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The Impact of Defense Counsel at Bail Hearings*

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Abstract

Roughly half of U.S. counties do not provide defense counsel at bail hearings and few studies have documented the potential impacts of legal representation at this stage. This paper presents the results from a field experiment in Pittsburgh, Pennsylvania that provided a public defender at a defendant's initial bail hearing. The presence of a public defender decreased the use of monetary bail and pretrial detention without increasing failure to appear rates at the preliminary hearing. The intervention did, however, result in a short-term increase in rearrests on theft charges, although a theft incident would have to be at least 8.3 times as costly as a day in detention for jurisdictions to find this tradeoff undesirable.

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1. Introduction

At the first court appearance after an arrest in the U.S., a judge makes critical decisions about the conditions necessary for defendants to be released from jail until the case is resolved. Most jurisdictions operate a cash bail system in which the judge determines an amount a person must pay to be released from detention (15). Recent studies have provided substantial causal evidence that pretrial detention leads to worse outcomes for the defendant and society at large, with longer jail stays and higher chances of conviction in the short term, and worse recidivism and employment outcomes over the long term (8, 9, 12, 13, 16, 19).

Despite the importance of the bail hearing, the U.S. Constitution does not guarantee the provision of legal representation for defendants at this stage. While the Sixth Amendment guarantees the provision of defense counsel at all critical stages of a criminal prosecution, the U.S. Supreme Court has not recognized the bail hearing as a critical stage, which would require that the presence of defense counsel at this hearing have a direct impact on the case outcome. As a result, whether defense counsel is provided at bail hearings has been left up to states and local jurisdictions to decide. Although the exact number is not known, up to half of the counties in the U.S. do not provide defense counsel at this stage (7).

In this current landscape, research on the impact of defense counsel at bail hearings is crucial because it can simultaneously shed light on whether the bail hearing should be considered a critical stage at which defense counsel must be provided, as well as help state and local jurisdictions assess the efficacy of their policies regarding the provision of defense counsel. The latter is especially important if states and localities argue that providing defense counsel is too costly and/or that defense counsel does not have any real impact on defendant outcomes at these hearings (7). In particular, the reality of these hearings—which in many large jurisdictions are assembly-line style hearings usually lasting less than three minutes via poor-quality video feeds—has bred some skepticism about the potential of attorneys to affect the outcome (18). Understanding the extent to which providing defense counsel at the bail hearing can impact the use of monetary bail and pretrial detention will thus provide policymakers with necessary information on the effectiveness of this intervention.

Despite the importance of this issue, there is surprisingly little known regarding the benefits of providing defense counsel at the bail hearing. The empirical evidence in this area is

limited to three studies, two of which are now-dated experiments that suffered some deviation from the research design during implementation (6,11). The third is a study examining a policy change, comparing outcomes after the change to those before, without a comparison group (23, 24). A recent related study examined the impact of providing bail advocates to support public defenders (14), although the study did not directly evaluate whether the public defenders themselves have an impact on bail hearing outcomes. While collectively these studies mostly support the claim that better defense representation at the bail hearing reduces pretrial detention with no increase in the rate at which defendants fail to appear at the next hearing, none are strong enough on their own to support wide-scale policy changes regarding the provision of defense counsel at bail hearings.

To address this gap, this paper presents the results of an evaluation of the impact of a year-long initiative to provide public defenders at some bail hearings within the Pittsburgh Municipal Court (PMC). The jurisdiction only had sufficient resources to provide public defenders for half of the shifts that did not already have public defenders. Our experimental design generated a public defender work schedule such that the shifts in which a public defender was working had defendants and judges who were on average virtually identical to those in which a public defender was not working. This research design, akin to a randomized control trial, allows us to rigorously evaluate the impact of providing a public defender at the defendant's initial bail hearing on a variety of defendant outcomes.

The results indicate that providing a public defender at the bail hearing led to a significant decrease in the use of monetary bail and short-term pretrial detention, with no impact on failure to appear rates or the probable cause determination at the preliminary hearing. However, the intervention did result in an increase in rearrests for third-degree felony theft charges within the first six months of the bail hearing. For jurisdictions whose primary concern about providing defense representation at this stage is ensuring defense counsel actually impact the proceedings, these results provide clear evidence of the benefit of this intervention. For jurisdictions concerned about the additional criminal activity arising from this intervention, our analysis indicates that in order for the tradeoff between reduced pretrial detention and increased criminal activity to be problematic, the cost of a theft charge to society would have to be at least 8.3 times more than the cost to society of a day in detention. Current survey estimates indicate

that individuals perceive the societal cost of a theft charge and a day in detention to be roughly equivalent, implying this tradeoff should be acceptable for most jurisdictions (21).

2. Bail Hearings and the Role of the Public Defender

If an individual is arrested for alleged criminal activity within the Pittsburgh city limits at any time, or in an outlying area within Allegheny County outside of normal court business hours, their initial bail hearing takes place in the Pittsburgh Municipal Court (PMC).¹ Arrested individuals are brought to the jail, which is physically adjacent to PMC, where pretrial staff administer a risk assessment using a locally validated tool and provide the results to the judge overseeing the bail hearing. The risk assessment predicts both the risk that the defendant will fail to appear at future criminal hearings, as well as the risk they will commit new criminal activity during the pretrial period. The risk assessment algorithm recommends either unconditional pretrial release, release with non-monetary conditions, or no release. Although monetary bail is never recommended, judges set a monetary bail roughly half the time. Importantly, judges examine the risk assessment paperwork and make their bail hearing disposition *prior* to the bail hearing, without talking to the defendant.² During the hearing the judge simply reads their final disposition to the defendant, who is in the jail and appears via video in the courtroom. Judges can elect to either release the defendant with no conditions (ROR), release them with non-monetary conditions, assign a monetary bail, or detain the defendant without bail. Judges rarely use the detainment without bail option. Prosecutors have no role in these hearings.

In the absence of a lawyer for the defendant, the judge makes their decision solely based on the risk assessment and the charge for which the individual was arrested. When a public defender is present, they will speak to the judge in the courtroom while the judge is reviewing the risk assessment paperwork and making their decision (prior to the hearing). The public defender will have already spoken to the defendant and can make the judge aware of relevant information about the defendant, such as informing the judge that the defendant has a regular job

¹ Individuals arrested outside of Pittsburgh during business hours can still be brought to PMC for their initial bail hearing if it is determined that they should be taken to the jail.

² Pretrial release dispositions are made prior to the bail hearing so that during the hearing the final paperwork can be presented to the defendant for them to sign. Judges often make their decision about an hour before the actual bail hearing occurs. Judges are not supposed to talk with defendants during the initial bail hearing due to Fifth Amendment concerns.

for which they need to show up, or that the defendant has a place to live that is separate from where an alleged victim is living. Public defenders thus act as a conduit through which defendants can convey important mitigating information to the judge. Further, public defenders can try to increase judge concurrence with the pretrial risk assessment—in particular, they can try to get judges to avoid setting a monetary bail in situations where the risk assessment recommends the defendant be released with non-monetary conditions.

3. Experiment Design and Data

In April 2017, Allegheny County began providing public defenders for all bail hearings at PMC during regular business hours (Monday through Friday from 8am-4pm). Allegheny County conducted an internal evaluation using a pre-post research design, which showed that providing a public defender appeared to reduce the use of monetary bail and pretrial detention (3). As a result, in early 2019, the county decided to expand their provision of public defense services to the bail hearings that take place during non-business hours (bail hearings take place 24 hours a day, 7 days a week). To implement this expansion of services, the public defender’s office hired two new public defenders to cover the bail hearings occurring in these off-hours. Because these two attorneys could only staff about half of the shifts during the evening, overnight, and weekend hours, we worked with the public defender’s office to assign the attorneys in a way that would allow for a more rigorous evaluation of the impact of public defenders.

Our goal was to ensure that the cases in the shifts with a public defender (the treatment shifts) would look very similar to the cases in the shifts with no public defender (the control shifts). We also had to ensure that the resulting work schedule was relatively regular to make it amenable for the two attorneys staffing these shifts, and we could not reduce the staffing of business hour shifts. Figure 1 presents the schedule that was developed—bail hearings that occur in cells with a “PD” were staffed with a public defender, and empty cells indicated shifts where no public defender was present. The public defender’s office followed the Pay Period 1 schedule for two weeks, then alternated to the Pay Period 2 schedule for two weeks, then back to the Pay Period 1 schedule for two weeks, and so forth for the duration of the study. The study was in the field between April 1, 2019, through March 13, 2020. A public defender working a given shift

represented all defendants who had their bail hearing during that time period, regardless of their eligibility for a public defender at subsequent hearings.

To have balanced treatment and control groups, our analyses only included defendants that had bail hearings in shifts where the public defender status varied across pay periods. For example, over the year with which our study was in the field we expected that the set of defendants who had their bail hearing on Sunday between 4am-8am to be relatively similar from week to week—those who happened to have their bail hearing during Pay Period 1 were provided a public defender, while those who happened to have their bail hearing during Pay Period 2 were not. In this way, we can only study the bail hearings that occur in the blue and yellow cells in Figure 1. The bail hearings that occur in the blue cells correspond to the treatment group, and the bail hearings that occur in the yellow cells correspond to the control group.

Allegheny County provided data on all bail hearings that occurred between April 1, 2019 through March 13, 2020. For each hearing we observe information on the date and time the bail hearing took place, the disposition, the demographics of the defendant and their criminal history, who the judge was, the complete set of charges associated with the arrest, and the defendant's pretrial risk assessment. The county also provided information on preliminary hearing outcomes (failure-to-appear rates and probable cause findings), rearrests, and jail booking data, which details the jail stints for all individuals in our sample, as well as notes whether they had any holds that would require them to be detained in jail regardless of what happened at their bail hearing. The public defender's office provided data on all of the bail hearings they staffed, which allowed us to identify which of the bail hearings actually had a public defender.³ More details on the construction of the data are provided in the appendix. In total we have 2002 cases in the treatment group, and 2089 cases in the control group.

Table 1 examines whether our experiment design resulted in balanced treatment and control groups. To test for covariate balance, we use the criteria from What Works Clearinghouse (22), which says that for the experiment to be valid, the difference between the means for the treatment and control groups for a given covariate cannot be bigger than 25% of

³ During the year the study was in the field, research assistants observed bail hearings regularly to ensure that the research design was being adhered to, and to better understand the nature of the interactions between the public defender and the judge.

the standard deviation of the covariate. All of our covariates are balanced well within these bounds.

The appendix provides further evidence of the validity of our experiment design. Table ST1 shows that the treatment and control groups were balanced with respect to the bail hearing judge, and Table ST3 provides evidence that the courts were not manipulating who was in the treatment and control groups. Table ST2 indicates that there was extremely good compliance with the research design, such that public defenders worked the shifts they were supposed to and were not present when they were not on the schedule. This compliance, along with the fact that at the initial bail hearing private attorneys were rarely involved and everyone qualified for the public defender, results in a situation in which the treatment-control comparison will reveal the impact of going from a situation where essentially no one has a lawyer to one in which everyone has the services of a public defender.

4. The Impact of Public Defenders on Bail Hearing and Pretrial Detention Outcomes

Figure 2 presents our main results regarding the impact that providing public defenders at bail hearings has on bail hearing and pretrial detention outcomes. These are estimates of intent-to-treat effects in that we are directly comparing the outcomes of defendants assigned to the treatment group with the outcomes of those assigned to the control group. Because Table 1 indicated the covariate imbalances were not zero, and because controlling for covariates generally improves the efficiency of the estimator, all the treatment-control comparisons presented in Figure 2 control for an extensive set of defendant and case characteristics. Specifically, we identify the treatment effect by regressing a given outcome on a treatment indicator and all the variables presented in Table 1, as well as judge and shift controls.⁴ The outcomes shown for the control group in Figure 2 correspond to the average value of the outcome variable among the control group (i.e., the baseline value), while the outcomes for the treatment group are determined by adding the regression-adjusted coefficient on the treatment indicator to the baseline value for the control group. Standard errors were clustered by shift time

⁴ To increase precision, we control for each component of the risk assessment tool, rather than controlling for the three risk assessment levels.

and date, which corresponds to the level of randomization, to allow for correlation among outcomes of cases heard during any four-hour shift (10).

The results for the bail hearing outcomes show clearly that public defenders have a substantial impact on defendants receiving a favorable outcome at the initial bail hearing. While those in the control group received either an ROR or non-monetary release only 49% of the time, those in the treatment group received this favorable outcome 60% of the time, a 22% increase. We also examine the proportion in the treatment and control groups that are assigned a monetary bail falling below a given threshold, where those who received either ROR or non-monetary conditions are coded as being below the threshold. The results indicate that public defenders mainly influence outcomes for defendants that would have received a monetary bail of \$10,000 or less. We also find that public defenders increased judges' concurrence with the risk assessment tool, which is defined as occurring when the judge's decision either follows the recommendation from the risk assessment or is more lenient. This increased concurrence thus seems to be one mechanism through which public defenders reduce the likelihood a monetary bail will be set.

The results for detainment outcomes indicate having a public defender at the initial bail hearing resulted in a decline in immediate pretrial detention after the bail hearing of five percentage points. Note that there is not a one-to-one relationship between being assigned a monetary bail and being detained pretrial. Some of the defendants in the control group who were assigned a monetary bail paid the bail amount and were released, while some members of the treatment group who were released with either an ROR or with non-monetary conditions were subsequently detained in jail because they had another hold (such as a probation detainer). For this reason, the impact of the public defender intervention was naturally somewhat smaller for pretrial detention than it was for the bail hearing decision.

While the public defender had a significant impact on immediate pretrial detention, the results indicate that 14 days after the bail hearing, those in the treatment and control groups were equally likely to be in jail. The dissipation of this pretrial detention effect likely occurred because bail review hearings were conducted on all individuals who remain in jail solely because they were assigned a monetary bail they cannot pay, and for whom the pretrial risk assessment recommended release. At these review hearings, which typically took place within three days of

the initial bail hearing, public defenders were present for all defendants. Thus, eventually the treatment and control groups ended up in the same situation with respect to pretrial detention, but it took those in the control group longer to get there because it took them longer to get access to a public defender.

Results from a heterogeneity analysis—which examines whether certain groups benefited more than others from the provision of a public defender—are presented in the appendix. Table ST4 indicates that the observed reduction in pretrial detention only occurred among individuals not charged with a violent offense. We also estimate a significantly larger impact on receiving ROR or non-monetary release for non-violent offenders than for violent offenders. Both of these findings imply that judges might have been more open to listening to the public defender’s recommendation for non-violent offenders. The treatment effects do not appear to vary by the defendant’s gender or race, but the treatment did have a larger negative impact on pre-trial detention for defendants older than 30 than for younger defendants.

5. The Impact of Public Defenders on Downstream Defendant Outcomes

As noted in the introduction, prior research has demonstrated that interventions that impact pretrial detention rates can also affect failure-to-appear rates at court hearings, case outcomes, and rearrest rates. To better understand the broader impacts of providing public defenders at bail hearings, the results in this section evaluate the impact the intervention had on these downstream outcomes.

Our results in this section have two key caveats. First, public defenders were already being provided at bail review hearings that occurred within three days of the first hearing. Therefore, our estimates reflect only the impact relative to the status quo of a public defender at the bail review hearing. Second, due to the onset of the COVID-19 pandemic in Allegheny County, we were not able to use outcome data that was collected after March 13, 2020. After this date, several changes were made to various criminal justice processes within Allegheny County that had the potential to significantly impact the outcomes examined here.⁵ As the intent of this

⁵ In response to the COVID-19 pandemic, Allegheny County made it a priority to release all non-violent offenders who were in the jail solely because they could not pay their monetary bail. Arrest activity around the county also changed, and court cases were delayed for lengthy periods of time.

study was to identify the impact of this intervention in normal times, our analysis necessarily focuses on short-term outcomes. We thus examine the impacts the intervention had on preliminary hearing outcomes (as opposed to the final case disposition), as well as rearrest activity within 180 days from the bail hearing (versus a longer two or three-year follow-up period). Each of the outcomes examined in this section requires a different level of sample truncation to ensure that the outcome for everyone in the sample can be measured by March 13, 2020.⁶

Figure 3 shows the impact public defender provision at the bail hearing had on downstream outcomes; the methodology used to obtain these results mirrors that used to obtain Figure 2. The estimates indicate that the public defender intervention had no impact on whether the defendant failed to appear at their preliminary hearing, or on the outcome of the preliminary hearing. These results are unsurprising given the impact the intervention. With respect to failure to appear rates, these preliminary hearings typically do not take place until at least two weeks after the bail hearing. By that point the intervention no longer had any impact on whether a defendant was in jail, and thus there should be no impact on failure to appear rates.⁷ With respect to the outcome of the preliminary hearing, the public defender intervention only provided assistance to the defendant regarding the outcome of their bail hearing. A different public defender was then assigned to represent the individual at their preliminary hearing if they were eligible for a public defender. Those in the treatment group were not receiving any extra access to services from the public defender's office between the time of their bail hearing and their preliminary hearing that would decrease the likelihood that the judge would determine probable cause to exist (thus allowing the case to move to the next level of prosecution).

The final outcome we consider in Figure 3 is whether individuals were charged with a new crime by law enforcement within 180 days of their initial bail hearing, which we term a rearrest. The results indicate that those in the treatment group were 3.2 percentage points more likely than those in the control group to be rearrested for any crime within the first 180 days of

⁶ For example, to measure whether or not individuals were rearrested within 180 days of their bail hearing, we can only use individuals that had their bail hearings on or before September 15, 2019, so that the entire 180 day follow-up period occurred before March 13, 2020.

⁷ If individuals are in jail when the pretrial hearing occurs, the jail ensures the individual shows up at their hearing. Thus, failure to appear rates are often higher when individuals are out of jail during the pretrial period.

their bail hearing, although this difference is not statistically significant. The remaining rearrest specifications examine whether any type of crime increased after this intervention. Once we identify that the treatment only had a statistically significant impact on rearrests for felony crimes (as opposed to misdemeanor or summary offenses), we then further parse which sets of felony crimes drive this result. Notably, the intervention has no impact on rearrests for violent felonies. Instead, we find that the overall increase in rearrests was being driven by an increase in rearrests for third-degree felony theft charges.⁸ In particular, while 2% of those in the control group were rearrested within the first 180 days of their bail hearing for a third-degree felony theft charge, 5.5% of those in the treatment group were.⁹ Although third-degree felony theft charges can potentially involve theft of items worth a significant monetary amount, almost three quarters of these rearrests were for retail theft. Under Pennsylvania law, if the individual has two prior theft convictions, an incident of retail theft will be charged as a third-degree felony regardless of the value of the item stolen. While we do not observe the value of items stolen in our data, it is possible that many of these rearrests involved minor retail thefts.¹⁰

The rearrest results imply that reductions in monetary bail and pretrial detention (which are the main ways the intervention impacted individuals) led to an increase in rearrest rates. There are several potential reasons why this might have happened, although we cannot definitively determine why this increase occurred. While incapacitation mechanisms (whereby those in jail are physically prevented from reoffending) are often suggested to explain why reductions in pretrial detention can lead to increases in rearrest rates, our results are not generally consistent with this type of story. At an intuitive level, the decrease in pretrial detention caused by the intervention was not large enough to incapacitate individuals from offending over a 180-day time frame. In the appendix we show that our findings imply that the treatment causes a decrease in detention of .29 days for the average individual in the treatment group. The results in Figure 2 indicate that this decrease in detention likely didn't occur for everyone. In particular, the results for the detainment outcomes for the control group indicate that 55% of individuals would not have gone to jail, and 30% were still in jail 14 days later regardless of the intervention.

⁸ Third-degree felony theft charges include receiving stolen property, retail theft, and theft by unlawful taking.

⁹ Theft charges make up 55% of third-degree felony rearrests.

¹⁰ 86% of individuals who were rearrested for a third-degree felony theft charge had at least two prior convictions, although we do not observe if those prior convictions were for theft.

Thus, it is likely the decrease in pretrial detention was only felt for the remaining 15% of the sample. If we attribute the total drop in pretrial detention for the treatment group to just 15% of the individuals, those 15% of treatment individuals saw a decline in pretrial detention of 1.9 days, while the other 85% of treatment individuals were unaffected. It is unlikely that having the opportunity to offend for roughly two extra days over the course of six months was enough to explain why those in the treatment group were almost three times as likely to be rearrested for a third-degree felony theft charge. As further evidence that our results are not the result of incapacitation, we find that the treatment effect remains virtually unchanged in the rearrest specifications in Figure 3 when we add in an explicit control for the number of days (over the 180-day period) that the individual was out of jail. Thus, the additional time outside of jail does not seem to be driving the impact on rearrests.

Beyond an incapacitation effect, there are a couple deterrence-based reasons why the intervention might have led to an increase in rearrests. Specifically, because those in the control group were more likely to have to pay a monetary bail and more likely to be detained pretrial, the negative experience of those events might deter them from offending in the future. Alternatively, those in the treatment group who received public defender services might have been emboldened by their experience of getting out of pretrial detention and thus perceived the consequences of being arrested again to not be as serious.¹¹

In terms of why the increase in rearrest rates only occurred for third-degree felony theft charges, one reason this might have occurred is that the people who were most affected by the intervention were more likely to commit these types of offenses. Specifically, the heterogeneity analyses presented in Table ST4 indicate that the public defender intervention only reduced the likelihood of detention for those who had a non-violent arrest charge. This group was much more likely to have their focal arrest charge classified as a theft charge, implying their future rearrests might fall in this category as well.¹²

¹¹ Allegheny County does not require individuals to forfeit their bail if they reoffend during the pretrial period. Thus, while the experience of having to pay a monetary bail (which includes permanently losing a 10% deposit if a bail bondsman is used) might deter individuals from offending again, the monetary bail itself should not directly incentivize individuals to avoid offending during the pretrial period.

¹² Examining focal charges (i.e., the charge that led to the bail hearing), 0% of those with a violent focal offense have their charge classified as theft, whereas 27% of those with a non-violent offense have their charge classified as theft.

Finally, several previous studies evaluating the impact of pretrial detention on rearrest rates have found that pretrial detention caused rearrest rates to increase, while our results imply the opposite. One potential reason for these different findings is that we are only able to examine short-term rearrest outcomes, while previous literature has followed the impact on arrest over a two or three-year follow-up period. This longer follow-up period allows the impact of pretrial detention to change over time. For example, early on, those who are detained pretrial might be deterred from reoffending. In the long-term, however, even two extra days of pretrial detention can be extremely disruptive to individuals if it causes them to lose their job and custody of their children, as well as increases their exposure to criminogenic influences, which can then lead to disruptions in the individual's living situation and health (4). This pattern, whereby the causal relationship between pretrial detention and rearrest rates is first negative but then becomes positive as the follow-up window increases, has been observed in several studies (9, 13). Future work should thus evaluate the impact of this intervention on rearrest rates over a longer time window.

6. The Tradeoff Between Pretrial Detention and Rearrests

Providing a public defender at bail hearings appears to involve a tradeoff between lowering pretrial detention rates and increasing rearrests for third-degree felony theft charges. In this section we discuss how to think about this tradeoff, albeit recognizing that this tradeoff will not be relevant to all jurisdictions. First, for some jurisdictions, the question of whether to provide a public defender at this stage will be normative. Within this perspective, because the bail hearing can have important consequences for a defendant, representation should be provided to defendants at this stage regardless of what any analysis shows. Second, some jurisdictions might be willing to staff public defenders at bail hearings so long as these attorneys are shown to have a positive effect on defendant outcomes at these hearings. The results presented in Section 4 provide clear evidence of this, and thus a discussion of the tradeoffs between pretrial detention and rearrest rates would be irrelevant for these jurisdictions as well. However, given the intense public focus that often occurs whenever changes in pretrial policy are thought to increase crime rates (17), it is likely that some jurisdictions will consider both the immediate and downstream consequences of potential interventions and may only support the provision of representation at

the initial bail hearing if the tradeoffs between pretrial detention and rearrests are favorable. We thus directly consider these tradeoffs in this section to help inform these discussions.

While monetary cost-benefit analyses can often be helpful in situations where an intervention involves clear tradeoffs, in this setting—where there is a wide amount of variation in estimates of the benefit of staying out of jail—the results can be more difficult to interpret. Here, a monetary cost-benefit analysis will essentially identify a threshold in dollar terms such that the policy should be implemented if a day of someone’s freedom is worth more than the threshold. However, because there will inevitably be a large amount of variation in terms of the amount individuals are willing to pay to stay out of jail (i.e., the value of freedom), and this amount is likely to be related to income level, this monetary threshold is unlikely to help policymakers come to a consensus conclusion about whether the tradeoff the intervention presents is worth it. Instead, we follow a cost-benefit approach developed by Stevenson and Mayson (21), which involves directly comparing the number of pretrial detention days avoided with the number of additional crimes committed.¹³ Results from a traditional monetary cost-benefit analysis are presented in the appendix.¹⁴

The results from Figures 2 and 3 indicate that the average treatment group member served .29 less days of detention and committed .035 more third-degree felony theft crimes than the average control group member. This means that, for the tradeoff presented by this intervention to be undesirable, the cost of a third-degree felony theft charge to society must be at least 8.3 times more than the cost to society of a day in detention ($.29/.035=8.3$). Put another way, for this tradeoff to be bad, individuals would have to be willing to spend at least 8.3 days in jail to avoid being the victim of a third-degree felony theft crime. Stevenson and Mayson (21) surveyed individuals in the general population and found that the median respondent would only be willing to spend one day in jail to avoid being the victim of a burglary. A third-degree felony theft offense is less harmful than a burglary, and thus these survey results indicate the median individual would be willing to accept the tradeoff the public defender intervention induces.

¹³ Albright (2) also uses this approach in recent work examining the impacts of an automatic release policy.

¹⁴ The monetary cost-benefit analysis results indicate that if society values the damage from incarcerating an individual for one day to be greater than \$504 (which is only 3% of the higher estimate of the societal cost of a day in jail), this program should be considered cost-effective.

The analysis conducted in this section is constrained to considering the short-term tradeoffs. As discussed in Section 5, the relationship between pretrial detention and rearrest rates might have been neutral or even positive if we had been able to use a longer follow-up window, which would eliminate the need to consider the tradeoff between these two factors.

7. Conclusion

This paper presents experimental evidence that providing public defenders at bail hearings reduced the use of monetary bail and short-term pretrial detention, with no impact on failure to appear rates or the probable cause determination at the preliminary hearing. This new evidence is important for constitutional arguments about whether bail hearings should be considered a critical stage requiring a lawyer (14). Further, in the absence of this designation, these results should help inform local jurisdictions, who are currently responsible for deciding whether defense counsel will be provided at bail hearings. These results are especially relevant given that recent widespread efforts at the local level to reform the monetary bail system have focused almost exclusively on implementing risk assessment instruments that recommend to judges that they replace monetary bail with supervisory conditions. However, research has found that judges often do not follow these recommendations and continue to set monetary bail (20). The results we find in Pittsburgh indicate that, in these situations, providing a public defender at the bail hearing appears to increase concurrence with the risk assessment, which will subsequently help jurisdictions reduce their use of monetary bail and pretrial detention.

For jurisdictions that are concerned with the increase in rearrests for third-degree felony theft charges that arose as a downstream impact of this intervention, our analysis indicates that for the tradeoff between reduced pretrial detention and increased rearrests to be problematic, the cost of a theft charge to society must be at least 8.3 times more than the cost to society of a day in detention. Current survey estimates of how individuals value these costs indicate this tradeoff should be acceptable for most jurisdictions. It is important to note that, due to the COVID-19 pandemic, we were prevented from evaluating the long-term impact of the intervention on rearrest rates. This is important for future research to consider, as the impact on rearrest rates might have changed if we were able to examine a longer time window for rearrests, potentially nullifying the concern about these tradeoffs.

Finally, the fact that bail review hearings (during which a public defender is always present) occur within three days of the initial bail hearing in this jurisdiction means that, *a priori*, the public defender who appears at the initial bail hearing could only have a limited impact on the length of time spent in detention. In jurisdictions where bail review hearings are either not conducted, or conducted without a public defender, the provision of public defenders at bail hearings might have a bigger impact on the number of days a defendant was detained pretrial, which in turn might impact case outcomes and rearrest outcomes in different and more substantial ways.

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Figure 1: Public Defender Shift Schedule

Pay Period 1 Schedule

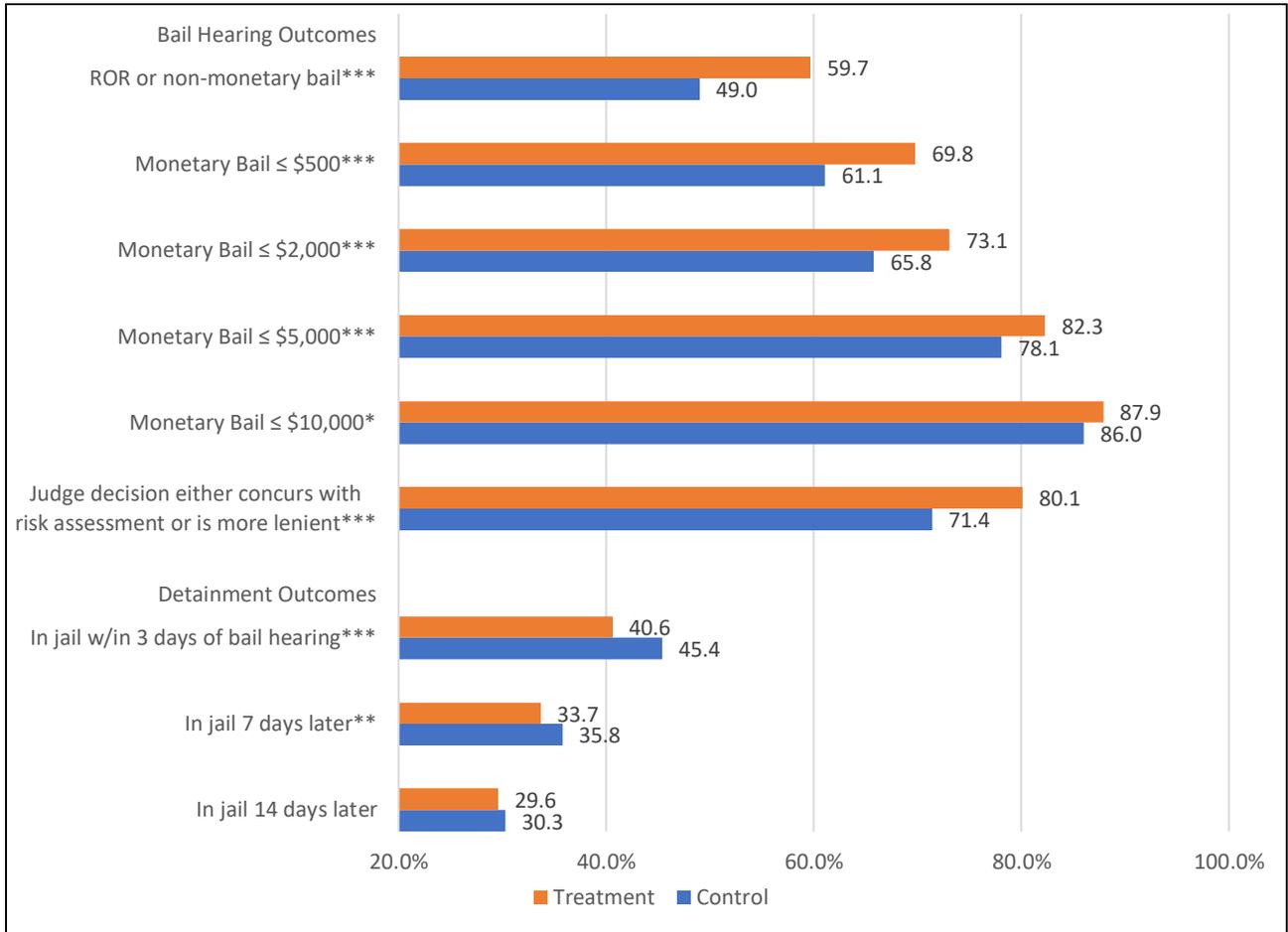
	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
12 Midnight – 4 AM		PD	PD	PD			
4 AM – 8 AM	PD	PD	PD	PD	PD		
8 AM – 12 Noon	PD	PD	PD	PD	PD	PD	
12 Noon – 4 PM		PD	PD	PD	PD	PD	
4 PM – 8 PM		PD	PD	PD	PD	PD	
8 PM – 12 Midnight		PD	PD				

Pay Period 2 Schedule

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
12 Midnight – 4 AM					PD	PD	
4 AM – 8 AM		PD	PD	PD	PD	PD	
8 AM – 12 Noon		PD	PD	PD	PD	PD	
12 Noon – 4 PM		PD	PD	PD	PD	PD	PD
4 PM – 8 PM			PD	PD	PD	PD	PD
8 PM – 12 Midnight				PD	PD	PD	

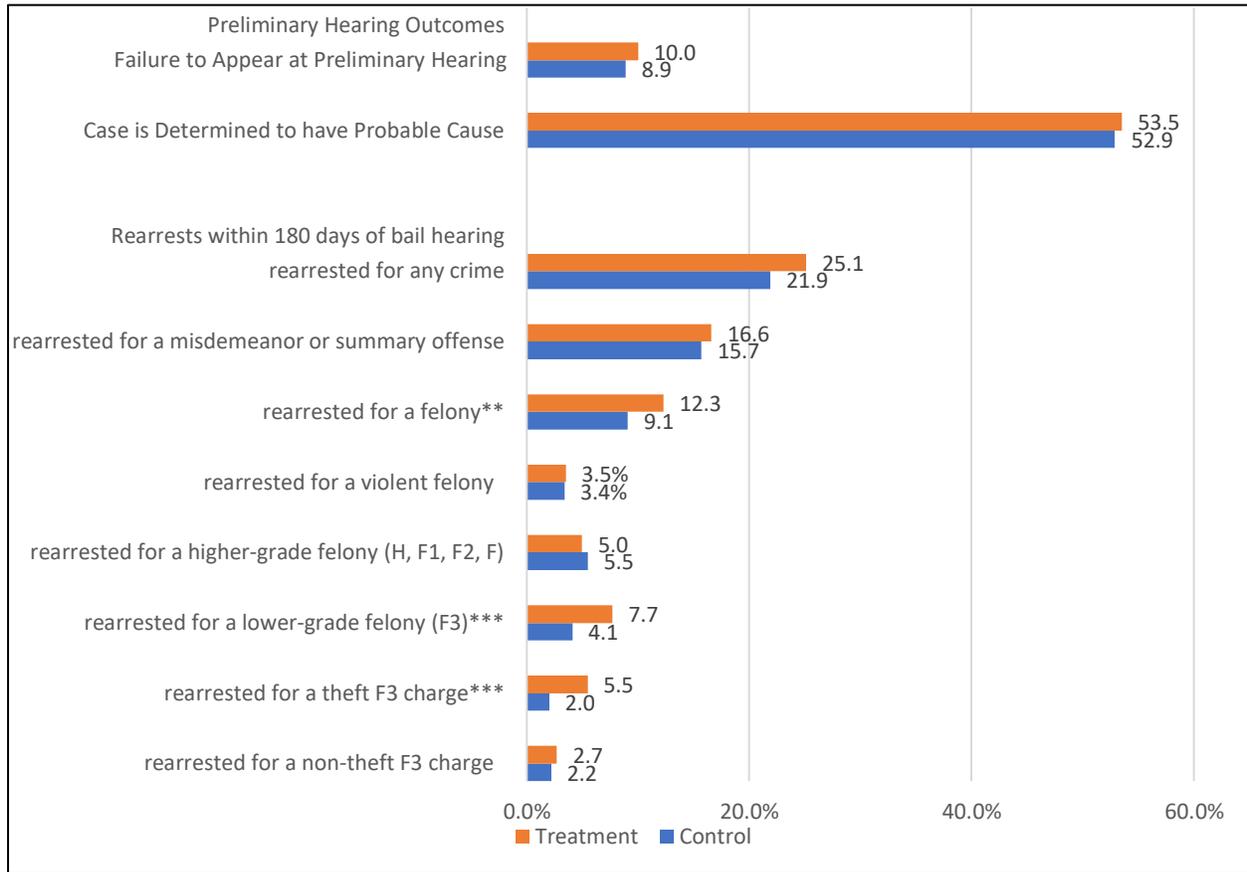
Note: The schedule alternates back and forth between these two shift schedules every two weeks. The blue shifts represent the treatment shifts, and the yellow shifts represent the control shifts.

Figure 2: Impact of Public Defender Provision on Bail Hearing and Pretrial Detention Outcomes



Note: ***, **, * indicate the difference between the treatment and control group is statistically significant at the 1%, 5%, and 10% level, respectively. The treatment-control comparisons are OLS regression-adjusted using controls for: gender, race, age, and education level of the defendant; whether the offense occurred within Pittsburgh (versus the greater county); grade and type of dominant charge; prior record and failures to appear; whether the defendant had other pending charges or was on probation at the time of their bail hearing; whether the defendant had any holds; judge; and the sixteen different four-hour shifts that composed the treatment and control groups. Standard errors were clustered by shift time and date. With the exception of the 7 and 14-day later detainment outcomes, all comparisons use the sample of 4091 bail hearings which occurred between April 1, 2019 through March 13, 2020. The 7 and 14-day later detainment outcomes truncate one week and two weeks from the sample, respectively, so that the detainment outcome can be measured before the onset of the pandemic.

Figure 3: Impact of Public Defender Provision on Downstream Outcomes



Note: ***, **, * indicate the difference between the treatment and control group is statistically significant at the 1%, 5%, and 10% level, respectively. The treatment-control comparisons are OLS regression-adjusted using the same specification as described in Figure 2. In order to only use data collected before the pandemic, sample sizes vary across the outcomes used. For failures to appear we used all bail hearings that occurred between April 1, 2019 through November 30, 2019 (n=2993); the probable cause determination dropped 261 additional observations that had not had their preliminary hearing as of March 13, 2020. For the rearrest within 180 days outcome, we use the 2167 bail hearings that occurred between April 1, 2019 through September 15, 2019. A crime of grade “F” corresponds to an ungraded felony drug charge. For this charge the maximum punishment is driven by prior convictions, and thus it does not have a specific grade attached to it like the other charges do.

Table 1: Covariate Balance Between Treatment and Control Groups

	Overall Mean	Overall Std. Deviation	Treatment Mean	Control Mean	T/C Difference as a Percent of SD
<i>Defendant Demographics</i>					
Age (years)	35.0	12.0	34.9	35.1	1.7
Black	0.561	0.496	0.560	0.562	0.4
White	0.422	0.494	0.422	0.421	0.2
Female	0.274	0.446	0.253	0.294	9.2
<i>Criminal History</i>					
Age at First Arrest (years)	21.3	8.2	21.2	21.3	1.2
Number of Prior Arrests	10.4	11.4	10.2	10.5	2.6
Number of Prior Felony Convictions	1.50	2.83	1.51	1.49	0.7
Number of Prior Misd. Convictions	2.70	3.48	2.59	2.80	6.0
Number of Prior Failure-to-Appears	1.20	2.14	1.16	1.23	3.3
<i>Case and Defendant Characteristics</i>					
Lead Charge is Felony	0.438	0.496	0.447	0.430	3.4
Number of Charges	3.58	2.89	3.56	3.60	1.4
Multiple Incidents Being Handled	0.064	0.244	0.067	0.061	2.5
Person Charge	0.370	0.483	0.370	0.370	0
Property Charge	0.228	0.420	0.223	0.233	2.4
Drug Charge	0.137	0.344	0.153	0.122	9.0
Weapon Charge	0.036	0.186	0.038	0.034	2.2
Public Order Charge	0.130	0.336	0.121	0.139	5.4
Other Pending Charges	0.350	0.477	0.352	0.348	0.8
Currently on Probation	0.297	0.457	0.281	0.312	6.8
Hold/Detainer Issued	0.221	0.415	0.216	0.225	2.2
Arrest within Pittsburgh	0.553	0.497	0.564	0.543	4.2
<i>Risk Assessment Recommendation</i>					
Pretrial Recommendation of ROR	0.084	0.278	0.085	0.083	0.7
Pretrial Recommendation of Non-Mon. Release	0.672	0.470	0.672	0.672	0.0
Pretrial Recommendation of Detention	0.243	0.429	0.243	0.244	0.2
Observations	4091		2002	2089	

Supplementary Appendix Materials for:

The Impact of Defense Counsel at Bail Hearings

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List of Supplementary Materials
Materials and Methods
Supplementary Figures SF1-SF3
Supplementary Tables ST1-ST4

1. Materials

1.1 Data Description and Sample Construction

Allegheny County provided data on all bail hearings that took place at PMC between April 1, 2019 through March 13, 2020. For each hearing we observe information on the date and time the bail hearing took place, the demographics of the defendant (age, gender, race), who the judge was, the complete set of charges associated with the arrest, and whether the defendant had any holds that would require them to be detained in jail regardless of what happened at their bail hearing. The latter occurs often in situations where the individual was already on probation and the new arrest triggers a probation detainer. We also observe the defendant's pretrial risk assessment—this includes information on all the individual components that go into the risk assessment tool, the overall risk assessment level given by the tool, and whether pretrial staff override the risk assessment. The public defender's office provided data on all of the bail hearings they staffed, which allowed us to identify which of the bail hearings actually had a public defender.

The county also provided detailed data on a wide range of outcomes, including the outcome of the initial bail hearing, as well as subsequent bail review hearings, which are automatic follow-up hearings that occur for lower-risk individuals who remain in jail because they cannot pay their monetary bail. Each defendant will have a preliminary hearing, which typically occurs within 14 days of the bail hearing and determines if probable cause exists such that prosecution can move forward. We coded two outcomes from this hearing—whether the defendant failed to appear at this hearing, as well as the outcome of this hearing (i.e. whether probable cause was established versus the charges were dismissed or downgraded to be handled in a lower court). The county provided data on all charges filed by law enforcement within the county between April 1, 2019-March 13, 2020. This information was used to determine if individuals in our sample were rearrested.

The court data does not indicate whether the individual was detained pretrial, but pretrial detention outcomes can be determined by merging in jail admission data. If the defendant is admitted to jail within three days of their initial bail hearing, we code them as being detained pretrial. It is important to note that jail admissions can be coded in two ways: as being booked into jail, which occurs the moment the intake process starts, and being moved to a pod within the jail, which occurs when the intake process has been completed and the defendant is transferred to their cell (as opposed to a temporary holding cell). The Allegheny County Jail begins their booking process right away such that almost everyone that is assigned a monetary bail is recorded as being booked into jail, even though many of these individuals pay their bail within a few hours and are released. In conversations with the county, they view being moved to a pod as the real measure of whether a defendant is admitted to jail. For this reason, throughout the paper the outcome “in jail” indicates the person was booked into jail *and* moved to a pod.

Fourteen percent of the observations in the data corresponded to situations in which a defendant had a bail hearing for multiple incidents at the same time. Each incident corresponds to a separate offending situation (i.e., the offense date might be different); each incident can

include multiple charges as part of the incident. Because the pretrial detention outcome corresponds to what happens across all incidents being handled at a given bail hearing, we collapsed the data down such that multiple incidents being handled at the same bail hearing only represented one record. We created an additional control which noted whether a given defendant had multiple offenses being adjudicated at the bail hearing. When a defendant had multiple incidents, most of the defendant characteristic variables (such as the pretrial risk assessment variables) were naturally exactly the same for both incidents, so we did not need to make any decisions regarding which variable values to use when we collapsed records. For case characteristics, we chose the case type and offense level associated with the most serious charge across all incidents. In terms of outcome variables, the way we collapsed these variables down depended on the outcome. To determine the level of monetary bail set, we summed the bail set across all of the incidents, as the defendant would have to pay all of these before they could be released. We defined the defendant as receiving monetary bail if they were assigned a bail on at least one of their incidents. We define a failure to appear occurring when the defendant fails to appear for at least one of their preliminary hearings for the separate incidents. Regarding the outcome of the preliminary hearing, we measure whether probable cause was determined for at least one of the incidents. Pretrial detention and re-arrest outcomes were already at the defendant level, and thus did not need to be collapsed when a defendant had multiple incidents.

Each bail hearing had a time stamp associated with it, and we used that to code whether the shift was in the treatment or control group. There were a few treatment shifts throughout the year that the public defender did not appear for due to either scheduled training, vacation days, or sick days. We dropped 337 observations that occurred in those shifts. To ensure we continued to have balance between the treatment and control groups, we offset each dropped treatment shift with a dropped control shift that was exactly two weeks later. This resulted in a loss of an additional 331 observations from the control group. We also dropped 10 observations where either the bail hearing didn't occur right after the arrest (i.e. it was a bail hearing that occurred after an individual had already failed to appear in court on their charge), or the bail hearing was being conducted for another jurisdiction. We dropped 52 observations because the individual's identifying information was missing, which would not allow the observation to be matched to the jail data. We dropped 25 observations where the defendant had multiple incidents being handled at the bail hearing, and these multiple incidents either spanned different shifts or different judges. Finally, we dropped one case where the time stamps indicated the bail hearing occurred before the pretrial risk assessment was conducted. These sample drops resulted in a final sample of 4091 observations—2002 treatment observations and 2089 control observations.

2. Methods

2.1 Further Analyses Validating Execution of Experiment Design

Judge Balance

To ensure the treatment and control groups are equivalent it is important that defendant characteristics are similar across groups, of which we showed evidence in Table 1. It is also

equally important that the same composition of judges is present across groups, as judges can vary substantially with respect to how likely they are to assign a monetary bail. We did not explicitly balance based on judge assignment, but due to the rotating pattern in which judges staff these hearings, we expected to have judge balance across the treatment and control groups. We first discuss the different types of judges that preside over these cases and their rotation schedule, and then provide evidence of balance for individual judges.

Over the course of our year-long study, 6 percent of cases were staffed by the 12 judges that were elected to represent districts within Pittsburgh. These city judges typically work the bail hearings that occur during business hours, although also work occasional shifts on the weekends, which is why they are observed somewhat rarely in our off-hours data. Thirty-six percent of cases were staffed by the 34 judges that were elected to represent suburban districts that are outside of Pittsburgh, but within Allegheny County. These judges cover the shifts that occur between 4pm-12am. Fifty-eight percent of cases were covered by seven senior judges who cover shifts that take place between 12am-8am. Senior judges are retired judges who are no longer serving in an elected position.

All judges within their groupings (i.e. city, suburban, and senior) work on a rotating basis for the shifts they are supposed to cover, although the rotation pattern differs across judge types. Both city and suburban judges usually work one eight-hour shift, and then are called to preside again when all the judges in their grouping have been called. Senior judges tend to work for a full week, and then often have several weeks off before they are called back again. However, judges can fill in for each other, and some senior judges seem to work more frequently than other senior judges.

When we test for imbalance in judge type, we find that all three types of judges were well balanced between treatment and control cases, suggesting that there were no efforts to divert cases to specific types of judges. The difference in the percent of judges in treatment and control cases, expressed as a percentage of the standard deviation of the overall percent of each type of judge, was 0.4, 3.6, and 6.5, for suburban, senior and city judges, respectively. All of these are well below the 25% threshold specified by What Works Clearinghouse (22).

We further test for judge balance by looking at balance for each individual judge. The data used to estimate Table ST1 is organized at the shift level (rather than at the case level), so we are examining whether the percent of treatment shifts each judge works is similar to the percent of control shifts they work. We organized the table in this manner because a judge is assigned to a shift as opposed to a case. We use the same What Works Clearinghouse (22) criteria for balance as we did in Table 1. While there are some imbalances, all judges fall below the What Works Clearinghouse threshold of difference of 25% of combined standard deviation; controlling for judge within the regression analysis can thus correct for this. These imbalances occurred because we did not explicitly balance on judge identity, and sometimes the way their rotating pattern mapped onto the shift schedule resulted in them serving in more of one shift type than another.

Compliance

In this section we examine how well our research design (outlined in Section 3) was complied with. Our examination of compliance breaks the bail hearing down into its component parts. Expanding on the basic structure of bail hearings that was outlined in Section 2, one can think of public defender involvement in a given bail hearing as consisting of three stages: (1) talking with the defendant; (2) talking with the judge before they make their decision; and (3) being present at the bail hearing when the judge reads their decision. As noted earlier, the judge makes their decision before the actual bail hearing. Public defenders told us that they consider the discussion with the judge before they make their decision to be the most important stage of involvement. Ideally, they would have already talked with the defendant by the time they talk with the judge, or if necessary they can talk to the defendant during the discussion with judge—the holding cell where the defendant is being detained is only a two-minute walk from the courtroom where the judge is, which enables the public defender to ask the defendant for any information the judge requests. However, even without talking to the defendant, public defenders can still lobby for defendants by referring to the risk assessment. Public defenders noted that their presence at the actual bail hearing is not likely to impact outcomes at all, because by this point the final decision has already been made.

The public defender's office collected information on their level of involvement in each case, including whether they talked with the defendant, whether they talked with the judge, and whether they appeared at the bail hearing. Table ST2 presents the public defenders' level of involvement for cases in both the treatment group and the control group. Among those in the treatment group, 60 percent of the time the public defender is involved in all three stages, which is how the intervention is designed to work. However, in an additional 29 percent of the cases they do at least talk with the judge, which we still consider to be an effective version of the intervention. The intervention is unlikely to be effective in the remaining 10 percent of the treatment cases where the public defender did not talk with the judge.

The reason that the public defender didn't always talk with the judge for the treatment cases appears to be related to how we classified cases as being in the treatment group. As noted earlier in our discussion of the data, we use the bail hearing's time stamp (provided in the court data) to determine whether a case belongs in the treatment group. However, this is not a perfect measure because the stages of the bail hearing can take place over the course of a couple hours. Typically, the time stamp corresponds to when the paperwork is filed, which is after the judge makes their decision but before the actual bail hearing. From Figure 1, one can see there are some treatment shifts in our data that come right after a shift where no public defender was present. It is possible that the time stamp for the paperwork makes the case fall into the treatment period, but the judge actually made the decision before that when no public defender was there. This results in a situation where the public defender is present at the actual bail hearing, but was not working when the judge was making their decision. For the 2.5 percent of treatment cases where there was no public defender involvement at all, this again tends to happen during treatment shifts that border a shift where no public defender is present; this indicates the time stamp was likely a bit off and that the case was actually handled in a non-treatment shift. Finally,

even when the public defender talks with the judge, we see that there is a non-negligible fraction of cases in which they did not talk with the defendant. Public defenders noted that this typically occurs when treatment shifts border shifts with no public defender—when the public defender first comes on the shift, the judge is already beginning to make their decision on the next set of defendants to have their bail hearing and will not provide the public defender time to go talk with the defendants. This also occurred because defendants can only talk to public defenders when they are in a holding cell, which occurs after they have completed their risk assessment with the Pretrial Department. Sometimes the Pretrial Department did not move them into the holding cell until a time very close to their actual hearing; in these situations the public defender was not given an opportunity to talk with the defendant.

Table ST2 indicates that public defenders had some level of involvement in 7 percent of the control cases. This tended to occur when control shifts occurred right after a shift where a public defender was present. The time stamp placed the case in the control shift, but it was likely that the judge actually made the decision in an earlier shift when the public defender was working.

Generally, the discussion above indicates public defenders complied extremely well with the research design in terms of working according to the schedule, but the fuzziness of the time stamp combined with the fact that the full bail hearing process takes a few hours results in the compliance statistics not being perfect. This almost-complete compliance, along with the fact that private attorneys are seldom used at these hearings and public defenders were provided to all individuals regardless of income, implies that the intent-to-treat analysis we conduct essentially picks up the impact of going from a situation where no one has access to an attorney to a situation where everyone has access to public defender services at their bail hearing. If anything, the small amount of non-compliance implies the actual impact of complete access to public defender services at bail hearings could be slightly greater than our estimates.

Potential Manipulation of Treatment and Control Group Composition

For our experiment design to produce balanced treatment and control groups, the individuals deciding when a defendant will have their bail hearing cannot make this decision based on when a public defender is likely to be present. Our conversations with court personnel indicate it would be exceedingly unlikely for the timing of an individual's bail hearing to be delayed or sped up so they could receive the services of a public defender. Once an individual is first brought to the jail they are assigned a bail hearing time slot that is the closest to being seven hours from when they were brought in; this implies the time of arrest on a given day (in a given pay period) is what solely determines whether they will be in the treatment group.

One way to empirically examine whether the composition of the treatment and control groups were being manipulated is to compare the average number of people in a given shift slot for the treatment group and the control group. (There are 16 shift slots in our study, which alternate between treatment shifts and control shifts with the pay period.) If court personnel were strategically trying to have more personnel receive the services of the public defenders, we

would expect to see more people in a given shift slot when it is a treatment shift compared to a control shift. Table ST3 shows these comparisons for the 16 different 4-hour shift slots included in our analysis. (These estimates include twenty treatment and control shifts that had zero defendants in them.) Of the sixteen shift slots, we only find statistically significant differences in the average number of people across the treatment and control shifts at the 5% level for two shifts. For these two shifts, the direction of the relationship is not consistent; the Wednesday 12am shift has more in the control group, and the Friday 4am shift has more in the treatment group. This suggests a slightly erratic pattern and does not indicate that the court is trying to manipulate who receives the services of the public defender.

2.2 Additional Specifications

Robustness of Main Results to Not Including Covariate Controls

Figures 2 and 3 presented our main results, where covariate controls were included to account for any potential covariate imbalances as well as to increase efficiency. However, as this study resulted in a reasonably well-balanced treatment and control groups, for clarity we also show what the treatment effects would be if covariate controls were not included. Figure SF1 presents these results for a core subset of the outcome variables that were included in Figures 2 and 3. The results in Figure SF1 do continue to cluster standard errors by the shift time and date (as was done in Figures 2 and 3). The results indicate that none of the main findings from Figures 2 and 3 are appreciably changed by not including covariate controls.

Robustness of Main Results to Specifications Preregistered on Open Science Framework

Regression specifications for this study were preregistered on Open Science Framework. The only difference between the preregistered specification and the current specifications used in the paper is that the preregistered specification stated we would only use individuals who had no pending charges, were not on community supervision, and were not currently incarcerated. For defendants that had multiple preliminary arraignments during the sample period we also stated we would only use their first preliminary arraignment. At the time these specifications were submitted we did not know an extensive amount regarding the characteristics of the individuals at preliminary arraignments. The primary reason we envisioned making this sample drop is that we did not think this group would constitute a large fraction of the sample, and we also thought this group may potentially not receive much benefit from the public defender intervention. In particular, if those on probation receive an automatic detainer upon arrest, the public defender will not be able to have an impact on pretrial detention for those individuals. However, once the data was collected, we realized that making these sample drops would have resulted in a loss of 53% of our observations. Further, our conversations with individuals in the public defender's office indicated that they not only felt they could have an impact in these cases, but that this group of cases was important to include in the analysis because they constitute such a large fraction of the population that they serve. As it is likely that many jurisdictions across the U.S.

will have a similar defendant make-up, we felt it was important to examine the impact the public defender intervention has on the full population having a preliminary arraignment. We thus decided to use the full sample of individuals in our main specifications in the paper. However, for completeness, in Figure SF2 we show the results for the two main preregistered outcomes using the exact preregistered specification. The treatment effects are not appreciably different from those presented in Figure 2. Note that the baseline values for these outcomes are somewhat different than in Figure 2 because here we are using a sample that generally would be considered to be lower-risk than a sample that includes those on probation and with other pending charges.¹⁵

Heterogeneity Analysis

Appendix Table ST4 presents the results from a heterogeneity analysis, which examines whether certain groups benefit more than others from the provision of a public defender. We first show that, as expected, a public defender can only have an impact on our main outcome—pretrial detention—if defendants did not have an active detainer (which would require them to be held in jail regardless of what happened at the bail hearing). The remainder of the heterogeneity analysis conducts sub-group analysis on the set of defendants who did not have detainer holds. The results indicate that the observed reduction in pretrial detention only occurs among individuals that are not charged with a person offense. Further results shown in the table indicate this result seems to be at least partly because public defenders have a bigger impact on the judge’s decision for this group of individuals, implying that judges might be more open to listening to the public defender’s recommendation for non-violent offenders. We also find that the public defender intervention only has an impact on pretrial detention for defendants older than age 30, but has no impact on pretrial detention outcomes for defendants age 30 or younger. Further analyses (not shown) indicate that this results pattern might occur because younger defendants are more likely to be able to pay a given monetary bail amount than older defendants, potentially making them less reliant on the public defender improving their outcome at the bail hearing. We do not find the impacts this intervention had on pretrial detention to vary by defendants’ charge severity (felony versus misdemeanor), gender, or race.

2.3 Monetary Cost-Benefit Analysis

This section presents the results from a monetary cost-benefit analysis of the provision of public defenders at bail hearings. We first provide an overview of our approach and the results, and then discuss in detail how each of the component values were calculated.

Overview of Results

¹⁵ Our original OSF specification also had a third main outcome—subsequent arrests within one-and-a-half years after the focal preliminary arraignment—which we do not include here because the COVID-19 pandemic inhibited our ability to examine longer-term outcomes.

Figure SF3 presents the results of a cost-benefit analysis of the provision of public defenders at bail hearings. The analysis combines the causal impacts of the intervention with monetary values provided by both Allegheny County and the criminal justice literature. Two types of costs and benefits are included—fiscal components are changes in government expenditures and welfare components are monetized estimates of changes to other important outcomes, such as the cost of a new crime to society and the value of defendant freedom. Each of the specific benefits and costs presented in the top two tables of Figure SF3 correspond to the monetary amount (in 2020 dollars) associated with that item for the average bail hearing. For example, the fiscal cost of jail corresponds to the reduction in the expected number of pretrial detention days that occurs (on average) for a single bail hearing when a public defender is provided, multiplied by the daily cost of incarcerating an individual. As the literature varies substantially with respect to proposed values of liberty, we present both low-end and high-end estimates from the literature. Details on how these costs and benefits were calculated are provided in the next section.

The bottom table of Figure SF3 presents several different constructions of the cost-benefit analysis, which vary according to which components are accounted for. In the first two rows of the table, we limit ourselves to the perspective of governmental agencies and perform a fiscal analysis that examines the impact of public defenders on cost outlays. The first estimate only considers the immediate impacts, with the cost of the public defender weighed against the averted incarceration costs and the avoidance of a bail review hearing. The second estimate includes the downstream impacts of the increased costs incurred due to rearrests. The last two rows of the table add in the costs of these additional crimes to society, as well as the value to defendants of avoiding incarceration, to provide a societal perspective on the impacts of this intervention.

The perceived cost effectiveness of this intervention is highly dependent on what outcomes are accounted for and how they are valued. If only the immediate fiscal impacts are considered, the provision of public defenders at bail hearings saves the county \$29.09 per bail hearing. This implies that over the roughly 15,000 bail hearings conducted annually at PMC, the county is saving \$436,350. However, when downstream fiscal impacts are added in, this impact turns negative due to the fiscal cost of a rearrest. Due to the wide range of estimates of the value of liberty to an individual, we cannot determine definitively whether this intervention has a positive or negative impact from a broader societal perspective. To provide more clarity on this we calculated the value of liberty that would be necessary for the program to run with zero net benefit—this analysis indicates that if society values the damage from incarcerating an individual for one day to be greater than \$504 (which is only 3% of the higher estimate of the societal cost of a day in jail), this program should be considered cost effective.

Components of the Cost-Benefit Analysis

Benefits (i.e., the averted costs)

Many of these estimates depend on the **estimated reduction in jail time per case**. This was calculated using the estimated impact on being in jail within 3, 7 and 14 days of the bail hearing, which can be calculated as the difference between the treatment and control values in Figure 2. Given that the impact is approaching zero after 2 weeks, we use the values in Figure 2 to estimate a simple mixture model. We assume there are two types of subjects – one for whom the public defender has an impact and the other for whom the public defender does not have an impact. Both types have constant hazard rates for release, which implies exponential distributions of jail spells. Using non-linear least squares to fit the mixture model to the jail duration probabilities, we estimate that the proportion of type 1 subjects is 0.607. Type one subjects in the treatment group have a daily jail hazard rate of 0.953 and an expected jail duration of 1.049 days. Type one subjects in the control group have a daily jail hazard rate of 0.657 and an expected jail duration of 1.523 days. The type 2 subjects make up 0.393 of the population. Both treatment and control type 2 subjects have a daily jail hazard rate of 0.0193 and an expected jail duration of 51.723 days. Therefore, the expected impact of treatment on jail duration is the estimated difference in expected jail time for type one subjects (1.049 minus 1.523 = - 0.474 days) times the proportion of the population who are type one: $-0.474 \times 0.607 = -0.287$ days.

Fiscal Cost of Jail: \$29.59 per case. The reduction in jail costs is calculated based on the daily cost of an inmate, provided by Allegheny County, of \$103, multiplied by the estimated reduction in jail time per case of 0.287 days.

Fiscal Cost of Bail Review Hearing per case: \$20.80. The reduction in bail hearing costs is calculated from our estimated impact on receiving a monetary bail, -0.104, multiplied by an estimated \$200 cost of a bail hearing.

Value of Liberty to Defendant – low-end estimate per case: \$10.68. Abrams & Rohlfs (1) estimate the value of liberty to a defendant as approximately \$30 per day in 2007 dollars, based on a defendant's willingness to pay bail. We inflate this to 2020 dollars and multiply by our 0.287 estimate of decreased days in jail per case.

Value of Liberty to Defendant – high-end estimate per case: \$4,596.81. Stevenson and Mayson (21) estimate the value of liberty in terms of survey respondents' willingness to be the victim of a crime rather than be incarcerated. The median respondent equates suffering a robbery to spending 3 days in jail. Cohen & Piquero (5) estimate the cost of a robbery as \$39,000 in 2007 dollars. We inflate the \$39,000 to 2020 dollars, divide by 3 and arrive at a value of liberty of approximately \$16,000 per day. Multiplying by 0.287 estimated impact on jail time, we find that the benefit of additional liberty is worth \$4,596.81 per case.

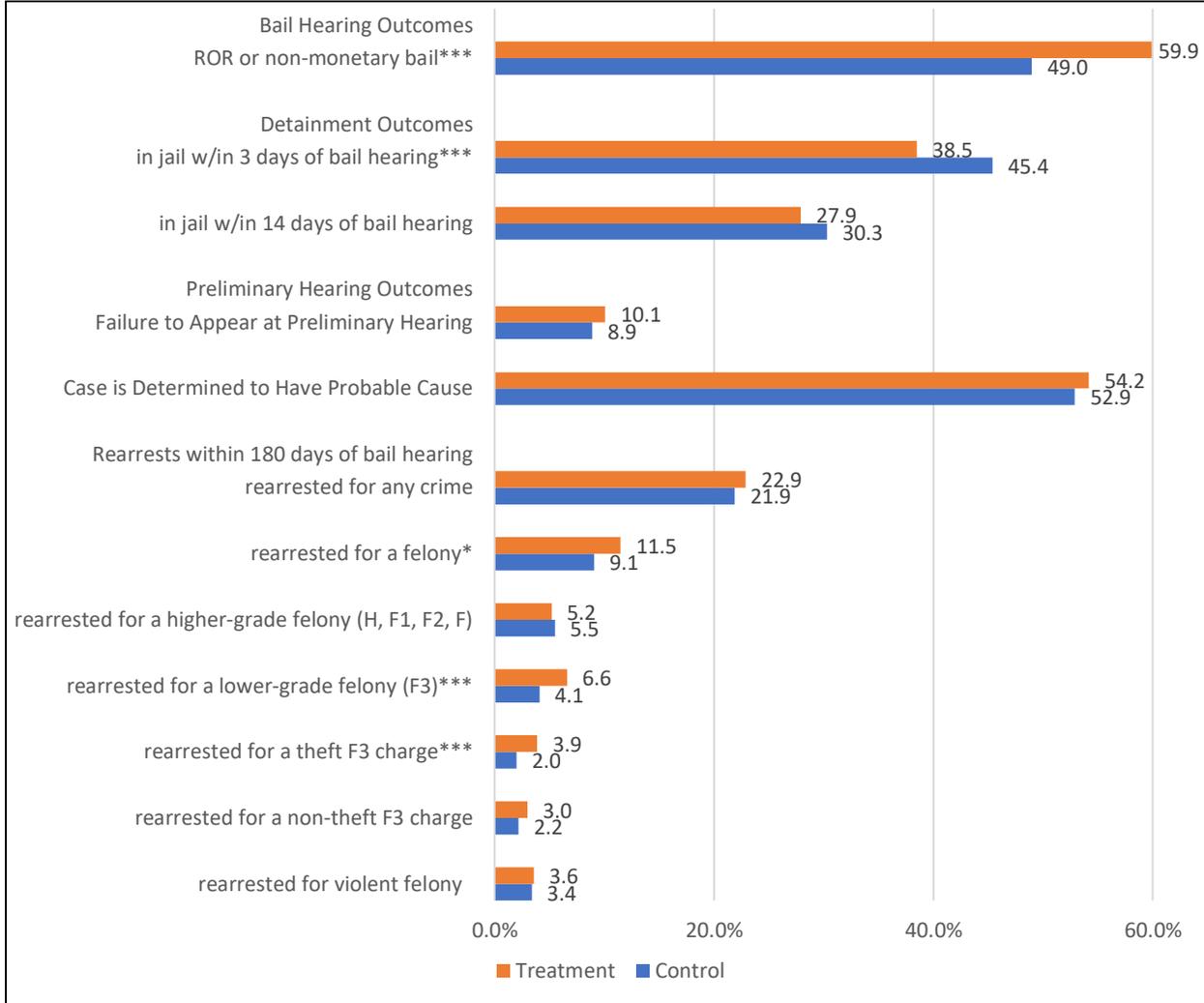
Costs

Fiscal Cost of Public Defender per case: \$21.30. Allegheny County says that the total compensation of a full time public defender is \$63,900 per year in 2021. We calculate that the average number of cases per 4 hour shift is 6. A full time defender works 10 shifts per week and 50 weeks per year, for an estimated cost of \$21.30 per case.

Fiscal Cost of Rearrest per case: \$73.78. We used Cohen and Piquero's (5) estimated criminal justice costs for a larceny offense, which indicated the cost for a theft rearrest of \$1,700 in 2007 dollars. We inflated to 2020 dollars and multiplied by the estimated impact on rearrest of 0.035 to obtain our estimated additional criminal justice costs of \$73.78 per case.

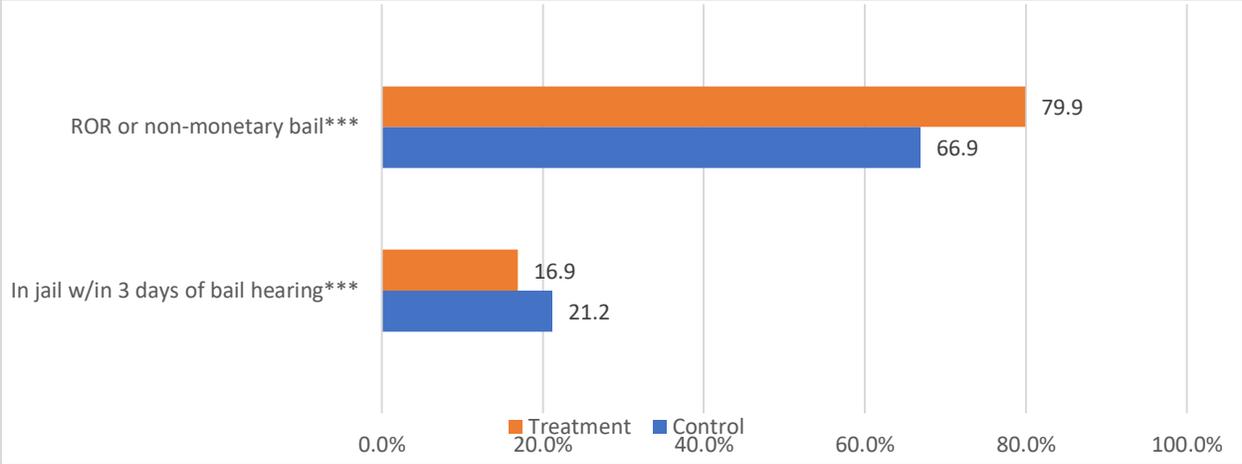
Social Cost of Rearrest per case: \$99.82. We used Cohen and Piquero's (5) estimated willingness to pay for a larceny arrest and subtracted off the estimated criminal justice costs, so as not to double count. This indicated a social cost for a theft rearrest of \$2,300 in 2007 dollars. We inflated to 2020 dollars and multiplied by the estimated impact on rearrest of 0.035 to obtain our estimated additional criminal justice costs of \$99.82 per case.

Figure SF1: Robustness of Main Results to Not Including Covariate Controls



Note: ***, **, * indicate the difference between the treatment and control group is statistically significant at the 1%, 5%, and 10% level, respectively. Standard errors were clustered by shift time and date; no covariate controls were included. Refer to Figures 2 and 3 for the sample sizes for each of the outcomes presented.

Figure SF2: Results from Specification Preregistered on Open Science Framework



Note: ***, **, * indicate the difference between the treatment and control group is statistically significant at the 1%, 5%, and 10% level, respectively. The treatment-control comparisons are OLS regression-adjusted using the same specification as described in Figure 2. Specifications presented here only include individuals that had no pending charges and were not currently incarcerated or on community supervision. For individuals that appeared in the sample multiple times, only their first bail hearing was included. The resulting sample included 1905 cases.

Figure SF3: Monetary Cost-Benefit Analysis of Providing Public Defenders at Bail Hearings

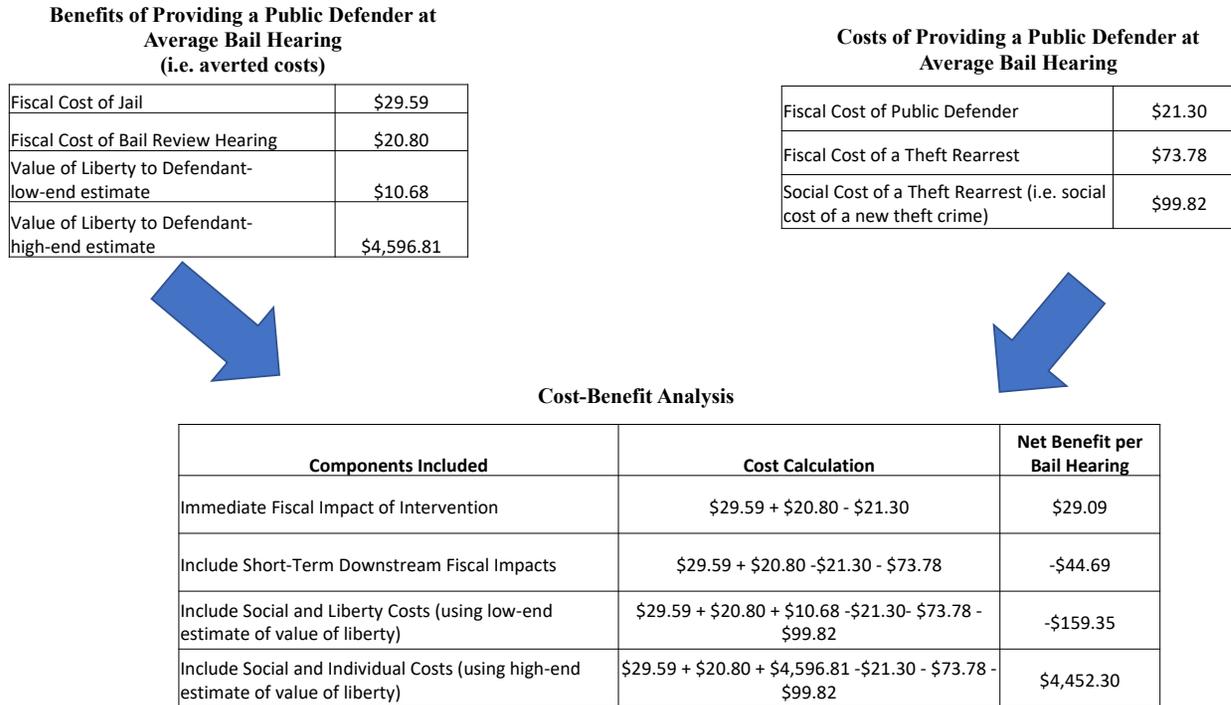


Table ST1: Percentage of Treatment and Control Shifts Each Judge Works

Judge Number	Overall Mean	Overall Std. Deviation	Treatment Mean	Control Mean	T/C Difference as a Percent of SD
1	0.012	0.109	0.009	0.015	5.5%
2	0.038	0.190	0.045	0.030	7.9%
3	0.014	0.116	0.015	0.012	2.6%
4	0.011	0.102	0.009	0.012	2.9%
5	0.012	0.109	0.012	0.012	0.0%
6	0.066	0.249	0.083	0.049	13.7%
7	0.006	0.077	0.009	0.003	7.8%
8	0.017	0.128	0.012	0.021	7.0%
9	0.005	0.067	0.000	0.009	13.4%
10	0.086	0.280	0.059	0.113	19.3%
11	0.009	0.095	0.009	0.009	0.0%
12	0.075	0.264	0.045	0.107	23.5%
13	0.020	0.139	0.024	0.015	6.5%
14	0.074	0.261	0.101	0.046	21.1%
15	0.017	0.128	0.024	0.009	11.7%
16	0.002	0.039	0.003	0.000	7.7%
17	0.014	0.116	0.009	0.018	7.8%
18	0.008	0.086	0.009	0.006	3.5%
19	0.017	0.128	0.009	0.024	11.7%
20	0.018	0.133	0.015	0.021	4.5%
21	0.033	0.179	0.036	0.030	3.4%
22	0.015	0.122	0.021	0.009	9.8%
23	0.002	0.039	0.003	0.000	7.7%
24	0.015	0.122	0.009	0.021	9.8%
25	0.015	0.122	0.024	0.006	14.8%
26	0.008	0.086	0.012	0.003	10.5%
27	0.014	0.116	0.012	0.015	2.6%
28	0.012	0.109	0.012	0.012	0.0%
29	0.005	0.067	0.009	0.000	13.4%
30	0.015	0.122	0.018	0.012	4.9%
31	0.012	0.109	0.012	0.012	0.0%
32	0.017	0.128	0.015	0.018	2.3%
33	0.008	0.086	0.006	0.009	3.5%
34	0.014	0.116	0.012	0.015	2.6%
35	0.011	0.102	0.015	0.006	8.8%
36	0.011	0.102	0.012	0.009	2.9%
37	0.005	0.067	0.003	0.006	4.5%
38	0.006	0.077	0.003	0.009	7.8%
39	0.012	0.109	0.012	0.012	0.0%
40	0.008	0.086	0.012	0.003	10.5%
41	0.005	0.067	0.003	0.006	4.5%
42	0.002	0.039	0.000	0.003	7.7%
43	0.008	0.086	0.009	0.006	3.5%
44	0.017	0.128	0.021	0.012	7.0%
45	0.012	0.109	0.012	0.012	0.0%
46	0.017	0.128	0.018	0.015	2.3%
47	0.024	0.153	0.015	0.034	12.4%
48	0.017	0.128	0.027	0.006	16.4%
49	0.018	0.133	0.015	0.021	4.5%
50	0.017	0.128	0.015	0.018	2.3%
51	0.012	0.109	0.003	0.021	16.5%
52	0.012	0.109	0.009	0.015	5.5%
53	0.078	0.269	0.080	0.076	1.5%
54	0.015	0.122	0.021	0.009	9.8%
Number of Shifts	665		337	328	

Table ST2: Public Defenders' Involvement with Treatment and Control Group Cases

	Treatment Group	Control Group
PD spoke w/judge and defendant; present at bail hearing	0.604	0.003
PD spoke w/judge and defendant; not present at bail hearing	0.045	0.047
PD spoke w/judge but not defendant; present at bail hearing	0.236	0.008
PD spoke w/judge but not defendant; not present at bail hearing	0.011	0.005
PD spoke with defendant but not judge; present at bail hearing	0.057	0
PD spoke with defendant but not judge; not present at bail hearing	0	0.001
PD did not speak with defendant or judge; present at bail hearing	0.022	0.010
PD had no involvement in case	0.025	0.926
Observations	2002	2089

Table ST3: Average Number of Cases in Each Shift Time for the Treatment and Control Groups

Shift Time	Total Number of Shifts	Average Number of Cases in Treatment Shift	Average Number of Cases in Control Shift	p-value
Sunday 4am-8am	43	5.410	6.050	0.374
Sunday 8am-12pm	43	6.320	6.480	0.838
Monday 12am-4am	39	7.650	8.050	0.682
Monday 4pm-8pm	44	2.780	3.570	0.195
Monday 8pm-Midnight	42	5.000	4.400	0.517
Tuesday 12am-4am	46	7.710	9.050	0.201
Tuesday 8pm-Midnight	48	4.240	4.260	0.978
Wednesday 12am-4am	42	6.270	8.650	0.030
Wednesday 8pm-Midnight	42	4.550	3.860	0.328
Thursday 12am-4am	46	8.140	8.380	0.832
Thursday 8pm-Midnight	44	4.380	5.170	0.342
Friday 12am-4am	42	6.950	8.090	0.316
Friday 4am-8am	42	11.000	8.590	0.065
Friday 8pm-Midnight	40	4.000	4.760	0.296
Saturday 12pm-4pm	41	5.200	3.900	0.145
Saturday 4pm-8pm	41	4.350	4.100	0.760

Table ST4: Heterogeneity Analysis

	Sample	Proportion Receiving ROR or Nonmonetary Release Among Control Group	Coefficient on Treatment for Receiving ROR or Nonmonetary Release	Proportion in Jail w/in 3 days of Bail Hearing Among Control Group	Coefficient on Treatment for in Jail w/in 3 days of Bail Hearing
Hold status	has a detention hold (n=903)	0.189	0.062 (.026)**	0.992	-0.002 (.016)
	has no detention hold (n=3188)	0.578	0.120 (.015)***	0.298	-0.061 (.015)***
	p-value for equality of treatment effects		0.051		0.007
Pretrial Risk Assessment Given no Holds	Pretrial Recommends ROR (n=335)	0.863	0.054 (.036)	0.089	-0.007 (.030)
	Pretrial Recommends Nonmonetary Release (n=2267)	0.673	0.138 (.018)***	0.210	-0.053 (.017)***
	Pretrial Recommends No Release (n=585)	0.059	0.077 (.027)***	0.747	-0.083 (.040)**
	p-value for equality of treatment effects		0.038		0.254
Charge Severity Given no Holds	Felony Charge (n=1356)	0.472	0.135 (.023)***	0.348	-0.067 (.023)***
	Misdemeanor Charge (n=1632)	0.670	0.113 (.021)***	0.257	-0.049 (.019)**
	p-value for equality of treatment effects		0.463		0.539
Charge Type Given no Holds	Person Charge (n=1292)	0.631	0.090 (.023)***	0.248	-0.030 (.022)
	No Person Charge (n=1896)	0.541	0.150 (.019)***	0.333	-0.080 (.019)***
	p-value for equality of treatment effects		0.045		0.066
Gender Given no Holds	Female (n=955)	0.669	0.129 (.028)***	0.245	-0.054 (.027)**
	Male (n=2227)	0.535	0.125 (.018)***	0.323	-0.062 (.017)***
	p-value for equality of treatment effects		0.896		0.788
Race Given no Holds	Black (n=1764)	0.535	0.145 (.020)***	0.296	-0.055 (.020)***
	White (n=1361)	0.629	0.099 (.022)***	0.308	-0.068 (.022)***
	p-value for equality of treatment effects		0.118		0.646
Age Given no Holds	Age ≤ 30 (n=1375)	0.618	0.107 (.022)***	0.227	-0.019 (.021)
	Age > 30 (n=1813)	0.546	0.141 (.020)***	0.353	-0.092 (.020)***
	p-value for equality of treatment effects		0.222		0.009

Note: The treatment results for the subsamples were obtained by regressing the relevant outcome on a treatment variable that was interacted with each subsample category, and the controls described in the notes to Figure 2. Each subsample stratification corresponds to a separate regression. Each regression uses the full sample of 4091 bail hearings. ***, **, * indicate whether the treatment effect for the associated subsample is statistically different than 0 at the 1%, 5%, and 10% level, respectively. The p-value at the bottom of each panel indicates whether the treatment effects are different across the stratification groups. A “Hold” indicates the individual arrested had a detainer issued for a separate violation, and thus must remain in jail regardless of what happened in the focal bail hearing.