

# The Effects of Brief Juvenile Detentions on Recidivism: Evidence from Low-Risk Youths

Diego Amador \*      Yu-Kuan Chen<sup>†</sup>

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## Abstract

Many youths accused of delinquent conduct are detained as their cases get processed by juvenile courts. In principle, detention should be reserved for youths who are deemed to be at risk of endangering themselves or others. Nevertheless, low-risk youth are also often detained for brief periods between the time of their arrest and their release to their families. Using a decade of detailed administrative data for all initial detention decisions made in one of the largest counties in the US, we find that these short-term detentions of low-risk youths lead to a sizeable increase in the likelihood of rearrest. We also find that these effects are concentrated on youths arrested for non-violent, less serious offenses and are unrelated to the actual amount of time spent in detention. To obtain our estimates, we implement the double/debiased machine learning estimator developed by Chernozhukov et al. (2018), which relies on selection on observables as the key assumption. Sensitivity tests show that our estimates are robust to plausible levels of violations of this assumption.

**Keywords:** Juvenile detention, criminal justice, youth, recidivism, sensitivity analysis

**JEL Classification:** K42, K14, J13

## 1 Introduction

Each year, juvenile courts in the United States handle more than 700,000 juvenile delinquency cases. Detaining some of these youths while their cases are processed is a common practice in

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<sup>†</sup>Department of Economics, Rice University.

juvenile justice systems across the country. Although the volume of detentions has steadily decreased in recent years—from 318,000 to 180,000 cases between 2009 and 2019—this reduction has essentially mirrored the generalized decrease in delinquency referrals during the same period (Hockenberry and Puzzanchera, 2021). Thus, the proportion of cases involving detention has essentially remained constant over the past 15 years. In 2019, 26% of disposed cases had involved some for pre-adjudicated detention.<sup>1</sup>

However, pre-adjudicated detention encompasses a wide range of experiences. While some youths spend months or even years waiting in detention as their cases proceed through the juvenile justice systems, others spend just a few days before being released to the custody of their families. Naturally, most of policy discussions about detention reform have focused on the procedures and decisions that determine formal and longer term detentions. Nevertheless, short-term detentions are common. In fact, many young people who are not formally detained may still spend very short periods of time in detention facilities between their arrests and an eventual release to their parents or guardians.

In this paper, we examine the impact of very short-term detentions on future justice involvement using detailed juvenile justice data from Harris County (Houston), the third largest county in the United States. Our data includes a wide array of records for the more than 40,000 youths who became involved with the juvenile justice system in Harris County between 2010 and 2020. For our analysis, we focus on low-risk youth without prior juvenile justice involvement who, according to existing policies and procedures, should not have been detained. Nevertheless, a significant fraction of them spent small periods of time - between a few hours and a few days - in detention following their arrest. Our results indicate that these short-term stays in detention increase the likelihood of recidivism. For youths in our sample, who were typically charged with misdemeanor offenses, spending any time in detention increases the likelihood that they are rearrested within 90 days by four percentage points (relative to a baseline rate of 7.1%).

To estimate the effect of detention, we implement the double/debiased machine learning lasso estimator developed by Chernozhukov et al. (2018), which assumes unconfoundedness or selection on observables. We show how discretionary decisions made by law enforcement at the time of arrest result in the temporary admission to detention facilities of a significant fraction of youths with low-level offenses and no additional risk factors. These youths were released within a few days without being formally detained, as their case characteristics did not warrant a detention. We argue that our key assumption is plausible in this context and

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<sup>1</sup>Throughout this paper, we use the term pre-adjudicated detention, and some times simply detention, to accurately reflect the characteristics of the juvenile justice system. The literature uses this term interchangeably with pretrial detention.

leverage the detailed information on youth available in the administrative data -including offense, demographic, and background characteristics- to compare youths who were briefly detained to youths who were not. Furthermore, we quantify the robustness of our estimates using the sensitivity analysis developed by Masten and Poirier (2018) and Masten et al. (2023), which allows us to gauge the degree of selection on unobservables that would invalidate our results.

Multiple mechanisms could explain the negative effects we observe. These include the trauma, stigma, and changes in self-perception of youth that result from the act of being detained itself; consequences of the time youths spend in the detention center, such as prolonged exposure to negative peers; and disruptions to other aspects of youths' lives, such as school attendance or family relations. Using a variety of empirical exercises, we show that the magnitude of the effect is not related to the amount of time spent in detention. We interpret these results as evidence that our findings stem from detrimental consequences of detentions themselves. Thus, stigma, trauma, and affectation to youths' perceptions of themselves appear to be the most likely mechanisms.

Research studying the causal effects of juvenile detention through convincing designs is scarce (Gilman et al., 2021). Two notable exceptions are Baron et al. (2022) and Walker and Herting (2020), both of whom rely on similar assumptions to the ones we use for our analysis. Using rich education and juvenile justice data for juveniles in Michigan, Baron et al. (2022) study the effects of pre-adjudicated juvenile detention on high school graduation and adult recidivism. They estimate that juvenile detention leads to a 31% decrease in high school graduation and a 25% increase in adult recidivism, with larger effects for felony offenses, relative to misdemeanor offenses. Walker and Herting (2020) use propensity score matching methods to analyze a large sample of youths from multiple locations. They also find negative effects of juvenile detention, estimating that being detained increases one-year felony recidivism by 33%.

The analysis in this paper expands and improves on the evidence provided by Walker and Herting (2020) and Baron et al. (2022). First, we focus on very short-term detentions. As we show in Section 2, short detentions are common events that nonetheless are not typically recorded as formal detentions. Furthermore, in the particular jurisdiction we analyze - but potentially in many others as well-, these detentions are not prescribed by official policy. Rather, they seem to stem from failures in the implementation of existing protocols or administrative delays in their implementation. Thus, their consequences may be overlooked by policymakers when designing formal detention policies. Additionally, and for the same reasons, the detentions we analyze affect a population that is not usually the target of pre-adjudicated juvenile detention. To the extent that detention may have different effects on

youth who are at lower risk of recidivism, our paper provides unique and novel evidence on a policy-relevant population. Finally, because the mechanisms by which detention can cause increased recidivism may depend on the length of the detention spell, analyzing very short term detentions indirectly provides evidence on the importance of specific mechanisms.

As mentioned, our estimates rely on unconfoundedness or conditional independence assumptions. This is also true for the estimates in Baron et al. (2022) and Walker and Herting (2020). However, there are important differences between our methodology and the ones used in those studies. Walker and Herting (2020) use simple propensity score matching methods and have access to a relatively smaller set of observable characteristics than we do. Baron et al. (2022) have much richer data and also apply a doubly-robust estimator (regression-adjusted inverse probability weighting) while guaranteeing exact matching on basic characteristics. We implement a double/debiased machine learning estimator (Chernozhukov et al., 2018), which allows us to better exploit the rich data we have access to. Furthermore, we explore the sensitivity of our results to the conditional independence/unconfoundedness assumption using procedures developed by Masten et al. (2023), which allows us to assess the sensitivity of our results to violations of unconfoundedness without relying on parametric functional form restrictions such as linearity.<sup>2</sup> Finally, our detailed detention records and the specific context of the detentions we analyze allow us to focus on a subpopulation of youths (low-risk youth with no prior contacts who should have not been detained) for whom unconfoundedness is more likely to hold.<sup>3</sup>

Other research has examined post-adjudicated juvenile incarceration and its effects on multiple outcomes, including recidivism. Aizer and Doyle (2015) found that incarceration of juveniles in Cook County (Chicago) led to lower high school graduation rates and increased adult recidivism. Their data did not allow them to distinguish detention from incarceration following adjudication, but they suggest that the bulk of cases in their data were comprised of incarceration as a sanction following an adjudication, typically lasting a couple of months. Earlier research by Hjalmarsson (2009) on residential programs in Washington state had found opposite results. Exploiting thresholds imposed by sentencing guidelines in a regression discontinuity design, Hjalmarsson (2009) estimates that being incarcerated decreases juvenile recidivism. These conflicting results suggest that the effect of incarceration may depend on the specific conditions of incarceration, as well as the characteristics of youth being incarcerated.

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<sup>2</sup>Oster (2019) is a popular method for assessing unconfoundedness in parametric linear models.

<sup>3</sup>Although not explicitly discussed in Baron et al. (2022), the comparison of samples in their paper suggests that the key characteristic leading to comparable sample of treated and untreated youths for their analysis comes from restricting their sample to petitioned youths. This reinforces the importance of our ability to focus on youths with very similar juvenile justice profiles and experiences.

However, pre-adjudicated detention and (post-adjudicated) placement or incarceration may differ substantially. In Harris County, in particular, pre-adjudicated detention facilities and residential facilities differ both physically and in terms of programming. Thus, to the extent that these conditions matter for recidivism outcomes, it is unclear how much the literature studying incarceration provides useful information regarding the effects of pre-adjudicated detention. Furthermore, many of the mechanisms by which incarceration may negatively or positively affect recidivism may not apply to short-term detentions like the ones we analyze here. For instance, one possible mechanism is the exposure of youths to negative peers. To the extent that this negative influence materializes over the course of multiple and extended interactions, short-term spells do not necessarily create the conditions for these to exert a strong influence on youth's subsequent behavior.

Finally, numerous studies have examined the consequences of pretrial detention in the adult criminal justice system. Leslie and Pope (2017), Heaton et al. (2017), Dobbie et al. (2018), and Stevenson (2018) all find that pretrial detention leads to an increase in convictions, typically through an increase in the likelihood of guilty pleas. Dobbie et al. (2018) find that pretrial detention has no effects on future crime, while Albright (2022) shows that a policy that reduced pretrial detention in Kentucky did not lead to an increase in pretrial arrests. Leslie and Pope (2017) show that, even if pretrial detention reduces rearrest while cases are being adjudicated (incapacitation effect), this reduction in crime is offset by later recidivism. Apart from this temporary reduction in crime due to incapacitation, the only benefit of pretrial detention in this literature comes from a reduction in non-appearances in court Albright (2022).

This literature finds mostly negative or null effects of pretrial detention of adults on a variety of outcomes. However, it is unclear to what extent, if any, these results extend to juveniles. For example, the evidence on the effects of adult pretrial detention on convictions is very strong. Nevertheless, the negotiation between defendants and prosecutors that results in guilty pleas - the process by which these effects occur - does not exist in the same way in the juvenile system. Similarly, although there is not as much evidence on specific mechanisms, many of the theoretical mechanisms linking pretrial detention to future crime (e.g. loss of employment) do not necessarily apply to juveniles. Overall, the strong evidence on the effects of pretrial detention in the adult system reinforces the need to study whether similar negative effects exist for minors.

## 2 Detention practices and their potential effects on recidivism

### 2.1 Trends in admissions to detention

Admissions to detention for juveniles are common, both nationally and in Harris County. In 2019, approximately 180,000 juvenile cases in the United States involved detention. Although the volume of detentions has steadily decreased over the last 10 or 15 years, this reduction has essentially mirrored the generalized decrease in delinquency referrals during the same period. In fact, the proportion of cases involving detention has remained stable over the last 15 years, with roughly one out of every four cases referred to juvenile courts in the United States resulting in detention (Hockenberry and Puzzanchera, 2021). Figure 1 shows that the share of referrals with admissions to detention (on the day of referral) in Harris County remained relatively stable for the most part of the same period, with the most recent years suggesting a declining trend in detention rates.

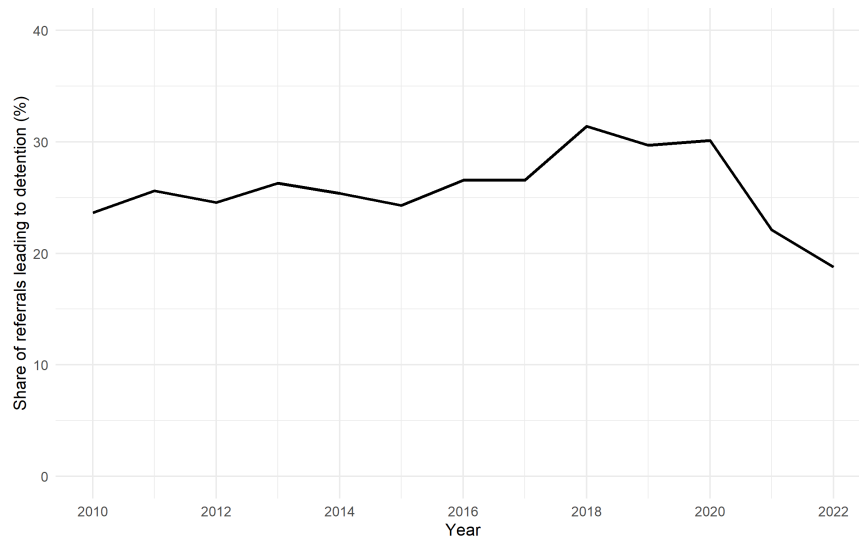


Figure 1: Share of referrals with admissions to detention by year.

As we explain in detail below, there are multiple decisions and decision-makers involved in the process that determines whether a youth is admitted to detention, as well as for how long he or she stays in detention. Thus, admissions to detention vary considerably in length. Whereas some youths are only held in detention for a couple of hours, others remain detained until their case is disposed, often for several months. Furthermore, although the majority of admissions to detention occur on the same day of the arrest (referral), youths may also be detained, prior to adjudication, at any time between the time of referral and the time of

disposition. For instance, a judge may order the detention of a youth prior to a hearing to ensure that he or she attends that hearing.

In this paper we focus exclusively on admissions to detention that begin at the time of referral, which constitute the typical detention case. We will also restrict our analysis to youths' first contact with the system. These choices allow us to focus on a uniform set of decisions, which apply to every youth who was ever involved with the juvenile justice system in Harris County. Furthermore, these decisions occur immediately after youths' first (formal) contact with the system, minimizing the amount of prior interaction that youths may have had with the system. These conditions make the assumptions underlying our identification strategy more likely to hold.

## **2.2 The process leading to detention in Harris County**

As mentioned, we focus on admissions occurring at the time of referral during a youth's first contact with the system. The first step in this process occurs right around the time of arrest. At this point, the law enforcement officer must determine whether to take (transfer) the youth to the Juvenile Detention Center (JDC) in downtown Houston. To make this decision, the officer may consult with the District Attorney's (DA) office and the Juvenile Probation Department (HCJPD). As described below, youth transferred to the JDC are assessed using a screening instrument. Thus, when deciding whether to transfer the youth to the JDC, the officer and other parties may conduct an informal screening to determine the likely outcome of the formal screening. Crucially, for the purpose of our analysis, these informal screenings are not recorded in the data.

Upon arrival to the JDC, HCJPD staff conduct a formal screening using the applicable screening instrument at the time. Throughout the period analyzed in this paper, HCJPD used a screening tool known as the Risk Assessment Instrument (RAI). In 2021, as part of multiple changes to detention practices, the RAI was replaced with a new screening instrument. Thus, to maintain consistency, our analysis concludes in 2020, before the implementation of these changes.

The screening instrument calculates a score based on a series of questions about the alleged offense(s) and information about prior referrals and other instances of involvement with the system. In our analysis, however, we focus only on a youth's first contact, where there is no applicable history of prior involvement. Thus, in the specific cases we analyze, the screening instrument produced a score based solely on characteristics of the current alleged offenses(s), namely the number of alleged offenses and their seriousness. The limited amount of information required to produce this score allows us to calculate a score for every youth,

even for those who were not actually screened at the time of referral, and use it in our econometric analysis.

To determine whether a youth should be admitted to detention, the scores are compared to a series of thresholds. Crucially, a score of 15 points or more leads to a recommendation of secure detention. However, admission protocols also gave the Juvenile Probation Officers (JPOs) some discretion, allowing them to override the recommendation based on other specific circumstances, such as when the offense involved significant violence towards the victim, or when there was no parent or guardian to assume responsibility for the youth. Finally, the screening instrument also specified situations that prompted a mandatory detention, such as an “Offense involving the use, exhibition or possession of a firearm”, regardless of the score.

In principle, youth should only have been admitted to detention when they scored above the 15-point threshold, when the characteristics of their offense or prior history triggered a mandatory detention, or when special circumstances prompted the JPO to override the “recommendation”. Exceptional circumstances, such as when parents or guardians are not immediately available to pick up the youth, may also prompt short admissions to detention. However, and crucially for our identification strategy, the data shows that youth were regularly admitted to detention despite not meeting any of the three stipulated conditions.

Once admitted, a youth cannot stay in detention for more than 48 hours – counted only during business days – without a detention hearing. In a detention hearing, a magistrate judge (or sometimes a District Court judge) reviews the case and evidence and determines whether the circumstances warrant a detention. If the magistrate decides that the youth should remain in detention, the youth is then considered to be formally detained. In this case, a new hearing must be scheduled at most 10 days later to determine whether the youth should remain detained or be released.

To characterize and summarize this process, we define five key outcomes, starting at the time of referral and ending at the (potential) first detention hearing. We do not observe in the data when a detention hearing occurs, only whether a youth is formally detained. Thus, we cannot distinguish cases where the magistrate ordered the release of a youth in a hearing from cases where the youth was released prior to a hearing. To approximate the occurrence of a hearing, we measure whether a youth stayed in detention for two business days (the maximum time without a hearing, as determined by law). Thus, the five outcomes are:

1. Screened (equivalent to being transferred to the JDC)
2. Admitted to detention
3. Admitted to and spent at least one night in detention



4. Admitted to and spent at least two business days (48 hours) in detention
5. Formally detained in hearing

Table 1: Detention outcomes by Risk Assessment Instrument (RAI) category (%)

RAI category <sup>1</sup>	Screened	Detention			
		admitted	1+ night	2+ business days	in hearing <sup>2</sup>
Very low	15.9	15.0	8.4	2.0	1.4
Low	35.0	32.5	21.3	5.7	3.5
Medium	54.6	33.5	25.5	11.9	9.4
High	91.3	83.8	79.7	65.5	41.5
Total	28.8	26.6	18.9	9.2	5.9

<sup>1</sup> RAI categories are based on calculated RAI scores for all youths. We use screening instrument guidelines and available data to calculate these scores.

<sup>2</sup> Only youths detained in hearing are considered formally detained.

Table 1 shows the share of all youth with each of these outcomes. We present rates separately by ranges of the calculated score of the screening instrument. The instrument specifies three categories - low, medium, and high risk - which correspond with release, conditional release, and detention recommendations, respectively. In our sample, the majority of youths fall into the low risk category, which we disaggregate further into low and very low scores.

Following a referral, 29% of youth in this sample were transferred to the JDC and formally screened. As expected, the rate at which youth were screened is strongly correlated with risk, as measured by the screening instrument. Screening rates range from 15% for those with very low scores to more than 90% for youth with high scores. This shows that the decision to transfer a youth to the JDC for screening is indeed based - though imperfectly - on the expected screening score. However, even among youth with low scores, for whom there should be little uncertainty regarding their expected score, more than a third were transferred for screening at the detention center.

Notably, the data also suggests that almost every youth who was screened was also admitted to detention. Many circumstances could explain why a screening would almost surely lead to an admission. For instance, if law enforcement officers, after conducting an informal screening, chose to only transfer youth for whom they expect that the formal screening will recommend detention, then it would be expected that most screenings result in admissions to detention. Nevertheless, as already mentioned and also implied by the screening rates for low-risk youth, the data does not support this explanation. In fact, only

32% of girls and 43% of boys who were screened during this period were recommended for detention based on the three criteria described earlier (score, mandatory detention, or override). Thus, the majority of admission decisions were inconsistent with the results of the screening procedure. Overall, these statistics show that many youths who were admitted to detention had been actually recommended for release.

Many of the admissions to detention were very short. These short stays likely reflect cases in which the youth needed to be formally processed after being transferred to the detention center because specific circumstances prevented their immediate release (such as parents not being immediately available). Nevertheless, the bulk of admissions still involved spending the night in the detention center. As shown in Table 1, 19% of all referred youths spent at least one night in detention (roughly 70% of youths admitted to detention). As the data shows, lower risk youth are more likely to be released on the same day of their admission to detention.

Most of the youths admitted to detention were released within a few days of their admission. Only 9% have stays long enough to require a detention hearing (two business days). These rates are, as expected, much lower for youth not in the high-risk category. Consistently, only a small fraction of youth admitted to detention were later formally detained, particularly among those for whom the screening instrument recommended a release (i.e. low and medium risk).

## 2.3 Short-term admissions of low-risk youth

As shown in Table 1, a large share of youths were admitted to detention despite having scores below the detention recommendation threshold. This group constitutes the key population in our analysis. These are youths with low-level offenses, for whom protocols did not recommend a detention. For those who were admitted, we will further restrict our sample to those who were not formally detained in a detention hearing.<sup>4</sup> Thus, for treated youths in our sample, either HCJPD staff or a magistrate judge concluded that there was no need or justification for the youth’s detention.<sup>5</sup> Consequently, detention stays in our treatment group should last only up to two business days. In practice, less than 1% of initial detention stays in our sample last more than four calendar days.

By restricting our analysis to this population, we are choosing to focus on youths for whom protocols stated that no detention should have occurred. The admission to detention, for the treated group, appears to reflect choices made by law enforcement and other parties at

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<sup>4</sup>As shown in Table 1, only a small share of youth in this group were formally detained

<sup>5</sup>Recall that our data does not allow us to distinguish youths who had a detention hearing but were not detained from youths who were released prior to a hearing.

the time of arrest. These choices were not supported by the screening procedure, nor by other decision-makers' review of the circumstances in the days that followed. We interpret their admission to detention as a consequence of a combination of incorrect application of policies, potential biases, and delays in the release process following and admission to detention.

## 2.4 The potential effects of short-term admissions

*Ex-ante*, these short-term admissions to detention have ambiguous effects on recidivism. Even among the group of low-risk youth that we analyze, this brief detention spell may serve as a deterrent for subsequent delinquent behavior, especially when compared to an alternative path without detention and relatively less severe dispositions, such as diversion or deferred adjudication. On the other hand, detention may have negative consequences on youth, which, in turn, may increase the likelihood of future delinquent behavior. The limited evidence from research estimating the causal effect of juvenile detention on recidivism (Baron et al. (2022), Walker and Herting (2020)) supports the latter.

Theoretically, multiple mechanisms may explain why detention may lead to increased recidivism. The experience of the detention process itself, from being arrested to being transported in a police car to the detention center, may itself have disruptive effects, either by leading to trauma or by affecting youths' self perception. This could be particularly important for youth for whom personal and contextual circumstances put them at risk of juvenile justice involvement. For instance, the prevalence of various mental health disorders among justice involved youth (Domalanta et al., 2003; Schubert et al., 2011; Schubert and Mulvey, 2014; Teplin et al., 2002), suggests that some youth who are detained may be particularly vulnerable to the events surrounding a detention.

Detention may also disrupt other important aspects of youths' lives. For example, youth with ongoing physical or mental health treatment may receive inadequate treatment while detained (Teplin et al., 2002, 2006; Shelton, 2005). Likewise, detention disrupts normal school attendance either by temporarily keeping youths out of school and/or by forcing them to switch out of their particular school to attend specialized school while in detention. This may be exacerbated upon release if schools implement formal or informal barriers to re-enrollment (for example, by using disciplinary punishment).

Being detained may not only affect a youth's self-perception, but also lead to changes in how others perceive and relate to them (stigma). A youth who has been detained may be labeled as problematic or criminal among school peers, educators, or community members. In turn, this may lead to changes in the kind of support they receive, how isolated they become, as well as the characteristics of people who they can interact with (e.g. become

more likely to establish ties with other youth who have been labeled as problematic).

Detention may expose youths to negative peers (Gifford-Smith et al. (2005); Dodge et al. (2006)). Compared to adults, adolescents have a less developed ability to regulate emotions and are particularly sensitive to external influences, including peer pressure (Council, 2013). Thus exposure to negative peer influences, such as other youth with more serious criminal involvement or anti-social behavior, may exacerbate detained youths' existing challenges and hinder their rehabilitation process. These interactions may reinforce antisocial behavior patterns, such as aggression, disobedience, and disrespect for authority figures.

Finally, stays in detention themselves may be traumatic experiences. Freedom is restricted in multiple ways. Detention facilities tend to be oppressive spaces which may lead to feelings of isolation. Communication with family and other people with whom the youth has relationships is limited, as is access to outside spaces. Abuse, either from other youth or staff, is possible and has been documented (Smith and Stroop (2019)).

However, the relative importance of each of these mechanisms may vary with the specific conditions of detention, including the length of the detention stay. For instance, one could expect the effect of exposure to negative peers to increase with the length of stay, as it likely requires the development of social ties with other youths within detention facilities. The extent of disruption to the educational process depends on the kind of barriers youths face when returning to school after being in detention. Trauma from family separation and loss of freedom may also be relatively less severe in short term stays. On the other hand, the trauma associated with the detention process itself is independent of the amount of time spent in detention. Likewise, stigma might also be less sensitive to the amount of time spent in detention.

## **3 Data**

### **3.1 Juvenile justice records**

Following an arrest, youth under the age of 17 are referred to the juvenile court. Juvenile referrals are then handled and processed by HCJPD, who oversees and operates most interactions between the youth and the juvenile justice system. The process starts at intake, when HCJPD creates a referral record in the county's Juvenile Information Management System (JIMS2). The record includes the youth's demographic information and it is updated as the case is processed. As the case moves forward through the system, additional records with disposition, placement, offense, and field supervision information are also created. These records can all be linked to a specific youth and referral.

The data was extracted and prepared for us by HCJPD’s Data and Research team from their internal systems and is organized at the referral level.<sup>6</sup> The data includes the above mentioned offense, disposition, detention, placement, and screening records. Our data set include all referrals up to December 31, 2022 for every youth whose first referral occurred between 2010 and 2022. However, as described in Section 2, a new screening instrument was introduced in February of 2021. Thus, in our analysis, we use data for referrals occurring between 2010 and 2020 only.

## Detention records

Our data includes specific records for any entry and exit into pre-adjudicated detention. A youth may have multiple detention records for the same referral, which may reflect different entries and exist from detention or administrative changes, including temporary exits (i.e. leave for a medical visit outside of the detention center). We organize detention records into *spells* based on dates, defining a detention spell as a series of consecutive dates with an active detention record. These include spells with entry and exit on the same date. For convenience, we refer to spells with entry and exit on different dates as spells in which the youth spent at least one night in detention.

As described in Section 2, our analysis focuses on detention spells that occur at time of referral (i.e. detention spells with an entry date that matches the referral date). This means that youth in our sample, both treated and untreated, may have subsequent detention spells for the same referral. In our main analysis sample, 8.4% of untreated youths have a later detention spell, while 17.1% of treated youths have more than one detention spell.<sup>7</sup> Our empirical strategy is guided by the details of the detention process at the time of referral that lead to short-term and unwarranted detentions for low-risk youth. Subsequent detention spells do not follow this process and may, in fact, be the result of recidivism, making them an outcome themselves (i.e. recidivism may prompt a detention that is recorded under the original/first referral). Thus, we do not use any information about subsequent spells to define our treatment variables.

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<sup>6</sup>Local juvenile probation departments in Texas, such as HCJPD, are required to submit monthly extracts of their referral case management data to the Texas Juvenile Justice Department (TJJD) using the Electronic Data Interchange (EDI) record specifications. The EDI specifications serve as the core structure around which our data is organized. HCJPD has also provided us with additional variables not usually included in the EDI transfers. These additional variables are still organized within the EDI structure, using the same identifiers.

<sup>7</sup>On average, the total number of days in detention for subsequent spells in the same referral is 8 and 13, for untreated and treated youths, respectively.

## Screening records

As detailed in Section 2, youth who are transferred to the detention center after an arrest are screened using the RAI tool. RAI assessments, however, are not identified at the referral level. To match a specific referral to a screening, we use the screening and referral dates. Specifically, we assign each screening to the closest referral. However, some matches differ substantially in their dates. Thus, we only keep matches whenever the screening occurred within five days of the referral date. In the entire sample of first contacts, only 7.8% of linked referrals are more than five days apart. Out of the matched screenings (i.e. within five days), 93.3% are linked to a referral on the exact same date, and more than 99% are linked to referrals within one day.

Screening records, however, are only available for a small sub-sample of youths, which, as discussed, overlaps almost perfectly with our treated group. Thus, for our analysis we use the detailed information in our data to construct simulated RAI scores for every youth in our sample. Indeed, the calculated scores are the ones we used in Table 1. We compared our simulated score with the true score for the sample that has a true RAI score,. We are able to replicate 81.6% of the scores. Although we cannot replicate the exact score for the remaining 18.4% of youths, only 7.7% of cases result in a mismatch in the outcome of the RAI screening process (detain or release recommendation). We examined detailed, case-by-case, data for these cases in search for patterns that would explain the mismatches, as scoring guides are relatively straightforward and use a limited amount of data. The only patterns we could identify was that Jail Felonies were more likely to have a mismatch than other offenses. We further examined mismatches in collaboration with HCJPD’s Data and Research team. Based on this analysis, we are confident that the observed discrepancies are the result in normal errors in scoring when the actual screening was performed.

## Referrals versus contacts

Each specific alleged offense is recorded in a separate referral. Thus, one single incident could lead to multiple referrals. When this occurs, a single stream of detention decisions is made for all the distinct referrals. Thus, for the purpose of our analysis, we group all referrals occurring on the same day into a *contact* and reorganize our data accordingly. This grouping of referrals requires us to combine and consolidate offense-specific information. For each contact, we assign the most serious offense in the group of referrals and then assign all the other offense information corresponding to the chosen (most serious) offense.<sup>8</sup>

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<sup>8</sup>For example, a youth with two referrals on the same day, a second degree property felony and an against-person misdemeanor A, will appear in the contact-level data as having a second degree property felony (i.e. we use a lexicographic classification in which seriousness - felony or misdemeanors of different degrees - trumps

Detention records are typically attached to only one of the multiple referrals in a contact, so creating detention variables at the contact level is straightforward. In the very few cases where detention records are scattered across the multiple referrals in a contact, we create contact-level detention records using all of the referral-level records (i.e. we do not only chose detention records attached to the most serious offense).

## Main outcome variables

For our analysis, we use three main measures of juvenile recidivism: recidivism within 90 days, within 180 days, and withing a year of the initial referral. We define recidivism simply as whether a new referral was filed for the same youth within the specified time frame. This includes both new offenses and, in cases where the youth is put on probation, technical violations of probation (VOPs). We choose to include VOPs in our measures of recidivism because our interest in recidivism extends beyond public safety. Like new offenses, VOPs also trigger a response from the juvenile justice system, which itself carries potential sanctions and consequences for youth.

When estimating the effects of detention on these outcomes, we restrict our sample to youth for whom we can observe at least 90 (180,365) days after their initial referral. This implies that we only use cases with referral dates at least 90 (180,365) days before December 31, 2022, which is when our data stops. This restriction is automatically satisfied in our sample as we only analyze contacts up to 2020 because the screening instrument was updated in early 2021. More importantly, we only include cases in which youths were at least 90 (180, 365) days away from their 17<sup>th</sup> birthday. Thus, our relatively short time frames to measure juvenile recidivism reflect our desire to limit this age-based truncation of our sample.<sup>9</sup>

## 3.2 Auxiliary data sources

In addition to the records described in Section 3.1, we also have access to PACT (Positive Achievement Change Tool) assessments for a subset of youth in our sample. The PACT is a widely used assessment tool in juvenile justice settings, which measures protective and risk factors. It contains very detailed information collected through a variety of sources, including administrative records and direct questioning of youth and their families. Different versions of the assessment are used at different stages in a youth’s potential involvement with the system. For instance, a shorter version is used for screening, while a more exhaustive version is used at the beginning of probationary periods.

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other offense characteristics, such as whether it is against-person, weapon, drug, or property-related).

<sup>9</sup>When comparing results with the three outcomes, we will be able to test the extent to which potential differences are caused by the differential sample.

In principle, a screening version of the PACT should be collected for every youth who is referred to the juvenile justice system. In practice, however, there are many youths for whom a PACT assessment cannot be located in the data. Crucially, this is not a random event. Almost every detained youth has a PACT assessment, while a large share of non-detained youth do not have one. Thus, PACT availability is strongly correlated with treatment and, potentially, with other risk factors.

Furthermore, unlike other records, PACT assessments are not linked to specific referrals. Thus, we assign a PACT to a referral based on the proximity of their respective dates. But, because recidivating events may also prompt a PACT assessment, any expansion of the window used to assign PACTs to specific referrals creates bias, as the likelihood of locating a PACT assessment correlates with the likelihood of recidivism. This is further complicated by the fact that available PACT assessments for non-detained youth are commonly collected weeks or even months after the referral date. Thus, we do not use PACT data in our main analysis. However, given the richness of the data, we use it in auxiliary analysis to examine the sensitivity of our estimates to the inclusion of an even richer set of covariates.

### 3.3 Sample

Our main data includes all referral records for youths whose first referral occurred between 2010 and 2020. As described, we organize these referrals into separate contacts. Although youth may be referred starting at age 10, youth under 12 receive differential treatment based on their age. Furthermore, the age of criminal majority in Texas is 17, so youth older than 17 are no longer under the jurisdiction of the Juvenile Court and are therefore handled by the adult criminal justice system. Thus, our analysis is limited to youth whose first referral occurred between the ages of 12 and 16 and was originated in Harris County.<sup>10</sup>

Our analysis focuses on low-risk youth and the detention decisions during their initial (formal) contact with the juvenile justice system. Thus, we apply a number of additional restrictions to our sample. Tables 2 and 3 present descriptive statistics for key covariates, the treatment variable, and outcome variables for different samples as we progressively impose these restrictions. Column 1 shows the entire sample of contacts in our (age-restricted) data. In this sample, multiple observations may come from the same youth, as some youths have multiple contacts throughout their lives. Column 2 restricts the sample to the first contact of each youth in our sample. As shown there, over 40,000 youths were formally involved with the juvenile justice system during our sample period. Column 3 further restricts the

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<sup>10</sup>A small amount of referrals are originated in neighbouring counties, typically because the offense occurred there but the youth lives or moves to Harris County. It is unclear whether we have all the relevant information and history of referrals in these cases. We do not include them in our analysis.



sample to youths we have denoted as low-risk, namely those with a calculated RAI score under the 15-point threshold associated with a detention recommendation. Finally, Column 4 implements two small additional restrictions. We combine them in a single column as the individual effect of each of them on the sample is marginal. Specifically, we exclude (i) youths whose detention decision was confirmed by a formal detention during the initial detention hearing, inferring from this formal detention that unrecorded circumstances or case characteristics may have prompted the initial detention (as their case was reviewed by a magistrate judge, who confirmed the detention decision upon review of the case); and (ii) cases in which the referral source is not law enforcement or a school (e.g. when the referral is initiated by the probation department), as this is an indication that these are not, in fact, those youths' first referrals. Column 4 describes our main estimation sample.

Observations in the first column of Table 2 are at the referral level, while, by restricting the samples to first contacts, Columns 2-4 are at the youth level. Moving from all contacts to first contacts, we observe that the share of male referrals decrease from 75.9% to 70.1%, the share of referrals for Black youths decrease from 42.3% to 37.2%, and the share for white youths increase from 13.9% to 18.1%, which points to the relative shares recidivism across groups. Table 2 also presents other background information on youths in our sample, including whether they experience sexual, physical, or emotional abuse, their schooling status, and who they live with at home. In the most restrictive sample in Column 4, at least 5.2% of youths experience some kind of abuse, 6.7% are in special education, and 15.9% are behind in school by at least one grade. In terms of family structure, the largest category is youths who live with just their mother, which makes up 47.5% of our sample, followed by 32.4% of youths who live with two parents (biological or otherwise).

Table 3 presents information related to the referrals for the same set of samples. Because columns 2-4 restrict the sample to first contacts, thus excluding youth on probation, the share of violation of probation mechanically drops to zero in Columns 2-4. A small number of referrals made by the Probation Department come from youths who were previously put on probation outside Harris County, and we further exclude them in Column 4. The share of felonies decreases from 26.4% for all contacts, to 15.2% for low-risk youths, and then to 14.3% in our estimation sample as we progressively restrict our sample. This occurs almost mechanically, as the seriousness of the offense is the main determinant of a youth's risk score. A similar pattern can be observed for the share of against person offenses, which are considered to be more serious than other types of offenses.

Despite their low-risk assessments, Column 4 in Table 3 show that 18.5% of the youths are still detained. While prevalence of pre-adjudicated detentions is lower in our estimation sample, the difference with the share of youths detained for all first contacts (Column 2) is

Table 2: Descriptive statistics: youth background

	(1) All contacts	(2) First contacts	(3) Low-risk first contacts	(4) Main sample
Age				
Mean	14.9	14.6	14.6	14.6
SD	1.2	1.2	1.2	1.2
Min	12	12	12	12
Max	23	16	16	16
Calculated RAI score				
Mean	4.7	4.7	3.6	3.5
SD	4.1	4.1	2.3	2.2
Male (%)	75.9	70.1	69.1	69.1
Race (%)				
White	13.9	18.1	18.9	18.9
Black	42.3	37.2	35.9	35.7
Hispanic	42.9	43.5	43.9	44.1
Other	0.9	1.2	1.3	1.3
Experienced abuse (%)				
Sexual	6.8	5.7	5.1	4.7
Physical	7.9	5.9	5.4	5.2
Emotional	2.5	2.0	1.8	1.7
Schooling (%)				
Special education	9.7	7.6	6.9	6.7
Behind in school	25.3	16.4	16.2	15.9
Family: youth lives with (%)				
Two parents	31.1	32.5	32.4	32.4
Mother	47.7	46.9	47.3	47.5
<i>N</i>	70,462	41,739	37,752	36,152

This table describes youth characteristics as we impose multiple restrictions to arrive to our main estimation sample. Column 1 includes all contacts. Column 2 restricts to first contacts only. Column 3 restricts to youth with low risk scores. Column 4 imposes two additional restrictions: we exclude all formally detained youth from the treatment group and we exclude referrals for which the initiating source suggests they are not a youth's first referral.

Table 3: Descriptive statistics: youth background

	(1) All contacts	(2) First contacts	(3) Low-risk first contacts	(4) Main sample
Referral source (%)				
Law Enforcement Agency	61.4	63.0	61.8	63.0
School	25.8	34.0	35.7	37.0
Probation Department	12.0	2.3	1.8	0.0
Other	0.8	0.7	0.7	0.0
Multiple referral event (%)	11.4	7.3	6.6	6.4
Most serious event (%)				
Felonies	26.4	23.3	15.2	14.3
Misdemeanors	64.6	76.7	84.8	85.7
Violation of Probation	9.0	0.0	0.0	0.0
Most serious event type (%)				
Against person	23.6	23.8	16.7	16.6
Property	27.8	31.4	34.6	34.6
Drug-related	2.1	2.3	2.5	2.5
Weapon-related	2.5	2.6	2.8	2.3
Other	44.0	40.0	43.4	44.0
Treatment (%)				
Detained	26.7	26.7	20.6	18.5
Outcomes				
90-day recidivism	13.9	8.2	8.3	7.9
180-day recidivism	22.2	14.1	14.1	13.4
1-year recidivism	35.7	23.6	23.5	22.7
<i>N</i>	70,462	41,739	37,752	36,152

This table describes referral characteristics as we impose multiple restrictions to arrive to our main estimation sample. Column 1 includes all contacts. Column 2 restricts to first contacts only. Column 3 restricts to youth with low risk scores. Column 4 imposes two additional restrictions: we exclude all formally detained youth from the treatment group and we exclude referrals for which the initiating source suggests they are not a youth's first referral.

only around 8 percentage points. The difference in the outcome variables is even smaller, as the differences between Columns 2 and 4 for recidivism measured 3 months (90 days), 6 months (180 days), and a year after the initial referral are all less than 1 percentage point in all cases.

Overall, Tables 2 and 3 show that, as expected, first contacts are not necessarily representative of all contacts, because recidivism is correlated with some of the characteristics shown in these tables. Furthermore, when we focus on low-risk youths, we see a big change in the types of offenses (as expected), but not on background characteristics. The prevalence of detention in our sample is lower than in first contacts in general, which reflects that low-risk youth are, in fact, less likely to be detained, as policies indicate. Notably, even though recidivism is lower when we look at first contacts only (again, as expected), rates are barely affected by further restrictions, including by focusing on low-risk youths only.

## 4 Empirical strategy

Our goal is to estimate the effect of short-term detention on recidivism for youths, focusing on low-risk youths on their first arrest to exclude any potential biases from previous contacts with the juvenile criminal justice system. To do this, we use a selection-on-observable approach with the double/debiased machine learning (DDML) lasso estimator developed by Chernozhukov et al. (2018), which allows for a more relaxed sparsity assumption required for lasso and leverages the rich information on youth in our administrative data. We also quantify the robustness of our estimates using the sensitivity analysis developed by Masten et al. (2023), which allows us to gauge the degree of selection on unobservable that would invalidate our results.

### 4.1 AIPW using Double/Debiased Machine Learning

Let  $Y \in \{0, 1\}$  be the observed binary outcome variable indicating recidivism within a certain number of days. Let  $D \in \{0, 1\}$  be the binary treatment variable indicating pre-trial detention following an arrest. Following standard potential outcomes notation, denote the unobserved potential outcomes by  $Y_0$  and  $Y_1$ , and the observed outcome by  $Y = DY_1 + (1 - D)Y_0$ . A potentially high-dimensional vector  $X$  collects the control variables. Following the notation of Chernozhukov et al. (2018) but suppressing the index for individuals, the outcome model is

$$E(Y = 1|X, D) = g_0(D, x),$$

while the treatment model is

$$E(D = 1|x) = m_0(x).$$

Our goal is to estimate the average treatment effect (ATE) of short term detention on youths’ recidivism, which is defined by

$$\text{ATE} = E[g_0(1, x) - g_0(0, x)].$$

We estimate the ATE using augmented inverse-probability weighting (AIPW). By using AIPW estimators, having either the treatment or the outcome model being correctly specified is sufficient to obtain consistent estimates of treatment effects, a property known as being “doubly robust”. We use double machine learning to separately select the control variables for the treatment and outcome models using lasso. We use five-fold cross-fitting with 10 different sample splits for double machine learning for all specifications.

Including a large set of variables for lasso to select from in both models allows  $X$  to be rich enough in order for the assumption of conditional independence to be plausible, without violating the overlap assumption. All of our models are forced to include a set of variables that are very likely to play a role in determining the treatment or the outcome, without relying exclusively on lasso to select them. These variables include the RAI score, year of referral, demographic information (race, sex, and age), and information about the referral, including the type of the offense (felony or misdemeanor, and against person or otherwise), the referral source, and whether the contact involved multiple referrals. Lasso then selects additional variables and interactions, to include in each of the two models. Descriptive statistics for key variables are presented in Column 4 of Tables 2 and 3.

## 4.2 Sensitivity to Unconfoundedness

To estimate the ATE, our DDML estimator relies on two major identifying assumptions:

- Unconfoundedness:  $D \perp\!\!\!\perp Y_i | X$  and  $X \perp\!\!\!\perp Y_0 | X$  and
- Overlap:  $m_0(x) \in (0, 1) \forall x \in X$

In principle, it is possible for lasso to select a set of variables that leads to a lack of common support. However, the implementation of AIPW using lasso takes this into account and mechanically satisfies the overlap condition. On the other hand, the unconfoundedness assumption is not testable. Even conditional on the variables included in the treatment

and outcome models, it is possible that unobservable characteristics correlate with both the treatment of pretrial detention and with outcomes.

We leverage recent developments in the literature on assessing robustness to unconfoundedness and directly assess the degree of selection on unobservables that would invalidate our results. Masten and Poirier (2018) define a class of nonparametric assumptions called conditional  $c$ -independence.

Let  $c$  be a scalar between 0 and 1. In our notation,  $D$  is conditionally  $c$ -independent with  $Y_d$  given  $X = x$  if for all  $x \in \text{supp}(X)$ ,

$$\sup_{y_d \in \text{supp}(Y_d|X=x)} |P(D = 1|Y_d = y_d, X = x) - P(D = 1|X = x)| \leq c.$$

The first term is the unobserved conditional probability of being detained that could depend on the unobserved potential outcome  $Y_d$ . If selection on unobservables were present in our data, this unconditional probability would be different from the propensity score, which is the second term in the definition. The assumption of conditional  $c$ -independence allows the difference to be at most  $c$ , thereby allowing for some selection on unobservables but in a quantified way. In the case of  $c = 0$ , conditional  $c$ -independence is equivalent to the usual unconfoundedness assumption.

The ATE is no longer point-identified when we replace the unconfoundedness assumption with conditional  $c$ -independence. Masten and Poirier (2018) derive bounds on various treatment effect parameters, and define the breakdown point for the ATE as the value of  $c$  for which bounds on the ATE start containing zero.

### 4.3 Workflow

To draw robust conclusions from nonexperimental but detailed data, we suggest combining the approaches described above into a workflow. First, we estimate the ATE of short-term detention on youths' outcomes using AIPW with lasso. Next, we take the union of variables selected by lasso across all specifications and perform the sensitivity analysis of Masten and Poirier (2018). To interpret the size of the breakdown point, the procedure leaves out each control variable one-by-one in the estimation of the propensity score and compares the changes in propensity scores to the breakdown point. A breakdown point smaller than changes in the propensity score for a substantial portion of the sample following the omission of a seemingly unimportant control variable would suggest that selection-on-unobservables could easily change the sign of the estimated ATE. On the other hand, if the breakdown point were larger than changes in the propensity score for most of the sample when a major control variable, such as race or severity of the offense, is removed, then it is less likely that

selection-on-unobservables would invalidate our results. For each estimate, we focus on the comparison of the breakdown point with the 90th percentile of changes in the propensity from dropping each variable, which we denote as  $\Delta^{p90}$ .

## 5 Results

We first present our main results in this section, showing that (1) pre-adjudicated detention leads to greater likelihood of recidivism, (2) the effect is larger as we increase the period used to determine recidivism, and (3) the effect is unlikely to be driven by selection on unobservables, which would need to be as severe as leaving out important controls like race to reduce the estimated effect to zero. We then show that the effect is not related to the length of time a youth spends in detention. We also show how the effect is driven by youths detained for misdemeanors or offenses that are not against a person.

### 5.1 Main Results

Table 4 reports the estimated treatment effect of pretrial detention on youths’ likelihood to be arrested again within 3 months (90 days), 6 months (180 days), and a year. In a 3-month period after a youth’s arrest, pretrial detention increases his or her recidivism likelihood by 4.02 percentage points. The effect increases as we lengthen the period over which recidivism is determined, with the ATE rising to 5.48 percentage points over a 6-month period, and 8.81 percentage points for a 1-year period. All of these estimates are statistically significant.<sup>11</sup>

To assess the robustness of the ATE being nonnegative to the unconfoundedness assumption, the first row of Panel B in Table 4 reports the breakdown point of Masten et al. (2023). The sensitivity analysis of Masten et al. (2023) builds upon baseline estimates computed by inverse probability weighting (IPW) using the usual propensity score estimator. Our estimates, on the other hand, rely on lasso to select potentially different sets of variables for the treatment and outcome models. Thus, to perform the sensitivity analysis, we take the union of variables selected in both treatment and outcome models for all outcomes. Moreover, if lasso selects a particular value or interaction of discrete characteristics, we supplement the union of selected variables with indicators for other values or interactions of the discrete variable. For example, if lasso selects the interaction of the indicator for against person

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<sup>11</sup>The samples used for the estimation of the effect on each outcome measure are different, as we need to allow enough time to observe the outcome (prior to the age of criminal majority). However, the pattern of increasing size of the effect as we extend the period for measuring recidivism is not due to the selection imposed by the use of these different samples. Table A2 in the Appendix presents the estimates obtained when we use the same sample for all outcomes (the sample for which 1-year recidivism data is available), and the results are close to the estimates with the full samples in Table 4.

Table 4: The effect of detention on recidivism

Months after first referral	3	6	12
Panel A: Effect of detention on recidivism			
ATE	0.0402*** (0.0073)	0.0548*** (0.0085)	0.0881*** (0.0089)
Control mean	0.0705	0.1219	0.2061
$N$	33,119	30,300	24,560
Panel B: Masten-Poirier-Zhang sensitivity analysis			
Breakdown point	0.048	0.038	0.040
90th percentile propensity score changes ( $\Delta^{p90}$ )			
Age	0.007	0.006	0.000
Calculated RAI score	0.035	0.035	0.031
Black	0.053	0.052	0.052
Hispanic	0.023	0.023	0.021
Male	0.005	0.004	0.000
Multiple referral event	0.003	0.004	0.003
Felony	0.017	0.016	0.020
School referral	0.093	0.093	0.095

<sup>1</sup> In addition to restrictions in Column 4 of Tables 2 and 3, each column further restricts the sample to the period for which there are corresponding number of months (3, 6, and 12) remaining in the data for recidivism to be determined.

<sup>2</sup> Covariates used for estimating IPW in the sensitivity analysis include the union of always-included controls described in Section 4 and variables selected by double machine learning. The variables are listed in Table 5 except for the year of referral.

<sup>3</sup> Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



offense with the indicator for Black youths, we also include the interaction of against person offense with the indicator for Hispanic youths.

As a benchmark for the breakdown point, Masten et al. (2023) suggest estimating order statistics of changes in the propensity score from excluding each observed covariate. Since conditional  $c$ -independence is formulated as the maximum change in the conditional probability of assignment to treatment from including the unobserved potential outcome along with the observed covariates, the marginal impact of leaving out each covariate on the propensity score provides reference values for the breakdown point.

The bottom panel of Table 4 reports the 90th percentile of changes in the propensity scores ( $\Delta^{p90}$ ) after leaving out each variable in the set of variables that are always included in our AIPW estimates, except for the year dummy variables. Across 3-month, 6-month, and 1-year periods, all but two of the 90th percentile changes in the propensity score following the removal of each variable are smaller than the breakdown points. If we take the 90th percentile change in propensity scores for variables such as the calculated RAI score, the youth’s sex, or the crime being a felony as reference values, which are likely important determinants of youths’ experience in the juvenile justice system, we could conclude that our estimated ATEs are robust to selection on unobservables. Conceptually speaking, for a violation of the unconfoundedness assumption to change the sign of our ATE estimates, changes in assignment probability from selection on unobservables would need to be greater than changes in the propensity score from ignoring whether the crime is a felony for 90% of youths’ first offense.

While the omission of most variables in our set of always-included controls result in smaller 90th percentile changes in the propensity score than the breakdown point, the dummy variables for the youth being Black and the referral originating at school remain the exception for all three outcomes. For 3-month recidivism,  $\Delta^{p90}$  is 0.053 from excluding the indicator for being Black, while the breakdown point is 0.048. The breakdown points decrease to 0.038 and 0.040 respectively for 6-month and 1-year recidivism, but the  $\Delta^{p90}$  for excluding the indicator Black youths stays at 0.052 in each case. Appendix Table A1 presents 50th ( $\Delta^{p50}$ ), 75th ( $\Delta^{p75}$ ), and 90th percentiles of changes in the propensity scores from each variable, and the breakdown points are larger than the 75th percentile change in propensity score of the indicator for Black youths. For example,  $\Delta^{p75}$  is 0.032 for the youth being Black in the case of 3-month recidivism. A similar pattern holds for the referral originating from school. In the case of 3-month recidivism, while  $\Delta^{p90} = 0.093$  is larger than the breakdown point, its  $\Delta^{p75}$  is only 0.030. Therefore while the more stringent threshold of the 90th percentile propensity score change indicates potential vulnerability of our estimates to omission key variables such as a youth’s race, the sensitivity is less concerning when we also consider thresholds such as

the 75th percentile.<sup>12</sup>

The sensitivity results in Table 4 use the variables that are always included in our estimation as benchmark. However, the set of variables selected by lasso could be larger. Thus, we follow Masten and Poirier (2018)’s suggestion of also considering which variables could have a substantial impact on the baseline point estimates of the ATE (as opposed to the propensity score, as in Table 4). Table 5 presents the 90th percentile change in propensity score from removing variables selected by lasso ( $\Delta^{p90}$  columns) and the percentage change in the ATE from removing each variable ( $\Delta\text{ATE}$  columns). The variables are the union of variables selected by lasso across the treatment and outcome models of all outcomes, along with the complementing interaction term if an interaction is selected.<sup>13</sup> The 90th percentile change in the propensity for all but three of the selected controls fall below the breakdown points. The interaction of Black and the offense being against a person has the highest change in propensity score, between 0.064 and 0.067. However, omitting this variable from our specification changes the estimate of the ATE by 1.5 percentage points in the worst case scenario. Leaving out the indicator for the offense being against person or involving a weapon leads to the largest changes in the ATE, with the changes ranging between 5.6 and 7 percentage points. The 90th percentile change in the propensity score at 0.05 is also greater than the breakdown point of 0.048 for 3-month recidivism, but not by a great margin. The changes in the propensity score are also similar for 6-month and 1-year recidivism, as the only difference in the treatment model is the reduction in the sample from defining recidivism over a longer period.

Overall, if we take variables such as youths’ demographic characteristics as the benchmark, our main result that the ATE of pre-adjudicated detention on youths’ recidivism is nonnegative appears to be robust to violations of unconfoundedness. While the exclusion of a few specific and obviously relevant variables (e.g. against-person and weapon-related offenses) leads to large changes in the propensity score for more than a quarter of youths, these variables have already been taken into account as observed covariates in our estimates. Furthermore, substantial changes in the propensity score from their exclusion do not directly indicate a violation of the unconfoundedness assumption, only that the procedures of pre-adjudicated detentions are heavily influenced by these variables. Precisely, our double machine learning procedure selects these variables as appropriate controls for the AIPW estimator. Thus, the main conclusion of the sensitivity analysis is that, to invalidate our

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<sup>12</sup>Note that we never omit race from any specification. The sensitivity analysis points to vulnerabilities when variables as important as race is not taken into account.

<sup>13</sup>For example, if the interaction of a variable with any race indicator variable is selected, we include the interaction terms for both Black and Hispanic youths, and the interaction with the white youth indicator variable serves as the reference category.

Table 5: Change in propensity score and percentage change in ATE from leaving out each variable.

Monsths after first referral	3		6		12	
Breakdown point	0.048		0.038		0.040	
Statistic	$\Delta^{p90}$	$\Delta\text{ATE}$	$\Delta^{p90}$	$\Delta\text{ATE}$	$\Delta^{p90}$	$\Delta\text{ATE}$
Youth experienced						
behind in school	0.040	0.007	0.037	0.001	0.027	0.008
emotional abuse	0.003	0.009	0.003	0.009	0.004	0.004
physical abuse	0.011	0.019	0.012	0.012	0.011	0.019
sexual abuse	0.012	0.004	0.012	0.013	0.013	0.012
Special education	0.020	0.029	0.021	0.023	0.024	0.021
Offense type						
Against property	0.051	0.041	0.051	0.002	0.051	0.001
Against person or weapon-related	0.050	0.070	0.050	0.056	0.051	0.060
Against person $\times$ Black	0.064	0.003	0.065	0.015	0.067	0.006
Against person $\times$ Hispanic	0.033	0.001	0.034	0.001	0.034	0.002
Lives with 2 parents	0.004	0.001	0.003	0.001	0.004	0.001
$\times$ Black	0.003	0.001	0.002	0.000	0.000	0.000
$\times$ Hispanic	0.002	0.001	0.002	0.001	0.002	0.000
$\times$ male	0.001	0.001	0.001	0.001	0.004	0.002
School referral $\times$ male	0.018	0.011	0.018	0.005	0.016	0.005

<sup>1</sup>  $\Delta^{p90}$  denotes the 90th percentile change in the propensity score of leaving out each variable.  $\Delta\text{ATE}$  denotes the change in the estimated ATE as a share of the original estimate.

main conclusions, the bias from remaining unobservables would need to be larger than the bias we would obtain from excluding almost any of the most relevant variables included in our specifications.

## 5.2 Mechanisms

We explore whether youths are likely to experience different outcomes because of differences in experiences of their detention spells. We first limit the sample to youths who are either not detained or are released on the same day of their detention. The top panel of Table 6 reports the estimated ATE for this subsample of youths who do not stay overnight in detention. The results are almost identical to our main results. This suggests that it is the fact that youth were detained, rather than what occurs during the detention spells, what drives the detrimental effects of short detention spells.

Table 6: Effect of duration of detention on recidivism.

Months after first referral	3	6	12
DDML estimates: sample not detained overnight			
ATE	.0437*** (0.0078)	.0580*** (0.0100)	.0813*** (0.0129)
<i>N</i>	29,663	27,120	21,968
Number of days detained			
OLS	-0.0028 (0.0045)	0.0005 (0.0058)	0.0001 (0.0077)
IV	0.0049 (0.0390)	0.0123 (0.0508)	-0.0320 (0.0681)
<i>N</i>	6,176	5,667	4,630
Montiel Olea-Pflueger Weak Instrument Test			
Effective F-statistic (at 5% confidence level)	27.26	23.52	18.43
Critical Value ( $\tau = 10\%$ of worst case bias)	13.94	13.77	13.77

<sup>1</sup> The top panel reports the same specifications, using Double/Debiased Machine Learning (DDML) as in Table 4, restricting the sample of treated youth to detained youths who do not spend a night in detention.

<sup>2</sup> The bottom panel reports OLS and IV estimates from linear models, with recidivism as the binary outcome and the number of days detained as the treatment variable. The sample for these specifications is limited to treated youths.

<sup>3</sup> Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

We further test this result with a different set of assumptions by using an instrumental variable strategy. For this, we rely on the fact that the legal maximum of 48 hours between the time of admission and a detention hearing uses business days, not calendar days. Thus,

the day of the week in which a youth was admitted to detention serves as a plausible source of exogenous variation on the number of days spent in detention. This is particularly relevant in our sample -composed of youth who should not have been detained based on their screening results and who are not detained by a judge- virtually all of whom are released within 5 calendar days.

Table 7: Share of youths by number of days detained for each day (%).

Day referred	Days detained						<i>N</i>
	0	1	2	3	4	$\geq 5$	
Sunday	46.5	46.5	6.0	0.6	0.0	0.3	664
Monday	44.2	44.5	10.8	0.0	0.2	0.4	1,037
Tuesday	44.3	45.1	10.1	0.2	0.0	0.3	1,169
Wednesday	47.6	42.0	9.2	0.2	0.1	1.0	1,131
Thursday	44.4	43.5	2.2	1.2	7.0	1.7	1,136
Friday	40.0	43.4	6.2	9.1	0.7	0.6	985
Saturday	38.4	53.5	7.4	0.4	0.0	0.4	568

Table 7 shows the distribution of the number of days spent in detention by day of the week in which the youth was arrested and referred. For referrals made between Sunday and Wednesday, less than 1.5% lead to detentions of three days or more, since the 48 hours of detention allowed before a detention hearing is conducted fall on business days. For youths arrested on Thursday, 9.9% are detained for more than 3 days, and number rises to 10.4% for referrals made on Friday. Referrals made on Saturday do not lead to as many detentions of more than 3 days, but only 38.4% of youths arrested on Saturday do not spend a night in the detention facility; between Sunday and Thursday, at least 44% of youths arrested on each day do not spend a night in detention. Building on this pattern, we instrument the number of days detained with a variable classifying referrals into 4 categories: referrals made on Thursday, Friday, Saturday, or between Sunday and Monday.

The bottom panel in Table 6 reports the results from both the OLS and IV specifications of a linear probability model of the effect of the length of detention on recidivism. We find that the estimated coefficient of days detained on recidivism is close to zero and statistically insignificant in all specifications. The test proposed by Montiel Olea and Pflueger (2013) also rejects the null hypothesis of weak instruments. Thus, the IV estimates corroborate the results in the top panel of Table 6. Within this short detention spells we analyze, the effect of detention on recidivism appears to be unrelated to the length of the detention spell.

### 5.3 Heterogeneous Effects

**Type of Offense** Our estimates using only treated youths who did not spend the night in detention strongly suggests that all the effects stem from mechanisms not associated with the length of the detention spell. Rather, it appears that the fact of being detained itself explains the negative consequences we have documented. Potential mechanisms consistent with these results include the trauma associated with the initial process of detention, stigma and other changes in behavior of others towards the youth, or changes in the self-image of a youth who has been detained. While we cannot directly test these mechanisms, we further explore the possibility of effects from the detention process through examining whether there are heterogeneous effects by the type of offense.

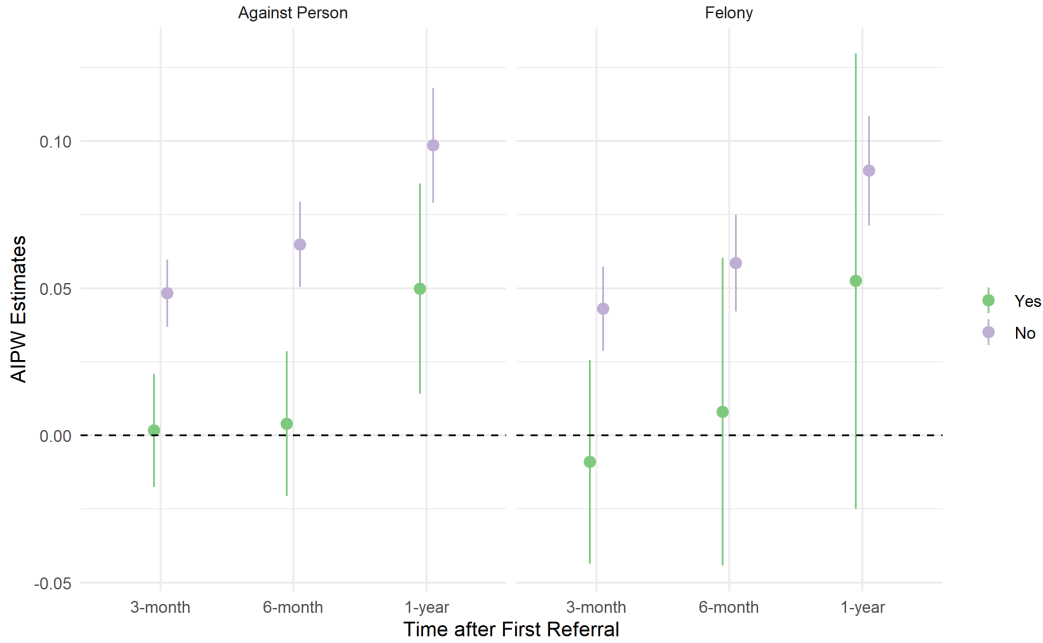


Figure 2: Effect of detention on recidivism by type of offense.

We estimate conditional ATEs separately for referrals with felony and misdemeanor offenses, and for offenses against persons and other offenses.<sup>14</sup> Figure 2 reports the conditional ATE's, obtained using the same procedure as in our main results. The effects for referrals of felonies are all close to zero in magnitude and statistically insignificant, while the effects for referrals of misdemeanors are all close to our main results in Table 4 and statistically significant. Thus, our main results are driven entirely by youths who were arrested and

<sup>14</sup>Because we focus on low-risk youths (as defined by the screening instrument), felony offenses in our sample are almost exclusively "low-level" felonies, typically jail felonies and category 3 felonies. Likewise, against-person offenses in our sample are against-person misdemeanors, as against-person felonies would almost surely lead to a higher risk score.

detained for misdemeanors. We find similar results when we compare the conditional ATE's for youth arrested for an offense against a person and other offenses, with the ATE's being small and statistically insignificant for offenses against persons. The only exception is for 1-year recidivism, for which the effect of detention for youths arrested for offenses against persons is also positive and statistically significant, although the estimated is half the size of the estimate for youths arrested for other types of offenses (0.0498 and 0.0986, respectively).

These results suggest that youths detained for misdemeanors or offenses not against persons drive most of our results. While youths included in all of our specifications are first-time offenders who have been determined to be low risk by the screening instrument, a referral for a felony offense or an offense against a person is usually considered to be more severe. Given that misdemeanors and offenses not against a person are less serious, the fact that youths are detained could indicate more acrimonious encounters between the youths and the police. Similarly, these differences in results are consistent with stigma or changes in self-perception being particularly strong for youth engaged in minor transgressions, as compared to those engaged in more serious delinquent behavior. For instance, a youth arrested for a (low-level) felony may have already been labeled as problematic regardless of whether he or she was detained, while detention may be the cause of stigma for youth with less serious offenses. However, our data and strategy does not allow us to further examine these hypotheses.

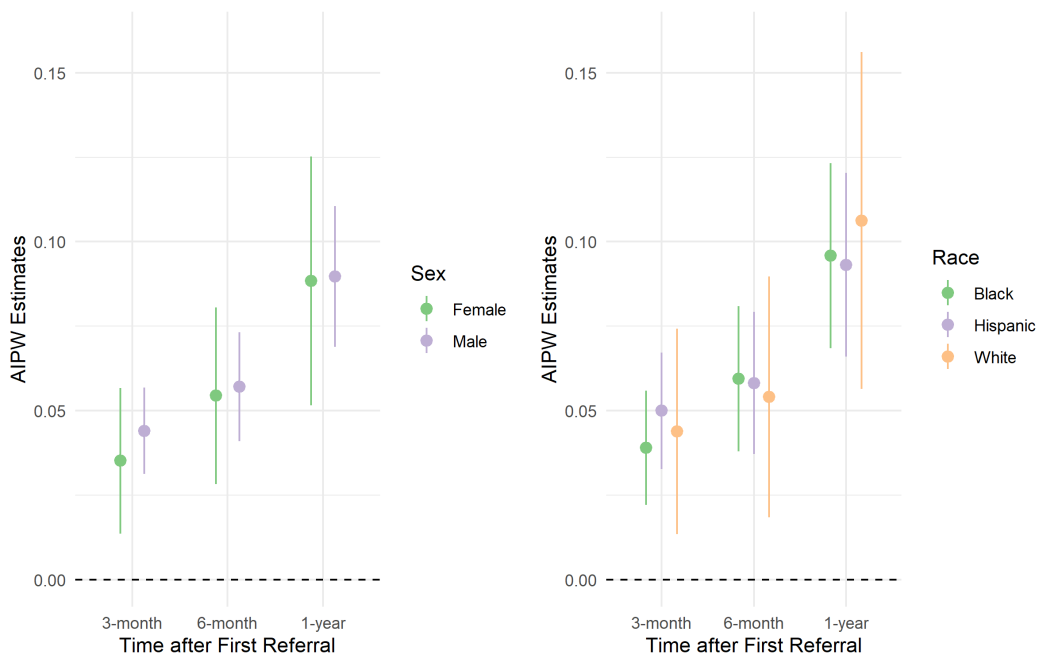


Figure 3: Effect of detention on recidivism by sex and race.

**Demographics** Finally, we examine where there are heterogeneous effects by sex or race. Figure 3 reports the conditional ATE’s by sex and race of youths separately. All results are similar, with overlapping confidence intervals between youths of different sex or race. Thus, we do not observe heterogeneous effects across broad demographic classifications.

## 6 Robustness

In Section 3 we introduced an additional source of data, the detailed items within the PACT assessments. As we described, a short version of the PACT is available for a subset of youths in our sample. However, the availability of PACT assessments is not random. Specifically, detention almost always ensures that a youth receives the PACT. Among non-detained youths, the majority of whom do not receive a PACT at the time of referral, recidivating events (i.e. new referrals) are likely to lead to an assessment. Thus, PACT availability is correlated with treatment and outcomes. This severely limits our ability to use this data. However, given the richness of the data available in the PACT assessments, in this section we use it to further examine the unconfoundedness assumption.

Columns 1-3 in Table A3 show the ATE estimates for the effect of detention on 90-day recidivism using progressively restricted samples. In Column 1, we present the estimate using our main analysis sample. Column 2 then restricts the sample to the years in which PACT assessments were used (starting in March, 2017). Column 3 further restricts the sample to youths with a matched PACT assessment. To construct these matches, we only use assessments conducted prior to any observed new referral. The estimates in Column 3, however, do not use any of the PACT variables for the estimates. Thus, comparison of the estimates in Columns 1 through 3 allows us to gauge the selection introduced by using the sample with available PACT data. As this comparison shows, the use of specific years does not affect the estimate of the ATE. On the other hand, restricting the sample to cases where there is an available PACT assessment introduces some bias in the estimated ATE, while the sample is reduced to about 16% of the main specification. The ATE estimate rises to 0.0527, and the control group mean more than halves from 0.07 to 0.03. This confirms our concerns about using the PACT information in our estimates, as the corresponding sample selection introduces substantial bias.

Column 4 shows the estimates of the ATE using the sample in Column 3 but including the PACT variables as potential covariates that lasso could select. The comparison of estimates in Columns 3 and 4 then allows us to gauge the changes in estimates after including a richer set of covariates, while holding the estimation sample constant. The estimated effect of 0.0375 in Column 4 is smaller than the one in Column 3, potentially suggesting a small



degree of selection on unobservables.

However, even after accounting for an extremely rich set of observable characteristics detailing a youth’s background and environment, our main conclusion remains. In fact, the estimated effect is close to our main estimate of 0.0402 (and larger in relative terms). Thus, although only applicable to a selected subsample (one with much lower baseline risk of recidivism), these results reinforce our main findings that short-term detentions increase the likelihood of recidivism.

Additionally, Appendix Table A3 presents sensitivity analysis for each specification below the ATE estimates, which shows that both Columns 3 and 4 have breakdown points that are larger than the 90th percentile change in the propensity score from removing any one of the variables that are always included in the AIPW estimation. Therefore, any bias in our estimates from restricting the sample to youths with PACT assessment results is more likely to stem from the selection of the sample, rather than violations of the unconfoundedness assumption in the remaining sample.

The patterns are different if we analyze the robustness of 6-month and 1-year recidivism by comparing the same process of restricting the sample and including PACT variables in the selection set for lasso. For both 6-month and 1-year recidivism, restricting the sample to the years in which PACT assessments were conducted leads to estimates that are smaller than our main estimates, while further restricting the sample to youths with available PACTs returns the estimated effect to close to the results from our main sample. However, including PACT variables into the set for lasso to select from reduces the estimated ATE; in the case of 1-year recidivism, the estimated ATE becomes statistically insignificant. To analyze this change in the estimated ATE, we again consider the sensitivity analysis presented in Appendix Table A3. Compared to the main specification, both restricting the years in our sample (Columns 6 and 10) and including PACT variables in lasso (Columns 8 and 12), in addition to having smaller ATE estimates, also show greater vulnerability to violations of unconfoundedness, with more variables having 90th percentile propensity score changes that are larger than the breakdown points. The only specifications that does not have more such variables is the restriction of the sample to youths with PACT results available, and their estimates are both close to the main specification for 6-month and 1-year recidivism.

In summary, incorporating the PACT assessment results into our analysis leads to very similar estimated effect of short-term detention on 3-month recidivism, and the results are also robust to selection on unobservables. In the case of 6-month and 1-year recidivism, while including the PACT variables leads to lower ATE estimates, the resulting specifications are more vulnerable to selection on unobservables than our main specification.

## 7 Conclusion

Temporarily confining youth in a detention center - a common tool in juvenile justice systems across the United States- aims to improve public safety by reducing the likelihood that arrested youth commit new offenses while their cases are disposed. At the same time, this crucial decision prevents youth from returning to their families and communities, exposes them to potential risks, and disrupts normal life at a particularly vulnerable developmental stage. Thus, in principle, pre-adjudicated detention is reserved for youths who are deemed to be at risk of endangering themselves or others. Nevertheless, as we have shown, low-risk youth are often detained for brief periods following their arrest. Although previous research has provided evidence on the (negative) sequences of typical detentions, we are the first to study the effects of these short-term detentions on recidivism outcomes of low-risk youth. We find that these short-term detentions increase the likelihood that a youth is rearrested in the 90 days following their initial arrest by more than 50% (4 percentage points, relative to a baseline rate of 7.1%).

Our results highlight the large scope of potential negative consequences that stem from the detention of youth. We analyze detention stays lasting from a few hours to less than a week. Thus, the negative consequences we document underscore that detentions themselves, even if they are short-lasting, can have profound consequences. We have focused on recidivism outcomes, but other potential consequences may include disruption to school and other aspects of youths' lives, and trauma associated with the experience of detention itself.

Our sample is limited to youths with low-level offenses, typically misdemeanors, with no prior involvement with the juvenile justice system. Thus, the specific detentions we analyze here affect a subset of justice-involved youth whom decision makers have already determined they should not be detained. Policy and procedural changes to avoid their unnecessary detention may receive more broad support than other detention-related changes in policies that would involve a reform of the system.

As we have mentioned, we limited our analysis to referrals prior to 2021 because a new screening instrument was introduced in Harris County in that year. This change was part of a general effort to rethink detention in this jurisdiction. Indeed, the data shows other important changes in practices happened at the same time. For example, the share of youth admitted to detention at the time of referral decreased from around 30% between 2018 and 2020 to 22% in 2021 and 19% in 2022. In fact, this sharp decrease is most pronounced for youth on their first contact, only 16% of whom were admitted at the time of referral in 2022 (compared to 32% in 2020). The data also shows a decrease in the fraction of youth who were screened (thus transferred to the JDC), which we have shown is the main driver of the

kind of detentions we analyze. Thus, in Harris County, changes in practices seem to already be underway. Our results show these changes are expected to have positive consequences, reducing recidivism and preventing youth from returning to the juvenile justice system.

Nevertheless, many low risk youths continue to be admitted to detention at the time of their arrest. This calls for the implementation of interventions to reduce their unnecessary and detrimental detention. Although we do not have access to national data to gauge the extent to which short-term detentions of low-risk youth are common elsewhere, we expect this practice to occur in many other jurisdictions as well. In fact, the recent effort in our jurisdiction to try to limit the use of detention suggest the practice could be even more widespread in other places. Thus, our results are likely applicable beyond the specific setting of this paper.

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## A Appendix: Supplemental Tables

Table A1: Masten-Poirier-Zhang sensitivity analysis for main sample.

Months after first referral	3			6			12		
	0.048			0.038			0.040		
Breakdown point									
Percentile of propensity score changes	50	75	90	50	75	90	50	75	90
Age	0.002	0.004	0.007	0.002	0.004	0.006	0.000	0.000	0.000
Calculated RAI score	0.007	0.014	0.035	0.007	0.014	0.035	0.006	0.013	0.031
Black	0.007	0.032	0.053	0.007	0.031	0.052	0.007	0.030	0.052
Hispanic	0.004	0.015	0.023	0.004	0.014	0.023	0.004	0.012	0.021
Male	0.001	0.003	0.005	0.001	0.002	0.004	0.000	0.000	0.000
Multiple referral event	0.000	0.002	0.003	0.001	0.002	0.004	0.000	0.001	0.003
Felony	0.001	0.002	0.017	0.001	0.002	0.016	0.001	0.002	0.020
School referral	0.012	0.030	0.093	0.012	0.030	0.093	0.012	0.031	0.095
Youth experienced behind in school	0.010	0.018	0.040	0.009	0.017	0.037	0.007	0.012	0.027
emotional abuse	0.000	0.001	0.003	0.000	0.001	0.003	0.000	0.001	0.004
physical abuse	0.002	0.005	0.011	0.002	0.005	0.012	0.002	0.005	0.011
sexual abuse	0.002	0.004	0.012	0.002	0.004	0.012	0.002	0.004	0.013
Special education	0.004	0.009	0.020	0.004	0.009	0.021	0.005	0.010	0.024
Offense type									
Against property	0.025	0.038	0.051	0.024	0.038	0.051	0.024	0.038	0.051
Against person or weapon-related	0.012	0.021	0.050	0.012	0.021	0.050	0.012	0.021	0.051
Against person $\times$ Black	0.011	0.023	0.064	0.011	0.023	0.065	0.011	0.024	0.067
Against person $\times$ Hispanic	0.005	0.009	0.033	0.005	0.010	0.034	0.004	0.009	0.034
Lives with 2 parents	0.001	0.002	0.004	0.001	0.002	0.003	0.001	0.002	0.004
$\times$ Black	0.001	0.001	0.003	0.000	0.001	0.002	0.000	0.000	0.000
$\times$ Hispanic	0.001	0.002	0.002	0.001	0.001	0.002	0.000	0.001	0.002
$\times$ male	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.002	0.004
School referral $\times$ male	0.006	0.011	0.018	0.006	0.011	0.018	0.006	0.010	0.016

1 Estimation performed for the main sample used for each outcome in Table 4.

2 Covariates used for estimating IPW in the sensitivity analysis include the union of always-included controls described in Section 4 and variables selected by double machine learning.

Table A2: Effect of detention on recidivism for estimation sample of 1-year recidivism.

Months after first referral	3	6	12
ATE	0.0425*** (0.0063)	0.0624*** (0.0076)	0.0882*** (0.0089)
Control mean	0.0715	0.1221	0.2061
<i>N</i>	24,560	24,560	24,560

<sup>1</sup> Specification same as Table 4, with the estimation sample restricted to youths for whom one-year recidivism could be determined, i.e. first referrals that happened at least one year before the end of the period in our data and youths who are not yet 16 at the time of first referral.

<sup>2</sup> Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table A3: Effect of detention on recidivism accounting for PACT information.

Months after first referral												
Sample	3			6			12					
	Full (1)	PACT years (2)	PACT available (3)	PACT included (4)	Full (5)	PACT years (6)	PACT available (7)	PACT included (8)	Full (9)	PACT years (10)	PACT available (11)	PACT included (12)
Panel A: Effect of detention on recidivism												
ATE	0.0402*** (0.0073)	0.0317** (0.0104)	0.0527*** (0.0093)	0.0375*** (0.0108)	0.0548*** (0.0085)	0.0410** (0.0126)	0.0592*** (0.0127)	0.0357* (0.0143)	0.0881*** (0.0089)	0.0524*** (0.0159)	0.0804*** (0.0165)	0.0336 (0.0184)
Control mean	0.0705	0.0781	0.0313	0.0313	0.1219	0.1243	0.0758	0.0758	0.2061	0.1977	0.1437	0.1437
N	33,119	8,396	5,452	5,452	30,300	7,701	5,014	5,014	24,560	6,305	4,095	4,095
Panel B: Masten-Poirier-Zhang sensitivity analysis												
Breakdown point	0.048	0.027	0.147	0.109	0.038	0.024	0.093	0.048	0.040	0.021	0.071	0.032
90th percentile propensity score changes												
Age	0.000	0.020	0.038	0.047	0.006	0.017	0.036	0.044	0.000	0.011	0.022	0.031
Calculated RAI score	0.031	0.038	0.041	0.031	0.035	0.039	0.042	0.032	0.031	0.031	0.037	0.029
Black	0.052	0.065	0.085	0.076	0.052	0.068	0.089	0.079	0.052	0.066	0.091	0.077
Hispanic	0.021	0.030	0.031	0.029	0.023	0.033	0.033	0.031	0.021	0.033	0.034	0.032
Male	0.000	0.008	0.008	0.002	0.004	0.006	0.004	0.006	0.000	0.003	0.002	0.014
Multiple referral event	0.003	0.002	0.012	0.014	0.004	0.003	0.013	0.015	0.003	0.002	0.014	0.017
Felony	0.020	0.039	0.047	0.051	0.016	0.037	0.045	0.049	0.020	0.039	0.049	0.051
School referral	0.095	0.113	0.141	0.102	0.093	0.111	0.140	0.102	0.095	0.113	0.141	0.103

1 Estimation method of ATE uses double machine learning to select controls using lasso, with variables from the PACT questionnaire added to the set of variables to select from.

2 Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .