SOCIAL SCIENCES

The impact of defense counsel at bail hearings

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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 Allegheny County, 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 8.5 times as costly as a day in detention for jurisdictions to find this tradeoff undesirable.

INTRODUCTION

At the first court appearance after an arrest in the United States, 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 that a person must pay to be released from detention (*1*). 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 (*2*–*7*).

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 United States do not provide defense counsel at this stage (*8*).

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 (*8*). In particular, the reality of these hearings, which, in many large jurisdictions, are assembly line style hearings usually lasting less than 3 min and conducted via video feed, has bred some skepticism about the potential of attorneys to affect the outcome (*9*). Understanding the extent to which providing defense counsel at the bail hearing can affect the use of monetary bail and pretrial detention will thus provide policymakers with the necessary information on the effectiveness of this intervention.

Despite the importance of this issue, there is unexpectedly 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 (*10*, *11*). The third is a study examining a policy change, comparing outcomes after the change to those before, without a comparison group (*12*, *13*). 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 of these studies are able to identify the causal impact of providing 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), which holds the majority of the bail hearings that occur within Allegheny County. 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 6 months of the bail hearing. For jurisdictions facing similar tradeoffs, whether this tradeoff is acceptable will depend on the factors that they consider. For jurisdictions whose primary concern about providing defense representation at this stage is ensuring defense counsel actually affect the proceedings, these results provide clear

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evidence of the benefit of this intervention. For jurisdictions concerned about the additional criminal activity arising from this intervention, our analysis indicates that 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 8.5 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 that this tradeoff should be acceptable for many individuals (*15*). However, the distribution of individuals with outlying views and the workings of political and bureaucratic processes will determine whether this tradeoff is accepted in any given jurisdiction.

BACKGROUND

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 normal court business hours, then their initial bail hearing takes place in the PMC. 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 (which is similar to the Public Safety Assessment tool that is commonly used in many jurisdictions) 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 that they will commit new criminal activity during the pretrial period. The risk assessment algorithm recommends either unconditional pretrial release, release with nonmonetary conditions, or no release. Although monetary bail is never recommended, judges set a monetary bail roughly half the time. Judges examine the risk assessment paperwork and make their bail hearing decision before the bail hearing, without talking to the defendant. During the hearing, the judge typically will just read their final decision to the defendant, who is in the jail and appears via video in the courtroom. While it is technically possible for the judge to change their decision at the bail hearing (and redo the paperwork), our court observation indicates that, in practice, this rarely happens. Judges can elect to either release the defendant with no conditions (ROR), release them with nonmonetary 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 on the basis of 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 (before 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 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. Furthermore, 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 nonmonetary conditions. Note that while these are some potential mechanisms through which the public defender can affect the outcomes of bail hearings, our study will not be able to definitively determine the exact mechanism responsible.

MATERIALS AND METHODS

In April 2017, Allegheny County began providing public defenders for all bail hearings at PMC during regular business hours (Monday through Friday from 8 a.m. to 4 p.m.). 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 (*16*). 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 nonbusiness 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 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 2 weeks, then alternated to the Pay Period 2 schedule for 2 weeks, then back to the Pay Period 1 schedule for 2 weeks, and so forth for the duration of the study. The study was in the field between 1 April 2019 and 13 March 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. The study received approval from RAND's Human Subjects Protection Committee, and all the guidelines were adhered to. We were not required to obtain informed consent because our study had minimal impact on defendants, as the intervention was going to happen anyways and our study did not alter the average probability of a defendant having a public defender at their bail hearing.

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 4 a.m. and 8 a.m. 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 orange cells in Fig. 1. The bail hearings that occur in the orange cells correspond to the treatment group, and the bail hearings that occur in the blue cells correspond to the control group. While our research design

Pay Period 1 schedule

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
12 midnight–4 a.m.		PD	PD	PD			
4 a.m.–8 a.m.	PD	PD	PD	PD	PD		
8 a.m.–12 noon	PD	PD	PD	PD	PD	PD	
12 noon–4 p.m.		PD	PD	PD	PD	PD	
4 p.m.–8 p.m.		PD	PD	PD	PD	PD	
8 p.m.–12 midnight		PD	PD				

Pay Period 2 schedule

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
12 midnight–4 a.m.					PD	PD	
4 a.m.–8 a.m.		PD	PD	PD	PD	PD	
8 a.m.–12 noon		PD	PD	PD	PD	PD	
12 noon–4 p.m.		PD	PD	PD	PD	PD	PD
4 p.m.–8 p.m.			PD	PD	PD	PD	PD
8 p.m.–12 midnight				PD	PD	PD	

Fig. 1. Public defender shift schedule. The schedule alternates back and forth between these two shift schedules every 2 weeks. The orange shifts represent the treatment shifts, and the blue shifts represent the control shifts.

will only allow us to estimate the impact of public defenders for bail hearings that occur outside business hours, these off-hour hearings compose about 63% of all bail hearings in Allegheny County.

Allegheny County provided data on all bail hearings that occurred between 1 April 2019 and 13 March 2020. For each hearing, we observe information on the date and time that the bail hearing took place, the outcome, 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 detail 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 that they staffed, which allowed us to identify which of the bail hearings actually had a public defender. More details on the construction of the data are provided in the Supplementary Materials. 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 with respect to the key defendant and case covariates. For completeness, tables S1 and S2 examine balance for the full set of relevant covariates, which include judge indicators, month indicators, shift indicators, and a more detailed version of the key defendant and case covariates presented in Table 1. In particular, table S2 breaks out many of the core variables presented in Table 1 into multiple categories, which reflects how these variables factor into the pretrial risk assessment. To test for balance, we used two complementary approaches. First, we examined whether the treatment means were statistically different from the control means after accounting for shift and month controls. We account for shift (which reflects both the day of the week and the specific 4-hour time block) and month controls because we assume that after conditioning on these timing factors, defendants arrive randomly to shifts with and without an assigned public defender. Column 5 of Table 1 presents P values from t tests that compare the treatment and control means for each of the key covariates. In addition, we regressed a treatment indicator on the full set of 128 covariates presented in tables S1 and S2 and conducted an F test to determine the level at which the covariates were jointly significant. While only four of the differences in covariate means shown in Table 1 are statistically significant at the 0.05 level, the P value for the F test of joint significance was 0.000.

Because the results above indicate that at least some covariate means vary by treatment status, our second approach to checking for balance follows Imbens and Rubin (*17*), who note that good balance does not necessarily require that there be no statistically

Table 1. Covariate balance between treatment and control groups.

	Overall mean	Overall SD	Treatment mean*	Control mean*	*P* value from *t* test comparing T and C means*	T/C difference as a percent of SD*
Defendant demographics						
Age (years)	35.0	12.0	35.0	35.1	0.761	1.0
Black	0.561	0.496	0.561	0.562	0.931	0.3
White	0.422	0.494	0.421	0.421	0.980	0.1
Female	0.274	0.446	0.257	0.294	0.008	8.4
Criminal history						
Age at first arrest (years)	21.3	8.2	21.2	21.3	0.802	0.8
Number of prior arrests	10.4	11.4	10.3	10.5	0.510	2.1
Number of prior felony convictions	1.50	2.83	1.52	1.49	0.741	1.0
Number of prior misd. convictions	2.70	3.48	2.60	2.80	0.062	5.9
Number of FTAs	1.20	2.14	1.17	1.23	0.391	2.7
Case and defendant characteristics						
Lead charge is felony	0.438	0.496	0.449	0.430	0.233	3.7
Number of charges	3.58	2.89	3.55	3.60	0.605	1.6
Multiple incidents being handled	0.064	0.244	0.068	0.061	0.375	2.8
Person charge	0.370	0.483	0.367	0.370	0.829	0.7
Property charge	0.228	0.420	0.227	0.233	0.677	1.3
Drug charge	0.137	0.344	0.153	0.122	0.004	9.1
Weapon charge	0.036	0.186	0.040	0.034	0.320	3.1
Public order charge	0.130	0.336	0.118	0.139	0.043	6.4
Other pending charges	0.350	0.477	0.350	0.348	0.905	0.4
Currently on probation	0.297	0.457	0.281	0.312	0.034	6.7
Hold/detainer issued	0.221	0.415	0.214	0.225	0.407	2.6
Arrest within Pittsburgh	0.553	0.497	0.564	0.543	0.179	4.2
Risk assessment recommendation						
Pretrial recommendation of ROR	0.084	0.278	0.085	0.083	0.817	0.7
Pretrial recommendation of nonmonetary release	0.672	0.470	0.671	0.672	0.934	0.3
Pretrial recommendation of detention	0.243	0.429	0.244	0.244	0.980	0.1
Observations	4091		2002	2089		

*The treatment and control means, as well as the last two columns, are OLS regression–adjusted for shift and month controls. Each characteristic was regressed on a treatment indicator, as well as shift and month controls. The control mean represents the average value in the control group, and the treatment mean reflects the sum of the control mean and the coefficient on the treatment indicator in the regression. Each month of the intervention includes approximately two treatment instances and two control instances of each shift. Treatment assignment is effectively randomized if there is no systematic difference between being arrested during a treatment instance and a control instance of each shift during each month.

significant differences between the treatment and control means across all covariates. Rather, what is required is that the differences between the treatment and control covariate means are small enough that simple regression methods will be reliable for removing biases associated with the differences in covariates. Imbens and Rubin (*17*) note that for a simple regression methodology to estimate unbiased treatment effects, the difference between the treatment and control means for a given covariate should be smaller than 25% of the SD of the covariate. We thus use this criterion based on the standardized difference in covariate means, after accounting for shift and month controls, to examine balance. The last column of Table 1 shows that all of these covariate differences are well within the required bounds; tables S1 and S2 further show that each of the 128 covariates included in these tables are also within the required bounds. When checking for balance simultaneously across many covariates, Imbens and Rubin (*17*) recommend

calculating the Mahalanobis distance between the means of the treatment and control groups (which results in one number that summarizes how the treatment and control group compare with respect to the means of all of the covariates simultaneously). Using the full set of covariates listed in tables S1 and S2, we find an average scaled deviation of 0.0067, which is well below the 0.25 threshold. These results indicate that our experiment resulted in well-balanced treatment and control groups and that we should be able to identify unbiased treatment effects as long as we include covariate controls.

The Supplementary Materials provides further evidence of the validity of our experiment design. Table S4 provides evidence that the courts were not manipulating who was in the treatment and control groups. Table S3 indicates that there was extremely good compliance with the research design, such that public defenders worked the shifts that 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.

The analysis plan for this project was preregistered on Open Science Framework. The specifications used in this paper mirror the initial analysis plan closely, although we note in the Supplementary Materials exactly how the final specifications used differ from the preregistered specifications.

RESULTS

The impact of public defenders on bail hearings 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 our discussion in the previous section indicated that the covariate imbalances were not zero, all the treatment-control comparisons presented in Fig. 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 the full set of 128 covariates included in tables S1 and S2. The outcomes shown for the control group in Fig. 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. Figure S1 in the Supplementary Materials presents our main outcome results with no covariate controls included and shows that none of our main findings are appreciably changed by not including covariate controls.

The standard errors for our regression specification were clustered following the guidelines provided by Abadie *et al.* (*18*), which note that a cluster should be defined as a set of cases where the regression errors will be correlated with each other and where all cases received the same treatment status. As we are controlling for month, as well as the specific 4-hour shift block (which picks up both day of the week and time of day effects), we expect the main reason the remaining regression errors should be correlated is that the cases that happen in time periods that are close together are likely handled by either the same judge, the same public defender, or both. For example, within a 4-hour shift block, all bail hearing decisions are made by the same judge/public defender combination and all have the same treatment status; we thus must cluster by at least the shift time and date level. However, if there are adjacent shifts included in our sample where treatment status remains the same and either the same public defender or judge (or both) carry over, then those SEs could be correlated as well, and we thus group them into the same cluster. For example, on a given Monday, the 4 p.m. shift and the 8 p.m. shift are combined into one cluster (as the same judge and public defender staff both); the Tuesday 12 a.m. shift however falls into a separate cluster as both the judge and public defender change at 12 a.m. In this way, the 16 4-hour shifts included in our analysis each week are grouped into 12 clusters. As a sensitivity check, we also calculated *P* values using randomization inference and found similar results.

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 nonmonetary release only 49% of the time, those in the treatment group received this favorable outcome 59.2% of the time, which is a 21% 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 nonmonetary 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 that a monetary bail will be set. One potential reason public defenders may increase judge concurrence with the risk assessment tool is that their presence will likely require a judge that is deviating from the risk assessment to explain why. While sometimes judges might deviate from the risk assessment because of well-defined reasons, in situations where they do not have well-defined, legitimate reasons, they may decide to go along with the risk assessment when questioned about it by the public defender.

The results for detainment outcomes indicate that having a public defender at the initial bail hearing resulted in a decline in immediate pretrial detention after the bail hearing of 4.6 percentage points, which is a 10% decrease. 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 nonmonetary 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.

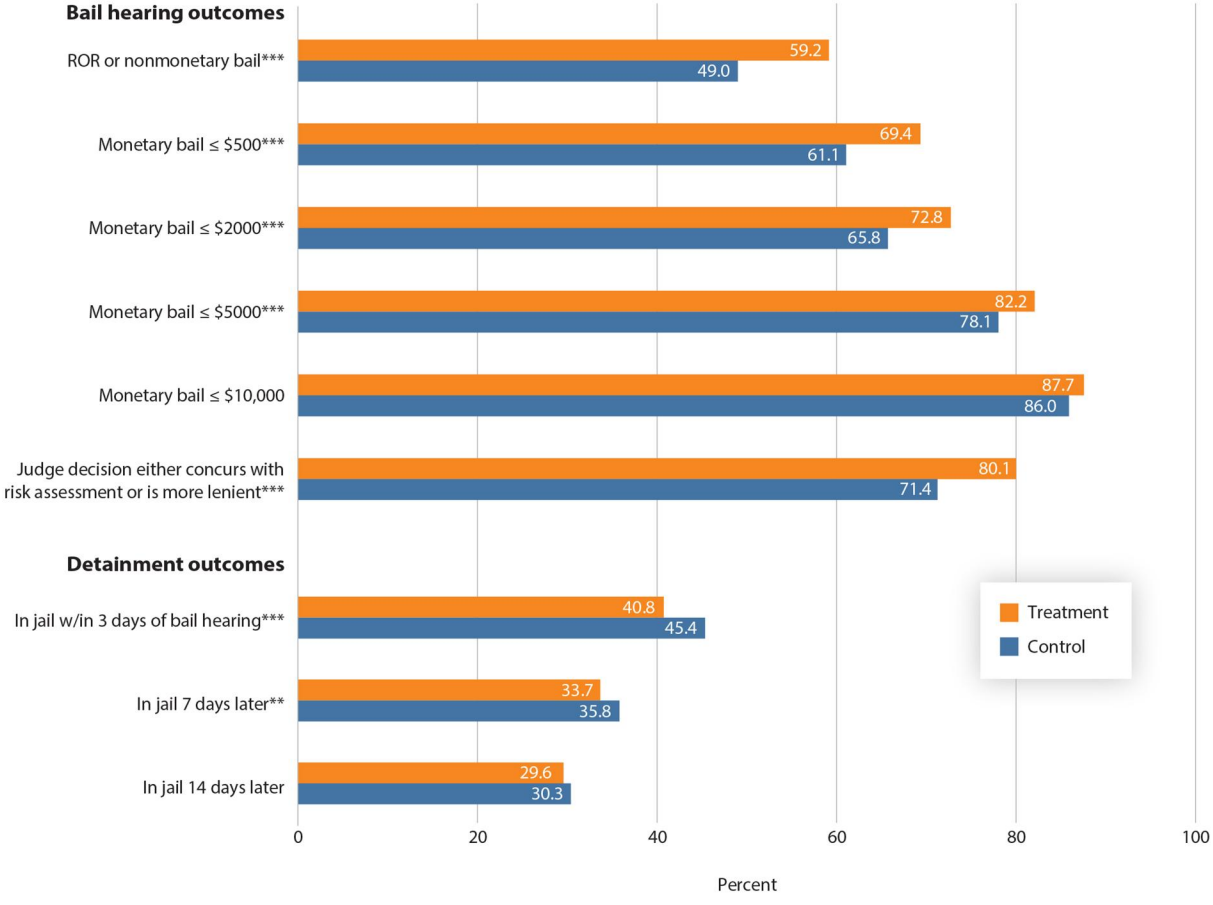

Fig. 2. Impact of public defender provision on bail hearing and pretrial detention outcomes. Asterisks ***, **, and * indicate that the difference between the treatment and control group is statistically significant at the 1, 5, and 10% level, respectively. The treatment-control comparisons are ordinary least squares (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 month controls, as well as indicators for the 16 different 4-hour shifts that composed the treatment and control groups. SEs were clustered by shift time and date; shifts that were adjacent, which shared the same treatment status and either the same public defender or judge, were grouped into the same cluster (see Results for more details). With the exception of the 7- and 14-day later detainment outcomes, all comparisons use the sample of 4091 bail hearings that occurred between 1 April 2019 and 13 March 2020. The 7- and 14-day later detainment outcomes truncate 1 and 2 weeks from the sample, respectively, so that the detainment outcome can be measured before the onset of the pandemic.

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 that they cannot pay and for whom the pretrial risk assessment recommended release. At these review hearings, which typically happen within 3 days of the initial bail hearing, public defenders are 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 Supplementary Materials. Table S5 indicates that the observed reduction in pretrial detention only occurred among individuals charged with a nonviolent offense. We also estimate a significantly larger impact on receiving ROR or nonmonetary release for individuals charged with a nonviolent offense versus those charged with a violent offense. Both of these findings imply that judges might have been more open to listening to the public defender's recommendation for individuals charged with nonviolent offenses. The treatment effects do not appear to vary by the defendant's gender or race at a statistically significant level, but the treatment did have a larger negative impact on pretrial detention for defendants older than 30 than for younger defendants.

The impact of public defenders on downstream defendant outcomes

As noted in the Introduction, prior research has demonstrated that interventions that affect 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 that 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 3 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, at the onset of the coronavirus disease 2019 (COVID-19) pandemic in Allegheny County, we decided that we were not going to use data on any outcomes that occurred after 13 March 2020; we thus stopped requesting data from our partners at this time. After this date, several changes were made to various criminal justice processes within Allegheny County that had the potential to significantly affect the outcomes examined here. In particular, court cases were substantially delayed, and eventual outcomes were potentially affected, as there was likely a higher propensity to dismiss cases to reduce the backlog in the courts. Arrest activity around the county also declined once the pandemic began, and the county also made it a priority to release all individuals charged with a nonviolent offense who were in jail solely because they could not pay their monetary bail. Using data after the pandemic began would thus identify the impact that this intervention had on outcomes under the policies and trends present during the pandemic, which, while interesting, would not provide generalizable insights about the impact of a public defender at this stage. Because the intent of this study is to provide an estimate of the impact of this intervention in prepandemic times, our analysis necessarily focuses on short-term outcomes, as these are unaffected by the pandemic. We thus examine the impacts that 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 2- or 3-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 13 March 2020. For example, to measure whether individuals were rearrested within 180 days of their bail hearing, we can only use individuals that had their bail hearings on or before 15 September 2019 so that the entire 180-day follow-up period occurs before 13 March 2020.

Figure 3 shows the impact that public defender provision at the bail hearing had on downstream outcomes; the methodology used to obtain these results mirrors that used to obtain Fig. 2. The estimates indicate that the public defender intervention had no statistically significant impact on whether the defendant failed to appear at their preliminary hearing or on the outcome of the preliminary hearing. These results are expected given the impact of the intervention. With respect to failure to appear rates, these preliminary hearings typically do not take place until at least 2 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. 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 that we consider in Fig. 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. Note that this measure of rearrest does not include arrests for failures to appear in court on the initial charge, as those incidents were already examined in the failure to appear outcome. 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 their bail hearing. The remaining rearrest specifications examine which specific crime types 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. (While the significance levels for the rearrest outcomes in Fig. 3 are not corrected for multiple hypothesis testing, we obtain similar results when we apply the conservative Bonferroni test.) Notably, the results indicate that 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 (which make up 55% of third-degree felony rearrests). 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, 5.4% of those in the treatment group were. 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.

The rearrest results suggest that reductions in monetary bail and pretrial detention (which are the main ways that the intervention affected individuals) led to an increase in rearrests for lower-grade theft charges. There are several potential reasons why this might have happened. While incapacitation (whereby those in jail are physically prevented from reoffending) is often put forth as an explanation for why reductions in pretrial detention can lead to increases in rearrest rates, our results are not consistent with an incapacitation effect. The decrease in pretrial detention caused by the intervention was not large enough to incapacitate individuals from reoffending over a 180-day time frame. In the Supplementary Materials, we show that our findings imply that the treatment causes an average decrease in detention of 0.29 days, which is a very small change in incapacitation relative to the 180-day time frame. This average reduction in detention ignores the possibility that some people see no impact on their detention and others have a larger impact. Using the findings at the bottom of Fig. 2, we see that 55% of the control group did not go to jail (i.e., were not in jail within 3 days of their bail hearing) and 30% were still in jail after 14 days. If we assume that all the impact on detention was on the remaining 15% who were in jail between 1 and 14 days, then this subset would have experienced a 1.9-day decrease in detention days (as 0.29/0.15 = 1.9). We think that it remains unlikely that having the opportunity to offend for roughly two extra days over

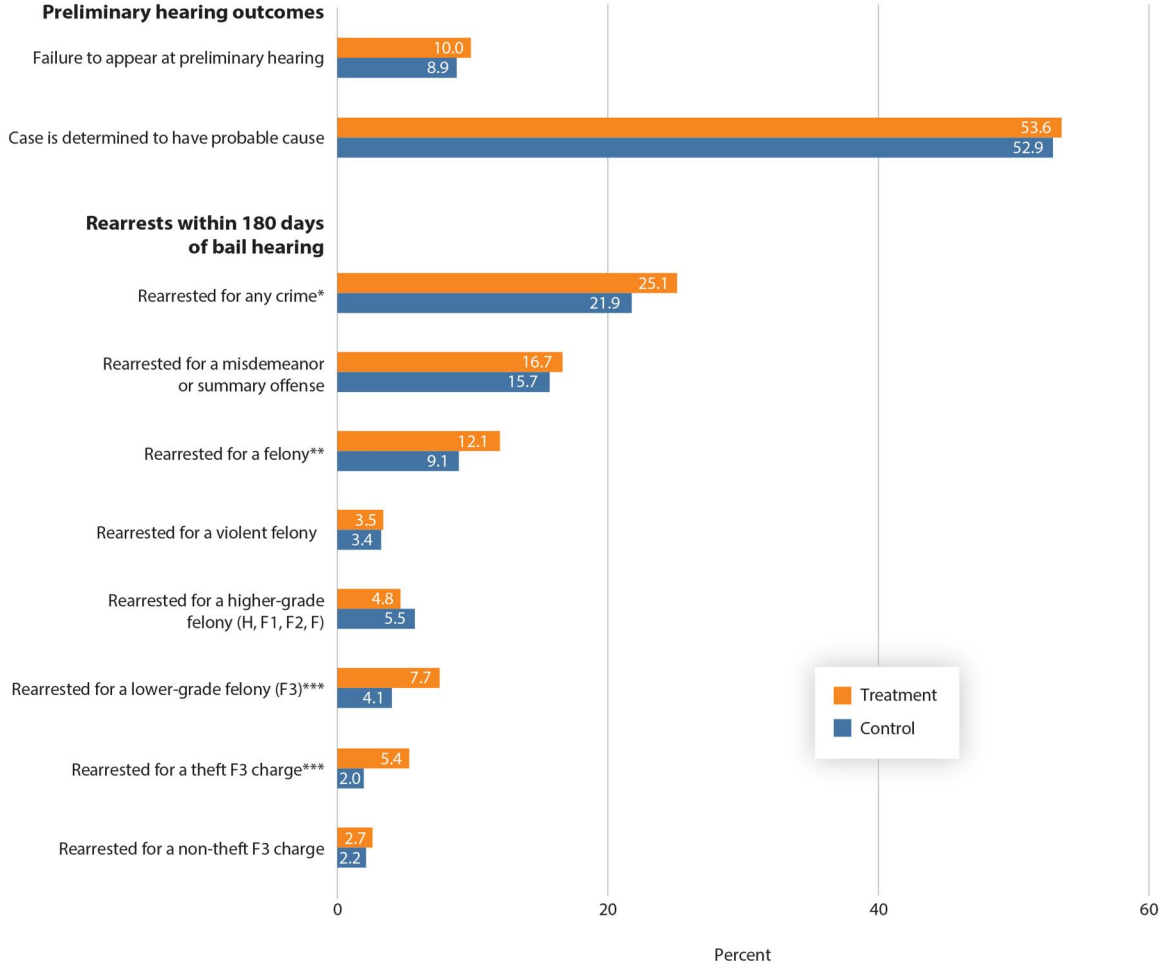

Fig. 3. Impact of public defender provision on downstream outcomes. Asterisks ***, **, and * indicate that the difference between the treatment and control group is statistically significant at the 1, 5, and 10% level, respectively. The treatment-control comparisons are OLS regression–adjusted using the same specification as described in Fig. 2. 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 1 April 2019 and 30 November 2019 (n = 2993); the probable cause determination dropped 261 additional observations that had not had their preliminary hearing as of 13 March 2020. For the rearrest within 180-day outcome, we use the 2167 bail hearings that occurred between 1 April 2019 and 15 September 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.

the course of 6 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 estimated treatment effect on rearrest presented in Fig. 3 remains virtually unchanged when we add 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.

Although the relatively small average reduction in jail time suggests that an increase in overall incapacitation is not driving the increase in minor felony rearrests, it is possible that the public defender's presence leads to "selective incapacitation," whereby more defendants at risk for these minor felonies are being released while awaiting trial but fewer other defendants are being released. Such reallocation of pretrial confinement could account for increased minor felony rearrests without changing the average number of days of pretrial confinement in the population much. We do not have any reason to think that the public defender's presence would increase confinement of some other group of defendants who are not at risk of reoffending nor do we find evidence of reductions in other types of rearrests, but we raise this as a possibility for consideration.

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. Note that Allegheny County does not require individuals to forfeit their bail if they reoffend during the

pretrial period, and thus, the monetary bail itself should not directly incentivize individuals to avoid offending during the pretrial period (although the experience of having to pay the monetary bail might).

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 S5 indicate that the public defender intervention only reduced the likelihood of detention for those who had a nonviolent arrest charge. This group was much more likely to have their focal arrest charge classified as a theft charge, implying that their future rearrests might fall in this category as well. An alternative possibility is that the experience of paying a monetary bail deters individuals specifically from committing financial crimes. For example, the benefit to an individual of committing a financial crime should decrease if they feel that they may have to pay a monetary bail.

Last, 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 2- or 3-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 (*19*). 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 (*3*, *5*). Future work should thus evaluate the impact of this intervention on rearrest rates over a longer time window.

The tradeoff between pretrial detention and rearrests

Our results indicate that, in this setting, 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 here 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 (*20*), 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, with wide variation in estimates of the benefit of staying out of jail, the results can be more difficult to interpret. 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 that the intervention presents is worth it. Instead, we follow a cost-benefit approach developed by Stevenson and Mayson (*15*), which involves directly comparing the number of pretrial detention days avoided with the number of additional crimes committed. Results from a traditional monetary cost-benefit analysis are presented in the Supplementary Materials and indicate that if society values the damage from incarcerating an individual for 1 day to be greater than $488 (which is only 3% of the higher estimate of the societal cost of a day in jail), then this intervention should be considered cost-effective.

The results from Figs. 2 and 3 indicate that the average treatment group member served 0.29 less days of detention and committed 0.034 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 8.5 times more than the cost to society of a day in detention (0.29/0.034 = 8.5). Put another way, for this tradeoff to be bad, individuals would have to be willing to spend at least 8.5 days in jail to avoid being the victim of a third-degree felony theft crime. Stevenson and Mayson (*15*) surveyed individuals in the general population and found that the median respondent would only be willing to spend 1 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. We provide Stevenson and Mayson's (*15*) valuation of the tradeoff between incarceration and burglary merely as a point of reference as individuals, policy-makers, and jurisdictions will have their own valuations of this tradeoff. For simplicity, our analyses of these tradeoffs only use the point estimates from our empirical analysis and are not accounting for the uncertainty in these estimates.

The analysis conducted in this section is constrained to considering the short-term tradeoffs. As noted earlier, 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.

DISCUSSION

This paper presents experimental evidence that providing public defenders at bail hearings increased the probability of receiving an ROR or nonmonetary release at bail hearings by 21%, reduced

the probability an individual was in jail 3 days after their bail hearing by 10%, and had no impact on failure to appear rates or the probable cause determination at the preliminary hearing. This evidence is important for constitutional arguments about whether bail hearings should be considered a critical stage requiring a lawyer (*14*). Furthermore, 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 (*21*). The results that we find in Allegheny County 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 8.5 times more than the cost to society of a day in detention. Current survey estimates of how individuals value these costs indicate that this tradeoff should be acceptable for most individuals. Note that, because of 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.

Last, note that there are many aspects regarding how bail hearings are conducted that vary by jurisdiction. The bail hearing process in Allegheny County does not include prosecutors, does provide judges the use of a risk assessment score, typically does not involve the judge basing their decision on information that they learn from the defendant during the bail hearing, and allows for bail review hearings within a few days of the initial hearing. While none of these components are unique to Allegheny County, there are likely to be many other jurisdictions that have a bail hearing process that differs in important ways than the one we study here. More research in this area is needed to understand the extent to which the results that we find here are generalizable to other jurisdictions with different process components. For example, the fact that bail review hearings (during which a public defender is always present) occur within 3 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 affect case outcomes and rearrest outcomes in different and more substantial ways.

Supplementary Materials

This PDF file includes:

Supplementary Materials and Methods
Figs. S1 to S3
Tables S1 to S5
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Acknowledgments: We would like to thank the following agencies in Allegheny County for invaluable support throughout this project: the Public Defender's Office, Department of Human Services, Pretrial Services, and the Criminal Courts. We would also like to thank G. Baker, S. Lanna, A. Nugroho, and R. Lawrence for assisting with court observation. We are grateful to A. Ouss, A. Agan, several anonymous reviewers, and conference participants at the Convening on Research and Data in Indigent Defense, the Transatlantic Workshop on the Economics of Crime, and the NBER Summer Institute Crime Meetings for helpful comments and feedback. **Funding:** This research was supported by funding from Arnold Ventures. **Author contributions:** S.A., S.B., and J.E. jointly developed the study design. S.A. conducted the data analysis for all components of the study except the cost-benefit analysis. J.E. conducted the

cost-benefit analysis. All three authors discussed the results and jointly wrote the final manuscript. **Competing interests:** The authors declare that they have no competing interests. **Data and materials availability:** All data needed to evaluate the conclusions in the paper are present in the paper and/or Supplementary Materials. The Data Use Agreement (DUA), which was executed between RAND and the Allegheny County Department of Human Services, precludes us from making the individual-level data publicly available. Other researchers wishing to gain access to these data will need to execute a DUA with the Allegheny County Department of Human Services.

Submitted 12 August 2022
Accepted 4 April 2023
Published 5 May 2023
10.1126/sciadv.ade3909

ScienceAdvances

The impact of defense counsel at bail hearings

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Sci. Adv., **9** (18), eade3909.
DOI: 10.1126/sciadv.ade3909

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