied, and suggested that failure to comply with testing conditions could be an early warning of later misconduct. However, those studies failed to provide evidence bearing on the question of whether urine testing serves as an effective element of a supervision strategy.

In one of several studies examining supervision in the context of probation, Petersilia et al. (1992) examined the impact of ISP or intensive supervision of probationers in seven jurisdictions using an experimental design. Over the 12-month follow-up period, ISP probationers produced rearrest and violation rates that differed little from their non-intensively supervised control groups. Similar findings had been reported earlier by Petersilia and Turner (1990) in a study of intensive probation in three California counties. In a non-experimental study, Erwin (1986) found that intensively supervised probationers generated higher rearrest rates than those given regular supervision, leaving open the question of whether the higher rates were the result of a supervision or surveillance effect (i.e., that the more closely supervised defendants are, the more violations are likely to be noticed). More recently, Solomon et al. (2005) analyzed 1994 Bureau of Justice Statistics parole data contrasting post-prison release with and without supervision and found no difference associated with supervised forms of release. That non-experimental study did not identify the relative effects of various components of parole supervision.

Overall, empirical studies did not address the question of whether supervision reduced unfavorable conduct among defendants compared to what would have resulted if no supervision had been applied. Nor did studies examine the extent to which particular elements of a supervision approach proved more or less effective

in dealing with specific categories of defendants. Perhaps even more frustrating in the search for "best practices" from the professional and empirical literature was the failure to deal with a definition of supervision explicitly. What supervision "was" appeared to be self-evident to the authors in those studies. In short, supervision mainly was what it was assumed to be, but those assumptions were seldom made explicit. Yet, whatever "it" was, supervision was generally portrayed as somehow beneficial, even in studies failing to show supervision impact.

Policy context: jail crowding and its effects in Philadelphia

The population of inmates confined in the institutions serving collectively as the City's jail system (known as the "Philadelphia Prisons") more than doubled, from just under 3,000 in 1960 to over 7,000 at the turn of the century, and stood above 6,000 in mid-1998 when the pretrial supervision strategy was being developed and implemented.[9] By the mid-1990s the Philadelphia justice system had been operating under the constraints of various crowding-reduction procedures for about a quarter of a century, with the result that the overcrowding "emergency" and its associated court-ordered intervention from state and Federal courts had become chronic; in fact, the emergency had become the new status quo.

In November 1995 the Federal judge issued a temporary stay of the major population-related emergency provisions (including the population MAP or "cap") to give the local court system a chance to implement improved justice procedures that would make Federal intervention in the City's crowding problem unnecessary.[10] City officials hoped that implementation of Philadelphia's alternatives-to-incarceration plan could, if successful, be parlayed into a permanent stay or even termination of Federal court jurisdiction entirely.

The submerging of judicial discretion in the local courts ("If I had a hammer...")

During the years when the jail emergency was in effect, the pretrial release decision-making process was seriously disrupted in Philadelphia. A population cap and related procedures limiting admissions to the jail system mandated by the Federal and state courts had the effect of superseding and rendering nearly meaningless pretrial release decisions made in Municipal Court at the first appearance (preliminary arraignment) stage. Bail judges and commissioners in Philadelphia learned that, no matter what they might decide to be rational and appropriate pretrial release decisions in individual cases, the detention of certain categories of defendants simply would not be permitted, while others who were detained would find themselves released immediately, if confined in contravention of the Federal court order.

Another effect of the superseding emergency procedures was to divert scarce pretrial services agency resources from work normally conducted to facilitate first-stage ("front-end") releases to activities supporting post-hoc reviews of the

detained population. Instead of focusing on the pre-first appearance interview and information-collection processes to promote safe pretrial release at the earliest stage of criminal proceedings, pretrial services personnel were forced to concentrate their energies on identifying individuals who had been detained but who, under emergency release rules, should be released (without supervision).

In short, the attempt to implement any effective system of pretrial supervision would have to be carried out within a larger strategy that returned pretrial release decisions to a more rational approach and recognized the importance of judicial discretion in the local courts but, nevertheless, provided clear guidance and required accountability in the exercise of that discretion so that the use of confinement could be reasonably controlled. The critical lesson to be drawn from the justice system dysfunction evident at that time was that simple, unilateral, externally imposed, discretion-eliminating, mandatory rules could not succeed in solving the system problems contributing to overcrowding. The discretion, authority and perspectives of the local judicial officials had to be given a central role in efforts to bring about serious change—or the discretion would be moved, accommodated or adapted "hydraulically" in other ways and other processing stages. At the same time, however, the crowding crises made the need for clear policies and consistent procedures powerfully evident. Unchecked judicial discretion in assigning local pretrial confinement was a luxury that the Philadelphia government could no longer afford.

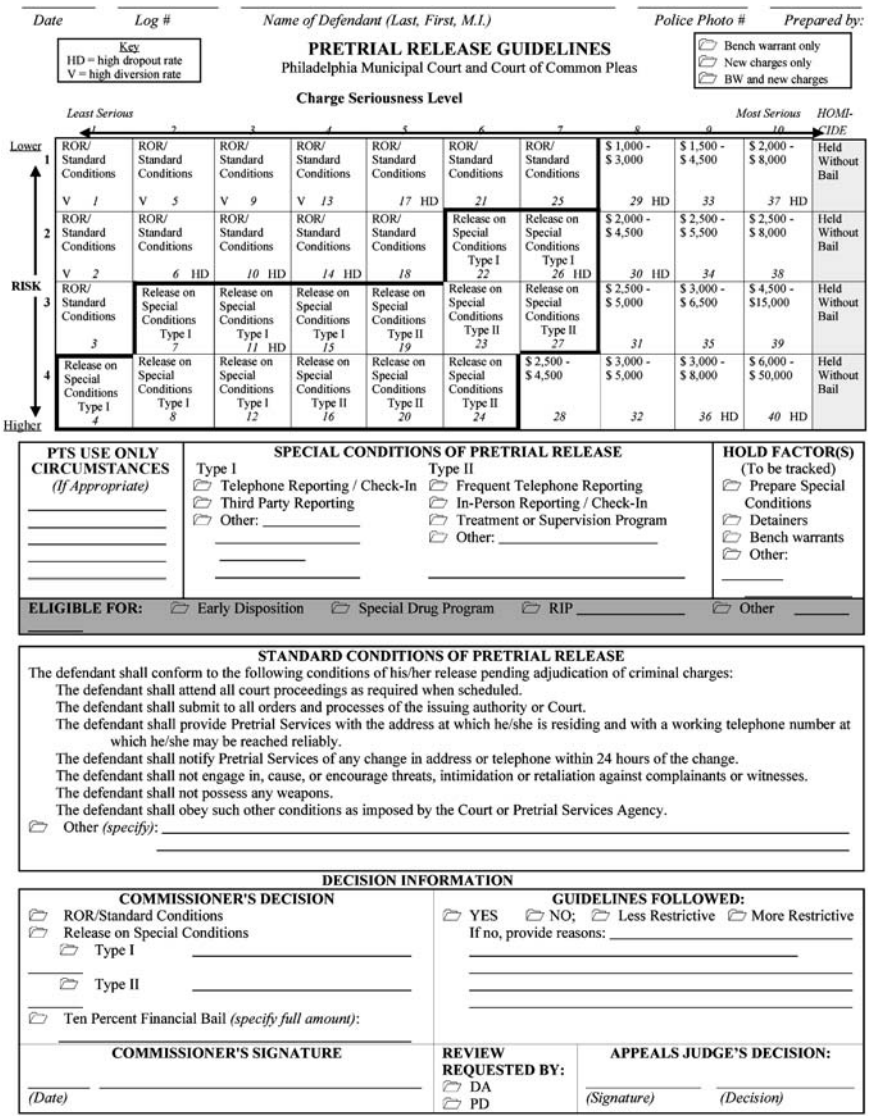
Targeted pretrial supervision as an element of pretrial release reform

Pretrial release (or "bail") guidelines were pioneered in Philadelphia in the early 1980s in an experiment designed to promote more equitable and effective pretrial release decisions.[11] Shortly after their implementation by the Philadelphia Municipal Court, court-imposed crowding reduction procedures began going into effect, ultimately undermining the judicial decision guidelines strategy. In considering how to carry out a systematic review and reform of pretrial release practices more than a decade later, however, the Mayor's Overcrowding Task Force returned to the judicial guidelines strategy, which combined focused research, empirical evidence on impact and policy debate in a continuing process of system change.

The decision guidelines matrix resembles a grid formed by two principal dimensions: the seriousness of the current charges on a scale of one to ten (with ten being the most serious) and a four-level risk classification that ranked defendants according to the likelihood of flight or rearrest (with four being the highest risk).[12] This matrix served as a classification of Philadelphia defendants into a limited number of categories by means of which specific performance data could be generated and release options could be targeted. The nature of the defining dimensions and the specific decision policies to be adopted for each category was determined through a process of empirical analysis of thousands of previous Philadelphia defendants and policy debate at the multi-agency task force level,

including judges, defenders, district attorneys and other agency staff. The resulting pretrial release guidelines classified defendants into 40 possible categories and suggested four basic types of release options, with one type representing the presumptive choice in each category (see Figure 1).

To promote pretrial release of targeted categories of defendants formerly very likely to be held in detention, the guidelines matrix emphasized use of non-financial conditions of pretrial release.[13] Two large zones of the guidelines



Date Log # Name of Defendant (Last, First, M.I.) Police Photo # Prepared by:

Key
HD = high dropout rate
V = high diversion rate

PRETRIAL RELEASE GUIDELINES
Philadelphia Municipal Court and Court of Common Pleas

☐ Bench warrant only
☐ New charges only
☐ BW and new charges

Charge Seriousness Level

RISK	Least Serious 1	2	3	4	5	6	7	8	9	Most Serious 10	HOMICIDE
Lower 1	ROR/ Standard Conditions V 1	ROR/ Standard Conditions V 5	ROR/ Standard Conditions V 9	ROR/ Standard Conditions V 13	ROR/ Standard Conditions 17 HD	ROR/ Standard Conditions 21	ROR/ Standard Conditions 25	$ 1,000 - $ 3,000 29 HD	$ 1,500 - $ 4,500 33	$ 2,000 - $ 8,000 37 HD	Held Without Bail
2	ROR/ Standard Conditions V 2	ROR/ Standard Conditions 6 HD	ROR/ Standard Conditions 10 HD	ROR/ Standard Conditions 14 HD	ROR/ Standard Conditions 18	Release on Special Conditions Type I 22	Release on Special Conditions Type I 26 HD	$ 2,000 - $ 4,500 30 HD	$ 2,500 - $ 5,500 34	$ 2,500 - $ 8,000 38	Held Without Bail
3	ROR/ Standard Conditions 3	Release on Special Conditions Type I 7	Release on Special Conditions Type I 11 HD	Release on Special Conditions Type I 15	Release on Special Conditions Type II 19	Release on Special Conditions Type II 23	Release on Special Conditions Type II 27	$ 2,500 - $ 5,000 31	$ 3,000 - $ 6,500 35	$ 4,500 - $15,000 39	Held Without Bail
Higher 4	Release on Special Conditions Type I 4	Release on Special Conditions Type I 8	Release on Special Conditions Type I 12	Release on Special Conditions Type II 16	Release on Special Conditions Type II 20	Release on Special Conditions Type II 24	$ 2,500 - $ 4,500 28	$ 3,000 - $ 5,000 32	$ 3,000 - $ 8,000 36 HD	$ 6,000 - $ 50,000 40 HD	Held Without Bail

PTS USE ONLY CIRCUMSTANCES *(If Appropriate)*

SPECIAL CONDITIONS OF PRETRIAL RELEASE

Type I
☐ Telephone Reporting / Check-In
☐ Third Party Reporting
☐ Other:

Type II
☐ Frequent Telephone Reporting
☐ In-Person Reporting / Check-In
☐ Treatment or Supervision Program
☐ Other:

HOLD FACTOR(S) (To be tracked)
☐ Prepare Special Conditions
☐ Detainers
☐ Bench warrants
☐ Other:

ELIGIBLE FOR: ☐ Early Disposition ☐ Special Drug Program ☐ RIP ☐ Other

STANDARD CONDITIONS OF PRETRIAL RELEASE

The defendant shall conform to the following conditions of his/her release pending adjudication of criminal charges:
The defendant shall attend all court proceedings as required when scheduled.
The defendant shall submit to all orders and processes of the issuing authority or Court.
The defendant shall provide Pretrial Services with the address at which he/she is residing and with a working telephone number at which he/she may be reached reliably.
The defendant shall notify Pretrial Services of any change in address or telephone within 24 hours of the change.
The defendant shall not engage in, cause, or encourage threats, intimidation or retaliation against complainants or witnesses.
The defendant shall not possess any weapons.
The defendant shall obey such other conditions as imposed by the Court or Pretrial Services Agency.
☐ Other *(specify)*:

DECISION INFORMATION

COMMISSIONER'S DECISION
☐ ROR/Standard Conditions
☐ Release on Special Conditions
☐ Type I
☐ Type II
☐ Ten Percent Financial Bail *(specify full amount)*:

GUIDELINES FOLLOWED:
☐ YES ☐ NO; ☐ Less Restrictive ☐ More Restrictive
If no, provide reasons:

COMMISSIONER'S SIGNATURE
(Date)

REVIEW REQUESTED BY:
☐ DA
☐ PD

APPEALS JUDGE'S DECISION:
(Signature) *(Decision)*

Figure 1. Example of pretrial release guidelines.

classification – referred to as type I and type II defendants – encompassing categories of type I or medium-to-type II or higher-risk defendants, would call presumptively for assigning "supervision" in the community to defendants, instead of the usual cash bail amounts responsible for pretrial detention. In this way the pretrial release guidelines specified categories of usually detained defendants for whom the new presumption would be release before trial under supervision or some form of monitoring (at that time yet to be determined). Thus, a specific gambit of the pretrial release guidelines strategy was to create pretrial release supervision for the first time in Philadelphia, to promote and manage sufficient release of defendants using supervision, and, ideally, to keep pretrial detention to levels that were, at minimum, no higher than existed under Federal emergency procedures. The major challenge faced in moving the pretrial release guidelines from theory to practice was that targeted "supervision" of defendants now had to be invented.[14]

Four sequenced field experiments testing elements of a pretrial release supervision strategy

Because of the lack of empirical evidence relating to "best practices" in pretrial supervision and the City's legal and political needs for the strategy to produce evidence of positive impact, the Philadelphia task force leadership employed research as a tool for formulating, implementing and testing the supervision strategy. As opposed to making general assumptions about what supervision might be, in the Philadelphia pretrial release experiments supervision would be defined as an overall strategy to constrain defendant misconduct during pretrial release based on a number of evidence-based practices. In this way the task force would have information testing assumptions shaping the supervision approach, would have evidence about the impact of supervision and would have the ability to make adjustments to the approach based on this evidence.

This research-grounded policy development approach adopted a working definition of supervision that posited five testable elements of an overall strategy intended to constrain the misconduct of defendants granted pretrial release. These elements included (a) gaining decision-maker compliance with pretrial release guidelines so that supervision was indeed assigned as a condition of pretrial release at initial appearance for the targeted categories of defendants; (b) notification and reminder procedures by pretrial services staff to remind defendants of important court dates in advance; (c) education of defendants at the first stages of the process through an orientation explaining the requirements of the pretrial release supervision process; (d) supervision itself, consisting of case management, in-person and telephone contacts, and (e) timely, early identification of noncompliant defendants for enforcement procedures to bring them back into compliance with the requirements of supervision and the judicial process.[15]

The first element – the need to have judicial decision makers use the pretrial release guidelines to assign supervision – was studied separately (Goldkamp et al., 1997) and was found to have been reasonably achieved. The remaining elements

were organized, implemented for the first time and then tested in four sequenced field experiments that examined the feasibility of the supervision strategy (its practical implementation) and addressed assumptions about the nature of the noncompliant defendant (the "intentional" versus "disorganized" types) as well as about the utility of its deterrent and incapacitative features.

Experiment 1 – testing the impact of in-court notification at first appearance

> Conditions of release should be accompanied by the means to facilitate compliance. Defendants must be informed of where or when they are to appear to comply with the conditions of appearance. This requires a system of written notification to defendants which details the date, time, and exact location of required appearances and provides a telephone number for the defendant to call if he has questions....In many jurisdictions the function of providing notice is carried out by a pretrial services agency and is often accompanied by a telephone contact with the defendant to confirm that he understands...it should be the ultimate responsibility of the court to assure that adequate notice is provided...NAPSA (1978, Commentary, Standard VI; A:30)

Some critics of traditional pretrial release systems have argued that an unknown but probably large portion of defendant failure to appear (FTA) can be explained by defendants' lack of comprehension of the proceedings and/or their confused frame of mind after arrest. Having understood poorly what was transpiring in court, this "disorganized" or "unintentional" noncompliant defendant line of reasoning suggests, defendants would predictably have difficulty later remembering details such as the scheduling of court appearances, locations, etc. The first field experiment, designed to test this element of a prospective supervision strategy, was carried out in advance of implementation of the pretrial release guidelines (the mechanism for the larger reform effort in Philadelphia) and its supervision component. Testing the "disorganized defendant" explanation for defendant noncompliance with pretrial release conditions, judicial leaders wished to learn whether simple notification approaches in themselves could demonstrably reduce defendant absences from court proceedings.

Under the experimental condition, for the first time in a Philadelphia courtroom, a pretrial services staff member would play an active role in court: the pretrial services staff person would explain to the defendant what had just occurred, what would occur next, what the defendant would be expected to do, and where and when the defendant should report next. The staff person would then give the defendant a card on which these instructions were summarized and on which a telephone number for a new pretrial services automated [automatic voice response (AVR)] phone system was indicated. All defendants were required to call the agency within 24 hours of release. The automatic phone system was to serve as a second experimental condition designed to test early compliance among released

defendants and allow pretrial services staff to follow-up on defendants who did not call in as required. The control group condition, representing the until then normal state of affairs, was to have defendants processed at the first judicial stage in the normal fashion, with no in-court contact, no explanation by pretrial services staff, and no requirement to call in to the AVR system.

Random assignment of defendants to experimental and control group conditions was carried out by alternating conditions in court on successive days between November 13 and November 26, 1995. Thus, on the first day of the study period, the preliminary arraignment courtroom would be staffed by a pretrial services representative who would carry out the experimental approach over all three shifts. On the second day, normal, non-experimental procedures would be in place. With some minor deviations, this plan was carried out on alternating days during a 14-day period, generating a population of 1,285 defendants appearing for preliminary arraignment after arrest.[16] Because of resource considerations, we drew random samples of 175 cases from each of the conditions (after we had removed problem cases, 136 control and 163 experimental group defendants were included in the study).[17]

The in-court notification experiment hypotheses

In effect this experiment sought to test two hypotheses:

- *Hypothesis 1.1* Defendants who had the in-court contact and explanation of the pretrial process and its requirements by pretrial services staff should generate notably lower rates of FTA. The expected effect of the innovation on defendant rearrest for crimes committed during pretrial release, however, is not clear. From the perspective that sees FTA and rearrest as related forms of misconduct, one would expect rearrests to be lower among experimental group defendants as well. From the perspective that sees failed attendance in court as a very different phenomenon from rearrest, no effect on rearrest rates would be expected.
- *Hypothesis 1.2* The requirement imposed on experimental group defendants to call the pretrial services AVR number should help identify early "noncompliers" (within 48 hours) and help pretrial services staff target those who would be likely to fail to appear in court subsequently. This would result in lower rates of noncompliance among experimental defendants than among control group defendants who were not required to call.

Findings from the in-court notification experiment

Preliminary comparative analysis confirmed that the two defendant groups were very similar on selected attributes, suggesting that random assignment worked fairly well to produce roughly equivalent groups of releasees.[18] Table 1 displays the outcomes for defendants in the two groups over a 120-day follow-up period. Contrary to the hypothesized outcome, failure to appear (FTA) did not differ

Table 1. Notification experiment (1): comparison of 120-day outcomes for experimental and control group defendants entering the judicial process at preliminary arraignment, November 13, 1995 to November 26, 1995 (pre-guidelines).

	Experimental Group		*Control group*	
Notification Experiment Outcomes	*(Number)*	*Percent*	*(Number)*	*Percent*
Failure to appear (FTA)*				
Total	(140)	100.0	(114)	100.0
None	(97)	69.3	(75)	65.8
One or more	(43)	30.7	(39)	34.2
Rearrest*				
Total	(140)	100.0	(114)	100.0
None	(115)	82.1	(98)	86.0
One or more	(25)	17.9	(16)	14.0
Fugitive status*				
Total	(140)	100.0	(114)	100.0
No	(120)	85.7	(98)	86.0
Yes	(20)	14.3	(16)	14.0

*Differences between groups were not significant at $P < 0.05$.

significantly between the two groups, with 31% of experimental and 34% of control group defendants failing to appear in court at least once during the follow-up period, and 14% of each officially in fugitive status at the end of the follow-up period. In addition, the two groups did not differ significantly in rearrests for crimes occurring during pretrial release, with 18% of experimental and 14% of control group defendants rearrested.

Implementation context: the in-court notification experiment

These findings do not appear to support the first hypothesis that notification, explanation and written instructions would reduce defendant misconduct. There are two possible explanations for these findings. The first is that, for some reason, implementation of the treatment condition was not successfully carried out. The second is that the in-court strategy was carried out but, given other factors, was not effective in assisting defendants to appear in court as required. Observation of the experiment suggested that the way the experimental treatment was introduced may not have given enough emphasis to the pretrial services role and that the in-court contact could have been better implemented. However, for practical reasons, it was necessary to introduce the approach without disrupting or slowing down the preliminary arraignment process. (The introduction of pretrial services personnel at this hearing represented quite an innovation in its own right; previously, pretrial services staff were not permitted in the preliminary arraignment courtroom.)

Although we can imagine more effective implementation of this strategy, we feel more comfortable accepting the second explanation as the more reasonable one. The in-court person-to-person notification procedures failed to bring about the hypothesized increased attendance in court (and reduced rearrest rate).

The second element of this notification experiment tested an automated call-in system that would serve as an early indicator of non-compliance with release conditions (i.e., not calling in as required) and would allow pretrial services staff to target those noncompliers with phone calls and letters reminding them of court dates and locations. This part of the experiment had serious implementation difficulties. After the experimental period it was discovered that the technology involved in the automated system was mostly dysfunctional during the study and that the results were unusable. Because the experiment could not be repeated, we are unable to report findings addressing the second hypothesis. We do draw cautious inferences from this malfunctioning technology about the danger of making assumptions about the introduction of technology as an "easy" solution to difficult policy problems.

Experiment 2 – testing the impact of supervision conditions

The second experiment tested assumptions related to key elements of the basic supervision strategy set in place under the pretrial release guidelines. The problem then was to determine how to form an appropriate "package" of actions that would provide supervision for defendants identified. The final strategy was based on an evolutionary and incremental approach; one that, in respecting the principle of release under the least restrictive conditions necessary, tested the effect of supervision elements starting with less restrictive and then moving toward more restrictive forms.

An ideal and basic experimental design would randomly assign target group defendants to either supervision or non-supervision conditions of release. Such a design would compare the performance of supervised defendants with defendants released with no supervisory conditions (i.e., tantamount to personal recognizance release) and answer the question of whether supervision offers any improvement over doing nothing at all. We did not adopt this approach for three reasons. The first reason was that a classical experimental design would oversimplify the questions posed by Philadelphia's supervision strategy. The aim was not only to investigate the relative impact of supervision, but also to examine the ingredients of supervision that contribute to an impact. Moreover, an objective was to determine the relative impact of supervising approaches for particular categories of targeted defendants.

The second reason for not employing the classic design was that, in the Philadelphia context, this type of experiment was unnecessary. We believed we had a sufficient functional (if not perfect)[19] equivalent of the no-release-conditions test in what amounted to a "natural experiment." We already had data showing how defendants in the type I and type II categories performed under the

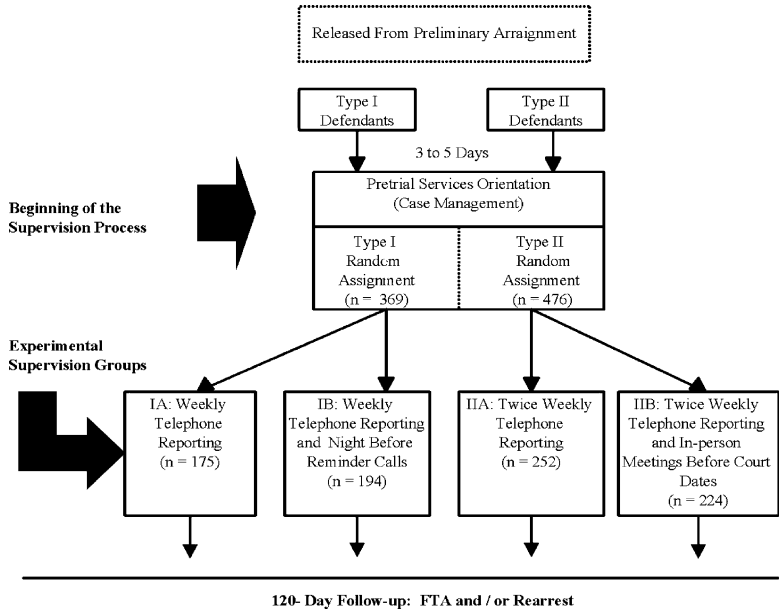


Figure 2. Design of the Philadelphia pretrial supervision experiment.

Federal consent decree *Harris v. Reeves* (or "*H.v.R.*") emergency release procedures when they were released before trial without supervision or other constraint. Depending on defendant category, these baseline data showed rates of failure-to-appear ranging from 36% to 56% and rearrest rates ranging from 13% to 20% among Philadelphia defendants released without supervision (Goldkamp et al. 1992).[20]

The third factor underlying the decision not to select a contemporaneous no-supervision control group was that it appeared unlikely that we would be able to secure agreement from judicial officials to release medium-to high-risk defendants without some form of constraint and on a random basis, all "in the name of science." Although getting support for use of an experimental design in a field setting is always a sensitive task, in this instance the sensitive political context made cooperation for the "all-or-nothing" test of supervision unlikely. Instead, the design tested the impact of varied supervision conditions of pretrial release and focused on the two groups of defendants targeted by the pretrial release guidelines for supervision (summarized in Figure 2). Based on odd–even last digits of their Philadelphia (person) identification numbers, type I and type II defendants entering pretrial services supervision were randomly assigned to two sets of supervision conditions (A and B) that differed in degree of "restrictiveness."

- *Type I* defendants with even digits were assigned to the A supervision condition that began with attendance at pretrial services orientation and included reporting

in by phone through the automated phone system once per week throughout the pre-adjudicatory period. Those with odd digits were assigned to the B condition, which included attendance at an orientation, calling in once per week, and receiving a personal phone call from the Warrant Unit pretrial services staff the night before each court date.

- *Type II* defendants with even digits (A condition) were required to begin the process with orientation and case management meetings and to call in to the AVR system two times per week. Those with odd digits (B condition) would attend orientation, call in twice weekly to the AVR and meet in person with their cases managers 3 days before every court date. In addition, if a defendant in the B group failed to attend the pretrial services meeting, the Warrant Unit would be notified and a warrant investigator would make a visit to the defendant's residence. The investigator would then instruct the defendant to attend the pretrial services meeting and remind the defendant of the upcoming court date.

The supervision experiment was carried out in the Philadelphia courts between August 1, 1996 and November 26, 1996. During that period, 845 defendants assigned type I and II supervision as a result of new charges appeared at the Pretrial Services Division, attended orientation, and then were randomly assigned to levels of supervision as shown in Figure 2.[21] This design permitted comparison of different levels of supervision within and across defendant type. The final assignment at the completion of the study period showed 175 type IA, 194 type IB, 252 type IIA, and 224 type IIB defendants. The study employed a 4-month (16-week) follow-up period to chart rates of failure-to-appear and rearrest among study defendants.[22]

The supervision experiment hypotheses

This second field experiment sought to test both a general hypothesis relating to the impact of supervision and hypotheses relating to specific conditions of supervision:

- *Hypothesis 2.1 – impact of supervision generally.* Each supervision group will produce lower rates of failure-to-appear and rearrest for new crimes during pretrial release than recorded by comparable pre-experiment (baseline) defendants. This hypothesis suggests that any supervision will produce an improvement over no-conditions release as shown in the baseline study of Federal emergency release.
- *Hypothesis 2.2 – added impact of agency-originated night-before reminder calls.* The calls from pretrial services staff to type IB defendants' residences the night before court dates will, compared to type IA defendants, produce lower FTA and rearrest rates because of the importance of personal contact from the judicial system in emphasizing the role of defendant accountability and possible consequences.
- *Hypothesis 2.3 – added impact of in-person meetings and active enforcement of compliance.* The requirements assigned to type IIB defendants (of calling-in, attending in-person meetings before each court appearance and active enforcement when requirements are not met) amount to the most restrictive of the

experimental supervision conditions. They will produce lower misconduct rates among type IIB defendants than will be recorded by their type IIA counterparts assigned only telephone reporting conditions.

- *Hypothesis 2.4 – equalizing effect of more restrictive conditions on higher risk defendants.* The more restrictive conditions applied to the higher-risk type IIA and IIB defendants should yield rates of misconduct at least as low as levels recorded by the lower-risk type IA and IB defendants, who had less restrictive conditions of supervision assigned but who would be expected to have lower rates of misconduct based on their guidelines risk classifications.

Findings from the supervision experiment

Table 2 presents the experimental results showing, first, that type IA and type IB defendants did not differ in their rates of misconduct over the 16-week follow-up period. The slight differences in the expected direction in failure-to-appear rates among type IA and IB defendants (at 22% and 20%, respectively) were not statistically significant. Similarly, the differences in rearrest rates between the two groups were in the opposite of the expected direction (9% and 11%), but were not significant. However, both types of misconduct rates were substantially lower than those generated by defendants classified as type I defendants in the baseline data (see Table 2, last three columns).

Table 2 further shows slight but non-significant differences between type IIA and type IIB defendants in rates of pretrial misconduct (with failure-to-appear rates of 23% and 26%, fugitive rates of 14% each, and rearrest rates of 16% and 14%). While these rates of misconduct are slightly higher than those produced by type I defendants (as would be predicted by their risk classification), type II defendants still exhibited much lower failure-to-appear rates than those produced among comparable baseline defendants in the earlier study, and rearrests that were slightly lower (or no higher) than among the baseline samples.

Columns 5 and 6 in Table 2 combine type I and type II defendants and then compare the subgroups with less restrictive options (A groups) to the groups with more restrictive options (B groups) to learn whether, viewed more generally, the restrictiveness of conditions makes a difference. That table again shows no significant differences in FTA or rearrest rates between A and B supervision-condition groups.

On the surface, these findings appear to suggest that gradations in restrictiveness of conditions of supervision in the experiment did not translate into commensurate differences in rates of misconduct. They do suggest, however, that the general content of supervision in the type I and II categories (of the sort applied to either the A or B group) produced rates of rearrest (for type 1 at least) and FTA (both types) substantially lower than those shown in the comparable baseline data.

The suggestion in *Hypothesis 2.1* that, across the board, some supervision produces better effects on defendant performance during release than no supervision (no-conditions) receives partial support. However, defendants in the more restrictive supervision groups (IB and IIB) did not show lower misconduct

Table 2. Supervision experiment (2): comparison of 120-day outcomes for supervision experiment groups, combined groups, and baseline defendants, August 1, 1996 to November 26, 1996.

	Group IA		*Group IB*		*Group IIA*		*Group IIB*		*All A defendants*		*All B defendants*		*1991 Federal releases**		*1992 Baseline**		*1992 Federal releases**	
Outcomes	*(Number) (#)*	*Percent %*	*(#)*	*%*	*(#)*	*%*	*(#)*	*%*	*(#)*	*%*	*(#)*	*%*	*(#)*	*%*	*(#)*	*%*	*(#)*	*%*
Failure to appear																		
Total	(175)	100.0	(194)	100.0	(252)	100.0	(224)	100.0	(427)	100.0	(418)	100.0	(374)	100.0	(1411)	100.0	(652)	100.0
None	(137)	78.3	(155)	79.9	(195)	77.4	(165)	73.7	(332)	77.8	(320)	76.6	(165)	44.0	(903)	64.0	(365)	56.0
One or more	(38)	21.7	(39)	20.1	(57)	22.6	(59)	26.3	(95)	22.2	(98)	23.4	(209)	56.0	(508)	36.0	(287)	44.0
Rearrest																		
Total	(175)	100.0	(194)	100.0	(252)	100.0	(224)	100.0	(427)	100.0	(418)	100.0	(374)	100.0	(1411)	100.0	(662)	100.0
None	(160)	91.4	(173)	89.2	(212)	84.1	(192)	85.7	(372)	87.1	(365)	87.3	(307)	82.0	(1228)	87.0	(530)	80.0
One or more	(15)	8.6	(21)	10.8	(40)	15.9	(32)	14.3	(55)	12.9	(53)	12.7	(67)	18.0	(183)	13.0	(132)	20.0
Fugitive (at 120 days)																		
Total	(175)	100.0	(194)	100.0	(252)	100.0	(224)	100.0	(427)	100.0	(418)	100.0						
No	(153)	87.4	(172)	88.7	(216)	85.7	(192)	85.7	(369)	86.4	(364)	87.1						
Yes	(22)	12.6	(22)	11.3	(36)	14.3	(32)	14.3	(58)	13.6	(54)	12.9						

*Differences were not significant at $P < 0.05$.

rates than their counterparts assigned less restrictive forms of supervision, thus failing to support *Hypotheses 2.2* and *2.3* relating to the relative impact of more restrictive conditions: night-before reminder calls and in-person visits.

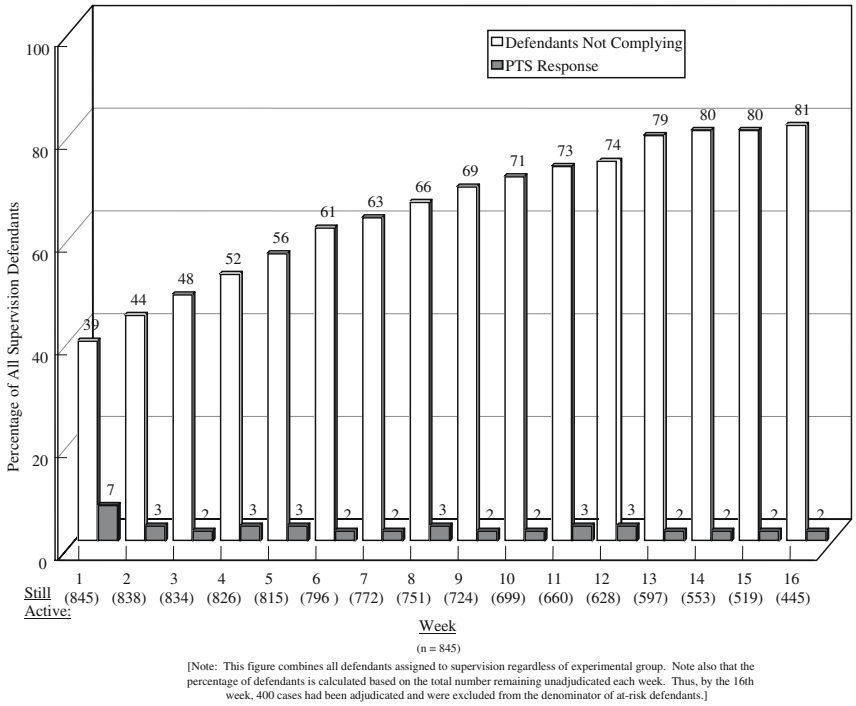
Hypothesis 2.4 posited that applying more restrictive conditions to the higher-risk supervision groups (IIA and IIB) should render their misconduct rates as low as those generated by the lower-risk supervision groups (IA and IB). The data are inconclusive regarding this "equalizing" effect. Certainly, the slightly higher rates shown by type II defendants are not significantly different from those shown by type I defendants in Table 2. The problem is that we have not demonstrated that more restrictive conditions have reduced commensurately the higher-risk propensities of type II defendants. It is equally probable that the minimum supervision conditions associated with type I defendants (principally telephone reporting) were sufficient to produce the lowered misconduct rates among type II defendants or that the "general supervision effect" (just being involved in supervision, regardless of conditions) was sufficiently strong to explain the lowered type II rates.

Implementation context: operationalizing the supervision experiment

Taken as a whole, the findings from the supervision experiment suggest that some minimum component of supervision improves on the no-conditions state of affairs represented by the emergency release captured in the Philadelphia baseline data. Reporting more or less frequently or adding other conditions did not appear to affect outcomes noticeably. In interpreting results of field experiments, however, the degree of confidence one may have in the findings depends partly on knowledge of how well the experimental conditions were implemented. Consideration of this aspect of the supervision experiment, unfortunately, raises some questions about whether the findings are so straightforward.

One important component of each of the supervision conditions involved calling in to the AVR telephone system. Although we cannot say that this system worked flawlessly, it appeared in this experimental application to work reasonably well (and was considerably improved over its attempted operation during experiment 1). However, it is one thing to examine the relationship between assignment of certain conditions of supervised release (such as calling in once or twice weekly) and rates of pretrial misconduct, and quite another to examine how that relationship actually could have operated, beginning with some measure of how well defendants accomplished those conditions. Implementation failure should not be mistaken for failure of the experimental treatment being tested.[23]

Figure 3 shows the proportion of all supervised defendants meeting their telephone reporting requirements by week over the 16-week observation period. (This figure combines data for all categories of supervisees regardless of experimental group assignment and telephone reporting requirements.[24]) These data certainly cast doubt on the assumption of full compliance with (or full implementation of) the supervision condition by defendants. The highest rate of compliance with telephone reporting among all defendants was found in week 1,



[Note: This figure combines all defendants assigned to supervision regardless of experimental group. Note also that the percentage of defendants is calculated based on the total number remaining unadjudicated each week. Thus, by the 16th week, 400 cases had been adjudicated and were excluded from the denominator of at-risk defendants.]

Figure 3. Pretrial services response (PTS) and noncompliance with telephone reporting within a 16-week follow-up period among all defendants (IA, IB, IIA, IIB) assigned supervision, August 1, 1996 to November 26, 1996, by week.

but even then only 61% of those required to call in did so. The compliance rate declined gradually but steadily in each of the succeeding weeks, until it reached a low of 19% compliance in week 16. Only 37 of the 845 defendants under supervision in the study made all required telephone contacts.

One could argue that these findings do not portray poor implementation of the supervision conditions but rather provide the predictive signs that persons who have not complied with telephone reporting are the evolving noncompliers, those next likely to fail to appear or be rearrested for committing new crimes. The problem with viewing the compliance rates each week as "early warnings" of more serious misconduct is that, from the first week on, these rates would greatly over-predict likely absconders (occurring among 23% of defendants by the end of week 16) and pretrial "criminals" (involving only 13% of defendants).

Relative frequency of night-before reminder calls made by pretrial services

Of the 376 court dates we could document[25] that were associated with group IB defendants, 325 were preceded by pretrial services reminder calls, an implementation rate of 86%. However, as in real life, this telephone-contact approach

experienced uneven results. First, some defendants simply did not have telephones in their own residences and had to be contacted through second parties. Second, the data in Figure 4 show that not all calls made resulted in an appropriate contact. In only about 55% of instances when defendants had to be in court the next day were calls made that resulted in acceptable contacts. In 14% of such instances, reminder calls were not placed by pretrial services. In 32% of cases with court dates the next day acceptable calls were not achieved (they were made but did not have an acceptable result).

Using defendants (not scheduled court dates) as a base, we found that 32% had no acceptable calls (including defendants who had no reminder calls placed), 30% had at least some acceptable calls made, and 38% had acceptable night-before calls made in advance of all court dates during the 16-week study period. Using both court date and defendant-based measures, the differences in pretrial misconduct by call outcomes (i.e., no acceptable, some acceptable, all acceptable) were not statistically significant. In short, whether reminder calls or appropriate contact were made appeared to have little or no effect on defendants' attendance in court the next day or on likelihood of rearrest.

Relative compliance with in-person meeting in advance of court dates

During the 16-week study period, the 224 type IIB defendants were scheduled for 487 court dates that we could document, and meetings were scheduled in advance

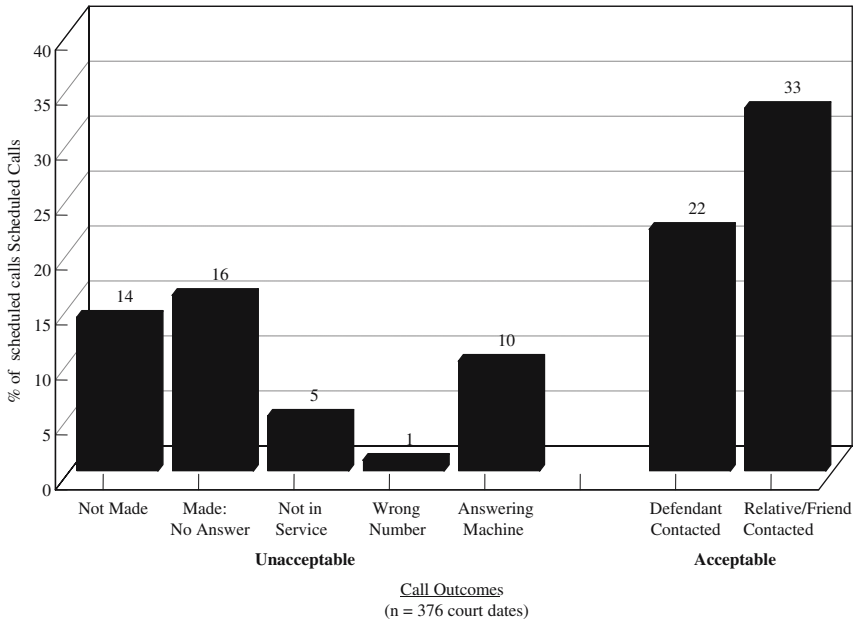


Figure 4. Outcomes of scheduled night-before reminder calls for type IB defendants (Scheduled Calls Measure) assigned supervision, August 1, 1996 to November 26, 1996.

of the court dates 452 times. Two-thirds of defendants kept their appointments as required, and 9% failed to attend but had a valid reason. Fourteen percent missed their appointment but did not have a Warrant Unit visit after failing to comply; 3% missed the appointment and had a visit from the Warrant Unit staff at their residence as a result. About 7% were never scheduled for appointments before their court dates. Although Warrant Unit staff were directed to make home visits when defendants did not attend the in-person meeting at pretrial services 3 days before the court date, they failed to do so in the majority of cases.

Figure 5 shows the relationship between meeting outcomes and pretrial misconduct using the court date based data. This figure suggests that defendants who had acceptable meeting outcomes showed significantly lower FTA and rearrest rates than those with unacceptable meeting outcomes. These findings seem to suggest that something about the meeting appointment process served as an effective reminder to defendants about their obligations to attend court, particularly if they kept their appointments (which most did).

For group IIB defendants, failure to comply with the meeting condition should have resulted in demonstrable consequences from the Warrant Unit. Because the Warrant Unit responded to fewer than one-fifth of the missed appointments, it cannot be argued that this deterrent element of the IIB supervision conditions was sufficiently well implemented to assess its effects.

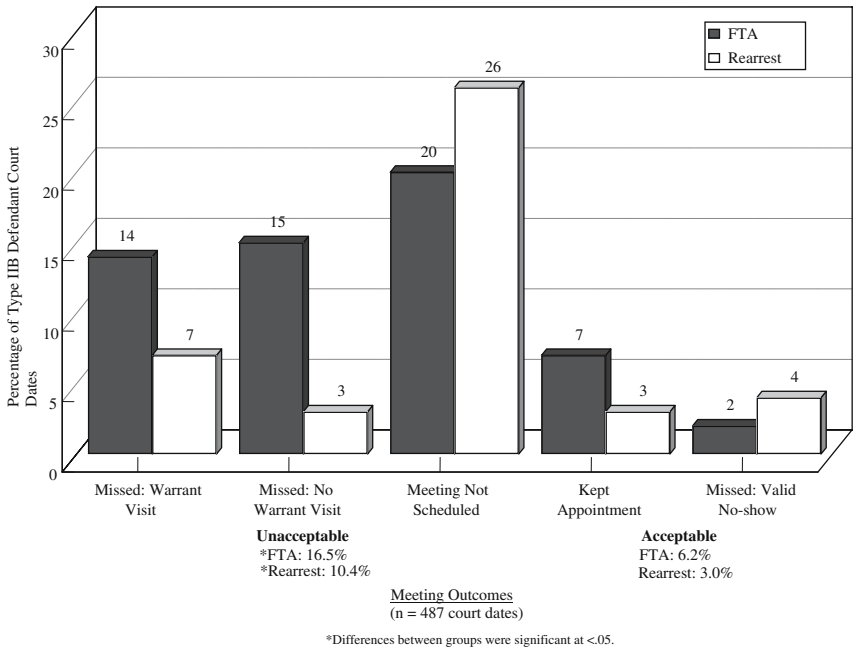


Figure 5. Relationship between compliance with in-person meeting requirement and pretrial misconduct (FTA, rearrest) among type IIB defendants assigned supervision, August 1, 1996 to November 26, 1996.

Experiment 3 – testing the impact of preventive notification of defendants prior to orientation and court

Implications of early no-shows for the supervision experiment: the rationale for a third experiment

During the supervision experiment (experiment 2), about 53% of defendants assigned type I or II supervised release failed to report as ordered to the pretrial services agency within 3 to 5 days to initiate the supervision process (and were not part of the supervision experiment).[26] The high no-show rate in advance of when supervision could start meant that (a) the guidelines strategy was successful in producing release among the target categories of defendants, but that (b) there was no means of supervising an important portion of the medium-to-high risk defendants identified as the key target population. Indeed, the supervision strategy was meant to target medium-to-high risk defendants detained under former practices now being released under type I or II conditions of supervision, not just those released defendants who actually attended orientation 3 to 5 days after release as the first required step in the supervision process. The "early no-shows" therefore represented the extent to which this policy intervention "missed the target" population.

Released type I and II defendants who failed comply with their release orders to attend pretrial services orientation and to undergo case management intake subsequently recorded higher rates of failure to appear in court (50% versus 21%, significant) and of being rearrested (17% versus 11%, not significant) during a 120-day follow-up period than those who did attend. One can infer from these data that either (a) the defendants who avoided supervision (no-show) were a priori higher-risk and more likely to perform poorly during pretrial release than their supervised counterparts or (b) the fact that they were not exposed to supervision of any type contributed to those higher rates.

The roughly 50% no-show rate at the important orientation (supervision intake) stage also raises a serious methodological concern of selection bias for the assessment of the impact of supervision conditions. The higher misconduct rates among the no-shows and the much lower rates among those attending the first stages of supervision suggest that the supervision process may be enrolling a more compliant or responsible subset of defendants assigned type I and II supervised release. If this indeed is the case, we would expect the defendants beginning supervision as a group to perform better than the defendants who failed to attend initial meetings at pretrial services. With relatively homogeneous, better risk defendants entering supervision (taken together), one would expect better results during pretrial release, regardless of the specific supervision packages, when compared with baseline defendants and with "no-shows." Quite conceivably, the results of the supervision experiment could have been much different had a greater proportion of the less compliance-prone defendants been successfully placed under supervision.

The second notification experiment

To address this early no-show phenomenon, we constructed a second proactive notification experiment. The notification strategy involved random allocation of 423 defendants ordered released to types I or II supervision by commissioners in Philadelphia's Municipal Court at preliminary arraignment to an experimental group ($n = 207$) and a control group ($n = 216$), based on odd–even last digits of I.D. numbers. The identification of defendants for random assignment began immediately the results of preliminary arraignment became known, rather than later at the pretrial services orientation stage, where attending defendants were randomly assigned to supervision conditions.[27] Instead, the experimental group was to be exposed to a proactive notification strategy that involved pretrial services staff placing calls during the day and/or evening hours within the 24-hour period immediately before the orientation (i.e., first appointment) date to remind defendants of the requirement to visit pretrial services, as well as of the specific date and time.[28] The control group would be handled in the normal, reactive fashion.

The outcome measures (or dependent variables) for this experiment included attendance at pretrial services orientation, and failure-to-appear and rearrest rates measured over a 1-month follow-up period. The reason for the relatively short follow-up period in this instance is because the effects hypothesized by the notification experiment were to be relatively immediate in nature.

Second notification experiment hypotheses

- *Hypothesis 3.1 – impact of advance notification on attendance and enrollment.* Advance notification and reminder of the pretrial services orientation to experimental group defendants will notably increase the rate of attendance at pretrial services and enrollment in the supervision process over control group defendants.
- *Hypothesis 3.2 – impact on pretrial misconduct.* Increased attendance and enrollment in supervision will translate into lower rates of failure to appear and rearrest among experimental group defendants when compared to the control group.

Findings from the second notification experiment

The differences between the two groups of defendants on the outcome measures of principal concern were not statistically significant (see Table 3). Experimental group defendants who received reminder calls attended orientation at a slightly higher rate (56%) than their control group counterparts (51%). Both groups also recorded similar rates of failure to appear, measured over the following 30-day period (at 18% and 19%, respectively), and both showed similarly low rates of rearrest (6% and 8% of experimentals and controls, respectively).[29]

Table 3. Notification experiment (2): Comparison of 30-day outcomes for experimental and control group defendants released on type I or II supervision at preliminary arraignment, October 21, 1997 to November 18, 1997.

	Experimental group		*Control group*	
Second notification experiment outcomes	*(Number)*	*Percent*	*(Number)*	*Percent*
Attendance at pretrial services response (PTS) orientation*				
Total	(192)	100.0	(216)	100.0
No	(84)	43.8	(107)	49.5
Yes	(108)	56.3	(109)	50.5
Failure to appear*				
Total	(204)	100.0	(215)	100.0
No	(167)	81.9	(175)	81.4
Yes	(37)	18.1	(40)	18.6
Rearrest*				
Total	(204)	100.0	(215)	100.0
No	(191)	93.6	(198)	92.1
Yes	(13)	6.4	(17)	7.9

*Differences between groups were not significant at $P < 0.05$.

Implementation context: the pitfalls of telephone notification in the second notification experiment

As in the previous experiments it would be misleading to accept the findings of this field experiment at face value, assuming that the experiment was straightforward and fully implemented as intended. As best we could assess, pretrial services staff performed the experimental regimen nearly flawlessly. However, because this strategy relied on telephone contact, the results of placing calls varied, and the variations in call completion appear to have been related to experimental outcomes.

About 9% of defendants apparently had no phone and could not provide a number located at a residence or belonging to a family member. Eight percent had provided numbers that were presumably fraudulent and resulted in "wrong numbers" when called. In another 9% of cases the number was called repeatedly but was never answered. In only 12% of the cases, the defendant was actually contacted and spoke with the pretrial services staff person as planned. In nearly half (48%) of the experimental cases the defendant was not contacted, but another person close to the defendant (immediate family member, relative, friend) was reached and a message then was left with that person. In 15% of the cases, we were unable to document the result of the telephone call. In short, these findings illustrate the difficulty with the in-advance telephone strategy (similar to the difficulties discussed for type IB defendants in the supervision experiment above).

Although certainly constrained by small numbers of cases in some categories, Figure 6 at least suggests that the nature of the phone contact may be related to

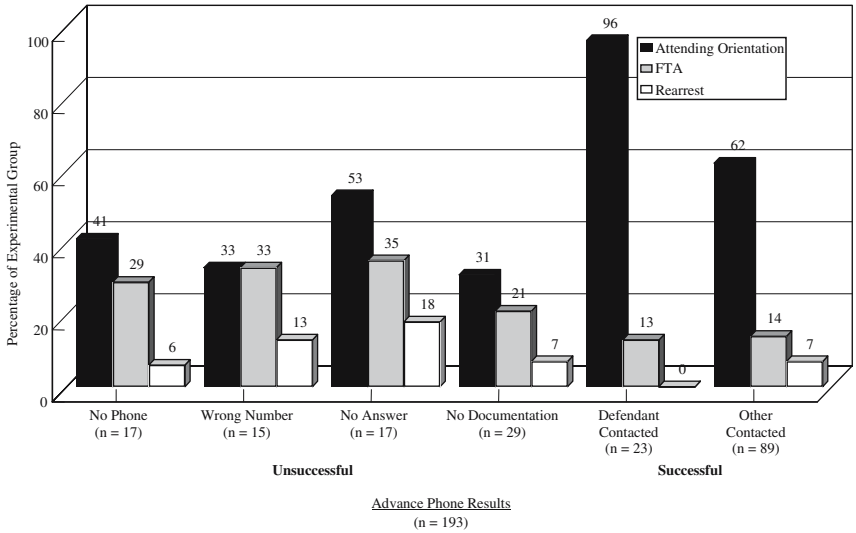


Figure 6. Relationship between phone outcomes and both attendance at pretrial services response (PTS) orientation and pretrial misconduct (FTA, rearrest) among experimental group defendants released on type I or II supervision, October 21, 1997 to November 18, 1997.

experimental outcomes. The "unsuccessful" categories with no contact (wrong number, no phone number, and no answer) showed the lowest rates of attending pretrial services orientation (overall, 43% versus 69% of those with contact). Almost all defendants who were successfully contacted attended as required. Calls resulting in contacting "other" persons were associated with medium rates of attendance, though notably higher than the no-contact categories of defendants. FTA and rearrest rates during the following 30 days conformed to the same pattern. The inference to be drawn from these findings may be that, when pretrial services actually makes contact, the advance telephone strategy has a beneficial effect.[30] The larger implications are that, as a means of a supportive or threat-communicating strategy (as deterrence would require), the telephoning strategy suffers from logistical difficulties – not so much in placing advance reminder calls, but in actually making contact with defendants in a large urban jurisdiction.

Experiment 4 – testing the impact of enforcement on early "no-shows" Bringing noncomplying defendants back into compliance

Having determined that a preventive strategy – at least as implemented – was apparently not going to resolve the no-show dilemma, the next approach was to design an enhanced reactive strategy. Experiment 4 tested a post-hoc enforcement initiative designed to act upon defendants once they had failed to begin the supervision process as required. The objective was to identify and "recover" early noncompliers and bring them back into compliance with the judicial process.

The targeted enforcement experiment

Upon analyzing the supervision experiment data, we concluded that defendants who had not made their initial pretrial services agency visit within 7 days of their scheduled date formed a critical group who were at high risk of subsequently failing to appear in court and/or being rearrested for new crimes. The 7-day cutoff allowed identification of defendants at a stage both late enough to document their serious (as opposed to inadvertent) noncompliance and early enough to prevent failure to appear in court, because most court hearings would not yet have occurred. The experimental enforcement strategy was simple in concept: As a first step, warrant/investigation officers would attempt to make contact with non-complying defendants through calls to the defendant, family, friends, etc. If the defendant could not be contacted, warrant officers would visit the defendant's residence. Whether by phone or by home visit, the content of the communication would inform the defendant that: (1) he/she had violated a condition of release by not attending the orientation; and (2) failure to attend within the next 2 days could result in arrest and revocation. This strategy was intended to allow for the helpful approach assuming an "unintentional" noncomplier or for clear communication of threat of sanction for defendants who were intentionally avoiding the court process.

Between November 17, 1997, and December 19, 1997, nearly 200 defendants were identified as early no-shows and were randomly assigned to an experimental ($n = 93$) or control ($n = 103$) group. Experimental group defendants were exposed to the targeted enforcement strategy, while control group defendants were tracked in the normal fashion without intervention. This experiment employed a 1-month follow-up period, which was deemed sufficient to determine whether defendants were successfully returned to compliance and to track their relative rates of early misconduct (FTA and rearrest).

Targeted enforcement hypotheses

- *Hypothesis 4.1 – targeted enforcement will* "*recover*" *early noncompliers to supervision.* Defendants who were documented as early noncompliers will be returned to the supervision process through the Warrant Unit interventions.
- *Hypothesis 4.2 –* "*recovered*" *defendants will show lower rates of misconduct.* Compared to control group defendants, experimental group defendants will produce lower rates of early FTA and rearrest as measured in the 1-month follow-up.

Findings from the enforcement experiment to recover early no-shows

Table 4 compares the misconduct rates of the two groups. Discouragingly, no defendant in either group attended pretrial services orientation within 48 hours of the 7-day intervention by the Warrant Unit. Eventually, 9% of the experimental and 4% of the control group defendants did later appear at orientation to begin the supervision process. This difference was not significant and suggests that the late enrollments that did occur could not be attributed to enforcement strategy efforts.

Table 4. Targeted enforcement experiment (4): comparison of 48-hour and 30-day outcomes for experimental and control group defendants failing to show at pretrial services response (PTS) orientation within 1 week of required date, November 17, 1997 to December 19, 1997.

	Experimental group		*Control group*	
*Targeted enforcement experiment outcomes**	*(Number)*	*Percent*	*(Number)*	*Percent*
Attendance at PTS orientation within 48 hours				
Total	(93)	100.0	(103)	100.0
No	(93)	100.0	(103)	100.0
Yes	(0)	0.0	(0)	0.0
Attendance at orientation anytime later				
Total	(93)	100.0	(103)	100.0
No	(85)	91.4	(99)	96.1
Yes	(8)	8.6	(4)	3.9
Failure to appear (FTA)				
Total	(92)	100.0	(100)	100.0
No	(65)	70.7	(66)	66.0
Yes	(27)	29.3	(34)	34.0
Rearrest				
Total	(92)	100.0	(100)	100.0
No	(81)	88.0	(85)	85.0
Yes	(11)	12.0	(15)	15.0

*Differences between groups were not significant at $P < 0.05$.

The same table shows that experimental group defendants had a slightly lower 1-month FTA rate (29%) than control group defendants (34%). More than that, measured at the 1-month mark, the FTA rates for both groups were relatively high. Finally, the rearrest rates among the two groups of noncompliers did not differ significantly, and again, these rates were comparatively high.

Implementation context: targeting early noncompliers

This experimental intervention depended on a somewhat more involved scenario than those described for the earlier supervision experiments. After the targeted early noncompliers had been identified and randomly assigned to the appropriate groups, the names of those in the experimental group were forwarded to the Warrant Unit for action. First, Warrant Unit staff were to call the defendant and be convinced that the defendant was fully aware of the obligation to report to pretrial services within the next 2 days. If satisfactory contact was not made, then warrant officers were to be deployed to visit the residence and to try to speak personally to the defendant, or, failing that, speak to another person at the residence, and, finally, if nothing else was successful, to leave a letter at the residence with the specified communication.

Warrant Unit records indicated that about one-third (33%) of the contacts proceeded no further than a phone call. In 43% of experimental group cases, a Warrant Unit visit was made. In 24% of the cases no contact was made or no record of a contact was kept. When telephone calls were the only action taken,

13% established no contact with anyone in the residence, 10% reached the defendant and 77% dealt with another person who was asked to convey the message to the defendant. When visits by warrant officers were made to the defendants, about one-fourth resulted in no contact, 18% involved an incorrect or fraudulent address, and a handful resulted in contact with the defendant (10% or four persons). In fully half the cases, the outcome of the visit was not recorded.

When the extent of implementation of the planned scenario was examined, it became apparent that few defendants were actually contacted, either by telephone or by warrant officers in person at their residence. As a result, it is clear that any "message" of threat of consequences was delivered to only a small proportion of the intended audience. As a result, we must conclude that the targeted enforcement strategy was not effectively implemented and that the results are not a serious test of the approach.

Conclusion: the utility of the experimental evidence in the Philadelphia pretrial release experiments

It is not uncommon in social science experimentation to read about the many ways in which experiments do not work out in the field as planned. Often, the inability to carry out the experiment according to the intended design makes the findings difficult to interpret. Difficulties in implementing experiments, particularly in random assignments, raise the methodological question of whether the prospects of a "broken" field experiment make use of experimental design less attractive than adoption of a second-best, quasi-experimental design that might have a better chance of being carried out successfully. In the experiments described here, the experimental conditions were mostly well operationalized, and, certainly, random assignment succeeded in each instance. On the surface, at least, the activities of the agencies involved appeared to provide sound tests of the hypotheses outlined.

In each experiment, however, implementation of the experimental conditions represented real innovations in the area of court and pretrial services operation. The exercises were simple in concept, but not so simple to implement in the context of the Philadelphia justice system. In each of the experiments, examination of the implementation context revealed developments and difficulties that affected the ability to draw clear inferences about the impact of the various experiments. The difficulties associated with operating a computerized telephone call-in system, with contacting defendants by telephone, with locating and visiting their residences for purposes of enforcement, with the ability to deliver sanctions when promised, as well as other real-world practicalities, raise serious questions about the findings of no difference between control and experimental group defendants on the key measures. In examining the threats to internal validity of the experiments, it becomes clear that the experimental findings need to be considered in the context of implementation findings.

This leaves us with questions about what the findings might have been, if certain factors were not involved. In the first notification experiment (in-court), having a pretrial services staff person personally explain expectations to the

defendant, and provide written instructions in the form of a card, did not make a difference in court attendance and rearrest rates. Perhaps placing greater emphasis on that role in court and eliminating other competing distractions could have produced a better test of the notification approach and better outcomes. Perhaps, to prevent the chronic high rate of no-shows among type I and II defendants given immediate street release, pretrial services staff should be deployed at police locations from which street release often occurs around the city to make an immediate supervision connection with each defendant. The supervision experiment produced favorable results when compared with past practices, showing higher compliance rates among those supervised when compared with three baseline samples of unsupervised defendants. But varying telephone reporting, staff reminder calls and in-person visits did not make much difference between experimental groups despite – or perhaps because of – the various difficulties associated with their operation.

The best illustration of the importance of implementation context in interpreting experimental results is the 50% early no-show rate among released defendants assigned to pretrial release supervision, that is, the "missed target" defendants who were successfully released (and who would have been detained under past practices) but who immediately disappeared from the judicial process. Although the supervision experiment appropriately focused only on those who entered supervision, the selection process that created the pool of supervised defendants (and allowed 50% to avoid enrollment) must be considered germane to understanding the experimental outcomes. Finally, the second notification and the early no-show enforcement experiments, both designed to address the early no-show phenomena, also struggled with implementation problems, which complicated interpretation of their findings.

The role of supervision in restoring accountability in pretrial release

When the close link between implementation context and experimental outcomes is taken into consideration, it can be seen that this research has identified important lessons about the role of pretrial release supervision and its elements – as well, perhaps, as the challenges of implementing change in justice practices. Not only did the guidelines and the supervision strategy represent major change in local justice practices, but the research also was conducted early on in the reform process so that feedback could be provided to local officials to inform necessary modifications and improvements. Thus, it is fair to add that our findings are drawn from early and evolving efforts, rather than longstanding and seasoned practice.

Two principal conclusions emerge from the collection of findings we have described in the body of this article. The first relates to the impact of notification strategies in reducing defendant misconduct (FTA and rearrest). The second relates to the role of deterrence in establishing conditions of supervision. First, we tested notification approaches as part of a supervision strategy in two experiments, both seeming to show no differences between experimental and control

group defendants. These findings suggest that, alone, the facilitative information strategy wields little influence on later defendant behavior during pretrial release. On the surface it may seem that this conclusion does not offer support for the "disorganized defendant" theory, that a large share of defendants who fail to appear in court do so because they are confused, ill-informed or forget. Apparently, the attempts to provide assistance to facilitate attendance in court through an active reminder process did not produce a notable response among targeted defendants. By implication, then, the findings instead might offer at least superficial support for the assumption that many defendants are "intentional" not inadvertent noncompliers.

This initial conclusion, however, should be tempered by the implementation findings from the second notification experiment. The experience with staff attempting to contact defendants by phone in advance of their pretrial services orientation date showed, first, that making contact by telephone within a narrow timeframe is difficult, but, second, that when defendants were actually reached, a very high rate of compliance was achieved. The lesson to be drawn from this finding is more hopeful than the experimental findings suggest. If supervisory staff can have an effective means of reaching defendants directly, defendant performance appears better. However, this is not easily accomplished in a large urban center where many defendants do not have telephones or have cell phones, and supervisors cannot get to know each assigned defendant personally when they are also responsible for hundreds of other defendants. Assuming a better methodology for establishing contact with defendants, however, we cannot be sure then whether the "disorganized defendant" is being constructively channeled into the required behavior or whether more effective threat communication is being delivered. This last possibility would credit deterrence as an effective strategy in ensuring defendant compliance. Both interpretations are speculative, however, and point out the challenges of assuming that technology (telephone) simply solves the problem.

The second principal lesson involves the deterrence-related aspects of the pretrial release supervision process, when the working assumption of the "intentional criminal" model of noncompliance is more salient. It is in this area that the demands of jail crowding and a successful supervision strategy collide. The structure of supervision can be viewed as heavily influenced by deterrent aims, balancing necessary requirements defendants must meet with the threat of consequences that will occur if they do not meet them. Conditions of supervision were intended to provide specific deterrence, both in communicating the threat of sanctions (in notification and orientation activities; see, e.g., Cook 1980) explicit in judicial release orders and in demonstrating consequences when individuals fail to meet obligations of reporting, etc. The guidelines strategy and its supervision core – an overall attempt to restore accountability to the pretrial release process – were also meant, in part, to serve as a general deterrent to mitigate the "culture of no consequences" in which noncompliance with court orders is the norm and is supported by defendants' perception of little threat (see, e.g., Nagin 1998) posed by the normal operation of the justice system. Quite simply, the planned strengthening of

accountability in the pretrial process sought to change the conventional wisdom "on the street" that one could just walk away from the criminal process with impunity.

The dilemma in setting up a pretrial release supervision system based on these deterrent aims was that the jail-crowding crisis in Philadelphia pretty much precluded making use of confinement to enforce compliance with conditions of release under supervision. To a great extent, the Philadelphia approach – like approaches in other cities – had to rely on the threat of sanction without the ability to impose the sanction or, at least, to make use of confinement as the ultimate sanction. Under these circumstances the threat of sanctions was perceived as an "empty threat." Individuals soon learned that failing to call in to pretrial services case managers would have little practical consequence. Certainly, with upwards of 50,000 fugitives already at large in Philadelphia, Warrant Unit officers were not going to be able to spend significant resources apprehending noncompliers. Confinement of such defendants, if they could be located, would not have been permitted under the terms of the emergency crowding litigation.

The system's lack of capacity to deliver consequences – to back up the threat implicit in pretrial release supervision conditions – handicaps an overall strategy to restore authority to the local judicial system over use of local confinement. Once the "bluff" of the implicit threats of enforcement is seen, when consequences for noncompliance are not applied, the bluff will be "called." The initial downward blip in defendant noncompliance caused by communication of the threat of sanctions associated with the new initiative will give way to a return to the former rates of pretrial misconduct generated under Federal emergency release measures. Commissioners deciding pretrial release will increasingly resort to use of cash bail instead of supervision, because they can, at least, be assured that defendants who cannot pay it will stay in jail – for a little while, at least – until they are released under the emergency crowding reduction rules imposed by the Federal court. Ultimately, both results – the lack of enforcement sanctions and the adaptive behavior of the pretrial release decision makers – adversely affect the jail population crisis and ultimately represent what amounts to a "backfire" of the innovation intended to relieve it.

Ironically, then, the success of the attempt to recapture responsibility for managing pretrial release and confinement from Federal emergency governance, by instituting a major re-engineering of the pretrial release system and by establishing a sound system of supervision, turns on the need for meaningful consequences in sanctioning noncompliance, including some selective role for confinement. One could argue that the results of this all-threat/no-sanction problem would be predicted from deterrence theory. Theoretical underpinnings aside, however, it was, in fact, identified as a critical issue for an effective pretrial release system 25 years ago in the *Performance Standards and Goals for Pretrial Release* established by the National Association of Pretrial Services Agencies (1978):

> Conditions of release are imposed in an effort to reduce the probability of nonappearance or pretrial crime, and therefore should be strictly enforced. (30)

> Setting conditions of release would be a futile exercise without an ability to monitor compliance with those conditions and to punish disobedience and reward compliance. (31)...Conditions of release imposed by the court should be treated seriously and rigorously enforced; otherwise, they should not be imposed at all. (32)

Perhaps the need to enhance the deterrent impact of pretrial release supervision can be addressed by drawing on lessons from other areas of justice innovation in which the need for intermediate punishment or graduated sanctions have been identified and a variety of options developed. [This is a lesson that has been learned in some of the best drug courts; see Goldkamp (2000)]. One of the lessons of years of crowding in state and local institutions, however, has been the need to develop a range of responses or sanctions that are measured and appropriate and that adequately reflect the nature of the violations involved. The preoccupation with crowding and its impact may have stimulated an over-emphasis on confinement as the main missing ingredient in justice decisions, particularly when compliance with court conditions is in question.

The body of research findings on the effect of elements of a pretrial supervision strategy we present must be seen in the context of the justice system's lack of credibility when it comes to delivering on the threat of conditions enforcement: rarely does it impose the threatened sanctions when defendants or offenders fail to comply with conditions of release. The challenge faced by the Philadelphia justice system is faced by many other jurisdictions as well: how to set requirements that are meaningful and enforceable, and how to devise measured responses that can and will be delivered when violations occur – without resorting automatically to confinement and without overwhelming the fair and rational functioning of the system itself.

Pretrial supervision and community context

In Philadelphia, as in many other jurisdictions, system reform leaders have learned that some of the problems of the criminal justice system cannot be solved by the criminal justice system itself. Community policing signaled that problems of public safety may be more productively addressed when officials engage in a working relationship with members of the communities that experience crime. The growth of community courts has further emphasized the connection between the apparatus of justice and the community it purportedly serves. The same theme is central to initiatives that deal with community crime problems through community prosecution and community probation, just as joint problem solving and reintegration formed a critical element in community corrections more generally in the past. Some of the issues raised in the research findings relating to pretrial supervision in this article could be addressed effectively through what might be viewed as a community justice approach to pretrial release.

Two examples are offered by way of conclusion. First, the development of a supervision capacity in the pretrial services agency is resource intensive, involv-

ing personnel, office space and computerization, and difficult to effectuate – as we have seen in our experiments. Certainly, the capacity to supervise and the overall effectiveness of supervision could be enhanced by developing partnerships with community organizations, ranging from church and civic groups to neighborhood associations in the areas in which defendants reside. The second example is the initial 50% no-show rate among persons assigned to attend supervision that plays such an important role in interpreting our findings (and in the effective functioning of the criminal process). Because pretrial release now occurs in Philadelphia via video from police holding locations throughout the city, defendants are "streeted" directly from those locations back into their neighborhoods.

One productive strategy for reducing the "no-show" phenomenon might be to recognize the role played by community context and the lessons learned from community justice initiatives across the United States of America – like the Neighborhood D.A. in Portland, Oregon or the Hartford (Connecticut) Community Court, which is tied directly to that city's 17 neighborhoods.[31] In short, the no-show-problem begins in the neighborhoods where defendants are released. Justice functions – including police, pretrial services, probation, courts and treatment services – could address the problem and greatly enhance supervision overall by moving to community locations and developing new, community focused collaborations. The aims of such a strategy would be diverse, including emphasis on neighborhood safety, accountability to the justice system, but also interest in seeing that neighborhood residents who are defendants or probationers are afforded the drug treatment and related services they may need. One of the goals of such a community justice strategy would be to improve the compliance of arrested persons with the requirements of the justice process. However, that narrow pretrial release goal would be linked to larger goals and contribute to improvements in the community as well as in the justice system.

Notes

1 Criticism of the role of financial bail has long formed the centerpiece of bail reform initiatives in the United States of America; see, e.g., Fosdick et al. (1922); Beeley (1927); Foote (1954); Goldfarb (1967); Goldkamp (1979). The District of Columbia preventive detention law [DC Code Ann. :: 23–1321, et seq (1970)], the Federal Bail Reform Act of 1984 [18 U.S.C. :: 3141–56 (1984)], and the American Bar Association (ABA) Criminal Justice Standards on Pretrial Release [2002: Standard 10–1.4 (e,f)] each prohibit the use of financial bail to cause pretrial detention of a defendant.

2 See also Morse and Beattie (1932); Alexander et al. (1958); Rankin (1964); Single (1972); Landes (1974).

3 Although it is true that, on a given day, Federal and state prisons hold more the twice the number of persons confined in jails in the United States of America, the high-turnover pretrial process exposes more individuals to incarceration (however brief) than would be exposed to prison. To illustrate, using Philadelphia data, at the time of this study during the late 1990s, an average of 100–125 arrests were processed into the courts per day

from police custody, translating into a minimum of 365,000 persons per year cycling into the system through custody. At year end in 2002, 40,168 inmates were held in Federal, state and local jail facilities in Pennsylvania. See Pastore and McGuire (co-eds.) (2002: Table 6.1 and 6.25).

4 For a detailed chronology of the developments in the crowding litigation under the state courts [in *Jackson v. Hendrick* (321 A. 2d 603, 1971)] and the Federal District Court [*Harris v. Reeves*, (654 F. Supp. 1042, 1987)] through 1990, see Babcock (1990) and Rudovsky (1985). *Harris* was filed in 1982 and reached a court-approved settlement in 1986. The respondents' names in Federal litigation changed over time (from *Harris v Pernsley*, *Harris v City of Philadelphia*, to *Harris v Reeves*).

5 See Clarke (1992); Cohen and Felson (1979); Eck (1995); Bentham (1781, 1988).

6 Though not addressed in this article, two major drug treatment initiatives – the Philadelphia Treatment Court and the Women's Criminal Justice Treatment Network for Women – were developed to address the categories identified in the pretrial strategy as in need of supervision as a second stage strategy supportive of the need for supervision under the pretrial release guidelines. For discussion of the Philadelphia Treatment Court, see Goldkamp et al. (1999), 2002b). For discussion of the Philadelphia treatment network for women charged with felonies, see Goldkamp et al. (2002b).

7 See, e.g., ABA (2002); the Federal Bail Reform Act of 1984 (18 U.S.C. :: 3141–56 (1984). The due process principle of least restrictiveness in setting conditions of release is closely compatible with deterrence theory, which calls for sanctions to be designed at levels only sufficient to outweigh the pleasure that might be derived by the offender from the prohibited act. Sanctioning beyond that level would be counterproductive and inappropriate. See Bentham (1781).

8 Since the Philadelphia experimental research relating to pretrial supervision of defendants, the American Bar Association Criminal Justice Standards on Pretrial Release (2002) has addressed supervision directly in black letter and commentary. The proposed revision of the National Association of Pretrial Services Agencies Pretrial Release Standards (draft under review, 2003) will also discuss supervision specifically.

9 In late 2005, the inmate population in the Philadelphia Prisons fluctuated at levels approaching 9,000.

10 See "Motion of the City of Philadelphia and the Honorable Edward G. Rendell, in His Official Capacity as Its Mayor, to Modify the December 30, 1986 Consent Decree and the March 11, 1991, Decree," in *Harris v. Reeves* (654 F. Supp 1042, 1987); No. 82–1847 (9/21/96). The motion was granted on November 22, 1995. Initially intended for a trial period of 90 days, the motion was subsequently extended indefinitely. See "On Thanksgiving Day, Judge Waives Prison Cap," *Philadelphia Inquirer* (1995) (11/22/95).

11 The development and implementation of what were first referred to as "bail" guidelines in Philadelphia were implemented in the context of a field experiment. See Goldkamp and Gottfredson (1985). For description of replication of the pretrial release guidelines approach in Maricopa County, Dade County and Boston, see Goldkamp et al. (1995).

12 For discussions of how the dimensions defining the pretrial guidelines matrix were arrived at, see Goldkamp and Gottfredson (1985); Goldkamp et al. (1995, 1997).

13 In this regard, the judicial pretrial release guidelines were influenced by the principles of release in the Federal Bail Reform Acts of 1966 and 1984. See also ABA, op. cit. (2002).

14 See Goldkamp et al. (1997) for discussion and evaluation of the first year of the new pretrial release guidelines in Philadelphia as part of the overall Alternatives-to-Incarceration strategy.

15 The elements of the overall strategy are described in more depth in Goldkamp et al. (1997).

16 On 11/19/95, an experimental day, procedures were not carried out on the 7–3 shift. On 11/23/95 no record was kept of what occurred and the day's cases were excluded from the study. On 11/24/95, no I.D. numbers were included for cases entering prior to 9:30 in the morning, resulting in their exclusion.

17 Forty-seven cases proved to have erroneous or unrecognizable identification numbers. Because we could not then link them to other data for follow-up, they were excluded from the study.

18 Several differences were identified: experimental group defendants more frequently gained outright release under Federal ("H.v.R.") procedures (61% versus 47%), had fewer prior convictions for serious property offenses (4% versus 11%), and had more prior weapons arrests (33% versus 22%). These differences were entered as controls when group outcomes were compared.

19 The baseline data offer a useful, if not perfect, approximation of defendant performance under no-supervision conditions that existed under Federal emergency release procedures prior to the introduction of the supervision strategy. We would expect that the baseline defendants would differ from the supervision experiment defendants principally in the broad mix of released defendants that would be found in the baseline population. The defendants targeted for the supervision experiment would represent a more focused, filtered and higher risk population because of their location under the pretrial release guidelines. (They were chosen to be higher risk and more likely to be detained.) This difference should mean that, a priori, the type I and II defendants, by definition, should show higher failure rates than the generalized population of released defendants overall represented in the baseline data.

20 See baseline comparisons with experimental and control groups below in Table 2.

21 Note that the pretrial release guidelines classified defendants into type I and II categories based on explicit criteria that provided presumptive recommendations for bail commissioners making the pretrial release decision at preliminary arraignment. The decisions commissioners made differed somewhat from the presumptions provided by the guidelines, with the effect that those assigned to type I and II levels of supervision were not all originally classified as falling within the level to which they were assigned under the guidelines. The experiment dealt with (randomly assigned) persons actually assigned to type I and II conditions, not with persons classified as presumptively appropriate (recommended) by the guidelines.

22 The random assignment of defendants to experimental groups was fairly successful but not without practical problems. The following are some examples of implementation difficulties. One of the problems with the AVR was that some defendants simply did not like to use it and preferred to call the case manager instead. Often this would result in a manual record – or no record – and so may have lead to some inaccuracies in tracking compliance with telephone reporting. In addition, when defendants entered an I.D. number inaccurately using the telephone system, the AVR would not show compliance when, in fact, the defendant had attempted to report in. Six defendants (out of 845) were randomly assigned to the wrong sample, an error rate of 0.7%. In addition, from November 11, 1996 to November 15, 1996 there was a computer "head" crash, which meant that the computer could not be used to identify defendants with upcoming courts dates who needed to be called or to come in to visit in person. Records during that 2-week period were deemed uninformative. On December 24 and 25, 1996 no incoming AVR calls were made by defendants and no outgoing staff calls to defendants were

made. Finally, when a new case manager was hired in November of 1996, the individual followed the incorrect procedure for IIB defendants. Instead of having them come in for meetings, he/she sent letters and made phone calls reminding them of court dates. As may occur even in successful random assignment, some differences in group attributes were identified. Type IB defendants seemed to have slightly more extensive prior histories of arrests than type IA defendants. A slightly greater proportion of type IIA defendants appeared to have drug-related prior arrests than type IIB defendants. Differences in group attributes were taken into consideration (controlled for) when the experimental outcomes were assessed.

23 This point was made by a National Academy of Sciences panel convened to review the effects of rehabilitation, when its members wrote that they did not find evidence that rehabilitation "worked," but they also did not find much evidence that it had been implemented (Sechrest et al. (1979)).

24 Thus, the measure is percent of compliance with the call-in requirement, whether the defendant was scheduled to make one or two calls weekly.

25 We believe that an unknown additional number of court dates may have existed that were not documented by the system for a variety of logistical reasons. These data deal only with dates that we could document.

26 Recall that defendants in the supervision experiment (experiment 2) were randomly assigned at the point of arrival at the orientation session conducted at the pretrial services agencies.

27 Random assignment occurred from October 21, 1997 through November 18, 1997. Random assignment was successful, as there were no significant differences in the composition of the two groups.

28 Staff called up to three telephone numbers that had been obtained during the pre-arraignment interview with the defendant. In the event that a defendant indicated no telephone at his/her residence, staff called persons listed as immediate references by the defendant. The communication to the defendant was based on the following exemplar:

> "This call is a reminder that you are scheduled to appear tomorrow at the main office of Pretrial Services at 121 N. Broad St. (2nd floor) for Pretrial Release Orientation. Attendance at this Orientation is a required condition of your pretrial release, as ordered by the Bail Commissioner, and failure to attend may result in arrest and/or revocation of your pretrial release."

29 Similar proportions (20% and 21%) of defendants in both groups were fugitives at the end of the 1-month observation period.

30 Yet, it is impossible to rule out an alternative explanation: defendants who can be reached by telephone live in more orderly settings or have more organized lives than those who cannot. Thus, telephone accessibility is really a measure of risk. Critics might argue that the phone contact system merely self-selects the better risks among defendants who, given their lower probabilities of misconduct, were more likely to attend pretrial services appointments any way.

31 See Berman et al. (2005); Goldkamp et al. (2000, 2002a).

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