New York City Criminal Justice Agency

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The Court Appearance Pilot Project:

A Randomized Evaluation of an Interactive Intervention to Encourage Court Date Attendance

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Executive Summary

This research brief presents results from a randomized controlled trial (RCT) to evaluate the Court Appearance Pilot Project (CAPP), an intervention implemented by the New York City Criminal Justice Agency (CJA) in Brooklyn from December 2018 through May 2019. Intended to serve individuals with an increased likelihood of failing to appear, CAPP consisted of both a post-arraignment meeting and a subsequent phone call to encourage individuals to attend their next court date.

Results are promising, with the treatment group consistently exhibiting a lower failure-to-appear (FTA) rate than the control group across all intent-to-treat and as-treated analyses. These results hold true both for FTA at the first post-arraignment appearance *and* FTA across the entire life of the case.

Two results were large enough to achieve statistical significance ($p < .05$). The first, the intent-to-treat analysis of first post-arraignment appearance FTA, revealed that only 10.4% of the treatment group received a warrant at their first post-arraignment appearance, compared to 14.4% of the control group.

The second, the as-treated analysis where treatment was defined as a post-arraignment meeting and a planning phone call attempt, revealed a case-based FTA rate of 15.2% for the treatment group, compared to 22.4% for the control group. In other words, individuals who were given the intervention were 32% less likely to fail to appear prior to case disposition than controls. The treatment group in this analysis was also 31% less likely to fail to appear at their first post-arraignment appearance compared to the control group, though this result falls just short of significance ($p = 0.07$).

This evaluation is the first RCT to examine how planning-focused reminders administered through direct engagement with arrested individuals (in person and via live phone calls) affect court appearance rates compared to more standard automated court reminders. In addition to highlighting the potential value of personalized court appearance-focused interventions, this RCT also offers lessons for researchers interested in conducting applied experimental research in real-world settings.

Introduction

By improving court appearance rates, pretrial service agencies can help individuals avoid warrants, families avoid the disruption of a loved one's pretrial detention, and jurisdictions save limited criminal legal resources. While the literature on court reminder notifications has explored the efficacy of reminder letters, text messages, and phone calls for promoting court appearance (see review in Thomas and Ahmed 2021), research has yet to fully examine more in-depth interventions to support court appearance among individuals at higher likelihood of failing to appear. This research brief addresses this critical gap by describing the Court Appearance Pilot Project (CAPP), a voluntary intervention implemented by the New York City Criminal Justice Agency (CJA) and evaluated through a randomized controlled trial (RCT). (At the time of implementation, CAPP was also variously known as the Court Appearance Support Project [CASP or CASPR]). This unique intervention was informed by evidence-based approaches in the notification literature and included both personalized and interactive elements. Despite certain logistical challenges (discussed in detail later), the intervention produced promising results, suggesting that in-depth, personalized court appearance support programs of this kind deserve more attention from the pretrial field.

Literature Review

A growing body of pretrial research – including several rigorous RCTs – has highlighted the value of notification as an intervention for promoting court appearance (see review in Thomas and Ahmed 2021). Notifying individuals of upcoming court dates by letter (Bornstein et al. 2013), live phone call (i.e., a phone call made by an actual person, rather than an automated service) (Ferri 2020), and text message (Fishbane et al. 2020) increases the likelihood that individuals appear for court. The effects can be substantial; for example, Ferri (2020) found that individuals who received any live reminder phone call were 37% less likely to fail to appear for court than individuals who did not receive a reminder call (Ferri 2020). Such findings about the potential efficacy of notifications are mirrored in nonexperimental studies as well (Crozier 2006; Howat et al. 2016; Schnacke et al. 2012; White 2006).

With regard to automated (rather than live) notification calls, an RCT by Lowenkamp et al. (2018) found no significant relationship between these automated calls and appearance rates, a finding echoed in non-experimental research (Nice 2006). Relatedly, Chivers and Barnes (2018) found that automated (rather than personalized) text message reminders did not have a relationship with court appearance rates. These null findings suggest that a lack of personalization or interaction may hinder the effectiveness of notification efforts. Additionally, the fact that a robust and growing body of research

has consistently found personalized notifications to be associated with increased court appearance rates suggests that these elements could be key to court notification efficacy (Bornstein et al. 2013; Crozier 2006; Ferri 2020; Fishbane et al. 2020; Howat et al. 2016; Schnacke et al. 2012; White 2006). However, it is essential to note that most research on personalized notifications has compared receiving personalized notifications to receiving *no notifications at all*; more research is needed that directly compares personalized and automated notifications. An unpublished study conducted by CJA specifically examined the efficacy of live calls compared to automated robocalls but found no significant difference in court appearance rates between the two (Ropac, unpublished), while Fishbane et al. (2020) found that different notification content and degrees of engagement could affect appearance rates. The current study builds on this limited current knowledge by directly comparing a personalized notification intervention with more standardized notification services.

The seeming importance of personalization and interaction in court reminders is perhaps not surprising, as this finding has long been noted in the more expansive literature on medical notification. Indeed, meta-analytic reviews of research on medical reminders indicate that, while any type of reminder is generally effective in improving either appointment appearance or treatment adherence rates, reminders that are personalized and/or provide more information to the patient are more effective (Lefforge et al. 2006; Mayer and Fontelo 2017; McLean et al. 2016; Parikh et al. 2010; Perri-Moore et al. 2016; Schwebel and Larimer 2018; Teo et al. 2017; Wagner 1998). One noteworthy finding from the medical notification literature is that reminders prompting recipients to write down the date and time they planned to attend a vaccine appointment were more effective at encouraging vaccination than reminders that did not include this element (Milkman et al. 2011), underscoring the importance of interaction.

In addition to personalization and interaction, the content of the notification message itself can have important impacts. Court date reminder letters that include additional information, such as the consequences of failing to appear, are more effective in improving appearance rates than letters without such additional information (Bornstein et al. 2013). An RCT of text message court reminders in New York City found that receiving any text reminder reduced the likelihood that an individual would fail to appear for court, but text messages that included *both* information about the consequences of failing to appear *and* tips to help plan one's attendance in court had the largest effect (Fishbane et al. 2020). The next section describes how such findings informed the design of the CAPP intervention.

Intervention

CAPP was a pilot intervention operated by CJA from December 2018 – May 2019 in the borough of Brooklyn. CAPP was designed to serve individuals with an elevated likelihood of failing to appear – specifically, individuals not recommended for release on their own recognizance under the CJA release assessment in use at that time, but who were nevertheless released on their own recognizance (ROR) at arraignment by the judge. Trained CJA personnel were stationed in court and approached eligible individuals post-arraignment to gain consent. For individuals who consented, the intervention began with a post-arraignment meeting conducted by a trained CJA staff member with strong engagement skills. The CJA staff member would ask the participant about possible obstacles that could hinder their ability to return for their next court date, and then would ask the participant to write down their court date. This latter component was inspired by findings from the medical notification literature about the importance of writing down information (Milkman et al. 2011). Individuals were also provided with water, a snack bar, and a MetroCard if needed. If the individual agreed, a CJA staff member would also provide a personal planning phone call a week or two prior to the individual's first post-arraignment appearance. The phone call's emphasis on planning messaging reflects the finding by Fishbane et al. (2020) that such content can be particularly effective in reducing failure-to-appear rates.

Therefore, the full intervention consisted of both an in-person meeting immediately following arraignment and a follow-up phone call some days or weeks later (depending on the timing of their next court date) to encourage individuals to appear.

Evaluation

This evaluation of CAPP constitutes the first RCT to examine how planning-focused reminders administered through direct engagement and interaction affect court appearance rates for individuals at high risk of failing to appear. Importantly, we compare personalized notifications with less personalized notifications (rather than no notification at all), allowing us to isolate the effects of this more targeted intervention. RCTs are considered the gold standard in scientific research because they allow for causal inference. By randomly assigning members of a study sample to either a treatment group (which receives the intervention of interest) or a control group (which does not receive the intervention), researchers can identify the impact of the intervention itself (Cartwright and Munro 2010; Deaton and Cartwright 2018). RCTs are uncommon in the social sciences since they require considerable resources, and also because in many contexts it is considered unethical to grant or deny a randomly designated group of people a given intervention (Fives et al. 2015; Nagin and Sampson 2019). Another key concern with the use of RCTs in the social sciences is that the method's emphasis on internal validity over

external validity may limit the generalizability of findings to other social contexts beyond that of the original evaluation (Cartwright and Munro 2010; Grossman and Mackenzie 2005; Nagin and Sampson 2019; Saint-Mont 2015).

> **External validity** refers to the replicability of a study's results outside of the study environment, while **internal validity** refers to the rigor of a study's methodological design (Bergin, 2018). RCTs are hailed for their strong internal validity, as their experimental design allows for causal inference; however, external validity may be reduced as findings may not hold in more practical, real-world settings that differ from the original experimental environment.
>
> (Deaton and Cartwright 2018)

CAPP is well-suited for evaluation via an RCT since interest in court notifications as a technique for promoting court appearance has increased nationally. The intervention was guided by the growing body of literature on these notifications, which suggests that reminders incorporating a person-to-person exchange of instrumental information about court appearance are the most effective in reducing FTAs (Bornstein et al. 2013; Fishbane et al. 2020). Since CJA already has an extensive court notification system in place to provide automated text messages, robocalls, and live reminder calls to all individuals released pretrial in New York City (New York City Criminal Justice Agency 2021), CJA was able to extend its live call operations to offer CAPP while still providing standard court notifications to the broader population, an important ethical necessity to ensure that the control group still received standard notification treatment (Fives et al. 2015). Further, by defining the control group as those who received only the standard CJA court notifications, this evaluation is able to examine how the specific elements of personalization and interaction in notifications affect FTA rates compared to more automated reminders, filling an important gap in the literature.

Only individuals at elevated risk of FTA were eligible for participation in CAPP; therefore, the chance of misattributing treatment effects for individuals who would appear regardless of their assignment to the treatment or control groups is reduced. Guided by the RCT literature, we used two distinct approaches to analyze outcomes and prioritize both internal and external validity. First, we employed an intent-to-treat (ITT) approach to analyze the treatment and control groups *as randomly assigned* despite how the intervention was implemented in practice; second, we employed an as-treated (AT) approach to analyze the treatment and control groups *as participants actually experienced the intervention* (Cartwright and Munro 2010; Ten Have et al. 2008). By utilizing both ITT and AT approaches, we were able to attain a more thorough understanding of how the CAPP intervention performed as it was meant to be implemented, as well as how it performed in practice (i.e., when subjected to the external forces of the real world, outside of the experimental RCT design).

Individuals' docket numbers were used to randomly assign them to either the treatment group or the control group, with the intention that approximately 60% of the sample would be assigned to the treatment group. In total, 1,103 individuals were screened for CAPP participation; 651 of these individuals were randomly assigned to the treatment group and 452 were randomly assigned to the control group. The effects of the CAPP intervention were measured through two distinct outcomes: FTA at first post-arraignment court appearance and any FTA throughout the lifespan of a case, prior to its disposition. By examining both outcomes, we were able to glean a more thorough understanding of how the CAPP intervention affects FTA rates and impacts the pretrial process. To ensure that the AT analyses were as informative as possible, separate analyses for "Any Treatment" (indicating that an individual received a post-arraignment meeting but may or may not have received a phone call attempt) and "All Treatment" (indicating that an individual received a phone call attempt). Therefore, in total, six analyses were conducted:

1. Intent-to-Treat (ITT): Warrant at First Post-Arraignment Appearance
2. As-Treated (AT) – Any Treatment: Warrant at First Post-Arraignment Appearance
3. As-Treated (AT) – All Treatment: Warrant at First Post-Arraignment Appearance
4. Intent-to-Treat (ITT): FTA Prior to Case Disposition
5. As-Treated (AT) – Any Treatment: FTA Prior to Case Disposition
6. As-Treated (AT) – All Treatment: FTA Prior to Case Disposition

These analyses are denoted by $FTA^{(1\text{-}6)}$ in the tables presented in the Results section.

Research Challenges

Before proceeding to the study's findings, it is important to discuss the practical challenges the team encountered when implementing the intervention and the RCT research design. These limitations provide important context for interpreting the study's results. The first stage of the intervention – the post-arraignment meeting – occurred in a busy courthouse environment, in which the process of identifying eligible individuals, correctly assigning them to control and treatment groups, and maintaining participant engagement was challenging. Certain hurdles further complicated this process, such as eligible participants' lawyers occasionally requesting that our team not approach their clients to discuss CAPP and their potential participation at all. Recording accurate data under these conditions was also difficult, and substantial subsequent cleaning and quality control efforts were needed to ensure data integrity. Those cases that could not be reconciled in the data cleaning process had to be dropped from the analyses, as outlined earlier in this document. Before analyzing the data, the team needed to resolve numerous inconsistencies in data collection and entry, often involving participant docket numbers.

Docket numbers were used not only to facilitate random group assignment but also to match cases across various data sources, including the CAPP screening data, interview data, and phone call data, which were all collected and recorded separately and later merged for analysis. Data entry errors that produced typos in docket numbers required manual checks and corrections to ensure accurate case matching across data sources. Participant names and arraignment dates as recorded both in the CAPP data and in CJA's official data sources were used to confirm docket numbers.

In the rare instance that a docket did not clearly match across data sources in the quality check process, that case was dropped entirely from the sample to avoid erroneous matching and subsequent false reporting on results. Most cases were resolvable, and many of the deleted cases were identified as duplicate cases that were accidentally entered multiple times by staff throughout data collection. Three extensive rounds of data cleaning were conducted by separate researchers at CJA at different times to ensure that no duplicate cases were mistakenly analyzed twice, and no incorrect case matching occurred. We are confident in the rigorous data quality work that led us to identify the total CAPP sample of 1,103 participants.

> **CJA's database** includes information on virtually every person arrested in New York City. Demographic information is obtained from CJA's pre-arraignment interview, arrest data are received from the New York City Police Department (NYPD), case-processing data from the Office of Court Administration (OCA), and bail making data from the New York City Department of Correction (DOC).
>
> (New York City Criminal Justice Agency 2021)

Additional practical limitations affected the implementation of the intervention as well; in a few instances, planning phone calls that were supposed to be made to consenting treatment group participants one to two weeks prior to their next scheduled court date were not executed following their post-arraignment meeting. To account for this, AT analyses were conducted twice for each outcome: first for individuals who received any treatment (a post-arraignment meeting that may or may not have been followed by a planning phone call), then for individuals who received all intended treatments (both a post-arraignment meeting and a planning phone call). Participants whom CJA staff members attempted to reach but did not reach for the planning phone call (e.g., voicemail left, bad number) were still included in the all-treatment group for the AT analyses. Finally, some participants assigned to the control group erroneously received one or both parts of the CAPP intervention, in which case they remained in the control group for the IT analyses but were analyzed accordingly as treatment group participants for the AT analyses.

Results

Table 1 illustrates the discrepancies between participants' intended group assignment and whether they actually received treatment. 444 of 452 (98.2%) participants assigned to the control group correctly did not receive any intervention. Of the 651 participants randomly assigned to the treatment group, 173 did not consent to participate and therefore received no treatment; one additional case consented to participate but erroneously received no treatment. The fact that only the treatment group received any kind of intervention and therefore was required to provide consent to receive that intervention introduces a potential comparability concern, as there could be inherent differences between the subset of individuals who choose to consent and who do not. To respond to this concern, we report results from both ITT and AT analyses, and in the ITT analyses, everyone assigned to treatment – regardless of whether they consented to receive the intervention – is included in the treatment group.

A total of 8 (1.7%) of the 452 participants assigned to the control group erroneously received some degree of treatment, likely due to data collection errors. These participants, despite their random group assignment, were still asked to consent to the intervention and all consented to participate in CAPP. Only one case erroneously received a planning phone call without a post-arraignment meeting. This case is kept in the ITT analyses in the treatment group based on its random assignment but excluded from the AT analyses for consistency purposes, as it does not conform to the criteria of the "any" treatment group (which analyzes those cases that received a post-arraignment meeting only) nor the "all" treatment group. Cases in the CAPP sample that were either disposed at arraignment (n=10) or missing FTA information (n=7) were excluded from the ensuing analyses. In total, 476 (73.1%) of the 651 randomly assigned treatment group participants received at least partial treatment – namely, the post-arraignment meeting. Only 278 of 651 (42.7%) participants assigned to the treatment group received the entire CAPP intervention as intended, consisting of both the post-arraignment meeting and the planning phone call.

Table 1: CAPP sample – Random group assignment vs. actual intervention received

Random group assignment	Intervention received				
	None	Meeting only	Meeting + call	Phone call only	Total
Control	**444 (98.2%)**	7 (1.5%)	1 (0.2%)	0 (0.0%)	452 (100%)
Treatment	174* (26.7%)	198 (30.4%)	**278 (42.7%)**	1 (0.2%)	651 (100%)
Total	618 (56.0%)	205 (18.6%)	279 (25.3%)	1 (0.1%)	1,103 (100%)

*173 of these 174 cases did not consent, so received no treatment. One additional case consented, but erroneously received no treatment.

Results from the intent-to-treat (ITT) analyses are presented in Table 2. The ITT analysis sample totals 1,086 participants, with 444 participants in the control group and 642 in the treatment group as randomly assigned. 131 (12.1%) total individuals were issued a warrant at their first post-arraignment appearance (Analysis 1) for failing to appear and 224 (20.6%) individuals failed to appear prior to their case disposition (Analysis 4). Across both analyses, FTA rates were demonstrably lower for the treatment group compared to the control group. Notably, the FTA rate among the treatment group at first post-arraignment appearance decreased by 27.8% compared to the control group. This difference was statistically significant at the .05 level ($p = 0.04$), indicating that the CAPP intervention as randomly assigned reduced the likelihood of FTA at the first post-arraignment appearance. The FTA rate prior to case disposition decreased by 11.3% with treatment as randomly assigned, but this result was not statistically significant ($p = 0.33$).

Table 2: Intent-to-Treat – FTA rates

	FTA[1] (at first post-arr. appearance)				FTA[4] (prior to case disposition)					
Group	Yes		No		Yes		No		Total	
Control	64	**14.4%**	380	85.6%	98	**22.1%**	346	77.9%	444	100%
Treatment	67	**10.4%**	575	89.6%	126	**19.6%**	516	80.4%	642	100%
Total	131	**12.1%**	955	87.9%	224	**20.6%**	862	79.4%	1,086	100%

[1] $x^2 = 3.82$; $p \leq 0.05$* [4] $x^2 = 0.96$; $p = 0.33$

The first set of as-treated (AT) analyses includes participants who received any treatment (at least a post-arraignment meeting) in the treatment group. One case that erroneously received a planning phone call but no post-arraignment interview was excluded; therefore, a total of 1,085 participants are included in the AT analyses. The results from the "any" treatment analyses are illustrated in Table 3 below. The "any" treatment sample consists of 607 participants in the control group and 478 participants in the treatment group. As in the IT analyses, those who received treatment in the AT "any" analyses exhibited consistently lower FTA rates than controls. Specifically, FTA rates at first post-arraignment appearance were 14.0% lower for the treatment group compared to the control group; FTA rates prior to case disposition were 11.1% lower for those treated. However, neither of these results was statistically significant ($p = 0.38$ and $p = 0.35$, respectively).

Table 3: As-Treated (any) – FTA Rates

	FTA[(2)] (at first post-arr. appearance)				FTA[(5)] (prior to case disposition)					
Group	Yes		No		Yes		No		Total	
Control	78	**12.9%**	529	87.1%	131	**21.6%**	476	78.4%	607	100%
Treatment	53	**11.1%**	425	88.9%	92	**19.2%**	386	80.8%	478	100%
Total	131	**12.1%**	954	87.9%	223	**20.6%**	862	79.4%	1,085	100%

[(2)] $x^2 = 0.78$; $p = 0.38$ [(5)] $x^2 = 0.89$; $p = 0.35$

The second set of AT results, shown in Table 4 below, includes in the treatment group only those who received all of the treatment as intended (post-arraignment meeting and subsequent planning phone call). In this analysis, participants who did not receive a planning phone call following their post-arraignment interview were included in the control group alongside the other participants who received no form of the treatment. This sample includes a total of 808 participants in the control group (including those who received no intervention and those who only received a post-arraignment interview) and 277 participants in the treatment group. Note that these analyses face the largest imbalance between the sample size of treatment and control groups, and thus possess less statistical power than the other four analyses where the groups are more balanced in size.

Statistical power refers to the probability that a Type I error does not occur when analyzing study results. A Type I error occurs when the null hypothesis (in this evaluation, that the CAPP intervention does not reduce FTAs) is incorrectly rejected. Typically, as a study's sample size increases, its statistical power increases while the chance of Type I errors decreases.

(Akobeng 2016)

The results of these analyses, presented in Table 4 below, show a 31.3% decrease in FTA rates for the treatment group compared to the control group at first post-arraignment appearance, and a 32.1% decrease prior to case disposition. These are the greatest reductions in FTA rates between control and treatment groups of all analyses conducted for this study. While the decreased FTA rate for the treatment group at first post-arraignment appearance approaches but does not achieve statistical significance ($p = 0.07$), results for the FTA rate prior to case disposition are statistically significant ($p = 0.01$). This finding suggests that receiving the two-part CAPP intervention as intended significantly reduces FTA rates over the lifespan of a court case.

Table 4: As-Treated (all) – FTA Rates

	FTA[3] (at first post-arr. appearance)				FTA[6] (prior to case disposition)					
Group	Yes		No		Yes		No		Total	
Control	106	**13.1%**	702	86.9%	181	**22.4%**	627	77.6%	808	100%
Treatment	25	**9.0%**	252	91.0%	42	**15.2%**	235	84.8%	277	100%
Total	131	**12.1%**	954	87.9%	223	**20.6%**	862	79.4%	1,085	100%

[3] $x^2 = 3.26$; $p = 0.07$ [6] $x^2 = 6.62$; $p \leq 0.01$**

Discussion

Overall, study results show that participants in the CAPP treatment groups exhibited consistently lower FTA rates than participants in the control groups. Of the six analyses performed, the greatest reductions in FTA rates were seen in the AT analyses that included only those participants who received all of the CAPP intervention as intended (both a post-arraignment meeting and a planning phone call) in the treatment group. Two of the six total analyses achieved statistical significance; first, participants in the ITT treatment group had an FTA rate at first post-arraignment appearance that was 27.8% lower than that for the control group, a result significant at the .05 level ($p = 0.04$). Second, the FTA rate prior to case disposition for participants in the AT (all) treatment group was 32.1% lower than that for the control group, a result that was significant ($p = 0.01$). The 31% decrease in FTA rates at first post-arraignment appearance for the AT (all) treatment group is worth noting, too, for although this result does not achieve statistical significance it falls just short ($p = 0.07$), likely due to sample size limitations given that the subsequent AT (all) analysis mirrors its results closely. The consistency of these results – FTA rates were lower in the treatment groups across all six analyses – combined with the statistical significance achieved by two of these analyses suggests that CAPP was ultimately effective in reducing FTAs.

This evaluation of CAPP fills an important gap in current research about court notification. Since CAPP was implemented by CJA, an organization that sends out court notifications to individuals released pretrial in New York City, the control groups analyzed in this evaluation – which received standard notifications – are categorically different than those in many other similar studies, where control groups often receive no form of court notification at all. In turn, these findings substantially build upon the growing research that suggests that court notifications involving direct, person-to-person exchanges of

instrumental information are more effective in reducing FTAs. Indeed, CAPP provides significant evidence that court notifications of this type are *particularly* effective in reducing FTAs when compared with their automated, less interactive notification counterparts, which serve as the control group in this study. Importantly, these results reflect improved FTA rates specifically for those individuals at heightened risk of FTA, an understudied subpopulation in court notification research.

Recommendations for Future Research and Practice

Future studies that seek to understand how court notifications affect court appearance should recognize that notifications, particularly those that require person-to-person engagement such as those studied here, require significant resources beyond those necessary for the more standard, less personal notifications typically seen in the pretrial realm. Court notification research should continue to prioritize studying planning-focused, personally engaging notifications to better understand the mechanisms behind such reductions, as well as provide cost-benefit analyses of their impacts. Future research should also examine whether the effects presented here differ by race, sex, age, or other key demographic variables that have been studied in the general notification literature (Bornstein et al. 2013; Ferri 2020; Fishbane et al. 2020; Nice 2006).

Conclusion

CAPP provides promising evidence of the effectiveness of court notifications that include personalized or interactive elements for individuals at elevated risk of FTA. By offering in-person meetings to discuss court appearance and by providing instrumental information to help individuals plan to appear, CAPP consistently (and, in some analyses, significantly) reduced FTA rates. CAPP provides a promising route for future court notification research, as more studies on personalized interventions – for different populations, in different jurisdictions, and using different delivery mechanisms – are greatly needed, and would fill a key gap in our current knowledge about pretrial services.

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