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PRETRIAL JUVENILE DETENTION

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ABSTRACT

Roughly one in four juveniles arrested in the U.S. spend time in a detention center prior to their court date. To study the consequences of this practice for youth, we link the universe of individual public school records in Michigan to juvenile and adult criminal justice records. Using a combination of exact matching and inverse probability weighting, we estimate that juvenile detention leads to a 31% decline in the likelihood of graduating high school and a 25% increase in the likelihood of being arrested as an adult. Falsification tests suggest the results are not driven by unobserved heterogeneity.

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“Fairly viewed, pretrial detention of a juvenile gives rise to injuries comparable to those associated with the imprisonment of an adult.”

— Justice Thurgood Marshall
Dissenting Opinion in *Schall v. Martin*, 1984

I Introduction

Pretrial juvenile detention is a crucial phase early in the juvenile justice process. It is the point at which a court decides whether or not to confine a youth pending a court hearing. Each year, over 200,000 youths are admitted to detention facilities in the United States, and approximately 16,000 are held in detention on any given night. Roughly one in four individuals involved with the juvenile justice system in 2019 spent time at a juvenile detention center (JDC) prior to their court date, with an average length of stay of 27 days.¹ The criteria for determining whether a juvenile should be detained prior to his/her court hearing is at the discretion of individual court referees and varies widely. While many juveniles in detention were arrested for a violent offense, the slight majority of them were arrested for other offenses including drug and property crimes (Figure 1).

The high rate of detention and the racial disparities in its use have contributed to an ongoing debate regarding its effectiveness. Critics argue that detention can disrupt defendants’ education and community ties, fostering disengagement with school and furthering criminality. Proponents argue that detention for certain offenses ensures that youths will show up to their court date and is necessary for public safety, as those awaiting trial and not detained could commit other crimes prior to their court date. As with the adult criminal justice system, these debates are currently taking place in a number of jurisdictions across the country, with many of them exploring alternatives to pretrial detention such as home detention, supervised evening programs, shelters, and electronic monitoring.

Despite the prevalence of detention and the widespread debates surrounding its practice, little is known about its consequences. While there are high-quality studies examining the impact of pretrial detention in *adult* criminal justice systems (Dobbie, Goldin and Yang, 2018; Leslie and Pope, 2017), to the best of our knowledge no study has examined the effects for juveniles.² Most research

¹In contrast, the average length of pretrial detention in the adult system ranges from 50 to 250 days, depending on the offense type. On any given day, roughly 460,000 individuals are detained prior to their court date in the adult system (Liu, Nunn and Shambaugh, 2018).

²Studies examining the effects of pretrial detention in the adult criminal justice system find that detention imposes substantial short- and long-term economic harms on detained defendants in terms of conviction, lost earnings, recidivism, and government assistance, and provides little in the way of decreased criminal activity for the public interest. While non-appearances at court do significantly decrease for detained defendants, these studies typically conclude that the magnitudes do not justify the economic harms to detainees (Dobbie, Goldin and Yang, 2018; Gupta, Hansman and Frenchman, 2016; Heaton, Mayson and Stevenson, 2017; Leslie and Pope, 2017; Stevenson, 2018). Other studies examine disparities in pretrial detention and show that bail judges are racially biased against Black defendants (Arnold, Dobbie and Yang, 2018). For a detailed review of this literature, see Dobbie and Yang (2021).

involving juveniles has focused on the effects of post-trial incarceration instead (Aizer and Doyle Jr, 2015; Eren and Mocan, 2021).³ However, incarceration and pretrial detention are quite distinct practices. Juveniles in detention are presumed innocent unless/until they are found guilty in court. Furthermore, while the purpose of a detention center is to temporarily confine the juvenile while his/her case is being handled in court, post-trial placements in correctional facilities are substantially longer, and for youth who have been convicted and sentenced to be confined.

Estimating the causal impacts of detention on youths' critical outcomes such as high school graduation and adult criminality has been complicated by two main issues. First, there are few datasets that include information on both juvenile detention records and later outcomes for a large number of individuals. Second, defendants who are detained before their hearing are likely systematically different from defendants who are not detained, leading simple ordinary least squares estimates to be biased. For example, defendants detained prior to their hearing may be more likely to be guilty or more likely to have committed a serious crime, thereby overstating the costs of detention and understating its benefits.

This paper provides the first examination of how pretrial juvenile detention influences youths' later-in-life outcomes. To do so, we match the universe of public school records in Michigan to juvenile petition and detention records. Our outcomes include high school graduation as well as adult crime, which we obtain via a match to the universe of adult arrests, convictions, and incarcerations in the state's adult criminal justice system.

In contrast to the adult criminal justice system, in which pretrial decisions are primarily made by bail judges, detention decisions in the juvenile justice system are much less formal and typically made by police officers and court referees. To identify the causal effect of detention, we combine exact matching, inverse probability weighting (IPW) and regression adjustment. While this approach relies on relatively stronger assumptions than other quasi-experimental methods, the richness of our administrative education and juvenile justice data allows us to (1) compare individuals with quite similar observable characteristics prior to their JDC placement, and (2) conduct a number of sensitivity/placebo checks that lend support to the conditional independence assumption underlying our identification approach.

For each treated observation, we identify exact matches based on the filing of a juvenile petition that year along with student race, sex, grade, and school district (all in that same year). We then implement propensity score matching via IPW to compare only ex-ante similar individuals within exact match groups. The propensity score model includes a rich set of predictors including prior history of juvenile justice contact (e.g., prior petitions, crime types, and convictions), prior educational measures (e.g., prior attendance rates, suspensions, and test scores), and type of juvenile petition (e.g., violent, property, or drug offense). Thus, we compare the outcomes of individuals

³Aizer and Doyle Jr (2015) use the incarceration tendency of randomly assigned judges in Cook County as an instrumental variable, and show that juvenile incarceration results in substantially lower high school completion rates and higher adult incarceration rates. Eren and Mocan (2021) use the same identification strategy in Louisiana and find that juvenile incarceration increases the propensity of being convicted for a drug offense in adulthood while lowering the propensity to be convicted of a property crime.

who, in a given academic year, had a similar type of juvenile offense, were in the same grade and school district, are of the same sex and race/ethnicity, and have a similar history of educational and juvenile justice outcomes, but one group is detained prior to the hearing while the other group is either released to their parents or placed in home detention. Finally, we conduct post-matching regression adjustment using a rich set of pre-detention characteristics. This results in “doubly robust” estimates, implying that our estimates of the effects of JDC placement will be unbiased if either the underlying matching model or the regression model is correctly specified.

We first provide evidence that matched treated and control students are quite similar along a range of observable characteristics in the years prior to JDC placement. We then show that detention has large, negative effects on youths’ later-in-life outcomes. Specifically, we find that juvenile detention leads to a decline of 8 percentage points (31%) in the probability of graduating from high school. We also find that juvenile detention increases the probability that a youth will be arrested as an adult by 9.4 percentage points (25%). These results are larger for felony offenses (40%) than for misdemeanor offenses (21%). While we find increases in the probability of arrest for all major types of crimes, our results are largest for violent crimes. We also show that detention leads to large increases in the probability of being convicted and incarcerated as an adult. We implement the sensitivity analyses proposed by [Oster \(2019\)](#), and find that selection on unobservables would need to be over four times as large as selection on observables in order to nullify the estimated treatment effects.

To examine the potential mechanisms driving the large negative impacts of juvenile detention, we take advantage of our detailed educational records which allow us to observe how key educational inputs and outcomes change before and after a juvenile detention spell. Interestingly, we find that JDC placement increases school engagement in the year following detention. Students were 25% *less* likely to drop out of school in the year following JDC placement than otherwise observationally equivalent peers who were not placed. Conditional on school enrollment, students who spent time in a JDC were 17% less likely to be chronically absent (defined as missing more than 10% of school days during the academic year). Students were also 15% more likely to be retained in grade in the year following JDC placement, and were 32% more likely to receive special education services. Together, these results suggest that school officials focused more attention on students who were placed in a JDC and attempted to provide them with additional support services, relative to observationally similar peers who had a juvenile petition but did not spend time in a JDC.

We observe similar patterns of effects in the second year following JDC placement and, for some outcomes, into the third year. However, four and five years post placement, students who spent time in a JDC are 6% and 8% more likely to have dropped out of school, respectively. This indicates that the initial supports provided to JDC students were not effective, or at least not sufficiently beneficial to overcome the human capital disruption or the negative criminogenic effects generated by detention. Accordingly, we show that individuals detained were substantially more likely to be re-arrested as juveniles in the years following the initial detention episode.

We also show that controlling for case outcomes (e.g., whether the youth was found guilty) has no

impacts on our main estimates. The case disposition likely reflects a range of factors that are visible to police, judges, and other decision-makers in the juvenile justice system but are unobservable to us. Indeed, we show that youth placed in a JDC are substantially more likely to be found guilty than observationally similar youth. The fact that controlling for this additional variable does not change our baseline estimates is additional evidence that remaining omitted variable bias is not driving our results.⁴

While a complete cost-benefit analysis of detention is beyond the scope of this paper, the large costs of detention due to reductions in high school graduation and increases in adult crime, as well as the monetary costs of detaining youth, suggest the benefits from detention would have to be quite large to justify its use.⁵ One purported benefit of detention is that it ensures juveniles show up to their court hearing. We find that detention reduces the likelihood that a youth misses his/her court hearing by 0.8 percentage points. However, the prevalence of this outcome is small (the control mean is 2.2%), which suggests any benefit from this channel is likely to be small in our context.

The other potential benefit of detention is its ability to increase public safety by preventing the juvenile from committing further offenses prior to his/her court hearing. As we explain below, we are unable to explore this issue due to data limitations. However, in a recent review article, [Dobbie and Yang \(2021\)](#) conclude that pretrial detention in the adult system has little effect on criminal activity during the pretrial period. Compared with the adult system, the length of detention in the juvenile system is substantially shorter. Approximately one third of youth in our sample are detained for less than a week whereas the average length of pretrial detention in the adult system ranges from 50 to 250 days, depending on the offense type ([Liu, Nunn and Shambaugh, 2018](#)). Together, these facts suggest that the public safety benefits of juvenile detention are likely small.

II Background

II.A Juvenile Justice Process

The juvenile justice process in the United States begins when a police officer, sheriff, or probation officer takes a juvenile into custody for violating a law, a status offense, or a local ordinance.⁶ In Michigan, a juvenile is defined as an individual who is under the age of 17, though recent legislation expanded the juvenile designation through age 17.⁷

Immediately following an arrest, a police officer takes the youth to the Family Division of the Circuit Court of the county in which the offense was allegedly committed, and files a juvenile

⁴As we discuss below, this finding also suggests that the negative effects of detention do not operate through a guilty disposition or juvenile incarceration.

⁵Though expenditures vary by region, a survey of state expenditures on confinement in 46 states conducted by the Justice Policy Institute found that the costs of confinement for a given juvenile are roughly \$400 per day.

⁶Status offenses are those deemed as criminal only in the juvenile justice system. These include offenses such as truancy, running away from home, violating curfew, or general ungovernability.

⁷Michigan’s “Raise the Age” legislation is effective as of October 1, 2021, and amends the age of a juvenile to under the age of 18.

delinquency petition. Alternatively, the police officer could decide to instead divert the case out of the system. Law enforcement makes this decision after talking to the victim, the juvenile, and the parents, and after reviewing the juvenile's prior involvement with the juvenile justice system. In 2019, 25 percent of all juveniles arrested were immediately diverted from the courts.

Conditional on filing a juvenile court petition, the police officer either (1) releases the child to his guardians until he has to appear in court, or (2) asks a court referee/judge to place the child in a JDC. If the court referee or judge agrees with the police officer's recommendation, the child is held until a preliminary detention hearing. In Michigan, an initial detention hearing must be held within 24 hours of initial detention, excluding Sundays and holidays. During the detention hearing, a judge reviews the case and determines whether continued detention is warranted. Thus, following the detention hearing, the youth may either be released to his guardians until the trial or may stay in detention until then. Police officers, court referees and judges make decisions regarding detention on the basis of several factors, most importantly the juvenile's risk of nonappearance in court later, and the potential harm to the juvenile or the public.

A JDC is generally a secure facility operated by local authorities. JDCs are primarily used to temporarily hold juveniles while they await a court hearing, disposition, or placement in a different facility. There are 625 JDCs across the United States. In 2018, 195,000 youths (or 1 in 4 juvenile delinquency cases) were placed in short-term detention, with an average length of stay of 27 days.

At the adjudicatory hearing (akin to a trial in the adult criminal justice system), witnesses are called and the facts of the case are presented. In nearly all adjudicatory hearings, a judge receives and weighs all available evidence in order to determine whether it proves beyond reasonable doubt the alleged charges in the petition. As in the adult justice system, however, the vast majority of cases in juvenile court are not actually contested in court. Rather, they are resolved via guilty or no contest plea agreements.

Conditional on the juvenile being adjudicated delinquent (found guilty), a disposition hearing is scheduled. This hearing is akin to a sentencing hearing in the adult criminal justice system. At this hearing—after weighing recommendations from the prosecution, probation staff, the defense, the juvenile's parents and/or other potential stakeholders—the judge determines the appropriate sanctions and/or treatment for the adjudicated juvenile. Common dispositions in juvenile courts are fines, restitution, community service, in-home placement under probation, and out-of home placement in commitment facilities. In 2019, 27% of adjudicated delinquents were placed in a residential facility, while 65% were placed on formal probation.⁸

II.B How Might Juvenile Detention Impact Later-in-Life Outcomes?

Juvenile detention could impact critical outcomes such as high school graduation and recidivism through multiple channels. Perhaps most obviously, detention removes youth from their family, school

⁸Authors' calculations from the Office of Juvenile Justice and Delinquency Prevention's *Easy Access to Juvenile Court Statistics: 1985-2019*.

and community for a period of time. Removal itself is disruptive, which could have a negative impact on a range of behavioral and academic outcomes. If the detention spell is short (roughly one third of juveniles are in a JDC for less than a week), the primary impact may be this disruption. However, for roughly 60% of juveniles the spell lasts between one week and three months. In these cases, the effects of detention will depend on how the psychological and educational supports provided in the JDC compare to what the youth would have received otherwise. As a result, the direction of the effect is ambiguous.

Federal laws guarantee youth numerous rights while in detention, including the right to due process, medical and mental health care, safe and humane treatment, and education ([AECF, 2020](#)). However, little is known about the structure or quality of educational programs in JDCs. A national survey of JDCs found that nearly three quarters did not always receive students' academic records from the home school district and that fewer than half of programs offered transitional services for exiting students ([Koyama, 2012](#)). JDCs face unique challenges due to students' uncertain lengths of stay, high variance in academic needs, delays in record transferring, interruptions due to legal meetings and court hearings, and large student and teacher turnover. For these reasons, one might expect the quality of instruction and support in JDCs to be low, and the experience of detention to cause a student to fall further behind in school. On the other hand, it is possible that youth receive more individualized attention in a JDC than they would otherwise. Center staff and social workers may facilitate the provision of additional services to address learning disabilities, mental health issues, or family conflict. Furthermore, a juvenile who is detained is required to attend school within the JDC.

Detention could also change the ways in which institutions regard and treat the juvenile (a labeling mechanism). Schools, for instance, may be unwilling to allow the juvenile to re-enroll once released, forcing him to enroll in an alternative school. Even if re-enrolled in the original school, detention could lead to schools treating the juvenile differently, perhaps increasing the likelihood of harsher disciplinary actions in the future. The criminal justice system might also regard the juvenile differently following detention. For instance, police may be less likely to offer diversion and judges might be more likely to convict if previous detention status is known.

In addition, detention could also encourage criminal capital accumulation through negative peer effects ([Bayer, Hjalmarsson and Pozen, 2009](#); [Billings, Deming and Ross, 2019](#); [Jacob and Lefgren, 2003](#); [Stevenson, 2017](#)), leading to increased probability of adult crime. It is also possible that detention could negatively impact juveniles indirectly through its impact on the disposition of the individual's petition. Previous studies have shown that pretrial detention in the adult criminal justice system increases the probability of conviction, which in turn harms future labor market performance ([Dobbie, Goldin and Yang, 2018](#); [Leslie and Pope, 2017](#)).⁹ As we describe below, we also find

⁹The increase in convictions is primarily due to two channels: (1) an increase in the probability of a guilty plea due to a decline in the bargaining power of defendants with prosecutors, and (2) detention status "biasing" the judge in charge of the case (i.e., seeing a defendant detained may increase the probability that a judge believes the defendant is guilty).

evidence that detention is associated with conviction in the juvenile system. However, conviction in the juvenile justice system may be less detrimental than in the adult system. In Michigan, juveniles found guilty of most offenses can apply for expungement either once they turn 18, or one year after disposition (whichever occurs later), which would limit the effect of a guilty disposition on expected future labor market outcomes.

On the other hand, detention could ensure that youth show up to their court dates, decreasing the probability of future penalties for not showing up to court. Detention could also improve youth outcomes via a deterrence mechanism. Early detention could increase a juvenile's perceived cost of involvement with the juvenile justice system. All else equal, these updated beliefs will make a youth less likely to engage in illegal behavior and more likely to invest in schooling.

III Data Sources and Analysis Sample

III.A Data Sources

In order to estimate the impact of JDC placement on educational attainment and criminal justice involvement, we construct a novel source of administrative data that link the universe of individual public school records in Michigan with juvenile petition records, juvenile detention records, and the universe of adult arrests, convictions, and incarcerations in the state. This section describes each of the administrative data sources used in the analysis and the process by which we link these records.

Individual Education Records and Juvenile Detention

Education data come from the Center for Educational Performance and Information (CEPI) and the Michigan Department of Education (MDE). This dataset contains the universe of K-12 public school student records in Michigan, including charter schools, from the 2002-03 to the 2019-20 academic years. A student appears in this dataset if he/she ever enrolled in a public or charter school in the state. The dataset contains information such as each student's sex, race/ethnicity, free or reduced-price lunch (FRPL) eligibility, and indicators for academic performance including attendance rates, standardized test scores, grade repetition, and high school graduation. Importantly, the education data also contain detailed information on each student's enrollment history during a given year. This allows us to identify all of the different schools that a student attended during an academic year, including whether the student was ever enrolled in an educational program offered at a JDC.

The Elementary and Secondary Education Act's Title I provides federal funding to improve the quality of education in facilities for neglected and delinquent youth. We used published reports on MDE's disbursement of these funds to identify 72 institutions in Michigan serving neglected and delinquent youth. These institutions include JDCs, youth commitment facilities, group homes, shelters serving abuse and neglected youth, and residential treatment centers. We researched each

of the 72 institutions in order to identify JDCs. We found that 20 of the 72 institutions were JDCs, while the remaining 52 institutions were mostly group homes and shelters serving victims of child maltreatment. To ensure we had isolated each JDC in Michigan, we compared our list to the names of JDCs that are part of the Michigan Juvenile Detention Association and to those institutions listed in the federal Juvenile Residential Facility Census (JRFC) Databook. Table A1 presents the names of the 20 Michigan JDCs where students in our sample are enrolled, as well as the total number of students enrolled in each.

Although individual enrollment records allow us to measure whether or not a student was placed in a JDC in any given year, the exact enrollment and exit dates for the JDC spell are either missing or implausible in 33% of JDC episodes.¹⁰ Therefore, our main analyses focus on the effects of JDC placement at the extensive margin, as opposed to treatment effects at the intensive margin (e.g., placement length).

We also assume that a spell in a JDC is for the purpose of pretrial detention. While the vast majority of placements in a Michigan JDC are indeed for detention (roughly 80%), a smaller share of placements are for juveniles waiting to be sentenced or transferred to another facility following a guilty verdict. From conversations with JDC administrators, a small share of cases (roughly 5-10%) could also be for longer-term, court-mandated treatment programs following sentencing. While our main analysis retains all placements in a JDC, we show that our results are robust to retaining only the set of episodes that we can be confident are not the result of post-trial sentences—those with non-missing data and lasting less than two weeks.

Juvenile Justice Records

Juvenile justice data come from the Michigan State Court Administrative Office (SCAO), and include all juvenile court petitions filed in 12 counties in Michigan between January 2008 and June 2019. These counties include Allegan, Alpena, Bay, Calhoun, Macomb, Midland, Monroe, Muskegon, Oakland, Saginaw, St. Clair, and Wayne County (home to Detroit), and together enroll roughly 52% of all students in the state. SCAO data completely exclude five urban and three rural counties in Michigan—Kent (home to Grand Rapids), Washtenaw (home to Ann Arbor), Ingham, Ottawa, Kalamazoo, Berrien, Delta and Keweenaw—and include minimal information for the remaining 63 counties.¹¹

A court petition is an official document filed following a juvenile arrest in cases where youth are not immediately diverted from the courts. Petitions can be dismissed by the court after filing and need not indicate that there was ever a formal court hearing. The juvenile justice records contain

¹⁰MDE records include information for students who are placed in a JDC during the summer.

¹¹Each county in Michigan operates its court system independently. While some counties report individual juvenile justice records to SCAO, they are not mandated to do so. The eight counties mentioned above do not report any individual records to SCAO, and 63 counties report minimal information (e.g., only for one or two years). This study includes only the 12 counties that do report full information to SCAO. We used publicly available, county-level reports from SCAO to validate the accuracy of the number of individual records reported by each of these 12 counties in each year.

information on the type of offense (e.g., violent, property, drugs, or public order), and the disposition of the petition (e.g., guilty plead, plead of no contest, dismissed, or no show).

An important limitation of the juvenile justice data involves the petition date. In theory, the petition date should be within a few days of the arrest date, and each arrest should correspond to a single petition, even if the arrest (and thus the petition) includes multiple offenses. However, based on our review of the data and discussions with SCAO officials, it appears that in some cases, multiple petitions (each with a different date) may be associated with the same arrest. For example, 20% (35%) of individuals with a petition have more than one petition filed within 7 (30) days of each other, and it is extremely unlikely that these correspond to distinct arrests. SCAO officials indicate that this may happen because updates to the charges of existing petitions may be wrongly coded as new petitions. One implication of this data limitation is that we cannot determine whether a youth committed additional offenses shortly after the initial incident. This precludes us from studying the impact of JDC placement on pretrial crime.

Adult Arrest Records

Adult arrest records come from the Michigan State Police (MSP), and include the universe of adult arrests (≥ 17 years old) in Michigan from January 2012 to May 2020. The dataset includes information such as each individual’s arrest date and the alleged offense. We use this dataset to construct adult crime outcomes including an indicator for whether the child was ever arrested in Michigan by age 19, arrest status by particular types of crime (e.g., violent or property), total number of arrests, and whether the individual was arrested on a felony or misdemeanor charge. MSP data also contain judicial information, which allows us to observe whether an individual was ever convicted and/or incarcerated by age 19.

III.B Matching Across Administrative Data Systems

The Michigan Education Data Center (MEDC) linked the public education records, juvenile justice, and adult crime data using a probabilistic matching algorithm. These data sources do not contain a common identifier, so MEDC staff linked the data based on first name, last name, date of birth, and gender using the Fellegi-Sunter model implemented via the *fastlink* R package (Enamorado, Fifield and Imai, 2019). Because MEDC manages the education data, the K-12 public school students served as the base population for the match. Staff linked the K-12 education data with the juvenile justice data and then matched the K-12 education data to the adult crime records. Both linkages performed well. For each of the matched records, the software rates the certainty level of the match using a posterior probability. Overall, 96% of records in the juvenile justice data matched to a public school student record and 93% of records in the adult crime data matched to a public school record with a high degree of certainty (over 99.6%). This match rate was nearly identical for males and females, and MEDC staff closely validated the match by manually matching a randomly selected

subset of 200 records. The match rate is quite high, given that some individuals arrested in Michigan could have gone to school in a different state, been enrolled in a private school, or been homeschooled.

III.C Analysis Sample

Table A2 describes in detail each step in the construction of our analysis sample. The starting point for our sample consists of first-time sixth-graders in Michigan public schools (including charters) during the 2007-08 through 2012-13 academic years. We construct an unbalanced panel at the student-by-year level, where we follow students from sixth grade and for up to seven years (to allow one additional year from on-time graduation). Individual students can drop out of the sample if they either drop out of high school, transfer to a private school, become homeschooled, or leave the state.

In step 1, we retain only the first JDC episode of individuals ever in a JDC. The median student with any JDC placements only had one episode, and the mean number of episodes is 1.6. In the main body of the paper we focus only on the first JDC episode since later episodes may be endogenous to initial JDC treatment effects. However, our results are robust, both in magnitude and statistical significance, to including each individual episode in the analysis instead. In step 2, we restrict the sample to JDC episodes in grades 7 through 12. We do this to be able to observe at least one year of baseline characteristics. Step 3 restricts the sample to student-by-year observations in the 12 counties for which we have juvenile justice data, as this is a critical variable in the matching strategy.

We observe 7,928 individual students with at least one episode in a JDC at some point between 7th and 12th grade in our 12 sample counties. However, for 2,633 (33%) of these individuals, there is no corresponding juvenile petition in the juvenile justice files in the year of their JDC placement. This is primarily driven by individuals who may be placed in a JDC for an extremely short period of time, and whose cases might be diverted from the formal court system prior to the filing of a formal petition with the court. We exclude these 2,633 students from the analysis because they are systematically different from individuals who were placed in a JDC and a subsequent petition was filed, as shown in Table A3.¹² Finally, we drop student-by-year observations in which key variables used in the match are missing, which only results in excluding 7 additional students with JDC exposure.

Table 1 describes our analysis sample, comparing students who were ever placed in a JDC to those who were not. There are 383,485 students in our sample. 5,288 of these students, or 1.4%, were ever placed in a JDC. The table reveals large demographic disparities in placement. 57% of students placed were Black despite their making up just 27% of the overall student population. 85% of placed students were economically disadvantaged despite their making up just 50% of the population; 71% of students placed in a JDC were male.

In sixth grade, before the vast majority of students had any experience with the juvenile justice system, there were already large differences in academic outcomes between students who would later

¹²Specifically, they are more likely to be White, less likely to be economically disadvantaged, and have better educational outcomes in grade 6: higher test scores and attendance rates, and lower rates of chronic absenteeism and IEP receipt. They are also more likely to graduate high school and less likely to be arrested as adults.

be placed in a JDC and those who would not. Students eventually placed in a JDC had much lower attendance rates and standardized test scores in sixth grade. The table also shows that youth who would later spend time in a JDC attended sixth grade schools that performed much worse on standardized tests, had a substantially higher share of dropouts, a higher share of Black students, and a higher share of students receiving FRPL.

Finally, children placed in a JDC had substantially worse critical outcomes. Notably, only 19% of placed students graduated high school, compared to 76% of students not placed. Similarly, nearly half of placed students were later arrested in Michigan by age 19, compared to only 6% of students not placed. Similar discrepancies in outcomes exist for the probability of being convicted and incarcerated by age 19.

Table A4 tells a similar story. It reports the probability of ever being placed in a JDC conditional on various characteristics of children in our analysis sample. The table reveals large differences across child characteristics in the probability of ever being placed in a JDC. For instance, while White females have less than a 1% chance of ever being placed, Black males have roughly a 4% chance. White students eligible for FRPL have a 1.7% chance of ever being placed, whereas eligible Black students have over a 3% chance. Students with an individualized education plan (IEP) in sixth grade, which automatically triggers special education services, have a 3% chance of being placed, relative to a 1% change for students with no IEP. Finally, students in the top quartile of the sixth-grade test score distribution have virtually no chance of ever being placed, whereas students in the bottom quartile have a 3% chance.

Figure 2 describes the experiences of students in JDC in our analysis sample. Panel A shows that most students first experience juvenile detention during grades 9–10. There is a small share of students who are first placed during grade 11 and grade 12, but the probability of first placement peaks in grades 9 and 10. Panel B shows that a significant share of placed students ($\approx 9\%$) spend only one day in a JDC. Roughly 33% of placed students were in the JDC for less than one week. The median number of days in a JDC, conditional on any placement, is only 15 days in our sample.¹³

IV Empirical Strategy

Simply comparing the outcomes of individuals who were placed in a JDC to those of individuals who were not placed is unlikely to recover causal estimates of the effects of JDC placement. As shown in Table 1, individuals placed differ from those not placed in a number of ways, even long before placement occurs. Previous studies examining the effects of post-trial juvenile incarceration (Aizer and Doyle Jr, 2015; Eren and Mocan, 2021), and those examining the effects of pretrial detention in the adult criminal justice system (Dobbie, Goldin and Yang, 2018; Leslie and Pope, 2017), have exploited the quasi-random assignment of judges to cases to estimate treatment effects. However, as mentioned above, such variation does not exist in our context, where police officers and a small

¹³Note that Panel B includes only the 67% of episodes for which we have reliable information on episode length.

number of court referees are responsible for the relatively informal decision to temporarily detain or not.

Instead, this paper employs a selection-on-observables approach that combines exact matching and IPW to recover the causal effects of JDC placements. While this strategy relies on relatively stronger assumptions than other quasi-experimental methods, the granularity and size of our microdata allow us to (1) compare individuals with quite similar observable characteristics prior to their JDC placement, and (2) conduct a number of sensitivity/placebo checks that bound the true treatment effects.

As noted above, our treatment group consists of individuals who had a juvenile petition and were placed in a JDC in a given year ($n=5,288$ unique youth). There are 2,432,884 potential control units, reflecting student-by-year observations for the cohorts in our sample counties described above. For each treated observation, we identify exact matches based on the filing of a juvenile petition that year along with student race, sex, grade, and school district (all in that same year). Of the 5,288 treated individuals in our analysis sample, 3,632 (68%) appear in a match group with at least one treatment and one control youth. We refer to these as non-degenerate match groups.¹⁴ There are 6,461 student-by-year observations (5,702 unique students) who did not spend time in a JDC but who appear in a non-degenerate match group, and thus form our set of control students.

Using the sample of 10,093 student-by-year observations in non-degenerate match groups, we use a logit model to estimate the predicted probability of being placed in a JDC. Specifically, we estimate a logit model where the dependent variable is an indicator for whether or not the youth was placed in a JDC and predictors include a wide range of pre-detention measures. Indeed, one of the main strengths of the analysis is the rich set of covariates we can include not only in the propensity model, but as controls in the outcome equation.¹⁵ Prior *educational measures* include academic achievement scores, disciplinary incidents (e.g., suspension or expulsion), attendance, services received for learning disabilities or for English Language Learners, and various measures of the child’s school such as enrollment size, school type, average poverty rate and academic performance. *Child welfare measures* include whether the student was previously ever the subject of a child maltreatment investigation or was placed in foster care. Prior *juvenile justice measures* include whether or not the student had contact with the system in the previous two years, (conditional on contact) the type of previous petition (e.g., violent or property), and whether the student was adjudicated (convicted). In addition, we control for factors associated with the student’s “target” petition, such as the most serious offense in the petition (e.g., violent or property).

Figure 3 shows the distribution of propensity scores separately for treatment and control youth in non-degenerate match groups. While the distribution of treated youth is shifted to the right as one

¹⁴Table A5 compares the characteristics of treated youth in a degenerate and non-degenerate match group. Treated youth in a non-degenerate match group are more likely to be Black, male, and low income. They also have worse baseline educational outcomes such as lower attendance rates and test scores. These students are also more likely to be eventually arrested, and are less likely to graduate from high school.

¹⁵Table A6 shows the results of the estimation of the propensity score model, as well as each of the variables that we include.

would expect, there is considerable overlap between the two distributions. To generate an estimate of the average treatment effect on the treated (ATT), we estimate the following Linear Probability Model:

$$Y_i = \gamma JDC_i + X_i\beta + \theta_g + \epsilon_i \quad (1)$$

where Y_i is the outcome of interest (e.g., whether juvenile i graduated high school or whether he/she was ever arrested as an adult); JDC_i is an indicator variable equal to one if the juvenile was placed in a JDC and is zero otherwise; X_i is the same vector of covariates that was used to estimate the propensity score model, and θ_g represents match group fixed effects. We weight control units by $\frac{\hat{\pi}}{1-\hat{\pi}}$, where $\hat{\pi}$ is the predicted propensity. Treatment units receive a weight of one. We cluster standard errors at the child and group levels, in order to account for both (1) the mechanical correlation that arises by including the same control student more than once in the estimation, and (2) potential correlations in the outcomes of individuals in the same match groups.

With the additions of IPW and regression adjustment in Equation 1, the estimator $\hat{\gamma}$ is a “doubly-robust” two-step estimator of γ . In other words, estimates of the effects of JDC placement will be unbiased if either the underlying matching model or the regression model is correctly specified. Regression adjusted matching estimators perform better in practice relative to either regression or matching on their own (Imbens and Wooldridge, 2009).

The identification assumption of our approach is that conditional on the observable characteristics included in the match and in the regression, JDC placement is as good as random between treated and control students. The primary threat to identification is therefore that unobservable student characteristics may be correlated with JDC placement. For instance, even after accounting for the type of petition (e.g., violent offense), a police officer may be more likely to recommend placement if the juvenile behaved in ways that we cannot observe. While our analysis cannot rule out omitted variable bias, we perform a number of robustness and specification checks that allow us to test the extent to which our estimates are driven by remaining unobserved heterogeneity, and to bound the true treatment effects.

IV.A Pre- and Post-Match Balance

The first three columns in Table 2 show pre-treatment covariate balance in our sample between the 5,288 treatment observations and 2,432,884 potential student-by-year control units. Prior to matching, there are substantial differences in demographics, prior academic achievement, and school characteristics. For instance, individuals placed in a JDC are 30 percentage points more likely to be Black, 36 percentage points more likely to be economically disadvantaged, score 81 percentage points of a standard deviation lower on test scores, and are 14 percentage points more likely to be suspended in the year before the JDC episode.

Column 4 shows differences in covariates between the matched treated and control observations.

Specifically, the point estimates in Column 4 come from a regression of the particular outcome on a treatment indicator, match group fixed effects, and inverse probability weights—as described above. This specification is estimated on the sample of 10,093 observations in non-degenerate match groups. The differences in this column shrink dramatically. By construction, there are no differences in the key demographics that were used in the exact match. Furthermore, in the year prior to the JDC episode, treated and control observations have similar test scores, attendance, IEP rates, and suspension rates. They also attend similar schools. Even though the point estimates in Panel C are statistically significant, they are modest in magnitude. For instance, treated students attend schools with a share of White students that is 1.8 percentage points lower than those of control students. Altogether, the results in the table indicate that treated and control students are quite similar along observable characteristics in the year prior to placement. We additionally control for all of these characteristics in the regression adjustment.

V Results

Table 3 presents our main results. The first row of each panel shows estimates of γ from Equation 1. Standard errors are clustered at the child and match group levels, and shown in parentheses below the point estimates. The table also presents the control mean—the average value of the dependent variable among individuals who were not placed—and the effect in percent terms—the point estimate divided by the control mean. Panel A shows estimates for high school graduation, while Panel B shows estimates for whether the student was eventually arrested as an adult.

Each column presents estimates from a separate regression. Column 1 presents a simple difference in means between treated youth ($n=5,288$) and all potential control observations ($n=2,432,884$). As suggested by the results in Table 1, students who were placed in a JDC have substantially worse outcomes relative to those who were not placed. They are 61 percentage points (76%) less likely to graduate high school and 42 percentage points (662%) more likely to be arrested by age 19.

Column 2 shows the difference in outcomes among youth in non-degenerate match groups ($n=3,632$ treated youth and 6,461 control observations). The magnitude of the estimates shrinks substantially in this restricted sample, but still shows large negative effects of JDC placement on outcomes. Each additional column further restricts the comparison: Column 3 also includes match-group fixed effects to restrict comparisons to individuals within the same group; Column 4 additionally implements IPW; Column 5, which also incorporates post-matching regression adjustment, is our preferred specification and the one we use throughout the remainder of the paper. These estimates indicate that placement is associated with an 8 percentage point (31%) lower likelihood of high school graduation and a 9.4 percentage point (25%) greater likelihood of arrest by age 19.

Table 4 presents estimates of JDC placement on other criminal justice outcomes. Beyond its impacts on adult arrests, detention also leads to an increase in the probability of a conviction by age 19 of 6 percentage points (30%) and incarceration of 6.6 percentage points (40%). JDC placement

has larger impacts on the probability of an arrest for a felony offense (40%) relative to a misdemeanor offense (21%). While we find that JDC placement increased the probability of an adult arrest across all major types of offenses, we find a particularly large increase in the probability of being arrested for a violent offense (39%).

Table 5 shows that JDC placement has large, negative effects across subgroups. For the effects on high school graduation, we find that the negative effects of JDC placement are larger for male and Black students. For adult arrests, the point estimates are also larger and more positive for male and Black students. However, given the differences in the control means for each group, the detrimental effects of placement are similar in percent terms for male and female students, and for Black and White students as well.¹⁶ We also find that the effects of detention on adult criminality are much larger (26% versus 19.5%) for students placed in detention due to a non-violent offense, relative to students detained for a violent offense.

V.A Sensitivity Analyses

Treatment Effect Bounding

The estimates above suggest that JDC placement has substantial negative effects on youths' critical outcomes. However, matching techniques cannot control for unobserved factors that may be correlated with both JDC placement and youth outcomes. Given the large number of highly predictive controls that we include in the propensity and outcome models, we suspect that remaining unobserved heterogeneity should be small. However, in order to determine how sensitive our results may be to omitted variable bias, we perform two exercises proposed by Oster (2019).

Building on earlier work by Altonji, Elder and Taber (2005), Oster (2019) proposes two complementary methods to assess the robustness of estimates of treatment effects to omitted variable bias. The first is to estimate a bias-adjusted treatment effect, which represents the value of the treatment effect assuming a given degree of selection on unobservables. Bias-adjusted treatment effects allow us to bound the true treatment effect of JDC placement. The second method is to examine the amount of selection on unobservables—relative to selection on observables—that would be needed for the true treatment effect to be equal to zero. If a relatively large amount of selection on unobservables is required, then treatment effect estimates can be considered robust to omitted variables bias. Both of these exercises require a proportional selection assumption: that selection on unobservables is proportional to selection on observables.

Table 6 presents the results of these two exercises. Panel A reports the results for high school graduation, while Panel B reports those for arrests by age 19. Column 1 shows our preferred baseline estimate of the effect of JDC placement, as previously reported in Column 6 of Table 3. This estimate would be equal to the estimate of the treatment effect if there were no remaining omitted variable bias. Column 2 shows the bias-adjusted treatment effect, assuming that the amount of selection on

¹⁶This is consistent with the results in Dobbie, Goldin and Yang (2018), which shows that the negative effects of detention are similar for White and Black individuals.

unobservables is equal to the amount of selection on observables. Both [Oster \(2019\)](#) and [Altonji, Elder and Taber \(2005\)](#) suggest this as an upper bound on the amount of omitted variable bias. Together, Columns 1 and Column 2 report bounds for the true treatment effect. Finally, Column 3 shows the amount of proportional selection needed such that the treatment effect would equal zero. Values smaller in magnitude than one indicate that selection on unobservables would not need to be as large as selection on observables, whereas values larger than one mean that selection on unobservables would need to be larger than the amount of selection on observables.

Our baseline estimates of JDC placement do not appear to be driven by omitted variable bias. We can tightly bound the effect on high school graduation to be -8 percentage points, and that for an arrest by age 19 between 8.1 and 9.4 percentage points. For both outcomes, selection on unobservables would need to be over four times as large as selection on observables in order for the true effect to equal zero. While our selection-on-observables identification strategy relies on strong assumptions, the results in [Table 6](#) suggest a large and detrimental causal effect of JDC placement on high school graduation and adult criminality.

The Effects of JDC Placement, Conditional on Case Outcomes

Column 1 of [Table 7](#) shows the impact of JDC placement on an indicator variable for whether or not the juvenile was adjudicated (convicted) in that academic year.¹⁷ The table shows that JDC placement is associated with a 17 percentage point (32%) increase in the likelihood of adjudication in the academic year. This estimate could be the result of two underlying channels. First, it could be that there is a causal effect of detention on case outcomes for juveniles. As mentioned above, previous studies have shown that pretrial detention in the criminal justice system increases the probability of conviction by lowering defendants' bargaining position (thereby increasing the likelihood of a guilty plea), and by potentially biasing judge's expectations ([Dobbie, Goldin and Yang, 2018](#); [Leslie and Pope, 2017](#)). Alternatively, this pattern could indicate the presence of remaining unobserved heterogeneity in our estimates. Specifically, the negative case outcomes may be a signal that, even conditional on all other controls, students with negative unobservables may be more likely to be placed in a JDC and, at the same time, more likely to be adjudicated.

In the remaining columns of [Table 7](#), we examine how controlling for case outcome influences the relationship between JDC placement and high school graduation and adult arrests. The intuition for this exercise is that if case disposition were a mechanism through which JDC placement operates, then we would expect that controlling for it would attenuate our JDC estimates. Similarly, if a guilty verdict captures negative unobservable characteristics, then we would expect the inclusion of this measure to attenuate the JDC effect.

As expected, we find that the case outcome is a strong predictor of youth outcomes. Even with the rich set of controls in the models, youth who are found guilty are 2 percentage points less likely to

¹⁷Specifically, a youth is adjudicated if he/she either pleaded guilty or no contest, or was found guilty by a judge. The youth was not convicted if the case was dismissed or he/she was found not guilty by a judge.

graduate high school and 3 percentage points more likely to be arrested as an adult than their peers who were not adjudicated. At the same time, the inclusion of this variable has virtually no impact on the estimated JDC coefficient. We view this result as evidence that (unlike the adult criminal justice system) JDC effects may not operate through case dispositions. This is an important finding, as an alternative explanation for our results is that detention harms future outcomes through its effects on the probability of youth incarceration.¹⁸

The results in Table 7 also offer evidence that the identifying assumption of our model is likely to hold. Specifically, the case disposition captures a host of factors that were available to police, judges, and other individuals involved in the detention decision, but are unobservable to us. The fact that controlling for this additional variable does not appreciably change our estimates is additional evidence that unobservable factors are not biasing our estimates.

Out-Of-State Migration

Because our administrative dataset contains the universe of adult arrests in Michigan, one may be concerned that it does not allow us to observe arrests for individuals who moved out of the state. In other words, a specific concern may be that detention does not increase criminal behavior, but rather that it simply increased the likelihood of staying in the state, and thus being observed in our arrest data.

Differential out-of-state migration in our context is unlikely, since most treated youth are detained around ages 14–16 and criminal justice contact is measured by age 19—leaving a relatively short time period to move out of the state. Nevertheless, we explore this concern in Table A8. In Column 2, we estimate our main specification but on a sample excluding any student who ever left the state in K-12. We measure this outcome using exit codes that are assigned to students who leave the Michigan Public School system. In Column 3, we exclude any student who ever attended a postsecondary education program (either two-year or four-year) from the sample. We measure this outcome using nationwide postsecondary enrollment information from the National Student Clearinghouse, which MEDC matched to the universe of Michigan public school records. The point estimates from these two distinct samples continue to show that detention led to large increases in the probability of being arrested by age 19. These results yield little evidence that our main estimates are driven by out-of-state migration.

Additional Robustness

Table 8 shows that our results are robust to several alternative specifications. Column 1 presents our baseline estimates. In Columns 2–4, we continue to implement exact matching and regression adjustment as in our main strategy. However, as opposed to implementing propensity score matching

¹⁸To further explore this concern, Table A7 replicates our main results in Table 3, but only on the subset of individuals who were not convicted (and were therefore not incarcerated). The effect sizes (in percent terms) are strikingly similar to our main point estimates.

via IPW, we test the robustness of our main estimates to alternative methods. In Column 2, we implement kernel matching with an Epanechnikov kernel function. In Columns 3 and 4, we use our predicted propensity scores to implement nearest neighbor matching instead. Column 3 shows the results when using a single nearest neighbor, while Column 4 shows the results when using the three nearest neighbors instead. All specifications yield similar estimates both in magnitude and statistical significance.

Finally, we assumed throughout the paper that a spell in a JDC was for the purpose of pretrial detention. While the vast majority of placements in a Michigan JDC are for detention purposes (roughly 80%), a smaller share of placements can be either for juveniles waiting to be sentenced or transferred to another facility following a guilty verdict, or for longer-term, court-mandated treatment programs following sentencing. Our main analysis retained all placements in a JDC. Table A9 replicates our baseline estimates in Table 3, but retaining only the set of 1,671 episodes with non-missing length information and that lasted less than two weeks. Thus, we retain only episodes that we can be confident were not the result of post-trial sentences—which are typically substantially longer. The estimates from this exercise are similar to those in Table 3 and point to substantial negative effects of JDC placement on later-in-life outcomes.

V.B Mechanisms

The results discussed above indicate that detention has substantial negative impacts on students’ critical outcomes. In order to shed light on which mechanisms may be driving these effects, Table 9 shows how several educational and juvenile justice outcomes evolve over time following JDC placement.¹⁹

High school dropout is cumulative in the sense that it is defined as permanently leaving school. That is, students who do not enroll in school in $t + 1$ but who enroll in $t + 2$ are *not* coded as having dropped out in year $t + 1$.²⁰ The remaining variables all refer to the current year and are coded as missing if the student has dropped out or simply not enrolled in that year. For example, if a student was placed in a JDC in 2014-15, the attendance, IEP receipt, and school mobility variables for $t + 1$ refer to the 2015-16 academic year. The $t + 1$ grade retention variable for this student indicates whether he/she was enrolled in the same grade in 2015-16 as in 2016-17.

The first column shows the “effects” of detention two years *prior* to placement. These estimates serve as a “placebo” or an additional robustness check.²¹ Reassuringly, we find that all of the “pre-intervention” estimates are small and statistically insignificant. The remaining columns show results for the first five years following JDC placement.

¹⁹Because students do not take standardized achievement tests regularly in high school and we do not have access to student grades, our analysis is limited to measures such as attendance and school mobility.

²⁰Note that youth who are detained will appear enrolled in a juvenile detention-based educational program, and will not be counted as dropouts.

²¹We do not show results one year prior to placement because many of the one-year lagged outcomes are used in the matching strategy in Equation 1.

Interestingly, we find that JDC placement may increase school engagement in the very short run. For example, students are 1.8 percentage points (roughly 25%) *less* likely to drop out of school in the year following JDC placement than observationally equivalent peers who were not detained. Conditional on school enrollment, students who spent time in a JDC were 11 percentage points (17%) less likely to be chronically absent. Students were also more likely to be retained in grade in the year following JDC placement (3.4 percentage points or 15%), and were 10.4 percentage points (32%) more likely to have an IEP, which requires them to receive special education services. Together, these results suggest that school officials focused more attention on students who were placed in a JDC and attempted to provide them with additional support services, relative to observably similar peers who had a juvenile petition but were not placed in a JDC.

Increases in support services do not appear to translate into better outcomes, however. In the year following their original juvenile arrest, youth who were detained are substantially more likely to be re-arrested as juveniles (21 percentage points, or more than 100%). These patterns persist for several years. Formerly detained youth remain attached to school at similar or greater rates than their peers, but continue to experience more juvenile arrests.

By the fourth and fifth years post detention, students who were detained are 2.4 percentage points (6%) and 3.4 percentage points (8%) more likely to drop out of school, respectively. During this time, those who were detained are also far more likely to be arrested in the adult criminal justice system. These results suggest that the initial supports provided to JDC students were not effective, or at least not sufficiently beneficial to overcome the human capital disruption and criminogenic effects of detention.

V.C Potential Benefits of Juvenile Detention

Proponents of juvenile detention point to two potential benefits: (1) ensuring that youth show up for their hearing (trial) and (2) ensuring public safety by preventing youth from committing other crimes prior to their court date.

Table A10 provides evidence relevant to the first of these purported benefits. We find that juvenile detention leads to a decline of 0.8 percentage points (36%) in the probability that the youth had a disposition of “no show” in a juvenile petition during that academic year. While this can be considered a benefit, the magnitude is quite small since only 2.2% of non-detained youth fail to show up for their trial.

We are unable to rigorously explore the effects of detention on public safety. First, we do not observe the exact enrollment and exit date for a large subset of JDC spells. Moreover, as described in Section III, we have reason to believe that a single arrest may produce multiple petitions within the month following the initial incident. As a result, we are unable to examine whether JDC placement prevents an individual from committing a new crime while awaiting trial. However, in a recent review article, [Dobbie and Yang \(2021\)](#) conclude that pretrial detention in the adult system has little effect on criminal activity during the pretrial period. Compared with the adult system, the length

of pretrial detention in the juvenile system is short. The median (average) length of detention in our sample is only 15 (36) days, and approximately one third of youth are detained for less than a week. In contrast, the average length of pretrial detention in the adult system ranges from 50 to 250 days—depending on the offense type (Liu, Nunn and Shambaugh, 2018). The relatively short length of detention in the juvenile system suggests there is even less scope for threats to public safety among non-detained youth.

VI Conclusion

Roughly 25% of youth involved with the juvenile justice system spend time in a juvenile detention center prior to their court date. Black, Hispanic, and American Indian youth are disproportionately represented in juvenile detention centers. The existing literature on the effects of pretrial detention in the *adult* system Dobbie and Yang (2021) yields strong reasons to believe that detention in the *juvenile* justice system may harm youths’ later-in-life outcomes. To the best of our knowledge, we provide the first empirical evidence on this topic.

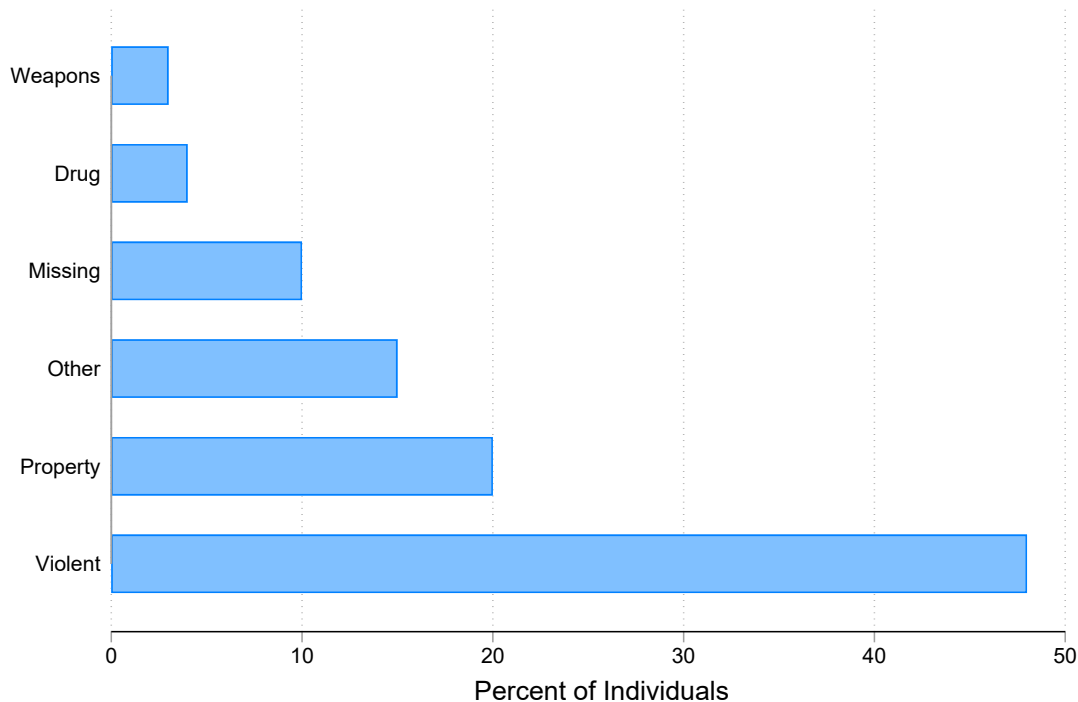
We find that juvenile detention significantly harms critical outcomes. Specifically, we find that detention leads to a 31% decline in the probability of high school graduation, and a 25% increase in the probability of an adult arrest. The sensitivity analyses we present provide strong evidence to support a causal interpretation of the estimates.

We present evidence that these costs likely overwhelm any potential societal benefits of detention. There is compelling evidence that the benefits of pretrial detention for adults (ensuring defendants show up to court and reducing the likelihood that they commit additional crimes before their trial) are small (Dobbie and Yang, 2021). Given that juveniles are accused of less serious offenses on average and the length of detention for juveniles is considerably shorter than for adults, it is likely that the benefits of detention in the juvenile justice system are even smaller. For example, fewer than 2% of youth in our sample fail to show up for their court date. Although we find that JDC placement reduces the likelihood of no-shows by 36%, the absolute magnitude of these benefits are quite small due to the very low baseline rate. While data limitations prevent us from determining whether JDC placement significantly reduces crimes committed during the period prior to a juvenile’s trial, these effects would have to be extremely large for the benefits of detention to outweigh its costs.

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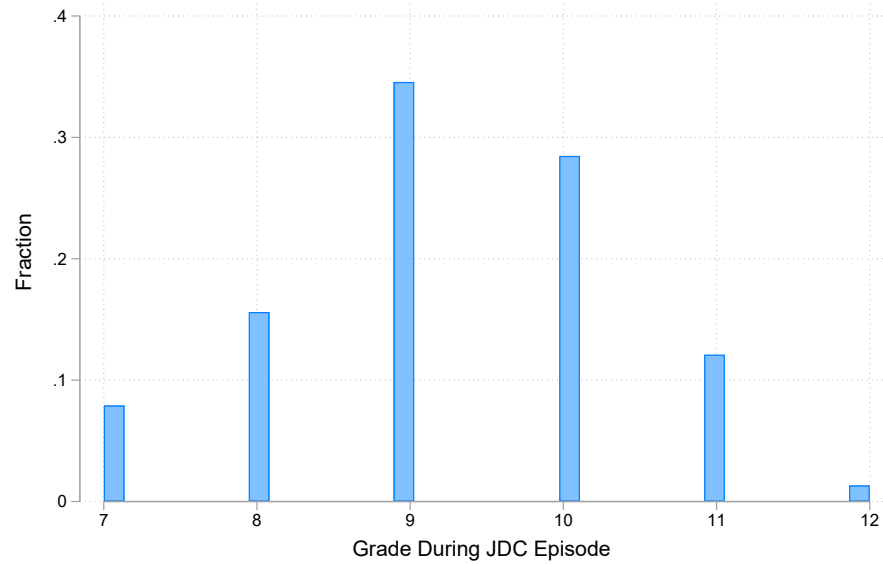
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Figure 1: Distribution of Most Serious Offense in Juvenile Arrest Leading to Detention

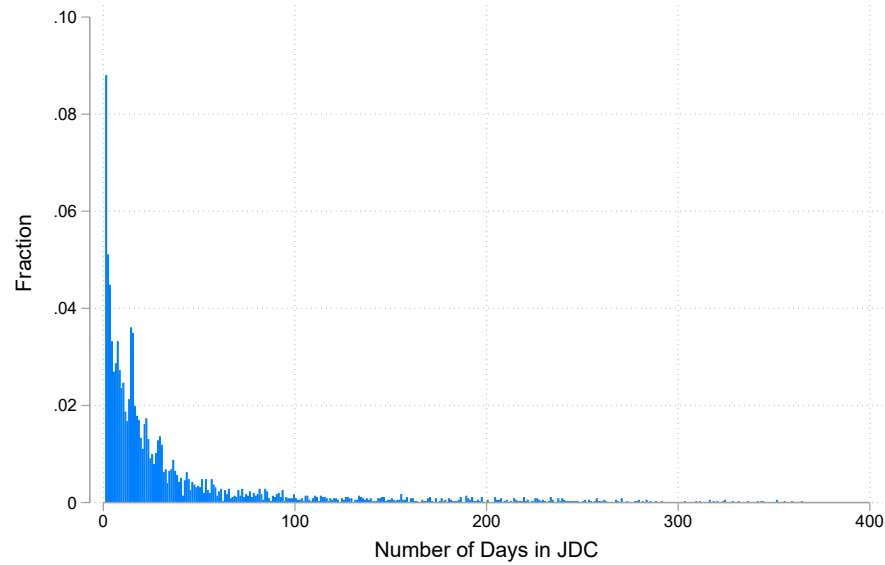


Notes: This figure plots the distribution of the most serious offense that a youth was arrested for in the academic year that he/she was detained prior to his/her court date. For instance, the figure shows that 48% of youth in detention in a given academic year were arrested for a violent offense in that year. We classify a violent offense as the most serious offense, followed by property, drug, weapons, and other offenses. The figure shows the distribution of most serious offenses for the 5,288 treated youth in our sample, described in detail in the main text. The variable measuring type of offense is missing for 10% of youth in our analysis sample.

Figure 2: Grade Distribution and Length of JDC Spells



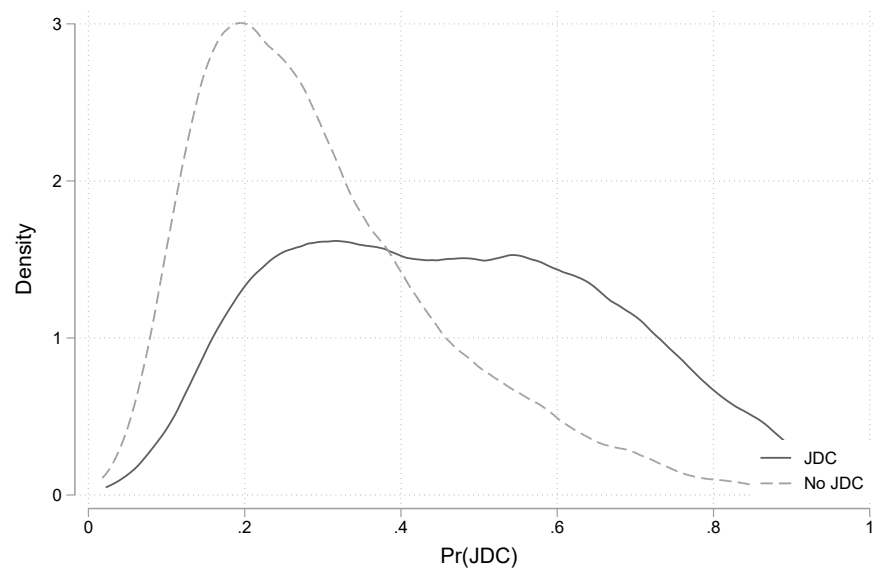
(a) Grade Distribution



(b) Length Distribution

Notes: This figure describes the experiences of youth's first episode in a JDC. Panel (a) shows the distribution of first placements by grade. Panel (b) shows the length distribution (in days), and includes only the 67% of episodes for which we have reliable information on episode length.

Figure 3: Distribution of Propensity Scores by Treatment Status



Notes: This figure shows the distribution of predicted propensity scores separately for the treatment and control groups. The prediction was obtained via a logit model, where the dependent variable is an indicator variable equal to one if the student was placed. See Table A6 for the complete list of predictors included in the model.

Table 1: Summary Statistics

	All Students	Ever JDC	Never JDC
Socio-Demographic Characteristics			
White	0.635	0.379	0.639
Black	0.273	0.571	0.269
Hispanic	0.050	0.042	0.050
Other	0.041	0.008	0.042
Economically disadvantaged	0.498	0.850	0.493
Female	0.491	0.292	0.493
Academic Outcomes in Grade 6			
Attendance rate	0.947	0.876	0.948
Chronically absent	0.131	0.427	0.127
IEP	0.146	0.315	0.144
Test scores	-0.031	-0.763	-0.021
Grade 6 School Characteristics			
School average mathematics score	-0.012	-0.383	-0.007
School 4-year dropout rate	0.106	0.256	0.103
Charter school	0.108	0.111	0.108
Share of students receiving FRPL	0.406	0.595	0.403
Share of students who are Black	0.264	0.482	0.261
Share of students who are Hispanic	0.047	0.056	0.047
Juvenile Justice			
Ever juvenile petition	0.046	1.000	0.032
Adult Outcomes			
Ever graduated high school	0.751	0.194	0.759
Ever arrested in Michigan	0.070	0.488	0.064
Ever arrested for a violent offense	0.017	0.234	0.014
Ever arrested for a property offense	0.027	0.255	0.024
Ever arrested for a drug offense	0.020	0.124	0.019
Ever arrested for a public order offense	0.031	0.268	0.027
Ever arrested for a felony offense	0.024	0.315	0.020
Ever arrested for a misdemeanor offense	0.058	0.369	0.053
Ever convicted	0.026	0.289	0.022
Ever incarcerated	0.021	0.243	0.018
Number of observations	383,485	5,288	378,197
Share of observations	1.00	0.014	0.986

Notes. This table reports summary statistics for three groups of students. Column 1 consists of the population of Michigan public school students in the analysis sample. Column 2 contains all students in the sample who were ever placed in a JDC, while Column 3 contains all students in the sample who were never placed.

Table 2: Balance

	Unmatched Sample		Matched Sample	
	Ever JDC	Never JDC	Difference	Difference
Panel A: Socio-Demographic Characteristics				
White	0.38 [0.49]	0.64 [0.48]	-0.262*** (0.007)	0.000 (0.000)
Black	0.57 [0.49]	0.27 [0.44]	0.301*** (0.007)	0.000 (0.000)
Hispanic	0.04 [0.20]	0.05 [0.21]	-0.006** (0.003)	0.000 (0.000)
Other	0.01 [0.09]	0.04 [0.20]	-0.033*** (0.001)	0.000 (0.000)
Female	0.29 [0.45]	0.49 [0.50]	-0.202*** (0.006)	0.000 (0.000)
Age	16.07 [1.12]	15.42 [2.09]	0.655*** (0.015)	-0.028 (0.018)
Economically Disadvantaged	0.84 [0.37]	0.47 [0.50]	0.361*** (0.005)	0.009 (0.008)
Juvenile petition this year	1 [0.00]	0.01 [0.08]	0.994 (0.000)	-0.000 (0.000)
Panel B: Academic Outcomes in t-1				
Share of School Days Attended	0.79 [0.20]	0.8 [0.35]	-0.004 (0.003)	-0.016*** (0.005)
Standardized Test Scores	-0.84 [0.71]	-0.03 [0.85]	-0.808*** (0.010)	-0.027 (0.018)
Chronically Absent from School	0.49 [0.50]	0.11 [0.31]	0.386*** (0.007)	0.017 (0.012)
IEP	0.32 [0.47]	0.11 [0.32]	0.204*** (0.006)	-0.006 (0.014)
Suspended (In School or Out of School)	0.15 [0.36]	0.02 [0.13]	0.137*** (0.005)	0.003 (0.010)
Old for Grade	0.39 [0.49]	0.18 [0.39]	0.206*** (0.007)	-0.002 (0.014)
Panel C: School Characteristics in t-1				
Share of FRPL Students	0.74 [0.23]	0.47 [0.27]	0.265*** (0.003)	0.029*** (0.003)
Average Test Scores	-0.6 [0.42]	-0.03 [0.39]	-0.563*** (0.006)	-0.059*** (0.008)
Share of White Students	0.41 [0.32]	0.64 [0.33]	-0.230*** (0.004)	-0.018*** (0.004)
Number of Observations	5,288	2,432,884	2,438,172	10,093

Notes. The table presents pre-treatment covariate balance. Column 1 consists of the 5,288 treated observations in the analysis sample. Column 2 consists of the 2,432,884 potential student-by-year control observations. Column 3 presents the point estimates of a test for equality of means between individuals in Columns 1 and 2. Column 4 presents the results of a regression of the outcome on an indicator for JDC placement, but re-weighted by the propensity score and including exact match group fixed effects. The sample in Column 4 includes 3,632 treated and 6,461 control observations, respectively. The individuals in this sample are those in non-degenerate match groups. Standard deviations are shown in square brackets in Columns 1 and 2, while standard errors are clustered at the child and group levels, and shown in parentheses below the point estimates in Columns 3 and 4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduated High School						
Placed in a JDC	-0.608*** (0.007)	-0.187*** (0.010)	-0.172*** (0.010)	-0.100*** (0.010)	-0.089*** (0.010)	-0.080*** (0.011)
Observations	2,438,172	10,093	10,093	10,093	10,093	10,093
R-squared	0.005	0.038	0.243	0.257	0.339	0.343
Control Mean	0.802	0.368	0.368	0.261	0.368	0.261
Percent Effect	-75.8	-50.7	-46.8	-38.2	-24.3	-30.5
Panel B: Arrested by Age 19						
Placed in a JDC	0.424*** (0.008)	0.176*** (0.011)	0.168*** (0.011)	0.136*** (0.013)	0.094*** (0.013)	0.094*** (0.014)
Observations	2,287,081	9,662	9,662	9,662	9,662	9,662
R-squared	0.006	0.030	0.231	0.274	0.264	0.314
Control Mean	0.064	0.326	0.326	0.381	0.326	0.381
Percent Effect	662.3	53.9	51.6	35.7	28.8	24.7
Students in Non-Degenerate Groups		Y	Y	Y	Y	Y
Group FE			Y	Y	Y	Y
IPW				Y		Y
Regression Adjustment					Y	Y

Notes. The table presents our main results. The first row of each panel shows estimates of γ from Equation 1. Standard errors are clustered at the child and match group levels, and shown in parentheses below the point estimates. The table also presents the control mean—the average value of the dependent variable among individuals who were not placed—and the effect in percent terms—the point estimate divided by the control mean. Panel A shows estimates for high school graduation, while Panel B shows estimates for whether the student was ever arrested by age 19. Each column presents estimates from a separate regression. Column 1 presents a simple difference-in-means between treated youth ($n=5,288$) and all potential control observations ($n=2,432,884$). Column 2 shows the difference in outcomes among youth in non-degenerate match groups ($n=3,632$ treated youth and 6,461 control observations). Each additional column further restricts the comparison: Column 3 also includes match-group fixed effects to restrict comparisons to individuals within the same group; Column 4 additionally implements IPW; Column 5, which also incorporates post-matching regression adjustment, is our preferred specification. The number of observations in Panel B is slightly smaller because a small group of students in our sample is not observed through age 19. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effects of JDC on Type of Criminal Justice Involvement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ever Convicted by Age 19	Ever Incarcerated by Age 19	Misdemeanor Offense	Felony Offense	Ever Arrested on a ... Violent Offense	Property Offense	Drug Offense	Public Order Offense
Placed in a JDC	0.060*** (0.011)	0.066*** (0.013)	0.060*** (0.014)	0.091*** (0.013)	0.063*** (0.011)	0.056*** (0.013)	0.032*** (0.009)	0.056*** (0.012)
Observations	9,662	9,662	9,662	9,662	9,662	9,662	9,662	9,662
R-squared	0.352	0.307	0.289	0.329	0.291	0.297	0.288	0.289
Control Mean	0.199	0.165	0.286	0.23	0.161	0.193	0.089	0.204
Percent Effect	30.2	40.0	21.0	39.6	39.1	29.0	36.0	27.5

Notes. The first row of the table shows estimates of γ from Equation 1. Standard errors are clustered at the child and match group levels, and shown in parentheses below the point estimates. The table also presents the control mean—the average value of the dependent variable among individuals who were not placed—and the effect in percent terms—the point estimate divided by the control mean. Each column presents the results of a separate specification, where the dependent variable in each is presented at the top of the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Heterogeneity by Observable Characteristics

	(1) All	(2) Male	(3) Female	(4) White	(5) Black	(6) Violent Offense	(7) Non-Violent Offense
Panel A: Graduated High School							
Placed in a JDC	-0.080*** (0.011)	-0.078*** (0.012)	-0.089*** (0.023)	-0.067*** (0.019)	-0.088*** (0.013)	-0.098*** (0.017)	-0.084*** (0.015)
Observations	10,093	7,075	3,018	3,409	6,526	4,121	5,972
R-squared	0.343	0.322	0.382	0.435	0.295	0.394	0.374
Control Mean	0.261	0.232	0.346	0.252	0.268	0.274	0.255
Percent Effect	-30.7	-33.6	-25.7	-26.6	-32.8	-35.8	-32.9
Panel B: Arrested by Age 19							
Placed in a JDC	0.094*** (0.014)	0.106*** (0.017)	0.060** (0.024)	0.091*** (0.025)	0.100*** (0.017)	0.078*** (0.026)	0.098*** (0.021)
Observations	9,611	6,764	2,847	3,236	6,230	3,052	6,559
R-squared	0.314	0.276	0.373	0.448	0.249	0.386	0.344
Control Mean	0.381	0.438	0.216	0.355	0.397	0.400	0.373
Percent Effect	24.7	24.2	27.8	25.6	25.2	19.5	26.3

Notes. The first row of each panel shows estimates of γ from Equation 1. Standard errors are clustered at the child and match group levels, and shown in parentheses below the point estimates. The table also presents the control mean—the average value of the dependent variable among individuals who were not placed—and the effect in percent terms—the point estimate divided by the control mean. Panel A shows estimates for high school graduation, while Panel B shows estimates for whether the student was ever arrested by age 19. Each column is the result of a separate specification, where each is estimated on the subgroup of students described at the top of the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Sensitivity Analysis

	(1)	(2)	(3)
	Baseline	Bias-Adjusted Treatment Effect	Proportional Degree of Selection For Effect of Zero
Panel A: Ever Graduated High School			
Placed in JDC	-0.080	-0.080	8.761
Panel B: Ever Arrested by Age 19			
Placed in JDC	0.094	0.081	4.023

Notes. This table reports the results of robustness checks proposed in [Oster \(2019\)](#). Panel A reports the results for high school graduation, while Panel B reports those for arrested by age 19. Column 1 shows our preferred baseline estimate of the effect of JDC placement, as previously reported in Column 5 of Table 3. This estimate would be equal to the estimate of the treatment effect if there were no omitted variable bias. Column 2 shows the bias-adjusted treatment effect, assuming that the amount of selection on unobservables is equal to the amount of selection on observables. Finally, Column 3 shows the amount of proportional selection needed such that the treatment effect would equal zero. The estimates in Columns 2 and 3 were calculated using the STATA package *psacalc*. This exercise assumes that the ratio of bias due to unobserved and observed factors is 1. We also assume that the maximum r-squared from a hypothetical model which includes all observed and unobserved factors is 1.3 times the r-squared from our preferred specification in Column 1.

Table 7: Effects of Detention After Controlling for Case Outcomes

	(1)	(2)	(3)	(4)	(5)
		<i>Baseline Estimates</i>		<i>Holding Adjudication Constant</i>	
	Adjudicated in Academic Year	Graduated High School	Arrested by Age 19	Graduated High School	Arrested by Age 19
Placed in a JDC	0.168*** (0.012)	-0.080*** (0.011)	0.094*** (0.014)	-0.076*** (0.011)	0.089*** (0.014)
Adjudicated				-0.020* (0.011)	0.033** (0.016)
Observations	10,093	10,093	9,662	10,093	9,662
Control Mean	0.522	0.261	0.381	0.261	0.381
Percent Effect	32.2	-30.7	24.7	-29.1	23.4

Notes. The first row of the table presents estimates of γ from Equation 1. Standard errors are clustered at the child and match group levels, and shown in parentheses below the point estimates. The table also presents the control mean—the average value of the dependent variable among individuals who were not placed—and the effect in percent terms—the point estimate divided by the control mean. Each column presents estimates from a regression on a separate outcome, where the outcome is described at the top of the table. The outcome in Column 1 is an indicator for whether or not the individual was adjudicated (convicted) for a juvenile petition in the target academic year. Columns 2 and 3 present our baseline estimates of JDC placement on high school graduation and adult arrest, respectively. Columns 4 and 5 present the results of our baseline specification, but controlling for an indicator of whether or not the juvenile was adjudicated in the target academic year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Robustness of our Main Estimates to Alternative Specifications

	(1) Baseline	(2) Kernel Matching	(3) One Nearest Neighbor	(4) Three Nearest Neighbors
Panel A: Graduated High School				
Placed in a JDC	-0.080*** (0.011)	-0.099*** (0.009)	-0.089*** (0.011)	-0.074*** (0.012)
Panel B: Arrested by Age 19				
Placed in a JDC	0.094*** (0.014)	0.090*** (0.011)	0.097*** (0.014)	0.102*** (0.015)

Notes. The table shows robustness of our main estimates to several alternative specifications. Column 1 presents our baseline estimates. In Column 2, instead of implementing propensity score matching via IPW, we implement kernel matching with an Epanechnikov kernel function. In Columns 3 and 4, we use our predicted propensity scores to implement nearest neighbor matching instead. Column 3 shows the results when using the single nearest neighbor, while Column 4 shows the results when using the three nearest neighbors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effects of JDC Placement on Children's Outcomes by Relative Time

	(1) t-2	(2) t+1	(3) t+2	(4) t+3	(5) t+4	(6) t+5
Dropped Out		-0.018** (0.008)	-0.004 (0.011)	0.008 (0.013)	0.024 (0.015)	0.034** (0.015)
Control Mean		0.073	0.202	0.335	0.41	0.439
N		10,093	10,093	10,093	10,093	10,093
Chronically Absent	-0.021 (0.013)	-0.110*** (0.024)	-0.069*** (0.021)	0.009 (0.026)	0.009 (0.035)	-0.021 (0.083)
Control Mean	0.573	0.645	0.66	0.64	0.646	0.605
N	8,328	5,624	4,709	2,963	1,346	406
IEP	0.005 (0.006)	0.104*** (0.012)	0.046*** (0.011)	0.020 (0.015)	0.005 (0.022)	0.014 (0.052)
Control Mean	0.346	0.33	0.289	0.259	0.266	0.261
N	9,351	9,281	7,211	4,393	1,951	580
Retained in Grade	-0.002 (0.008)	0.034** (0.015)	0.025 (0.016)	0.029* (0.018)	0.009 (0.025)	0.034 (0.053)
Control Mean	0.087	0.225	0.219	0.159	0.104	0.082
N	9,246	7,855	5,773	3,453	1,568	500
Number of Schools in Year	0.014 (0.017)	0.587*** (0.028)	0.312*** (0.028)	0.243*** (0.033)	0.130*** (0.032)	0.184** (0.077)
Control Mean	1.347	1.447	1.355	1.258	1.194	1.183
N	9,349	9,279	7,211	4,391	1,951	580
Juvenile Arrest	0.000 (0.000)	0.211*** (0.015)	0.146*** (0.014)	0.097*** (0.015)	0.045** (0.020)	0.037 (0.032)
Control Mean	0.115	0.179	0.088	0.039	0.022	0.000
N	10,093	9,476	7,332	4,304	1,855	517
Juvenile or Adult Arrest	0.000 (0.000)	0.202*** (0.014)	0.112*** (0.013)	0.069*** (0.013)	0.068*** (0.013)	0.079*** (0.014)
Control Mean	0.115	0.187	0.167	0.241	0.314	0.357
N	10,093	10,093	10,093	10,093	10,093	10,093

Notes. The table shows how the educational effects of JDC placement vary by year relative to the juvenile petition. Specifically, the table presents estimates of γ from Equation 1. Standard errors are clustered at the child and match group levels, and are shown below the point estimates in parentheses. Each column presents the results of a separate regression, each on a different relative year. The first column shows the “effects” of JDC two years *prior* to placement. These estimates serve as a “placebo” or an additional robustness check. The remaining columns show results for the first five years following JDC placement. The dropout variable is cumulative, in the sense that it measures a student dropping out and not returning to the sample. The remaining variables all refer to the current year and are coded as missing if the student has dropped out or simply not enrolled in that year. For example, if a student was placed in a JDC as a 9th grader in 2014-15, the attendance, IEP (special education), and school mobility variables for $t + 1$ refer to the 2015-16 academic year. The $t + 1$ grade retention variable for this student indicates whether the student was enrolled in the same grade in 2015-16 as he/she will be in 2016-17. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Pretrial Juvenile Detention

E. Jason Baron, Brian Jacob, and Joseph Ryan

Online Appendix

Supplemental Online Tables

Table A1: Distribution of Enrollments by Facility

JDC Name	Number of Students	Percent of Students
Allegan County Juvenile Detention	52	0.98
Bay County Juvenile Home	132	2.50
Calhoun County Juvenile Home	212	4.01
Eaton County Youth Facility	3	0.06
Genesee Valley Regional Center	16	0.30
Jackson County Youth Center	1	0.02
Kalamazoo County Juvenile Home	1	0.02
Kent County Juvenile Detention Center	4	0.08
Macomb County Juvenile Justice Center	897	16.96
Maurice Spear Campus	3	0.06
Midland County Juvenile Care Center	108	2.04
Monroe County Youth Center	121	2.29
Muskegon County Juvenile Transition Center	225	4.25
Oakland County Children's Village	944	17.85
Ottawa County Juvenile Detention	8	0.15
Saginaw County Juvenile Detention	321	6.07
Spectrum Services Calumet Center	337	6.37
St. Ignace Juvenile Detention Facility	4	0.08
Washtenaw County Youth Center	2	0.04
Wayne County Juvenile Detention Facility	1,897	35.87

Notes. The table presents the names of the 20 Michigan JDCs where students in our sample are enrolled, as well as the total number of students enrolled in each. We identified a JDC in CEPI/MDE individual student records as follows: Part D of Title I, a provision of the Elementary and Secondary Education Act, provides federal funding to improve the quality of education in facilities for neglected and delinquent youth. We used published reports on MDE's disbursement of these funds to identify 72 institutions in Michigan serving neglected and delinquent youth. These institutions include JDCs, youth commitment facilities, group homes, shelters serving abuse and neglected youth, and residential treatment centers. We researched each of the 72 institutions in order to identify JDCs. To ensure we had isolated each JDC in Michigan, we compared our list to the names of JDCs that are part of the Michigan Juvenile Detention Association and to those institutions listed in the JRFC Databook.

Table A2: Sample Construction

Step Number	Step Description	(1) Number of Students	(2) Ever JDC	(3) Never JDC	(4) Student by Year	(5) Ever JDC Student by Year	(6) Never JDC Student by Year
0	Start with student-by-year unbalanced panel for cohorts of interest	717,363	13,791	703,572	4,775,690	93,150	4,682,540
1	Keep only first JDC episode	717,363	13,791	703,572	4,696,331	13,791	4,682,540
2	Keep only JDC episodes in seventh through 12th grade	717,013	13,441	703,572	4,695,981	13,441	4,682,540
3	Keep only observations in 12 counties with juvenile justice data	386,255	7,928	378,327	2,444,160	7,928	2,436,232
4	Keep only JDC episodes with juvenile petition in same academic year JJ petition	383,622	5,295	378,327	2,441,527	5,295	2,436,232
5	Drop observations with missing data on key variables	383,485	5,288	378,197	2,438,172	5,288	2,432,884

Notes. The table describes in detail each step in the construction of our analysis sample. The starting point consists of first-time sixth-graders in Michigan public schools (including charters) during the 2007-08 through 2012-13 academic years. In step 1, we retain only the first JDC episode of individuals ever in a JDC. In step 2, we restrict the sample to JDC episodes in grades 7th through 12th. Step 3 restricts the sample to student-by-year observations in the 12 counties for which we have juvenile justice data. Step 4 excludes treated students with no corresponding juvenile petition in the same academic year. The final step drops student-by-year observations in which key variables used in the match are missing.

Table A3: Comparing JDC Students with and without Accompanying Juvenile Petition

	All Students	Juvenile Petition	No Juvenile Petition
Socio-Demographic Characteristics			
White	0.390	0.379	0.411
Black	0.555	0.571	0.523
Hispanic	0.045	0.042	0.052
Other	0.010	0.008	0.014
Economically disadvantaged	0.846	0.850	0.838
Female	0.299	0.292	0.314
Academic Outcomes in Grade 6			
Attendance rate	0.880	0.876	0.888
Chronically absent	0.417	0.427	0.395
IEP	0.307	0.315	0.290
Test scores	-0.750	-0.763	-0.724
Grade 6 School Characteristics			
School average mathematics score	-0.367	-0.383	-0.335
School 4-year dropout rate	0.270	0.256	0.300
Charter school	0.110	0.111	0.106
Share of students receiving FRPL	0.587	0.595	0.571
Share of students who are Black	0.468	0.482	0.442
Share of students who are Hispanic	0.056	0.056	0.055
Outcomes by Age 19			
Ever graduated high school	0.210	0.194	0.244
Ever arrested in Michigan	0.453	0.488	0.381
Ever arrested for a violent offense	0.206	0.229	0.161
Ever arrested for a property offense	0.226	0.248	0.182
Ever arrested for a drug offense	0.110	0.120	0.089
Ever arrested for a public order offense	0.238	0.261	0.193
Ever arrested for a felony offense	0.277	0.309	0.214
Ever arrested for a misdemeanor offense	0.332	0.359	0.278
Ever convicted	0.255	0.281	0.204
Ever incarcerated	0.210	0.236	0.159
Observations	7,921	5,288	2,633
Share of observations	1.00	0.668	0.332

Notes. This table reports summary statistics for three groups of students. Column 1 consists of the 7,921 students in our base cohorts who had at least one episode in a JDC at some point between 7th and 12th grade. Column 2 consists of the 5,288 students in our base cohorts who had at least one episode in a JDC at some point between 7th and 12th grade, and who had a corresponding juvenile petition in the same academic year. Column 3 consists of the 2,633 students in our base cohorts who had at least one episode in a JDC at some point between 7th and 12th grade, and who had no corresponding juvenile petition in the same academic year. All three columns exclude the 7 treated students with missing observations in key variables used in our matching strategy.

Table A4: Conditional Probabilities of JDC Placement

x	$Pr(JDC X = x)$
Socio-Demographics	
White	0.008
Black	0.029
Hispanic	0.012
Female	0.008
Male	0.019
White Male	0.011
White Female	0.005
Black Male	0.042
Black Female	0.016
Hispanic Male	0.015
Hispanic Female	0.009
FRPL	0.024
Not FRPL	0.004
White and FRPL	0.017
Black and FRPL	0.033
Socio-Demographics $\times$ G6 Academic Outcomes	
Chronically Absent	0.044
Not Chronically Absent	0.009
White and Chronically Absent	0.030
Black and Chronically Absent	0.056
G6 Academic Outcomes	
ELL	0.006
Not ELL	0.014
IEP	0.030
Not IEP	0.011
G6 Test Scores	
First Quartile	0.031
Second Quartile	0.014
Third Quartile	0.006
Fourth Quartile	0.003

Notes. This table reports the probability of ever being placed in a juvenile detention center conditional on various characteristics for children in our analysis sample. For instance, the table shows that, while White female students have virtually no chance of ever being placed in a JDC, Black male students have roughly a 3% chance.

Table A5: Characteristics of Treated Students in Groups With and Without Variation

	All Students	Students in Groups With Variation	Students in Groups Without Variation
Socio-Demographic Characteristics			
White	0.379	0.350	0.444
Black	0.571	0.631	0.441
Hispanic	0.042	0.020	0.091
Other	0.008	0.000	0.024
Economically disadvantaged	0.850	0.860	0.827
Female	0.292	0.259	0.365
Academic Outcomes in Grade 6			
Attendance rate	0.876	0.868	0.894
Chronically absent	0.427	0.465	0.346
IEP	0.315	0.329	0.284
Test scores	-0.763	-0.811	-0.659
Grade 6 School Characteristics			
School average mathematics score	-0.383	-0.451	-0.235
School 4-year dropout rate	0.256	0.257	0.253
Charter school	0.111	0.121	0.089
Share of students receiving FRPL	0.595	0.627	0.528
Share of students who are Black	0.482	0.546	0.341
Share of students who are Hispanic	0.056	0.049	0.071
Outcomes			
Ever graduated high school	0.194	0.181	0.221
Ever arrested in Michigan	0.488	0.501	0.458
Ever arrested for a violent offense	0.234	0.244	0.211
Ever arrested for a property offense	0.255	0.263	0.238
Ever arrested for a drug offense	0.124	0.124	0.122
Ever arrested for a public order offense	0.268	0.275	0.251
Ever arrested for a felony offense	0.315	0.333	0.275
Ever arrested for a misdemeanor offense	0.369	0.370	0.368
Ever convicted	0.289	0.282	0.305
Ever incarcerated	0.243	0.246	0.234
Number of observations	5,288	3,632	1,656
Share of observations	1.00	0.687	0.313

Notes. This table reports summary statistics for three groups of students. Column 1 consists of the 5,288 treated individuals in our sample. Column 2 consists of the 3,632 treated juveniles in non-degenerate exact match groups—those in groups consisting of at least one treated and control observations. Column 3 consists of the 1,656 treated juveniles in degenerate groups.

Table A6: Logit Model for Estimation of the Propensity Score

	JDC
Child Welfare Controls	
Previously Ever Victim of Maltreatment	0.503*** (0.058)
Previously Ever Placed in Foster Care	0.243*** (0.093)
Socio-Demographic Characteristics	
Age	1.704*** (0.532)
Age Squared	-0.056*** (0.017)
Economically Disadvantaged in t-1	-0.172** (0.072)
Prior Academic Performance	
Most Recent Attendance Rate	1.276 (1.887)
Most Recent Attendance Rate Squared	-3.483 (3.448)
Most Recent Attendance Rate Cubed	1.597 (1.938)
Most Recent Test Scores	-0.088 (0.068)
Most Recent Test Scores Squared	0.061 (0.056)
Most Recent Test Scores Cubed	0.034* (0.021)
Number of Schools Attended in t-1	0.021 (0.043)
New to School in t-1	-0.044 (0.054)
Chronically Absent in t-1	-0.123* (0.074)
IEP Receipt in t-1	-0.386*** (0.071)
Suspended in t-1	0.011 (0.209)
Old for Grade in t-1	-0.074 (0.053)
Female X Suspended in t-1	-0.744*** (0.199)
Female X Retained in Grade in t-1	0.162 (0.142)
Female X IEP in t-1	1.656*** (0.118)
Female X Share of FRPL Students in School in t-1	-0.626*** (0.208)
Female X Attendance in t-1	0.094 (0.179)
Female X Test Score in t-1	0.224*** (0.086)
School Characteristics in t-1	
Share of FRPL Students	0.612* (0.368)
Average Test Scores	-1.411*** (0.138)
Share of White Students	1.898*** (0.389)
Total Enrollment	-0.000*** (0.000)
Charter School Indicator	-0.095 (0.078)
Prior Juvenile Justice Measures	
Adjudicated in t-1	0.223 (0.528)
Share of Total Petitions With Adjudicated Disposition t-1	0.148 (0.788)
Disposition of Dismissed in at Least One Petition t-1	-0.110 (0.408)
Disposition of Other in at Least One Petition t-1	-0.096 (0.387)

Disposition of Plead Guilty in at Least One Petition t-1	0.324 (0.358)
Disposition of No Show in at Least One Petition t-1	0.836 (0.673)
Disposition of Plead No Contest in at Least One Petition t-1	0.098 (0.334)
Disposition of Guilty in at Least One Petition t-1	-3.043** (1.412)
Disposition of Probation in at Least One Petition t-1	0.367 (0.335)
Adjudicated in t-2	0.217 (1.131)
Share of Total Petitions With Adjudicated Disposition t-2	-2.403 (1.716)
Disposition of Dismissed in at Least One Petition t-1	-0.473 (0.848)
Disposition of Other in at Least One Petition t-1	-0.601 (0.836)
Disposition of Plead Guilty in at Least One Petition t-1	1.197 (0.735)
Disposition of No Show in at Least One Petition t-1	-0.242 (2.002)
Disposition of Plead No Contest in at Least One Petition t-1	1.683** (0.703)
Disposition of Probation in at Least One Petition t-1	1.595** (0.720)
Most Serious Offense in t-1 Was Violent	0.431 (0.581)
Most Serious Offense in t-1 Was Property	0.661 (0.587)
Most Serious Offense in t-1 Was Other	0.794 (0.602)
Most Serious Offense in t-1 Was Weapons	-0.330 (0.698)
Most Serious Offense in t-1 Was Drug	-0.045 (0.628)
All Offense Descriptions in t-1 Are Missing	0.620 (0.592)
One Petition in t-2	-0.103 (0.347)
Two to Three Petitions in t-2	-0.163 (0.313)
Most Serious Offense in t-2 Was Violent	-0.914 (1.277)
Most Serious Offense in t-2 Was Property	-0.790 (1.278)
Most Serious Offense in t-2 Was Other	-0.520 (1.291)
Most Serious Offense in t-2 Was Weapons	-0.815 (1.366)
Most Serious Offense in t-2 Was Drug	-0.607 (1.365)
All Offense Descriptions in t-2 Are Missing	-0.685 (1.282)
One Petition in t-2	1.322* (0.720)
Two to Three Petitions in t-2	1.094* (0.656)
Controls for Target Petition Year (t)	
Most Serious Offense in t Was Violent	1.156*** (0.078)
Most Serious Offense in t Was Property	0.503*** (0.086)
Most Serious Offense in t Was Other	0.608*** (0.093)
Most Serious Offense in t Was Weapons	1.155*** (0.151)
Most Serious Offense in t Was Drug	0.323** (0.139)
One Petition in t	-1.913*** (0.155)
Two to Three Petitions in t	-0.726*** (0.158)
Observations	10,093

Table A7: Main Results for Sample of Students Not Convicted

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduated High School						
Placed in a JDC	-0.183*** (0.014)	-0.185*** (0.017)	-0.186*** (0.018)	-0.102*** (0.018)	-0.075*** (0.020)	-0.065*** (0.020)
Observations	8,410	4,229	3,690	3,690	3,690	3,690
Control Mean	0.395	0.394	0.394	0.274	0.394	0.274
Percent Effect	-46.3	-47.0	-47.2	-37.2	19.0	-23.7
Panel B: Arrested by Age 19						
Placed in a JDC	0.180*** (0.017)	0.181*** (0.020)	0.131*** (0.024)	0.102*** (0.026)	0.063** (0.025)	0.075*** (0.027)
Observations	8,001	4,053	3,505	3,505	3,505	3,505
Control Mean	0.280	0.298	0.298	0.344	0.298	0.344
Percent Effect	64.3	60.7	44.0	29.7	21.1	21.8
Students in Non-Degenerate Groups		Y	Y	Y	Y	Y
Group FE			Y	Y	Y	Y
IPW				Y		Y
Regression Adjustment					Y	Y

Notes. The table replicates our main results in Table 3, but estimated on a sample containing only individuals who were not convicted in the academic year of their juvenile justice petition. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Robustness to Out-of-State Migration

	(1) Baseline	(2) Excluding Ever K-12 State Leavers	(3) Excluding Ever College Outside MI
Placed in a JDC	0.094*** (0.014)	0.088*** (0.019)	0.090*** (0.019)
Observations	9,611	6,764	2,847
R-squared	0.314	0.276	0.373
Control Mean	0.381	0.406	0.409
Percent Effect	24.7	21.7	22.0

Notes. Column 1 reports our baseline estimate of the effects of detention on ever arrested by age 19, as previously reported in Column 6 of Table 3. Columns 2 and 3 replicate this result, but on a sample excluding children who ever left the state in K-12 (Column 2), and children who ever enrolled in postsecondary education outside of Michigan (Column 3). We measure the outcome in Column 2 using exit codes that are assigned to students who leave the Michigan public school system. We measure the outcome in Column 3 using data from the National Student Clearinghouse.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Main Results When Retaining Only Short-Term Placements

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduated High School						
Placed in a JDC	-0.608*** (0.007)	-0.187*** (0.010)	-0.172*** (0.010)	-0.100*** (0.010)	-0.089*** (0.010)	-0.080*** (0.011)
Observations	2,438,172	10,093	10,093	10,093	10,093	10,093
R-squared	0.005	0.038	0.243	0.257	0.339	0.343
Control Mean	0.802	0.368	0.368	0.261	0.368	0.261
Percent Effect	-75.8	-50.7	-46.8	-38.2	-24.3	-30.5
Panel B: Arrested by Age 19						
Placed in a JDC	0.424*** (0.008)	0.176*** (0.011)	0.168*** (0.011)	0.136*** (0.013)	0.094*** (0.013)	0.094*** (0.014)
Observations	2,287,081	9,662	9,662	9,662	9,662	9,662
R-squared	0.006	0.030	0.231	0.274	0.264	0.314
Control Mean	0.064	0.326	0.326	0.381	0.326	0.381
Percent Effect	662.3	53.9	51.6	35.7	28.8	24.7
Students in Non-Degenerate Groups		Y	Y	Y	Y	Y
Group FE			Y	Y	Y	Y
IPW				Y		Y
Regression Adjustment					Y	Y

Notes. The table replicates the specifications in Table 3, but retains only the set of 1,671 JDC episodes with non-missing data and that lasted less than two weeks. The first row of each panel shows estimates of γ from Equation 1. Standard errors are clustered at the child and match group levels, and shown in parentheses below the point estimates. The table also presents the control mean—the average value of the dependent variable among individuals who were not placed—and the effect in percent terms—the point estimate divided by the control mean. Panel A shows estimates for high school graduation, while Panel B shows estimates for whether the student was ever arrested by age 19. Each column presents estimates from a separate regression. Column 1 presents a simple difference-in-means between treated youth and all potential control observations. Column 2 shows the difference in outcomes among youth in non-degenerate match groups. Each additional column further restricts the comparison: Column 3 also includes match-group fixed effects to restrict comparisons to individuals within the same group; Column 4 additionally implements IPW; Column 5, which also incorporates post-matching regression adjustment, is our preferred specification. The number of observations in Panel B is slightly smaller because a small group of students in our sample is not observed through age 19. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Potential Benefits of Detention

	(1)	(2)	(3)	(4)	(5)
		<i>Baseline Estimates</i>		<i>Holding Disposition Constant</i>	
	No Show Disposition in Academic Year	Graduated High School	Arrested by Age 19	Graduated High School	Arrested by Age 19
Placed in a JDC	-0.008* (0.004)	-0.080*** (0.011)	0.094*** (0.014)	-0.079*** (0.011)	0.094*** (0.014)
Observations	10,093	10,093	9,662	10,093	9,662
Control Mean	0.022	0.261	0.381	0.261	0.381
Percent Effect	-0.364	-0.307	0.247	-0.303	0.247

Notes. The first row of the table presents estimates of γ from Equation 1. Standard errors are clustered at the child and match group levels, and shown in parentheses below the point estimates. The table also presents the control mean—the average value of the dependent variable among individuals who were not placed—and the effect in percent terms—the point estimate divided by the control mean. Each column presents estimates from a regression on a separate outcome, where the outcome is described at the top of the table. The outcome in Column 1 is an indicator for whether or not the individual received a disposition of “no show” in the target academic year. Columns 2 and 3 present our baseline estimates of JDC placement on high school graduation and adult arrest, respectively. Columns 4 and 5 present the results of our baseline specification, but controlling for an indicator of whether or not the individual received a disposition of “no show” in the target academic year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.