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IS THERE A FOSTER CARE-TO-PRISON PIPELINE?
EVIDENCE FROM QUASI-RANDOMLY ASSIGNED INVESTIGATORS

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ABSTRACT

Foster care placement is strongly associated with crime—for example, close to one fifth of the prison population in the United States is comprised of former foster children—yet there is little evidence on whether this relationship is causal. Leveraging the quasi-random assignment of child welfare investigators and administrative data from Michigan, we show that foster care placement substantially reduced the chances of adult arrests, convictions, and incarceration for children at the margin. Exploring mechanisms, we find that foster care also improved a range of children's safety, academic, and behavioral intermediate outcomes. A likely reason for children's improvements is that their birth parents made positive changes, as most children in our setting reunified with their parents after a short stay in foster care. In light of recent historic federal policy which prioritizes keeping children with their families, our analysis indicates that safely reducing foster care caseloads will require improving efforts to ensure child wellbeing in the home.

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A non-technical summary of the paper is available at http://www.nber.org/data-appendix/w29922

I Introduction

Foster care is surprisingly common in the United States and children placed in foster care are particularly likely to be involved in the criminal justice system as adults. Children are placed in foster care when government authorities find that they were abused or neglected and determine it is not safe for them to continue living in their homes. They can be placed in a variety of settings, such as with relatives, an unrelated family, or institutional settings. As many as 5% of children in the United States are placed in foster care at some point by age 18, including up to 9% of Black children and 11% of American Indian and Alaska Native children (Yi, Edwards and Wildeman, 2020). Close to one fifth of the prison population in the United States is comprised of former foster children (BJS, 2016) and about 70% of youth who exit foster care as legal adults are arrested at least once by age 26 (Courtney et al., 2011). Decades of research also show a positive association between foster care placement and criminality (Yang, McCuish and Corrado, 2017, 2021) and the media often cites a "foster care-to-prison pipeline" (Amon, 2021; Trivedi, 2020).

Despite these dismal descriptive statistics, there is little evidence for a causal relationship between foster care and later-in-life crime. Ex ante, it is unclear whether foster care would reduce or increase adult crime. On the one hand, keeping children in a harmful home environment could lead to worse adult outcomes. On the other hand, separating children from their families could lead to trauma and instability in their lives. Seminal work in Doyle (2008) provides the only causal evidence on the relationship between foster care and adult crime in the United States, and finds that placement tripled arrest, conviction, and imprisonment rates for children investigated in Illinois over two decades ago. However, in light of substantial changes to federal child welfare policy over time and the dramatic increase in foster care placements attributed to the opioid epidemic (Dallman, 2020; Evans, Harris and Kessler, 2022; Hou, 2022), it is critical to understand the effects of current foster care systems.

This paper provides new evidence on the causal relationship between foster care and adult crime. We assembled a rich administrative dataset linking child welfare and adult criminal justice records in Michigan. To explore mechanisms, we also linked these data to administrative records from Michigan's K-12 public school system, juvenile detention spells, and nationwide postsecondary enrollment information. We study nearly 120,000 child welfare investigations involving children ages 6 to 16 between 2008 and 2016. Our research design exploits plausibly exogenous variation in foster care placements created by the rotational assignment of child welfare investigators. Investigators are assigned to cases based on who is next up on a list, not for reasons specific to the child or family, and they have discretion over whether to recommend placement. These decisions are in part subjective and some investigators are stricter than others. Using investigator stringency as an instrument, we compare the outcomes of children who by chance are assigned a strict investigator and placed in foster care to those who are not placed only because they are assigned a more lenient investigator. This research design allows us to identify local average treatment effects (LATE), which are impacts for children at the margin of placement: those for whom investigators might disagree about foster care placement. These children, or compliers, are a relevant population for policy (Berrick, 2018).

We find that foster care placement substantially reduces the chances of later-in-life criminal involvement for children at the margin. Children placed in foster care are 25 percentage points less likely to be arrested by age 19 relative to a control complier mean of 37%, a decrease of 68%. We find even larger reductions in the likelihood of being convicted (28 percentage points or 80%) and incarcerated (21 percentage points or 81%). We find similar declines through age 21 for the subset of youth that we observe at older ages. These effect sizes are economically meaningful; for example, they are larger than those found for other interventions targeting youth that directly aim to reduce crime, such as cognitive behavioral therapy in the Becoming a Man program (Heller et al., 2017). The reduction in crime is driven by a large decline in violent crimes, which are the most costly to society (Chalfin, 2015). We show that these results are not driven by differential out-of-state migration and are robust to alternative designs and samples.

To contextualize these findings, we examine characteristics of the complier population, conduct subgroup analyses by gender, age, and race and ethnicity, and estimate marginal treatment effects (MTEs). We show that most compliers are initially placed in a family home as opposed to an institutional setting, have stable placements, are in foster care for one to two years, and ultimately reunify with their birth parents.[1] Subgroup analyses show that the reduction in adult crime is larger for male than female children and for children ages 6 to 11 relative to youth ages 12 to 16. We find similar impacts for White, Black, and Hispanic children. We also estimate MTEs to explore heterogeneity by unobservable characteristics. The MTEs indicate that foster care reduces adult crime the most for children whose cases likely involve relatively more severe maltreatment. The effects of foster care on adult crime are smaller and statistically insignificant for children whose cases likely involve less severe maltreatment.

We explore mechanisms for why foster care reduces adult crime by examining impacts on other indicators of child wellbeing. We find that foster care protects children from subsequent abuse and neglect; children who are placed are considerably less likely to be confirmed as victims of maltreatment in the future, even years after they exit foster care. We also see gains in children's short- and medium-term academic outcomes. Foster care substantially reduces absences from school, improves math test scores, and appears to increase the likelihood of high school graduation and college enrollment. We also examine impacts on children's behavior and find that placement reduces the likelihood of being held in a juvenile detention center, a measure of juvenile delinquency.

The gains in children's safety, academic, and behavioral outcomes due to placement could contribute to the estimated reduction in adult crime through several channels. Improvements in academic outcomes and educational attainment could increase the opportunity cost of crime through better outside options (Becker, 1968; Lochner, 2004). At the same time, the decline in juvenile justice contact as a result of foster care could itself reduce later-in-life crime (Aizer and Doyle, 2015; Eren and Mocan, 2021). In addition, recent work highlights the important negative relationship between mental health and adult crime (Anderson, Cesur and Tekin, 2015; Bondurant, Lindo and Swensen,

[1] We refer to the adult/s with legal custody of the child before foster placement as the child's birth parents throughout, even though in some cases the adult/s may not be their biological parent, e.g., stepparents or grandparents.

2018; Jácome, 2020), and clinical research shows that child abuse and neglect are closely tied to worsened mental health (Garno et al., 2005; Heim et al., 2010). Although we cannot distinguish between these channels, we rule out two other explanations: a mechanical short-term reduction in crime from staying in school longer and moving to more advantaged neighborhoods.

How did foster care placement improve these intermediate outcomes? We find evidence that birth parents make improvements while their children are temporarily in foster care. Nearly all children at the margin of placement have short stays in foster care (one to two years) after which more than 80% reunify with their birth parents. By examining the time pattern of impacts on children's outcomes, we show that the gains from foster care emerge after most children reunify and persist thereafter. In addition to these trends, there are institutional reasons to believe that foster care could lead birth parents to make improvements. After removal, birth parents work closely with social workers and receive fully funded services to address challenges in their lives, such as substance abuse treatment or counseling. A judge must also approve that it is safe for families to reunify. Accordingly, we find that birth parents whose children are placed are less likely to abuse or neglect children in the future.

This study provides new evidence on whether or not there is a foster care-to-prison pipeline. Consistent with the idea of a pipeline, we are able to reproduce descriptive statistics showing that children who are placed in foster care are substantially more likely to be involved in the adult criminal justice system. Indeed, we find that this relationship holds even after controlling for a range of observable characteristics. However, our instrumental variables strategy reveals that in our data, this relationship is the result of selection bias and does not represent the causal effects of foster care. Failing to isolate exogenous variation in foster care placement can lead to incorrect conclusions about the foster care-to-prison pipeline.

Our findings contrast those in Doyle (2008), the only other study to estimate the causal effects of foster care on adult crime in the United States. Doyle (2008) uses the same research design and finds large detrimental effects of foster care placement. We explore many potential explanations for the divergent results and conclude that the most likely explanation is that foster care in Illinois two decades ago was tremendously different than in Michigan more recently. First, child welfare policy has changed over time in ways that have likely improved foster care. The federal government has enacted several key policies after the start of the Doyle (2008) sample period focusing on reducing placement length, improving the quality of placement settings, and promoting the wellbeing of children while in foster care. Second, there are large and persistent discrepancies between the foster care systems in the two states. For example, placements were and continue to be considerably longer and less stable in Illinois, and there is an extensive literature showing a negative association between these indicators and children's outcomes (Rubin et al., 2007, 2004; Ryan and Testa, 2005). We find less evidence that other differences between studies, such as different sample restrictions and complier populations, can explain the divergent findings.

We complement Doyle (2008) in three important ways. First, we examine the effects of foster care in a more recent time period. Because of the substantial changes to federal child welfare policy, revisiting the effects of foster care is critical to understanding the efficacy of current systems. Second, we focus on a state that is more representative along key dimensions of foster care quality. As Doyle (2008) notes, foster children in Illinois remained in the system for nearly two years longer than the national average. They also changed foster homes at a higher rate than in all but two states. In contrast, placement length and stability in Michigan mirror nationwide statistics. Although these are two examples of foster care indicators, states track them as performance measures and make reducing placement length and instability a goal of their systems (Bald et al., 2022*b*). Third, we explore the specific mechanisms through which foster care influences later-in-life crime, which is crucial for policy purposes. By showing that birth parents likely make improvements while their children are temporarily in foster care, we highlight that foster care is a family intervention and that family structure and parental behavior contribute to criminal development.

This study builds upon Gross and Baron (2022), which found that removal improved shorter-term outcomes for Michigan children, such as school attendance and test scores, by examining effects on later-in-life outcomes. Studying longer-term outcomes is important because effects on short-run outcomes can fade out over time and may not persist into adulthood (Aizer, Hoynes and Lleras-Muney, 2022; Jacob, Lefgren and Sims, 2010). Moreover, perhaps due to unobserved improvements in non-cognitive outcomes, studies of early-life interventions often find impacts on long-term outcomes such as educational attainment and criminal activity even in the absence of improvements in short-run academic outcomes (Deming, 2009; Gray-Lobe, Pathak and Walters, 2021; Heckman, Pinto and Savelyev, 2013). Thus, looking only at short-term outcomes may miss important benefits of early-life interventions.[2]

Beyond studying longer-term outcomes than Gross and Baron (2022), our focus on crime in this study is particularly important because this outcome has been closely tied to child maltreatment and foster care. Furthermore, unlike improvements in children's cognitive skills which yield mostly private benefits, reducing crime generates large positive externalities for society.[3] Indeed, we provide the first estimate of the Marginal Value of Public Funds (MVPF) of foster care, and show that its social benefits from reducing crime alone are greater than its costs.

Our findings also contribute to the economics of crime literature identifying effective crime reduction strategies. Although many studies have focused on policing or tougher sanctions as strategies to reduce crime (Bell, Jaitman and Machin, 2014; Chalfin et al., 2021; Drago, Galbiati and Vertova, 2009; Helland and Tabarrok, 2007; Katz, Levitt and Shustorovich, 2003; Mello, 2019), we add to a growing literature emphasizing the efficiency gains of early policy interventions that prevent the development of offenders in the first place. For example, studies have shown that increasing access

[2]Other studies in this literature include Bald et al. (2022*a*) and Roberts (2019), who estimate the effects of foster care for young children in Rhode Island and South Carolina, respectively. Bald et al. (2022*a*) focus on academic outcomes such as test scores and grade repetition and find substantial gains for girls younger than 6 years old but null effects for other gender-age groups. Roberts (2019) finds positive impacts on on-time grade progression, yet noisy estimates on daily school attendance and test scores. In a related study, Warburton et al. (2014) studies the effects of foster care on a range of outcomes for 16- to 18-year-old male youth in Canada and finds mixed results.

[3]Although Gross and Baron (2022) studied the effects of foster care on juvenile crime, the data for that outcome were incomplete and the study could not rule out positive or negative effects.

to mental healthcare (Jácome, 2020), moving out of disadvantaged neighborhoods (Chyn, 2018), and limiting lead exposure (Billings and Schnepel, 2018; Grönqvist, Nilsson and Robling, 2020) can reduce adult crime. Our study shows that preventing child maltreatment is another effective way to reduce later-in-life crime.

The results of this study are especially relevant because of the historic changes to federal policy introduced in the Family First Prevention Services Act of 2018. A main goal of this bipartisan legislation is to keep families intact and reduce foster care caseloads. To do so, it allows states to redirect up to $8 billion in federal funds from foster care and adoption services toward evidence-based prevention-focused programs and services. We find that abused and neglected children who were not placed in foster care are more likely to be involved in the adult criminal justice system relative to those who were placed. Thus, our results indicate that safely reducing foster care caseloads will require improving efforts to ensure child wellbeing in the home.

II Overview of the Child Welfare System in Michigan

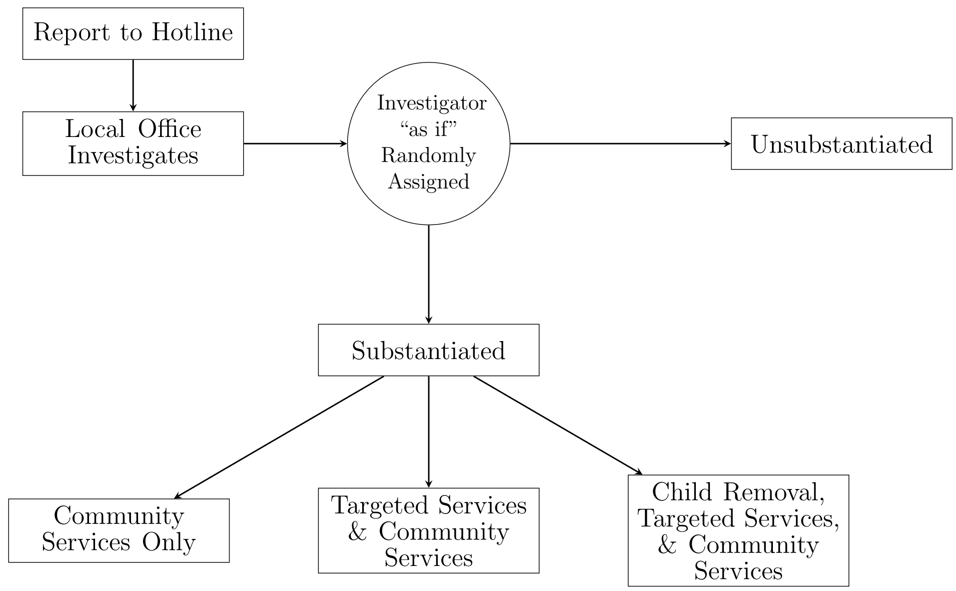
Child welfare involvement is not a rare experience for children in Michigan—close to one in five children are the subject of a maltreatment investigation before the third grade (Ryan et al., 2018). This section describes the maltreatment investigation process and the foster care system in Michigan.

II.A Child Maltreatment Investigations

In Michigan and across the country, a child maltreatment investigation is triggered by a report of suspected child abuse or neglect to a centralized hotline (Figure 1). Child abuse includes harm from non-accidental physical injuries, such as bruises or burns, and sexual abuse; child neglect includes harm from negligence, such as inadequate food or clothing. Anyone can call the hotline to report suspected maltreatment and the most common reporters are education and law enforcement personnel (Baron, Goldstein and Wallace, 2020). Hotline employees screen and transfer reports to the child's local child welfare office.

Upon receiving a report of suspected maltreatment, the local child welfare office assigns the report to a child maltreatment investigator according to a rotational assignment system. Reports cycle through investigators based on who is next in the rotation and investigators are not assigned based on their specific characteristics or skill sets, with two exceptions. Sexual abuse reports tend to be assigned to more experienced investigators, and repeat reports involving a child who was recently investigated are often re-assigned to the initial investigator. We exclude these exceptions from our analysis sample by dropping reports of sexual abuse and those involving children who had been the subject of an investigation in the year before the report. Typically, each county has its own local office but some large counties have multiple offices and some offices split investigators into geographic-based teams. As such, to compare children who could have been assigned to the same investigator, we include zip code by investigation year fixed effects in our analyses.

Figure 1: Child Welfare Investigation Process in Michigan



Child welfare investigators have about 30 days to make two key decisions that jointly determine the outcome of the investigation. Investigators first decide whether there is enough evidence to substantiate the allegation by interviewing the people involved, visiting the home, and reviewing any police or medical reports. About three quarters of cases go unsubstantiated (AECF, 2017) and in these cases, the investigation ends without any further follow up. In the roughly one quarter of substantiated cases, investigators then determine the level of risk the child faces in their home. To determine risk levels, investigators complete a 22-question risk assessment and cases deemed at the highest risk may result in removal from the home. Many of the questions on the risk assessment are subjective (e.g., one question asks whether the caretaker “views the incident less seriously than the department”) and investigators often manipulate responses based on their priors about the child’s risk level (Bosk, 2015; Gillingham and Humphreys, 2010). Consequently, investigators have immense discretion over child removal.

Investigator judgment over both evidence and risk determines the outcome of the investigation. As described earlier, unsubstantiated cases require no further follow up. If investigators substantiate a report and the risk level is low, they must refer the family to community-based services like food pantries, support groups, or other local nonprofits. After the investigator refers the family to community-based services, the child welfare office does not follow up further. If investigators substantiate the allegation and the risk level is high, the family also receives more intensive, targeted services based on their needs, which could include substance abuse treatment, parenting classes, and counseling. Lastly, substantiated allegations with particularly high risk not only trigger targeted and community services but also require investigators to file a court petition for child removal. Investigations that lead to removal tend to be short (e.g., investigations lasted 10 days for the median placement in our sample). The main analysis in this paper examines the combined effects of child removal and adult interventions on children's outcomes. In supplemental analyses, we demonstrate that the effects of removal alone are similar when excluding the effects of adult interventions.

II.B Foster Care System

Foster care is intended to be a temporary and family-oriented intervention. Children are placed with either an unrelated foster family, relatives, or in a group home. Child welfare typically tries to place children with relatives or, if no suitable relatives are available or willing, an unrelated family. Group home placements are considered a last resort for most children. In 2015, 41% of foster children in Michigan were placed with an unrelated family, 35% lived with relatives, 9% lived in group homes or institutions, and 14% lived in other settings, such as pre-adoptive homes or supervised independent living (AECF, 2017). It is common to switch placement settings while in the foster system: 60% of foster children in Michigan in 2015 lived in more than one setting, and 17% lived in at least four.

Birth parents work to regain custody in nearly all foster care cases. Child welfare caseworkers (who are different from the maltreatment investigator) meet with birth parents to create a reunification plan which stipulates how they can regain custody. Reunification plans might require birth parents to complete drug tests, secure housing, or get a job. Birth parents receive targeted services aimed at addressing the challenges in their own lives, which can range from counseling and parenting classes to job training and substance abuse treatment. Caseworkers monitor birth parents' progress and update the reunification plan as needed. Family reunification only occurs if the court decides that birth parents have made sufficient progress for their child to be safe in the home. About half of foster children in Michigan reunify with their birth parents. Close to one third of children are adopted or have legal guardianship transferred, 9% exit as legal adults, and others enter into an informal guardianship with relatives.

Foster care in Michigan is quite similar to other states. Foster care lasts for about 18 months on average in Michigan during which 70% of children experience two or fewer placements, compared to 20 months and 65% nationwide, respectively (AECF, 2017). Likewise, 47% of foster children in Michigan reunify compared to a national average of 51%. For these reasons, we believe the findings in this study could generalize to foster care systems in other states.

III Data Sources and Sample Construction

III.A Administrative Data Sources

This study uses four sources of administrative data spanning: (1) child welfare, (2) adult crime, (3) K-12 education, and (4) postsecondary education. This section describes each data source and the method used to link child records across sources.

Child welfare data come from the Michigan Department of Health and Human Services and consist of the universe of child maltreatment investigations in Michigan between August 1996 and

July 2017. These data include the details of each investigation, such as the allegation report date, allegation types as coded by the investigator, the child's zip code, and whether the investigator substantiated the allegation. The data also include placement records, such as whether a child was placed in foster care following an investigation, placement setting, and permanency outcome. We use this information to construct our main treatment variable: whether a child was placed in foster care due to a child welfare investigation. Crucial to our identification strategy, which leverages the quasi-random assignment of investigations, the files include unique investigator identifiers beginning in 2008.

Criminal justice data come from the Michigan State Police and contain our three main adult crime outcomes: arrests, convictions, and incarceration. To measure adult arrests, we use a dataset containing the universe of arrests in Michigan from January 2012–May 2020. For individuals who are arrested at age 17 or older (the age at which Michiganders are considered to be adults by the justice system during our sample period), these data include the date of the arrest, whether the arrest was for a misdemeanor or felony offense, and the type of crime, such as violent, property, or drug crime. We use a similar dataset that contains judicial information to measure whether an individual was convicted or incarcerated. To define outcomes consistently, our main analysis focuses on whether a person met each adult crime outcome before they turned 19 years old (for example, whether a person was ever arrested by age 19). In additional analyses, we examine outcomes through age 21 for the subset of children that we can observe at older ages.

K-12 education data come from the Michigan Department of Education and the Center for Educational Performance and Information. These data cover the universe of K-12 public school students in Michigan, including charter school students, between the 2002–2003 and 2020–2021 academic years. The K-12 education data include rich student-level demographic and socio-economic information, such as gender, race and ethnicity, and free- or reduced-price lunch eligibility, which we use as covariates. We also use the K-12 education data to explore intermediate outcomes, including standardized test scores, school attendance, and whether a student enrolled in an educational program at one of Michigan's 23 juvenile detention centers. Enrollment in a juvenile detention center is a behavioral outcome that indicates youth contact with the juvenile justice system; youth younger than 17 years old may be held in a detention center after being arrested. We focus on enrollment in a juvenile detention center instead of other behavioral outcomes commonly available in administrative education data, such as school suspensions or expulsions, because school discipline is not reported consistently in the Michigan data. The K-12 data also include the census block where students live, which we link to publicly available census block group characteristics from the Census Bureau.

Postsecondary education data come from the National Student Clearinghouse, which covers enrollment at most two- and four-year colleges in the United States. Specifically, it includes enrollment information from 97% of degree-granting institutions in the United States (Dynarski, Hemelt and Hyman, 2015). We use these data to examine whether an individual ever attended any college.

The Michigan Education Data Center (MEDC) linked the child welfare, adult crime, and education administrative 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 K-12 education data, the K-12 public school students serve as the base population. Staff linked the K-12 education data with the child welfare data and then matched the K-12 education data to the adult crime data. Both linkages performed well. For each of the matched records, the software rates the certainty level of the match using a posterior probability. Overall, 87.6% of records in the child welfare data matched to a public school student record and 92.4% 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 is nearly identical for males and females, and MEDC closely validated the match by manually matching a randomly selected subset of 200 records. Furthermore, this rate is quite high, given that some individuals investigated or arrested in Michigan could have gone to school in a different state, been enrolled in a private school, or been homeschooled.

III.B Overview of Analysis Sample

Using the administrative data sources, we construct an analysis sample unique at the child by investigation level of Michigan children subject to a child welfare investigation between 2008 and 2016. Because the child welfare and adult crime data are linked separately via the K-12 education records, we restrict the sample to children who ever enrolled in a public school in Michigan. We also restrict the sample to children who were 16 years old or younger at the time of their investigation because 17-year-olds can be arrested as adults in Michigan. We further restrict the sample based on the years of available child welfare and adult crime data. For a child to be in our analysis sample, we must observe both: (1) who investigated the case and (2) their adult crime outcomes by age 19. Because the child welfare data first record investigator identifiers in 2008 and the adult crime data end in May 2020, we restrict our sample to children investigated at ages 6 and older. Children younger than 6 who were investigated in 2008 or later would not have turned 19 years old before May 2020. Lastly, we exclude cases from the analysis sample where investigators were unlikely to have been quasi-randomly assigned: allegations of sexual abuse and those involving children from a recent prior report. Overall, we focus on 118,273 investigations of 87,100 students.

Table 1 compares the characteristics of all public school students in Michigan during the 2016-2017 school year (Column 1) to the children in our analysis sample—those subject to a child welfare investigation (Column 2). About half of students in both groups are female and their average age is 12 years old. There are notable differences in terms of race and ethnicity and socio-economic status, however, as Black children and children from families with low income are disproportionately involved in the child welfare system. 81% of investigations involved children with low income, despite these children making up just half of the overall population. Children subject to a child welfare investigation also had substantially lower baseline school attendance rates (83% compared to 95%), and scored about 0.4 standard deviations lower on standardized math and reading tests.

Table 1: Summary Statistics

	All MI Students	Analysis Sample: All	Analysis Sample: Foster Care
Socio-demographic Characteristics			
Female	0.49	0.50	0.46
White	0.67	0.63	0.49
Black	0.21	0.29	0.43
Hispanic	0.08	0.06	0.06
Other Race	0.05	0.02	0.02
Age	11.70	11.85	12.08
Grade in School	6.15	6.22	6.36
Low Income	0.49	0.81	0.86
Prior Schooling Characteristics			
Attendance Rate	0.95	0.83	0.75
Std Math Score	0.00	-0.39	-0.52
Std Reading Score	0.00	-0.36	-0.49
Adult Crime Outcomes			
Arrested by Age 19		0.14	0.21
Convicted by Age 19		0.08	0.11
Incarcerated by Age 19		0.06	0.09
Observations	1,262,665	118,273	2,595

Notes. Column 1 reports the characteristics of Michigan public school students enrolled in grades 1 through 11 in the 2016-17 school year. Column 2 includes all investigations in the analysis sample and Column 3 includes the subset of investigations that resulted in foster placement. For Columns 2 and 3, the socio-demographic variables are measured in the school year of the investigation and the prior schooling variables are measured in the school year before the investigation. Low income represents free or reduced-price lunch eligibility and math and reading test scores are normalized for the entire state to have a mean of zero and a standard deviation of one within every subject by grade by year cell.

About 2% of investigations in the analysis sample led to foster care placement (Column 3). Compared to all children subject to an investigation, those placed in foster care are slightly less likely to be female (46% compared to 50%) and considerably more likely to be Black (43% compared to 29%). Those placed in foster care also have lower baseline school attendance rates and test scores. Consistent with other research highlighting the association between foster care and crime, we find that children who are placed are disproportionately likely to be arrested, convicted, and incarcerated. For example, children who are placed are 50% more likely to be arrested by age 19 than those subject to an investigation who are not placed (21% compared to 14%). Altogether, these descriptive statistics indicate that children placed in foster care differ in systematic ways from children who are investigated but not removed.

IV Empirical Strategy

A naïve analysis of foster care might regress a measure of children's later-in-life criminal activity, such as an indicator for ever convicted as an adult, on a binary treatment variable equal to one if the child's investigation resulted in foster placement. Even with covariates to account for a wide range of observable characteristics, estimates of foster care from such a regression likely contain bias because foster children differ along unobservable dimensions from investigated children who are not removed. For example, children who are placed may be more severely abused or neglected than those who are not. Such unobserved features would bias OLS estimates to understate the benefits of foster care and overstate its costs.

IV.A Research Design

To address omitted variable bias, we implement the examiner assignment research design, which has been used in other studies of foster care (Doyle, 2007, 2008; Gross and Baron, 2022) as well as research on incarceration and prosecution (Agan, Doleac and Harvey, 2021; Aizer and Doyle, 2015; Bhuller et al., 2018, 2020; Kling, 2006; Mueller-Smith, 2015; Norris, Pecenco and Weaver, 2021), disability insurance (Dahl, Kostøl and Mogstad, 2014), and evictions (Collinson et al., 2021), among others. Specifically, we instrument for placement using the removal tendencies of quasi-randomly assigned investigators. Children assigned by chance to particularly strict investigators—those with high propensities to remove—are more likely to be placed than they would have been had they been assigned to a more lenient investigator. To reliably measure investigator removal tendencies, we focus on children assigned to investigators who we observe work at least 50 cases in the child welfare data, inclusive of quasi-randomly assigned cases outside of the analysis sample.[4] Overall, our analysis sample includes 3,027 investigators assigned to 235 cases each, on average.

Following the examiner assignment literature, we calculate the instrument as the fraction of all other investigations, both past and future, assigned to the same investigator that result in foster care placement. Specifically, for investigation i assigned to investigator w:

$$Z_{iw}^{R} = \left(\frac{1}{n_w - 1}\right) \sum_{k \neq i}^{n_w - 1} (FC_{kw}) \tag{1}$$

where n_w equals the total number of cases assigned to investigator w, and FC_{kw} is an indicator equal to one if investigation k results in foster care.[5] This instrument is equivalent to the investigator fixed effect from a leave-out regression where foster placement is the dependent variable.

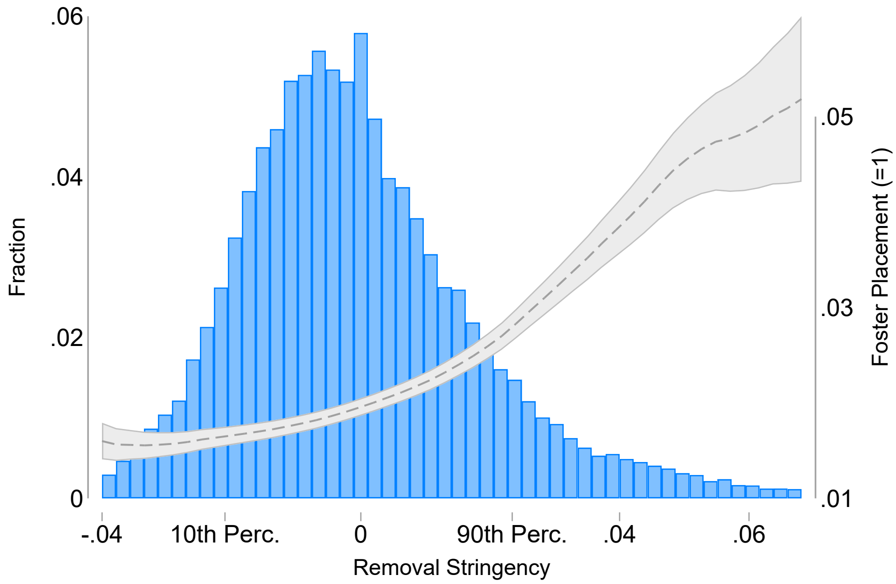
We find that there is considerable variation in investigator tendencies. The instrument has a mean of 0.033 and a standard deviation of 0.025. There is also variation in removal tendencies among

[4] In Section VII, we show that the results are robust to using a larger threshold.

[5] In Section VII, we show that the results are robust to alternative measures of investigator stringency, such as allowing tendencies to vary over time.

investigators who work in the same local office. Figure 2 shows the distribution of the instrument net of child zip code by investigation year effects. An investigator at the 10^{th} percentile removes at a rate 2.1 percentage points less than the average investigator in their local area, whereas an investigator at the 90^{th} percentile removes at a rate 2.3 percentage points greater. Relative to the average removal rate of 3%, this indicates that moving from the 10^{th} to the 90^{th} percentile represents an almost 150% increase in the likelihood of placement.

Figure 2: Distribution of Investigator Removal Stringency Instrument



Notes. This figure shows the distribution of the removal stringency instrument net of zip code by investigation year fixed effects. The dashed line shows point estimates from a non-parametric regression of placement on Z^R and the shaded region shows the 95 percent confidence interval.

We use the following two-stage least squares (2SLS) specification to estimate the causal effects of foster care:

$$FC_{iw} = \gamma_1 Z^R_{iw} + \gamma_2 X_{iw} + \Theta_r + \eta_{iw} \tag{2}$$

$$Y_{iw} = \beta_1 \hat{FC}_{iw} + \beta_2 X_{iw} + \theta_r + \epsilon_{iw} \tag{3}$$

where Y_{iw} is a child outcome, such as an indicator for ever arrested by age 19; X_{iw} is a vector of baseline covariates that includes a variety of socio-demographic and academic characteristics, such as gender, race and ethnicity, grade level fixed effects, and baseline standardized test scores.[6] Θ_r and θ_r represent child zip code by investigation year fixed effects to control for the level of investigator rotational assignment, which ensures that we only compare children who could have been assigned to the same investigator. There are 6,642 unique rotation groups, and the median rotation group consists of 15 investigators. Because including many covariates (e.g., over 6,600 rotation fixed effects) can induce bias in leave-out instrument approaches, we follow Norris, Pecenco and Weaver (2021) and

[6]See Table A10 for the full set of baseline covariates.

implement our main analysis using the unbiased jackknife instrumental variables (UJIVE) approach of Kolesar (2013) which is robust to this issue.[7] We cluster standard errors at the child level to account for the mechanical correlation in outcomes that arises by including the same child more than once in the dataset.[8]

Under standard instrumental variables assumptions, which we discuss in the next section, β_1 represents the impact of foster care placement on outcomes for children at the margin (compliers). Compliers in this setting are children for whom investigators might disagree about removal. Because most policy debates surrounding foster care focus precisely on the complier population, the LATE we identify in this study is for a particularly relevant population in child welfare policy.

IV.B Identification Assumptions

Four assumptions must be satisfied to interpret our estimates as the causal effects of foster care for children at the margin of placement: relevance, exogeneity, monotonicity, and exclusion.

Relevance. This assumption requires that investigator removal stringency predicts foster care placement ($\gamma_1 \neq 0$). Figure 2 above visually depicts the strong, positive relationship between investigator removal stringency and placement, and Table 2 reports the first-stage regression of foster placement on the removal stringency instrument.

Table 2: First Stage Relationship between Removal Stringency and Placement

	(1) Foster Care	(2) Foster Care	(3) Foster Care	(4) Foster Care
Removal Stringency	0.462***	0.435***	0.436***	0.435***
	(0.028)	(0.029)	(0.029)	(0.029)
Observations	118,273	118,273	118,273	118,273
F-Statistic	281.796	224.746	225.993	225.17
Zip Code by Year FE		✓	✓	✓
Socio-demographic Controls			✓	✓
Academic Controls				✓

Notes. This table reports estimates of γ_1 from Equation 2. Socio-demographic controls include gender, race and ethnicity, indicators for grade in school, an indicator for whether the child was the subject of a prior investigation, and the number of prior investigations. Academic controls include an indicator for free or reduced price lunch eligibility, an indicator for special education receipt, an indicator for ever expelled, daily attendance rate in the school year prior to the investigation, and the most recent pre-investigation standardized math and reading test scores. Standard errors are clustered by child.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[7]The UJIVE approach of Kolesar (2013) uses a leave-out approach to estimate investigator removal tendency conditional on the control variables included in the first and second stages in Equations 2 and 3.

[8]Section VII details several alternative inference approaches and shows that the main results are robust to other levels of clustering, such as by investigator, by rotation group, and two-way clustering by child and rotation group.

The correlation between the instrument and foster care is 0.46 (Column 1) and a one standard deviation (2.5 percentage point) increase in removal stringency increases the likelihood of placement by roughly one percentage point (Column 4). The F-statistic of 225 indicates that the instrument is strong.

Exogeneity. This assumption requires that the unobserved determinants of children's later-in-life outcomes are independent of investigator removal stringency ($\text{Cov}[Z^R, \epsilon] = 0$). We test an implication of exogeneity: that observable child and case characteristics are uncorrelated with the removal tendencies of the assigned investigator. As expected due to the rotational assignment of child welfare investigators, a rich set of characteristics are not jointly predictive of the instrument despite being highly predictive of placement itself (Table 3). As further evidence of exogeneity, the first stage F-statistic in Table 2 is stable with the inclusion of covariates.

Table 3: Balance Tests

Dependent Variable:	(1) Foster Care	(2) Investigator Removal Stringency
F Stat from Joint Test	11.306	1.224
P-Value from Joint Test	0.000	0.206
Observations	118,273	118,273

Notes. This table reports the results from regressions of the dependent variable (either foster care placement or investigator removal stringency) on zip code by investigation year fixed effects and the covariates listed in Table A7. Standard errors are clustered at the child level.

Average monotonicity. Recent advances note that pairwise monotonicity, the assumption that children who are removed by a particularly lenient investigator must also be removed by a stricter investigator, is neither realistic in most contexts nor necessary to estimate LATEs (Frandsen, Lefgren and Leslie, 2019; Norris, Pecenco and Weaver, 2021). Although the pairwise monotonicity assumption ensures that the instrumental variables estimator aggregates treatment effects across complier groups using Imbens and Angrist (1994) weights, if a weaker assumption of average monotonicity holds, then our estimates will still be a proper weighted average of treatment effects with the weights for each child equal to the scaled covariance between foster care placement and investigator's removal tendencies (Frandsen, Lefgren and Leslie, 2019; Norris, Pecenco and Weaver, 2021). For average monotonicity to hold, the covariance between each child's investigator-specific treatment status and investigator stringency must be weakly positive. It follows from average monotonicity that removal stringency and placement should be positively correlated for all child subgroups. Table A1 shows that the first stage is positive and statistically significant across gender, age, and race and ethnicity groups.

Exclusion. Our analysis requires an exclusion restriction in order for the estimates to be interpreted as local average treatment effects. We discuss the exclusion restriction in detail in Section VII.

V Causal Effects of Foster Care on Adult Crime

This section presents new evidence on the causal effects of foster care on adult crime. We first report our main findings on adult arrests, convictions, and incarcerations, and describe the complier population for whom these findings apply. We then explore sources of heterogeneity, including subgroup analyses and an examination of MTEs.

V.A Main Findings

A naïve OLS analysis suggests that placement increases adult criminality (Panel A, Table 4). For example, we find that foster care is associated with a 4 percentage point increase in the likelihood of being arrested by age 19. This represents a 28% increase, relative to a control mean of 14.2%. We see similarly large increases in the likelihood of being convicted and incarcerated by age 19 (Columns 2 and 3). These results show that even controlling for detailed socio-demographic, school, and neighborhood characteristics, there is a strong positive association between foster care and adult crime. However, because of unobservable differences between children who are and are not placed (e.g., the severity of the maltreatment), the OLS estimates likely conflate the impacts of placement with unobserved factors.

Table 4: Effects of Foster Care on Adult Crime

	(1) Arrested by Age 19	(2) Convicted by Age 19	(3) Incarcerated by Age 19
	Panel A: OLS Estimates		
Foster Care	0.040***	0.021***	0.025***
	(0.008)	(0.006)	(0.006)
	{0.142}	{0.076}	{0.056}
	Panel B: 2SLS Estimates		
Foster Care	-0.252**	-0.281***	-0.210**
	(0.126)	(0.095)	(0.083)
	{0.370}	{0.346}	{0.262}
Observations	118,273	118,273	118,273

Notes. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors clustered at the child level are in parentheses and control means (Panel A) or control complier means (Panel B) are in curly brackets.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In contrast, 2SLS estimates show that placement substantially decreases adult criminality for children at the margin (Panel B, Table 4). We find that placement decreases the likelihood of being arrested by age 19 by 25 percentage points. Relative to a control complier mean of 37%, this suggests placement reduces the likelihood of an adult arrest by 68%. We find an even larger reduction in the likelihood of being convicted by age 19 of 29 percentage points, or 80% compared to a control complier mean of 35%. The estimated impacts on incarceration by age 19 are similarly large, equal to a 21 percentage point (or 81%) reduction. The point estimates are statistically significant and economically meaningful. For example, they are larger in magnitude than the estimated reduction in crime from cognitive behavioral therapy in the Becoming a Man program, which directly aims to reduce crime among vulnerable youth (Heller et al., 2017).

To explore whether our main estimates are driven by particular types of crimes, we examine heterogeneity in the effects of placement by the type of offense (Table A2). For this analysis and the remainder of the paper, we study convictions as the adult crime outcome to focus on incidents with a higher likelihood that a crime occurred and for ease of exposition. We test the effects of foster placement on convictions for each of the following four crimes: violent, property, drug, and public order. Violent crimes are the most costly to society (Chalfin, 2015) and the most common examples in the Michigan State Police data are homicide, assault, battery, robbery, and sexual assault. Examples of property crimes include larceny, fraud, and damage to property; drug crimes include possession, manufacturing, and delivery of a controlled substance; and public order crimes include traffic violations, purchase or consumption of alcohol by a minor under age 21, and disorderly conduct. We find that placement reduces convictions for violent crimes by 14 percentage points or 78% compared to a control complier mean of 18%, and the estimate is statistically significant at the 5% level. We also find negative point estimates for the other three crime types, including a 71% reduction in property crimes, although these are less precisely estimated.

V.B Characteristics of Children at the Margin of Placement

The 2SLS estimates show that foster care placement reduces later-in-life crime for compliers: children at the margin of placement, or those for whom investigators might disagree over whether removal from the home is appropriate. Table 5 shows that compliers make up 5% of children in our sample; this is consistent with the fact that most child welfare investigations are unsubstantiated and placement is rare (e.g., only 2% of children in our sample are removed). Compared with the full sample, compliers are more likely to be female, Black, Hispanic, and from families with low income. For example, nearly 40% of compliers are Black compared to 29% of all children subject to an investigation in our sample. Compliers are also slightly older and are more likely to live in a low income neighborhood. The reason for the child welfare investigation also differs between compliers and the overall sample. Compliers are almost twice as likely to be investigated for parental substance abuse (30% compared to 16%) and are less likely to be investigated for physical abuse (24% compared to 35%). This could reflect that investigators may yield greater discretion in cases involving parental substance abuse and less discretion in physical abuse cases. The share investigated for neglect is more similar for both groups. Compliers also score lower on standardized math and reading tests prior to the investigation.

Table 5: Baseline Complier Characteristics

	Full Sample	Compliers
Socio-Demographics		
Female	0.500	0.557
White	0.629	0.505
Black	0.287	0.394
Hispanic	0.060	0.089
Low Income	0.811	0.873
Ages 12 to 16	0.559	0.572
Urban/Suburban County	0.648	0.662
Low Income Neighborhood	0.500	0.578
Investigation Characteristics		
Neglect Allegation	0.522	0.482
Physical Abuse Allegation	0.345	0.237
Substance Abuse Allegation	0.164	0.300
Previous Academic Performance		
Above-Median Math Score	0.500	0.400
Above-Median Reading Score	0.498	0.391
Share of Sample	1.000	0.050

Notes. We calculate the share of compliers as the difference in the first-stage coefficient between children assigned to investigators with removal stringency at the 99th and the 1st percentiles (Dahl, Kostøl and Mogstad, 2014). We calculate the characteristics of compliers as the fraction of compliers across each characteristic subgroup.

After removal, most compliers are initially placed in a family home and reunify with their birth parents after about 1.5 years. Table 6 compares the average experience of all children placed in foster care to the experiences of compliers. On average, 56% of foster children are initially placed with relatives, 30% with an unrelated family, and 14% in a group home. Compliers are equally likely to be placed with relatives (55%), slightly more likely to be placed with an unrelated family (33%), and less likely to be placed in a group home (12%). About 40% of all foster children in our sample live in just one or two different placements while in foster care and 60% live in three or more. Placements for compliers are more stable; over half (52%) of compliers live in one or two placements. Compliers also had shorter stays in foster care, spending about three fewer months in the system (18 months compared to 21 months). Conditional on exiting, nearly four in five compliers reunify with their birth parents (83%). Fewer compliers are adopted (7%), have legal guardianship transferred (6%), or are emancipated as legal adults (4%). These permanency outcomes are similar for the average foster child.

Our research design cannot identify the causal effects of certain placement experiences on adult crime, such as placement in family homes or more stable placements. However, the experiences of compliers suggest that foster care reduces crime in a setting where most are initially placed in a family home, have stable placements, are in foster care for a relatively short period of time, and ultimately reunify with their birth parents. This is consistent with a large body of research showing that these experiences correlate with improved outcomes (Rubin et al., 2007, 2004; Ryan and Testa, 2005; Ryan et al., 2008).

Table 6: Foster Care Experiences of Compliers

	All Placements	Compliers
Initial Placement		
With Relatives	0.556	0.550
With Unrelated Family	0.304	0.334
In Group Home	0.140	0.116
Placement Stability		
Number of Different Placements	3.468	3.272
One or Two Different Placements	0.397	0.523
Three or More Different Placements	0.603	0.477
Placement Length		
Months in Foster System	21	18
Permanency Outcomes		
Reunified	0.802	0.832
Adopted	0.087	0.065
Guardianship Transferred	0.064	0.060
Emancipated	0.047	0.044

Notes. Column 1 reports the mean outcome among all foster placements and Column 2 reports the results from 2SLS regressions of the outcome variable on foster care. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Permanency outcomes are conditional on having exited foster care by the last available day in the child welfare data.

V.C Effects of Foster Care by Gender, Age, and Race and Ethnicity

We test whether the effects of foster care on adult criminality differ by child gender, age, and race and ethnicity (Table 7). Studies show that male children are often more vulnerable to disruptions or disadvantages than female children (Autor et al., 2019; Kling, Ludwig and Katz, 2005). We find that the reduction in convictions is entirely driven by male children. Foster care has no statistically significant impact on adult crime for female children. Previous research also shows that younger children benefit more from changes to their environment than older children (Chetty, Hendren and Katz, 2016; Chyn, 2018). Consistent with this research, our results suggest that the reduction in crime is driven by children ages 6 to 11 at the time of the investigation. There is also policy interest in understanding whether the causal effects of placement vary by child race and ethnicity (Barth et al., 2020). Our results suggest that foster care reduces crime for White, Black, and Hispanic children. The point estimates are similar in magnitude for White and Black children and are more negative, yet less precise for Hispanic children given the smaller sample size. When benchmarked to the control complier means, however, the effects are larger for White (87%) and Hispanic children (79%) than for Black children (60%).

Table 7: Effects of Foster Care on Adult Convictions by Gender, Age, and Race/Ethnicity

	(1) Male	(2) Female	(3) Ages 6 to 11	(4) Ages 12 to 16	(5) White	(6) Black	(7) Hispanic
Foster Care	-0.496***	-0.114	-0.446***	-0.095	-0.243*	-0.253*	-0.282
	(0.189)	(0.094)	(0.135)	(0.140)	(0.144)	(0.133)	(0.346)
	{0.593}	{0.189}	{0.530}	{0.138}	{0.283}	{0.424}	{0.358}
Observations	59,976	58,297	52,675	65,598	74,127	33,045	5,975

Notes. Each column reports estimates from a separate 2SLS regression of whether a child was convicted by age 19 on foster care. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Control complier means are in curly brackets. Standard errors are clustered by child.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

V.D Marginal Treatment Effects

We estimate MTEs to explore heterogeneity in the effects of foster care by unobservables and to understand the treatment effects for different populations. MTEs in this setting are average treatment effects for children on the margin of foster care placement—where the margin varies across the distribution of the unobserved propensity to be removed.

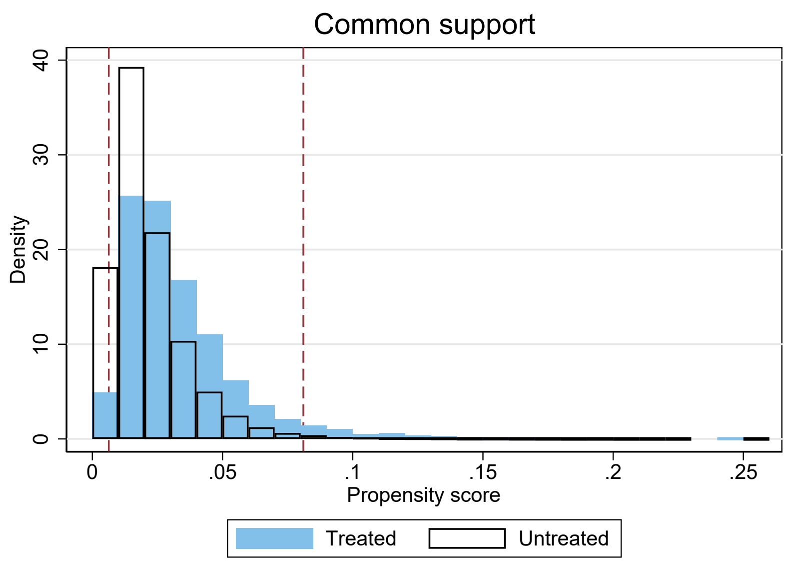
We follow the MTE framework described in Bhuller et al. (2020). We model the observed outcomes as $Y = I \times Y(1) + (1–I) \times Y(0)$. I is an indicator for foster care placement, $Y(1)$ is the potential outcome if children are placed and $Y(0)$ is the potential outcome if children are not placed. The assigned investigator will remove a child ($I = 1$) according to the following choice equation: $v(X, Z) - U > 0$, where $v()$ is an unknown function, X represents the child's observable characteristics, Z is the investigator's stringency, and U is an unobserved continuous random variable. In other words, the investigator's decision to remove is based on three factors: their own stringency, the child's characteristics observed in our data, and the child's characteristics not unobserved in our data. U represents a child's unobserved resistance to foster care placement, meaning children with lower values of U are more likely to be removed conditional on X and Z.

As Bhuller et al. (2020) show, after normalizing $U|X = x$ to be uniformly distributed over $[0, 1]$ for every value of X, then $v(X, Z)$ equals the propensity score of removal, defined as $p(X, Z) \equiv P[D = 1|X = x, Z = z]$. The MTE is defined as $E[Y(1)–Y(0)|U = u, X = x]$. That the MTE depends on U for a given X reflects unobserved heterogeneity in treatment effects. In addition to the assumptions of relevance, exogeneity, and monotonicity, estimating the MTE requires separability between observed and unobserved heterogeneity in treatment effects. This additional separability assumption is standard in the literature (Brinch, Mogstad and Wiswall, 2017) and is weaker than the standard additive separability assumption between I and X in the instrumental variables framework (Bhuller et al., 2020). Under these assumptions, the MTE is point identified over the common support of the propensity score.

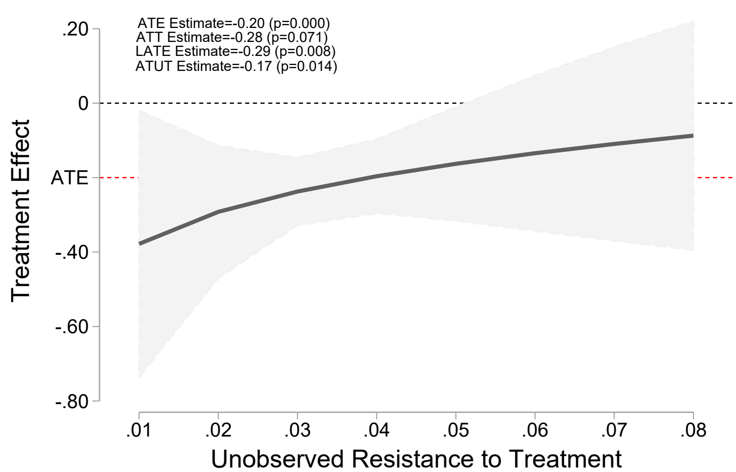
To characterize the region of common support, we plot the distribution of propensity scores for removal separately for children in our sample who are and are not placed (Panel A; Figure 3). The region of common support for which we can estimate the MTE ranges from 0% to 9%. Because of the small number of observations at the extremes, as is standard in the literature, we trim the top and bottom 1 percentiles of the overlapping sample prior to estimating the MTEs. The vertical dashed lines in the figure show the trimmed region of common support. Within this region, we estimate MTEs using a local instrumental variables approach and a global quadratic polynomial specification. We construct confidence intervals based on 100 bootstrap replications.

Figure 3: Common Support and MTEs

(a) Common Support



(b) MTE Estimates: Ever Convicted by Age 19



Notes. Panel A shows the distribution of the propensity score for treated (those that resulted in removal) and non-treated (those that did not result in removal) investigations. The dashed vertical red lines indicate the upper and lower points of the propensity score distribution with common support (based on one percent trimming). The MTE estimates in Panel B are based on a local instrumental variables approach using a global quadratic polynomial specification for the trimmed sample with common support. The shaded area represents 95% confidence intervals. Standard errors are constructed based on 100 bootstrap replications.

The MTE estimates of foster care on adult crime are negative across all values of the unobserved resistance to treatment. Panel B of Figure 3 plots the MTE point estimates on adult convictions by the unobserved resistance to treatment. The estimates are most negative for children with a low unobserved resistance to treatment and rise as the unobserved resistance to treatment increases. This implies that foster care placement reduces adult crime the most for children whose unobservables would make them likely to be placed regardless of the stringency of their investigator. These are likely cases involving relatively more severe maltreatment. At the other extreme, estimates of the MTE are smaller and statistically insignificant for children whose unobservables would make them unlikely to be placed regardless of investigator stringency. These are likely cases involving relatively less severe maltreatment.

We use the MTEs to estimate other economically interesting treatment effect parameters. As shown in Heckman and Vytlacil (2007, 2005), all conventional treatment effect parameters can be expressed as weighted averages of the MTEs, including the average treatment effect (ATE), the average treatment effect on the treated (ATT), and the average treatment effect on the untreated (ATUT). Recovering these parameters for the entire population requires full support of the propensity score over the unit interval (Bhuller et al., 2020), which Panel A of Figure 3 shows is not the case in our setting. Therefore, we follow Carneiro, Heckman and Vytlacil (2011) and Bhuller et al. (2020) and rescale the weights so that they integrate to one over our region of common support. We report estimates of the LATE, ATE, ATT, and ATUT in the upper left corner of Panel B. The estimated ATT shows that foster care reduces adult convictions by 28 percentage points for children who are placed, which is nearly identical to the estimated LATE. The estimated ATE is also negative yet smaller in magnitude than the ATT and LATE. Given the relatively narrow region of common support, however, these estimates should be interpreted with caution.

VI Mechanisms

The results presented so far show that foster care placement leads to large reductions in adult crime. This section explores potential mechanisms driving these effects. We begin by showing that foster care improves a variety of other outcomes, including safety, academic, and behavioral outcomes, that likely contribute to the reduction in adult crime. Consistent with Gross and Baron (2022), we then show that improvements that birth parents make while their children are temporarily in foster care likely explain the effects on children's intermediate outcomes.

VI.A Impacts on Safety, Academic, and Behavioral Outcomes

The main goal of removing abused and neglected children from their homes and placing them in foster care is to protect them from subsequent maltreatment. Columns 1 and 2 of Table 8 show that foster care achieves this goal. Placement decreases the chances that children are the subject of a subsequent maltreatment investigation by 22 percentage points, or 58% relative to a control complier mean of 36%. It also reduces the likelihood that children are confirmed as victims in a subsequent investigation by 8 percentage points (67%).[9]

Consistent with improvements in safety, we find that foster care improves academic outcomes and may increase educational attainment. Close to half of complier children who are not placed are chronically absent from school in the years after their investigation, meaning that they are absent for over 10% of school days in a given year. Foster care reduces chronic absenteeism by 21 percentage points. We also see gains in standardized math test scores of about 0.4 standard deviations. These improvements in children's short-term academic outcomes appear to translate to increased educational attainment. We find large yet somewhat imprecise positive impacts on high school graduation and college enrollment.

Table 8: Effects of Foster Care on Safety, Academic, and Behavioral Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Safety		Academic				Behavioral
	Subject to Maltreatment Investigation	Confirmed Victim of Maltreatment	Chronically Absent	Math Test Score	Graduated High School	Ever Attended College	Ever Detained as Juvenile
Foster Care	-0.215***	-0.081**	-0.211**	0.428*	0.184	0.268*	-0.172*
	(0.080)	(0.037)	(0.101)	(0.256)	(0.177)	(0.154)	(0.094)
	{0.364}	{0.123}	{0.463}	{-0.685}	{0.558}	{0.304}	{0.207}
Observations	118,273	118,273	118,189	93,764	114,601	118,273	118,273

Notes. This table reports the results from 2SLS regressions of the dependent variable on foster care. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors in parentheses are clustered by child and the control complier means are reported in curly brackets. Because the outcomes in Columns 1 through 4 are time-varying, we construct an unbalanced investigation by school year panel and follow students in the years after their investigation. The point estimates in Columns 1 through 4 come from a specification where we pool all available years following the focal investigation. The outcomes in Columns 5 to 7 are measured at a single point in time and we use our main approach to estimate effects for these outcomes. Certain outcomes are not available for the full sample: (1) chronically absent, due to a small amount of missing absences data; (2) math test scores, because some children were investigated after tested grades or exempt from state tests; and (3) high school graduation, because some students leave Michigan public schools before grade 9 and their graduation status is unknown.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We also find evidence that foster care may influence children's behavior. As a measure of children's behavior, we examine the probability that students are held in a juvenile detention center at any time after the investigation. Juvenile detention centers are secure facilities generally used to hold youth after an arrest but before a court hearing and the median youth is held for about two weeks (Baron, Jacob and Ryan, 2022). We find that placement reduces the likelihood of being held in a juvenile detention center by 17 percentage points or 80%.

[9]It is unlikely that the decrease in subsequent reports is driven by changes to reporting behavior because conditional on being the subject of a report, investigations involving children who are placed are no more or less likely to be substantiated. In addition, caseworkers (who are mandatory reporters) visit foster children regularly, both during their time in foster care and after they exit, suggesting that actual maltreatment against foster children would be reported.

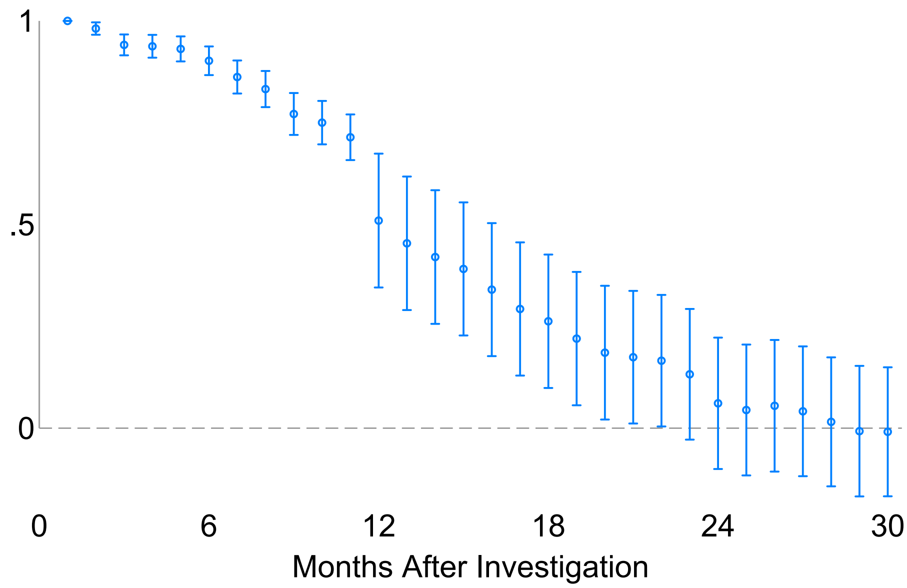
The improvements in children's safety, academic, and behavioral outcomes due to placement could contribute to the estimated reduction in adult crime through several channels. It is possible that improvements in academic outcomes and educational attainment increase the opportunity cost of crime through better outside options, which in turn might reduce adult criminality (Becker, 1968; Lochner, 2004). It is also possible that the decline in juvenile justice contact as a result of foster care itself reduces later-in-life crime. The literature has found that increased contact with the juvenile justice system causes increased involvement with the adult criminal justice system (Aizer and Doyle, 2015; Eren and Mocan, 2021). In addition, recent work highlights the important negative relationship between mental health and adult crime (Anderson, Cesur and Tekin, 2015; Bondurant, Lindo and Swensen, 2018; Jácome, 2020). Although we do not observe any measure of children's mental health in our administrative data, child abuse and neglect have been closely tied to worsened mental health. Clinical research shows that a history of abuse and neglect during childhood is a leading risk factor for mental illness (Arnow, 2004; Brown et al., 1999; Garno et al., 2005; Heim et al., 2010). Because we find that foster care protects children from subsequent abuse and neglect, placement could also improve their mental health and, consequently, reduce later-in-life crime. Any combination of these channels could contribute to the observed reduction in adult crime and we cannot disentangle the relative importance of each.

We can rule out certain alternative explanations for the reduction in adult criminality including a mechanical short-term reduction in crime from staying in school longer or moving to more advantaged neighborhoods or schools. As foster care appears to increase educational attainment, it is possible that placement reduces crime by age 19 only because youth spend more time in high school and mechanically have less time to commit crimes. Such an "incapacitation effect" would imply a short-lived reduction in criminality that ends after youth complete their schooling (Bell, Costa and Machin, 2021). To rule this out, we estimate the impacts of foster care using the subset of youth for whom we observe crime at older ages. We find sustained crime reductions through age 21, a time when youth would have either graduated or dropped out of high school (Table A3). It is also possible that, because children by definition move when they are placed in foster care, they move to more advantaged neighborhoods which could improve their outcomes (Chetty, Hendren and Katz, 2016; Chyn, 2018). However, children at the margin of placement spend only 1.5 years in foster care on average and more than 80% reunify with their birth parents (as previously reported in Table 6). Although it is possible that children exit foster care to more advantaged neighborhoods (e.g., if their birth parents move), we find that placement had little effect on the characteristics of children's census blocks and schools after foster care (Table A4).

VI.B Evidence That Birth Parents Make Improvements While Children Are in Foster Care

Thus far, we presented evidence that gains in children's safety, academic, and behavioral outcomes likely contribute to the observed reductions in adult crime—but how did foster care placement improve these intermediate outcomes? Recall that most complier children who are placed have short stays in foster care after which they reunify with their birth parents. Specifically, close to half exit foster care after one year and nearly all exit within two years (Figure 4). More than 80% of compliers reunify with their birth parents when they exit and return to similar neighborhoods and schools as compliers who are not placed.

Figure 4: Effects of Foster Care on Likelihood of Being in Foster System Over Time



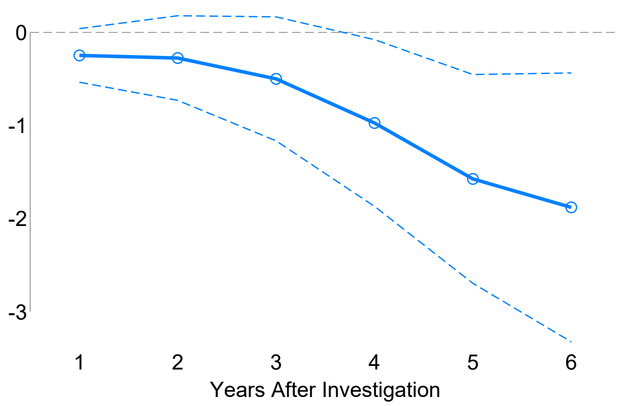
Notes. This figure reports 2SLS point estimates and 95 percent confidence intervals from regressions of the likelihood of ever being in the foster system during a given month on an indicator for foster placement. Because children must exit foster care by age 18, we use an unbalanced panel for this analysis where children who turn 18 years old exit the sample. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors are clustered at the child level.

These patterns begin to suggest that birth parents make improvements while their children were temporarily in foster care. To explore this mechanism, we examine the time pattern of impacts on children's safety and academic outcomes. Panel A of Figure 5 shows that the effects of foster care on the likelihood of being the subject of a subsequent maltreatment investigation are small and statistically insignificant in the first three years after the initial investigation. The gains in children's safety emerge in year four and persist in later years. We see similar trends for math test scores and chronic absenteeism (Panels B and C). That children's outcomes only improve after most foster children reunify offers further evidence that removal may have positive effects on birth parents.

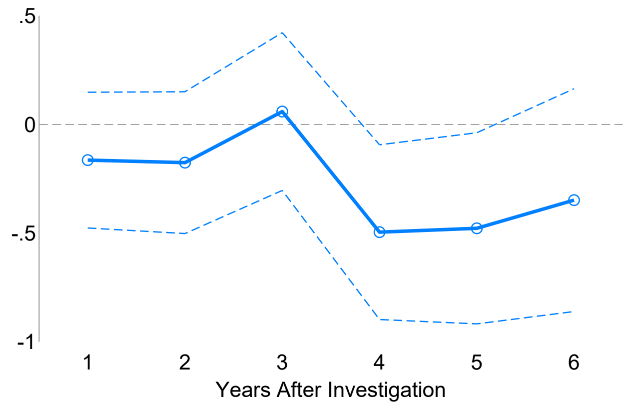
There are two institutional features that further support this channel. First, after their children are removed, birth parents work closely with social workers to address challenges in their own lives, such as confronting drug addiction and other substance abuse, finding stable employment, securing housing, or strengthening parenting skills. Birth parents receive fully funded services to help with these challenges, such as substance abuse treatment, parenting classes, or counseling. Second, a judge needs to approve that it is safe for children to return home before they can be reunified with their birth parents. Consistent with these features, we show that perpetrators of child maltreatment (almost always a birth parent), are less likely to abuse or neglect children years later if their initial child victim was placed in foster care (Panel D, Figure 5).

Figure 5: Effects of Foster Care on Safety and Academic Outcomes Over Time

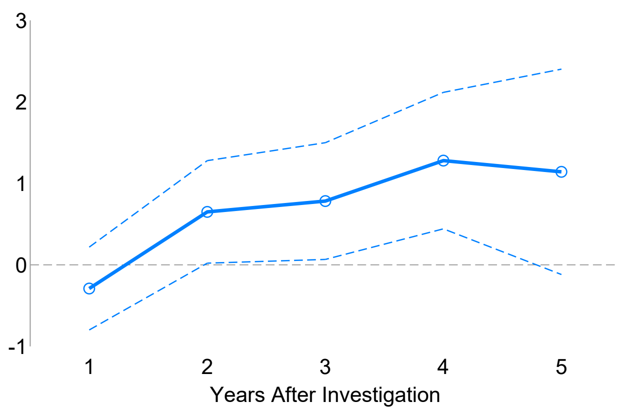
(a) Ever the Subject of a Subsequent Investigation



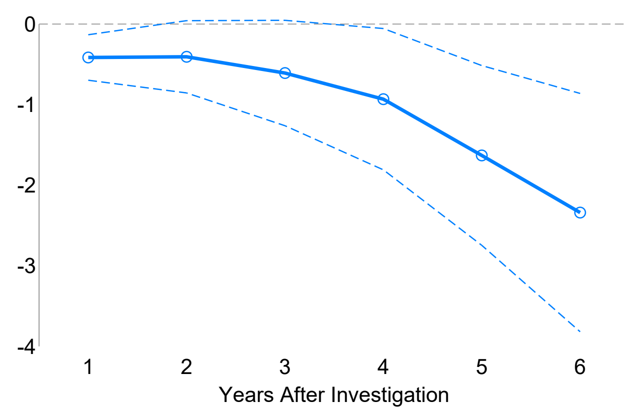
(b) Chronically Absent



(c) Math Test Scores



(d) Original Perpetrator Ever Alleged for Child Maltreatment Again



Notes. These figures report the 2SLS point estimates and 95 percent confidence intervals from regressions of the outcome variable on foster care. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors are clustered at the child level. Because children must exit foster care by age 18, we use an unbalanced panel for this analysis where children who turn 18 years old exit the sample. Panel C shows outcomes up to five years after the investigation (as opposed to six) because students in Michigan are only tested in grades three through eight. As a result, the sample size drops dramatically for this outcome after year five.

VII Threats to the Research Design and Robustness Checks

VII.A Exclusion and the Multi-dimensionality of Treatment

The exclusion restriction requires that the removal stringency instrument only influences children's later-in-life outcomes through foster placement. A potential concern is that investigators might influence children's experiences in foster care. However, investigators do not work with children after the investigation. Cases that require follow-up are transferred to child welfare caseworkers who work in different departments. Consequently, investigators are not involved in determining where children are placed, how long they remain in foster care, or the stability of their placements. For example, how long children spend in foster care depends on a variety of factors, none of which involve the initial investigator. Placement length in part depends on the progress that birth parents make toward regaining custody of their children, which is monitored by both the case worker and a judge. If parental rights are terminated, placement length depends on the supply of adoptive or guardian homes. As expected, we find that the instrument does not predict indicators of children's experiences in foster care, such as placement length, nor is it jointly related to these indicators (Table A5).

Although investigators do not influence children's experiences in foster care, they may affect children and families during the investigation in ways that could potentially influence outcomes. For example, investigators could vary in their sensitivity to a family's schedule or in the way in which they conduct themselves during the investigation process, which could influence outcomes. We are not able to observe all potential channels through which investigators could impact children's outcomes in the administrative data, however. Even detailed survey data on family experience would not fully capture all potential channels.

We empirically account for perhaps the most important way in which investigators might impact children's outcomes other than removal: investigators' influence over whether families are referred to prevention-focused services. Investigators place families on one of four tracks based on the strength of evidence that maltreatment occurred and the child's risk of future harm: (1) no services, (2) community-based services, (3) both community-based and targeted services, and (4) child removal plus community-based and targeted services (Figure 1). The exclusion restriction would be violated if investigators who are more likely to remove children are also more likely to recommend prevention services, and tendencies over prevention services are not included in the estimation (Mueller-Smith, 2015). We address this potential concern by leveraging the fact that quasi-randomly assigned investigators can also offer services to families whose children are not removed.

To explore the relative importance of removal and prevention services, we compare the impacts of prevention services without removal to the effects of services with removal. We create two new instruments according to Equation 1: investigator propensity to recommend community-based services alone (Z^C) and investigator propensity to recommend both community-based and targeted services without child removal (Z^{TC}). Together with the main removal stringency measure (denoted here by Z^{RTC}), we use these new measures to simultaneously instrument for tracks two, three, and four according to the following three first-stage and one second-stage equations:

$$RTC_{iw} = \gamma_1 Z^{RTC}_{iw} + \gamma_2 Z^{TC}_{iw} + \gamma_3 Z^{C}_{iw} + \gamma_4 X_{iw} + \kappa_r + \mu_{iw} \quad (4)$$

$$TC_{iw} = \alpha_1 Z^{RTC}_{iw} + \alpha_2 Z^{TC}_{iw} + \alpha_3 Z^{C}_{iw} + \alpha_4 X_{iw} + \chi_r + \nu_{iw} \quad (5)$$

$$C_{iw} = \delta_1 Z^{RTC}_{iw} + \delta_2 Z^{TC}_{iw} + \delta_3 Z^{C}_{iw} + \delta_4 X_{iw} + \pi_r + \zeta_{iw} \quad (6)$$

$$Y_{iw} = \beta_1 R\hat{T}C_{iw} + \beta_2 T\hat{C}_{iw} + \beta_3 \hat{C}_{iw} + \beta_4 X_{iw} + \Pi_r + \xi_{iw} \quad (7)$$

where RTC_{iw} is an indicator variable equal to one if the child is removed, TC_{iw} is an indicator equal to one if the family is referred to both targeted and community-based services, and C_{iw} is an indicator equal to one if the family is only referred to community-based services. Because the families of children who are removed also receive services, RTC_{iw} can only equal one when TC_{iw} and C_{iw} also equal one. In this specification, β_1 in Equation 7 measures the additional impact of placement relative to both targeted and community-based services without removal.[10]

Table A8 provides evidence to support the exclusion restriction that the removal stringency instrument operates only through foster care placement. We find that the impacts of prevention services without child removal (both community-based services alone and community-based and targeted services) are small and statistically insignificant. This indicates that investigator stringency over prevention services is largely unrelated to children's outcomes. In contrast, we find that the impacts of child removal above and beyond prevention services are even larger than the main estimates in Table 4. Overall, the evidence presented in this section suggests that there is little cause for concern regarding the exclusion restriction in our context.

VII.B Robustness Checks

Table A9 presents a variety of alternative specifications that probe the robustness of our main findings, including using alternative samples (Panel A), different measures of investigator stringency (Panel B), or other levels of investigator rotational assignment (Panel C). To assess whether certain sample restrictions influence the results, we conduct the analysis using two other samples: (1) the first investigation of each child and (2) excluding children assigned to investigators who worked fewer than 75 cases (the main analysis uses a threshold of 50). To assess robustness to other reasonable measures of investigator removal tendencies, we recreate the instrument in three ways: (1) randomly splitting the sample in half and defining the instrument as the investigator's removal rate from the other half of the sample, (2) allowing tendencies to vary over time by creating a leave-out-other-years measure, and (3) constructing a leave-out-same-year measure to account for removal decisions occurring around the same time potentially being correlated. Lastly, we examine sensitivity to the definition of investigator rotational assignment by including fixed effects for the county by investigation year instead of zip code by investigation year. Because some of the local offices in Michigan divide investigators into teams based on small regions, the main analysis includes zip code by investigation year fixed effects. However, a small share of zip codes in Michigan span more than one county, which could generate measurement error in our main analysis. Across all of these robustness checks, the estimates of foster care on adult crime are negative and statistically significant.

We also assess robustness to the specific covariates included in the 2SLS regressions and to the

[10] The first-stage relationships in Equations 4 through 6 are strong, with F-statistics ranging from 158 to 651 (Table A6). Furthermore, balance tests indicate that each of the three instruments is unrelated to a rich set of baseline child characteristics (Table A7). The three instruments are positively, but not perfectly, correlated with each other, indicating that there is independent identifying variation from each. Conditional on zip code by year fixed effects, Corr(Z^{RTC},Z^{C})=0.48, Corr(Z^{RTC},Z^{TC})=0.62, and Corr(Z^{TC},Z^{C})=0.80.

level of clustering standard errors. Our main analysis includes a variety of covariates in addition to the rotation group fixed effects and a potential concern is that the main findings are unique to that particular specification. However, the estimated impact of placement on adult crime is nearly identical regardless of what covariates are included (Table A10). In addition, although our main specification clusters standard errors at the child level to account for the correlation in outcomes that arises mechanically by including the same child more than once in the panel, we assess sensitivity to alternative levels of clustering. We find that the results are robust to the following alternative levels of clustering standard errors: investigator level, zip code-by-year level, two-way clustering at the child and investigator level, and two-way clustering at the child and zip code-year level (Table A11).[11]

Finally, one may be concerned that foster placement causes children to leave Michigan and commit crimes in other places, which we do not observe in the Michigan adult crime data. That is, the observed reduction in crime could be driven by increased out-of-state migration rather than an actual decrease in crime. We explore the extent to which out-of-state migration may influence our findings in Table A12. We find that foster care does not impact the probability that children leave the state during grades K–12 (Column 1) or for college (Column 2). We also estimate our main 2SLS specification while excluding children who left Michigan in K–12 (Column 3), attended college outside of Michigan (Column 4), or lived in a high out-of-state migration county (Column 5). Estimates of foster care on adult crime for these samples are very similar to our main findings.

VIII Marginal Value of Public Funds

The Marginal Value of Public Funds (MVPF) is a benefit-cost framework that produces a common metric for the relative effectiveness of spending on different programs. It compares the benefits that a policy provides to society, or society's willingness to pay, to the net cost to the government of implementing the policy (Hendren and Sprung-Keyser, 2020). In this section, we calculate the first estimate of the MVPF for a policy that promotes children's safety.

The first step to calculate the MVPF is estimate society's willingness to pay for foster care. To do so, we measure the social benefit as the reduction in social costs from foster care's effects on adult crime. We pair our detailed criminal justice records which show which types of crimes (if any) children committed with social cost estimates for each type in Chalfin (2015). We construct a variable equal to each child's social cost of adult crime, equal to zero for children who were never convicted by age 19 and equal to the sum of the cost of each conviction for children who were convicted. For example, if a child was convicted twice, first for homicide and then for carjacking, then we define

[11] Because the same investigator is assigned to multiple investigations throughout the panel, clustering at the investigator level accounts for potential correlations in the error term within investigator but across investigations and time. Clustering at the zip code-by-year level accounts for potential neighborhood- or child welfare office-level shocks. Two-way clustering by child and investigator or by child and zip-year level account for all of the potential correlations in outcomes described above.

the child's social cost as the sum of the social costs of homicide and carjacking. Because foster care placement occurs years before an adult conviction, we discount the social cost of each crime using a 3 to 5% rate (Anders, Barr and Smith, 2022). Using this variable as the outcome in our main 2SLS specification, we show that the reduction in social costs ranges from about $84,000 to $95,00 depending on the discount rate (Panel A of Table 9).

Table 9: Marginal Value of Public Funds

	(1) 3% Discount Rate	(2) 4% Discount Rate	(3) 5% Discount Rate
	Panel A: Society's Willingness to Pay		
Foster Care	-95,319***	-89,374***	-83,854***
	(33,667)	(31,619)	(29,715)
Observations	118,273	118,273	118,273
	Panel B: Direct Cost to the Government		
Direct Cost	$49,920	$49,920	$49,920
	Panel C: Cost Savings to the Government		
Foster Care	-13,742**	-12,937**	-12,188**
	(6,992)	(6,588)	(6,212)
Observations	118,273	118,273	118,273
	Panel D: Estimates of the MVPF		
Willingness to Pay	$95,319	$89,374	$83,854
Net Cost	$36,178	$36,983	$37,732
MVPF	2.63	2.42	2.22

Notes. All monetary amounts are inflated to 2012 dollars. Panel A reports the results from 2SLS regressions of the total social cost of the convictions for each child on foster care. The dependent variable is equal to zero for children who were never convicted by age 19. We discount the social cost of each crime using a 3 to 5% rate from the age at conviction to age 12, the average age at investigation in our sample. Panel B reports the cost of each out-of-home placement in Michigan in 2018 to the federal, state, and local governments (ChildTrends, 2021). Using the same methods as in Panel A, Panel C reports the results from 2SLS regressions of the child's total police, court, and incarceration costs on foster care. Panel D presents estimates of the MVPF, equal to society's willingness to pay divided by the net cost of foster care to the government. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors clustered at the child level are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The next step is to calculate the net cost of foster care to the government which includes both the direct costs of each placement as well as the cost savings from less criminal activity (for example, savings from fewer incarcerations). This improves on typical cost-benefit analyses which do not consider long-run government savings as part of a program's cost. The direct cost of each placement in Michigan is about $50,000 (ChildTrends, 2021) (Panel B).[12] For the cost savings associated with

[12]The direct cost of marginal placements in our setting may be even lower because compliers spend relatively less time in foster care than the average placement and are somewhat less likely to be placed in institutional settings (Table 6), which tend to be more expensive (Barth, 2002; Ward and Holmes, 2008).

less crime, we use estimates from Heckman et al. (2010) for the police and court costs associated with each arrest and the incarceration costs for a given incarceration spell. Similar to calculating the reduction in social costs, we conduct our 2SLS analysis where the dependent variable is the sum of the cost of each arrest and incarceration for each child in our sample. Panel C shows that cost savings range from \$12,000 to \$14,000 depending on the discount rate. Combining the direct cost of each placement with these cost savings, the net cost of foster care to the government is between \$36,000 and \$38,000.

We calculate the MVPF as the reduction in the social cost of crime divided by the net cost of foster care to the government. The MVPF ranges from 2.22 to 2.63 depending on the discount rate, which means that society receives more than \$2 in benefits for every \$1 in government costs (Panel D). These estimates are larger than the MVPF of other social policies for children and adults. For example, Hendren and Sprung-Keyser (2020) calculate an average MVPF of 1.78 across four health insurance expansions to children and an upper bound of 1.20 for adult policies such as housing vouchers, tax credits, and cash welfare programs. Moreover, the estimated MVPF in our study is likely conservative for several reasons. First, the benefits include only crime reductions and not the other benefits we estimate, such as increases in educational attainment, which could increase earnings. Second, as is common in the economics of crime, the social cost of crime reductions excludes relatively minor crimes, such as traffic or drug offenses (Chalfin, 2015). Lastly, the cost savings to the government exclude savings from fewer subsequent child welfare investigations or juvenile detentions. For these reasons, policies that promote child safety not only improve children's outcomes but also provide a substantial return to society.

IX Comparison to Doyle (2008)

The analysis in this study contrasts with the findings in Doyle (2008), the only other study to estimate the causal impacts of foster care on adult crime in the United States. Using administrative data on Illinois children investigated between 1990 and 2003, the study finds that placement increases adult arrests by 39 percentage points and convictions by 41 percentage points (see Table 4 in Doyle (2008)). We explore many potential explanations for why our findings differ from Doyle (2008) and conclude the most likely explanation is that foster care in Illinois two decades ago was tremendously different than in Michigan more recently.

1. Differences between study settings: The two studies examine different time periods and focus on different states. We next discuss how each of these dimensions could explain the different findings.

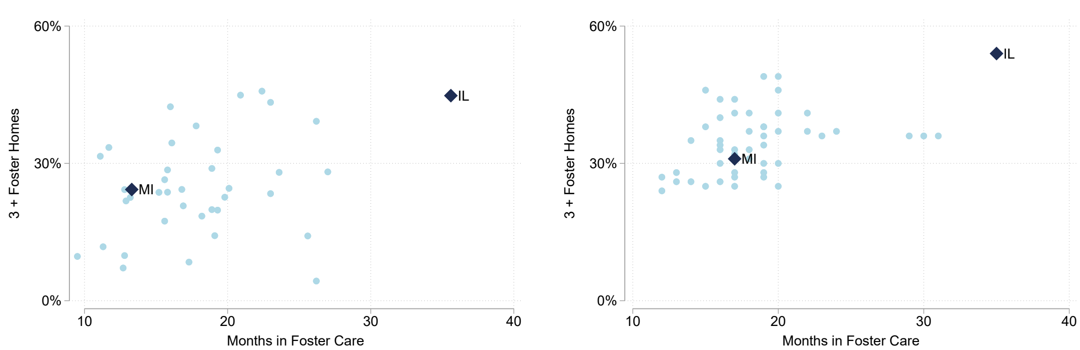
First, child welfare policy has changed over time in ways that likely have improved foster care. The federal government has enacted several key policies after the start of the Doyle (2008) sample period focusing on three areas: reducing placement length, improving the quality of placement settings, and promoting the wellbeing of children while in foster care. The Adoption and Safe Families Act of 1997 aimed to reduce the length of stay by generally requiring states to terminate parental rights for children who spent 15 of 22 consecutive months in foster care. Accordingly, the proportion of children in foster care with short stays (between one and two years) increased from 18% to 30% from 1998 to 2017 (ChildTrends, 2018). This could have improved foster care because there is an extensive literature showing a negative association between placement length and children's outcomes (Rubin et al., 2007, 2004; Ryan and Testa, 2005). There has also been a shift toward increasing placements with relatives and decreasing placements in institutional settings, in part driven by the Fostering Connections to Success and Increasing Adoptions Act of 2008. The share of children placed with relatives increased by 33% from 2008 to 2017 (24% to 32%) while the share placed in group homes decreased by roughly the same amount (16% to 11%) (AECF, 2017). Perhaps due to negative peer effects, research shows that group home placements are associated with worse outcomes (Bayer, Hjalmarsson and Pozen, 2009; Ryan et al., 2008). Lastly, there have been efforts to ensure children's safety and wellbeing while in foster care, including requiring more regular visits from caseworkers (e.g., through the Child and Family Services Improvement Act of 2006), mandating criminal background checks of prospective foster parents (the Adam Walsh Child Protection and Safety Act of 2006), and coordinating mental health care services for foster children (the Child and Family Services Improvement and Innovation Act of 2011). To the extent that these policies have improved child welfare practice, one would expect more recent studies to find less detrimental effects of foster care.

Even without changes in federal child welfare policy over time, one might expect different effects of foster care in Illinois and Michigan. There are large and persistent discrepancies between the foster care systems in the two states. For example, placements were and continue to be considerably longer and less stable in Illinois. Figure 6 shows that median placement length was 36 months in Illinois in 1998 and 35 in 2015, compared to 13 and 17 in Michigan, respectively. Likewise, about 45% of foster children in Illinois in 1998 and 54% in 2015 lived in three or more settings while in foster care, compared to 24% and 31% in Michigan, respectively. As described above, placement length and instability correlate with worse outcomes.

Figure 6: Comparison of State Foster Care Systems

(a) 1998 Statistics

(b) 2015 Statistics



Notes. For each state, this figure plots the median number of months spent in foster care and the share of foster children who lived in at least three different foster homes in 1998—the first year of publicly available data (USDHHS, 2003)—and in 2015 (ChildTrends, 2017; KCDC, 2015). 10 states did not report these statistics in 1998.

2. Differences in marginal placements. Because the examiner assignment research design identifies the impact of foster care for compliers, the estimated relationship between foster care and adult crime could depend on the population of compliers. To examine whether or not there were differences in marginal placements across settings, we compare the observable characteristics of the complier populations in each study (see Section V.C for complier characteristics in Doyle (2008)), and find that the complier populations are similar. Compliers in Doyle (2008) were more likely to be female, Black, and older relative to children in the overall sample. Likewise, compliers in the current study are also more likely to be female, Black, and older (Table 5).

Although compliers were similar in their observable characteristics, they may have differed in ways not captured in administrative data. For instance, placement could harm children who face little risk in the home and benefit those who face more risk, and it is challenging to quantify risk. One way to examine whether marginal children faced less risk in the home in Doyle (2008) than in our study is to compare the placement rate across studies. The placement rate (that is, the percentage of children subject to an investigation who were placed) was close to 20% in Doyle (2008) compared to about 2% in our study. Because Illinois removed a much larger percentage of investigated children, it is possible that the marginal child faced less risk in the home than in Michigan more recently, and thus placement could have had more detrimental effects. However, the placement rate depends on the number of children who are investigated for maltreatment, which fluctuates over time and across states.[13] Another approach that is less sensitive to changes in maltreatment reporting is to compare the share of the overall child population that is placed in foster care. All else equal, marginal children in a setting in which more children are placed likely face less risk in the home than those in a setting in which fewer children are placed. As discussed in Gross and Baron (2022), about 3 out of every 1,000 children were placed in both study settings (AECF, 2017; Wulczyn, Hislop and Goerge, 2000). Consequently, it is unclear to what extent unobserved differences in compliers across settings may contribute to the divergent findings.

3. Differences in sample restrictions: The sample in Doyle (2008) included children ages 4 to 16 who received Medicaid before their investigation. The sample in the current study includes investigated children ages 6 to 16 regardless of Medicaid receipt. To assess whether the differences in the sample selection could explain the differences in findings, we restrict our analysis to students who were eligible for free or reduced price lunch in any academic year prior to the investigation (a possible proxy for Medicaid receipt). We find estimates of foster care placement for this sample that are similar to our main analysis (Table A13). Although we cannot examine impacts for 4- and 5-year-olds because they are too young to appear in our adult crime data, it is extremely unlikely that excluding these children could explain the differences in findings because the reduction in crime in the current study is driven by younger children (Table 7). Therefore, differences in the analysis sample definition do not appear to contribute to the differences in findings.

Taken together, the most likely explanation for the differences in findings between the current

[13] In particular, laws passed after the Doyle (2008) sample period mandating that certain occupations report any suspicion of child abuse or neglect have been shown to increase the number of children investigated (Prettyman, 2021).

study and Doyle (2008) appears to be the large differences in foster care systems across the two settings. In other words, the "treatment condition" was fundamentally different in Illinois at the time and in Michigan more recently. We find little evidence that differences in the marginal placement across studies, and no evidence that differences in sample restrictions, contribute to the contrast in findings.

X Conclusion

This study provides new evidence on whether or not there is a foster care-to-prison pipeline. Studying nearly 120,000 child welfare investigations in Michigan between 2008 and 2016, and leveraging the quasi-random assignment of child welfare investigators, we find that foster care placement reduces the likelihood that children are arrested, convicted, and incarcerated as adults. We show that without accounting for selection into foster care, one may incorrectly reach the conclusion that foster care is criminogenic.

We find that a likely explanation for the reduction in crime is that birth parents make improvements while their children are in foster care. Children in our setting exit foster care within one to two years and most reunify with their birth parents; gains in intermediate outcomes only emerge after this time. This pattern could be explained by the fact that birth parents work closely with social workers after removal and receive fully funded services to address challenges in their lives. As a result, one might suspect that the improvements in children's outcomes are driven by adult services and not placement. That is, that we would observe similar impacts for children if their birth parents had received services while they remained in the home. However, the families of complier children who are not placed often also receive services, indicating that there is something different about the experiences of parents whose children are placed. For example, perhaps the incentives to make changes are stronger for parents working to regain custody of their children than for parents who face the threat of child removal. Parents might also benefit from time and space away from their children to focus on themselves. In addition, although the families of compliers who are and are not placed typically receive similar types of services, the services may differ in terms of dosage or content in ways that we do not observe in the data.

Because abused and neglected children who are not placed are more likely to be involved in the criminal justice system as adults, our results indicate that current efforts to protect vulnerable children in their homes are falling short. The findings in this study are particularly important in light of the Family First Prevention Services Act of 2018, bipartisan legislation which prioritizes keeping children with their families. For the first time, this historic policy allows states to use federal Title IV-E funding on evidence-based programs and services that aim to prevent foster care placement. Given the federal push to keep families intact, our analysis indicates that safely reducing foster care caseloads will require improving efforts to ensure child wellbeing in the home. Child welfare programs and services funded through Title IV-E represent one path to strengthen prevention, yet policies outside of child welfare can also promote child safety. For instance, an extensive literature has found that broader social policies, such as the social safety net, can reduce child abuse and neglect (Aizer et al., 2016; Berger et al., 2017; Raissian and Bullinger, 2017). The fact that foster care passes cost-benefit analyses even when accounting only for its effects on adult crime suggests that policies that promote child safety are likely to yield substantial positive externalities. To meet the federal goal of safely reducing foster care caseloads, identifying and scaling effective policies and programs to ensure child wellbeing in the home is a crucial frontier for future research.

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Is There a Foster Care-To-Prison Pipeline? Evidence from Quasi-Randomly Assigned Investigators

E. Jason Baron and Max Gross

Online Appendix

Supplemental Online Tables

Table A1: Testable Implications of Monotonicity: First Stage by Subgroup

	(1) Female	(2) Male	(3) Ages 6 to 11	(4) Ages 12 to 16	(5) White	(6) Black	(7) Hispanic
Removal Stringency	0.490***	0.385***	0.398***	0.479***	0.371***	0.551***	0.654***
	(0.042)	(0.041)	(0.039)	(0.045)	(0.034)	(0.059)	(0.146)
Observations	58,297	59,976	52,675	65,598	74,127	33,045	5,975

Notes. Separately for each child subgroup, this table reports the first-stage relationship between investigator removal stringency and foster placement. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors are clustered at the child level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Heterogeneity by Crime Type

	(1)	(2)	(3)	(4)
	Ever convicted for a ...			
	Violent Offense	Property Offense	Drug Offense	Public Order Offense
Foster Care	-0.140**	-0.059	-0.036	-0.064
	(0.059)	(0.062)	(0.042)	(0.064)
	{0.179}	{0.084}	{0.045}	{0.099}
Observations	118,273	118,273	118,273	118,273

Notes. This table reports the results from 2SLS regressions of the dependent variable on a foster care indicator, using removal stringency to instrument for foster care. Standard errors clustered by child are shown in parentheses and control complier means are shown in curly brackets. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10.

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

Table A3: Effects of Foster Care on Adult Convictions Through Age 21

	(1) Convicted by Age 19	(2) Convicted by Age 20	(3) Convicted by Age 21
Foster Care	-0.281***	-0.302**	-0.221
	(0.095)	(0.127)	(0.200)
	{0.346}	{0.367}	{0.311}
Observations	118,273	93,279	68,067

Notes. This table reports the results from 2SLS regressions of the dependent variable on foster care. Standard errors clustered by child are shown in parentheses and control complier means are shown in curly brackets. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10.

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

Table A4: Effects of Placement on Neighborhood Characteristics Following Reunification

	(1)	(2)	(3)	(4)	(5)
	Neighborhood			School	
	Median Income ($1000s)	Share BA Degree or Higher	Employment Rate	Test Scores	Percent Low Income
Foster Care	9.241	0.035	0.026	0.248	-0.010
	(7.924)	(0.055)	(0.043)	(0.222)	(0.083)
	{30.503}	{0.092}	{0.807}	{-0.494}	{0.616}
Observations	118,273	118,273	118,273	118,273	118,273

Notes. This table reports the results from 2SLS regressions of the dependent variable on foster care. Because the outcomes are time-varying, we construct an unbalanced investigation by school year panel and follow students in the years after their investigation. The point estimates come from a specification where we pool all available years following the focal investigation. To focus on the years after children exited foster care, we excluded the first and second years after the focal investigation. Standard errors clustered by child are shown in parentheses and control complier means are shown in curly brackets. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Testable Implications of the Exclusion Restriction

	Removal Stringency
Days in Foster System	0.000
	(0.000)
# Different Foster Placements	-0.000
	(0.001)
Initial Placement with Relatives	0.006
	(0.005)
Initial Placement with Unrelated Family	0.007
	(0.005)
Observations	2,595
F Stat from Joint Test	0.727
P-Value from Joint Test	0.574

Notes. This table reports the results from a regression of the removal stringency instrument on indicators of the child's experience while in foster care. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors are clustered at the child level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: First Stage Effects of Investigator Tendencies Over Removal and Family Services

	(1)	(2)	(3)
Dependent Variable:	Child Removal, Targeted Services, and Community Services	Targeted Services and Community Services	Community Services
Tendency Over Child Removal	0.357***	-0.195***	-0.207***
	(0.035)	(0.061)	(0.081)
Tendency Over Targeted and Community Services	0.046***	0.687***	0.128***
	(0.016)	(0.032)	(0.044)
Tendency Over Community Services	0.001	0.026	0.633***
	(0.010)	(0.020)	(0.029)
Observations	118,273	118,273	118,273
F-Stat	158	439	651

Notes. This table reports the results from regressions of each of the three dependent variables on three investigator tendency instruments. The regression includes the following controls: gender, race and ethnicity, indicators for grade in school, an indicator for whether the child was the subject of a prior investigation, the number of prior investigations, an indicator for free or reduced price lunch eligibility, an indicator for special education receipt, an indicator for ever expelled, daily attendance rate in the school year prior to the investigation, and the most recent pre-investigation standardized math and reading test scores. Standard errors are clustered by child.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Balance Tests of the Conditional Random Assignment of Investigators

Dependent Variable:	(1) Child Removal, Targeted Services, and Community Services	(2) Targeted and Community Services	(3) Community Services	(4) Tendency Over Child Removal	(5) Tendency Over Targeted and Community Services	(6) Tendency Over Community Services
F Stat from Joint Test	8.816	12.291	5.033	1.193	1.380	1.297
P-Value from Joint Test	0.000	0.000	0.000	0.228	0.094	0.142
Observations	118,273	118,273	118,273	118,273	118,273	118,273

Notes. This table reports the results from regressions of the dependent variable (either foster care placement or investigator removal stringency) on zip code by investigation year fixed effects and the following covariates: gender, race and ethnicity, indicators for grade in school in the year of the investigation, an indicator for whether the child was ever the subject of a prior investigation, the number of prior investigations, free or reduced price lunch eligibility, an indicator for ever special education receipt prior to the investigation, an indicator for ever expelled prior to the investigation, an indicator for ever retained in grade prior to the investigation, daily attendance rate in the school year prior to the investigation, and the most recent pre-investigation standardized math and reading test scores. Standard errors are clustered by child.

Table A8: Effects of Adult Interventions on the Child's Later-in-Life Adult Convictions

	Convicted by Age 19
(A) Child Removal, Targeted, and Community Services	-0.399***
	(0.132)
(B) Targeted and Community Services	0.027
	(0.051)
(C) Community Services	0.032
	(0.032)
Observations	118,273
P-value (A) = (B)	0.001
P-value (A) = (C)	0.000
P-value (B) = (C)	0.950

Notes. This table reports estimates of β_1, β_2, and β_3 from Equation 7. The regression includes zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors are clustered at the child level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Robustness to Alternative Samples, Stringency Measures, and Levels of Rotation

	Convicted by Age 19
	Panel A: Alternative Samples
Child's First Investigation	-0.366***
N = 87,100	(0.120)
Investigator Assigned > 75 Investigations	-0.225**
N = 113,661	(0.098)
	Panel B: Alternative Stringency Measures
Split Sample	-0.307***
N = 118,273	(0.105)
Leave-Out Other Years	-0.126**
N = 118,273	(0.061)
Leave-Out Same Year	-0.514***
N = 118,273	(0.198)
	Panel C: Alternative Level of Rotation
County by Year	-0.296***
N = 118,273	(0.095)

Notes. Panel A reports the results from 2SLS regressions using alternative sample definitions, Panel B uses alternative measures of removal stringency to instrument for foster care, and Panel C reports the results using the main stringency instrument but replaces zip code by investigation year fixed effects with county by investigation year fixed effects. Except for Panel C, all regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors are clustered at the child level. In Panel B, the split sample instrument is the removal rate of the assigned investigator from a random half of the sample. The leave-out other years measure is the leave-out removal rate of the assigned investigator from other children who had investigations in the same calendar year. The leave-out same year measure is the leave-out removal rate of the assigned investigator from other children who had investigations in different calendar years.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Robustness to Control Selection

	(1) Convicted by Age 19	(2) Convicted by Age 19	(3) Convicted by Age 19	(4) Convicted by Age 19	(5) Convicted by Age 19	(6) Convicted by Age 19
Foster Care	-0.269***	-0.278***	-0.275***	-0.281***	-0.282***	-0.281***
	(0.096)	(0.095)	(0.095)	(0.095)	(0.095)	(0.095)
Baseline Controls						
Grade 2		0.013***	0.014***	0.015***	0.015***	0.015***
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Grade 3		0.025***	0.026***	0.028***	0.028***	0.027***
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Grade 4		0.025***	0.023***	0.025***	0.024***	0.023***
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Grade 5		0.031***	0.029***	0.031***	0.031***	0.030***
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Grade 6		0.049***	0.047***	0.049***	0.048***	0.047***
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Grade 7		0.064***	0.061***	0.062***	0.062***	0.060***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Grade 8		0.082***	0.079***	0.080***	0.080***	0.077***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Grade 9		0.101***	0.098***	0.097***	0.097***	0.095***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Grade 10		0.110***	0.108***	0.108***	0.108***	0.106***
		(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Grade 11		0.098***	0.095***	0.097***	0.097***	0.094***
		(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Std Math Score		-0.012***	-0.011***	-0.014***	-0.014***	-0.013***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Std Reading Score		-0.013***	-0.011***	-0.011***	-0.011***	-0.011***
		(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Female		-0.068***	-0.068***	-0.067***	-0.067***	-0.067***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Black		0.001	0.008	0.007	0.006	0.007
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
White		-0.037***	-0.032***	-0.031***	-0.031***	-0.030***
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Investigation Controls						
# of Prior Investigations			0.009***	0.008***	0.008***	0.008***
			(0.001)	(0.001)	(0.001)	(0.001)
Allegation was for Physical Abuse			0.014***	0.016***	0.016***	0.017***
			(0.002)	(0.002)	(0.002)	(0.002)
Perpetrator was a Parent			-0.004	-0.004	-0.004	-0.004
			(0.004)	(0.004)	(0.004)	(0.004)
Prior Academic Characteristics						
Attendance Rate				-0.126***	-0.125***	-0.123***
				(0.011)	(0.011)	(0.011)
Special Education				-0.001	-0.001	-0.001
				(0.003)	(0.003)	(0.003)
Ever Expelled				0.167***	0.167***	0.167***
				(0.026)	(0.026)	(0.026)
Free or Reduced Price Lunch				0.005**	0.005*	0.004
				(0.002)	(0.002)	(0.002)

	(1)	(2)	(3)	(4)	(5)	(6)
				(0.002)	(0.002)	(0.002)
Std Math Score X Std Reading Score				0.004*	0.004*	0.004*
				(0.002)	(0.002)	(0.002)
Std Math Score Squared				-0.003***	-0.003***	-0.003***
				(0.001)	(0.001)	(0.001)
Std Reading Score Squared				0.000	0.000	0.000
				(0.001)	(0.001)	(0.001)
Std Math Score Cubed				0.001***	0.001***	0.001***
				(0.000)	(0.000)	(0.000)
Std Reading Score Cubed				0.000	0.000	0.000
				(0.000)	(0.000)	(0.000)
School Controls						
Urban					-0.003	-0.003
					(0.003)	(0.003)
Charter					-0.008**	-0.008**
					(0.003)	(0.003)
% White					0.009	0.010
					(0.015)	(0.015)
% Black					0.013	0.022
					(0.015)	(0.015)
% FRPL					0.013*	0.013*
					(0.007)	(0.007)
Neighborhood Controls						
# Neighborhoods Lived in Before Investigation						0.002***
						(0.000)
Household Median Income						0.000
						(0.000)
Employment Rate						0.004
						(0.011)
% Bachelor's Degree or Higher						0.002
						(0.011)
% White						-0.008
						(0.017)
% Black						-0.022
						(0.018)
Homeless in SY Before Investigation						0.370
						(0.468)
Observations	118,273	118,273	118,273	118,273	118,273	118,273
Rotation Group FE	✓	✓	✓	✓	✓	✓
Baseline Controls		✓	✓	✓	✓	✓
Investigation Controls			✓	✓	✓	✓
Academic Controls				✓	✓	✓
School Controls					✓	✓
Neighborhood Controls						✓

Notes. Our preferred 2SLS specification throughout the paper includes the covariates listed in the leftmost column of this table. This table shows that the main 2SLS results in Table 4 are robust to alternative selections of control variables. Column 1 includes only zip code by investigation year fixed effects. Column 2 incorporates baseline controls including gender, race and ethnicity, grade-level fixed effects, and controls for a student's most recent pre-investigation math and reading test scores. Column 3 adds investigation controls: whether the allegation was for physical abuse or neglect, the child's relation to the perpetrator, and the number of prior investigations that the child was previously the subject of. Column 4 includes academic controls measured in the year before the investigation. Column 5 includes the characteristics of the school that the child attended in the year before the investigation. Finally, Column 6 includes characteristics of the child's neighborhood in the year prior to the investigation. All columns include indicators for any missing covariates. Standard errors are clustered by child.

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

Table A11: Robustness to Alternative Levels of Clustering Standard Errors

	Convicted by Age 19
Baseline (by child)	
Foster Care	-0.281***
	(0.095)
By Investigator	
Foster Care	-0.281***
	(0.095)
By Rotation	
Foster Care	-0.281***
	(0.094)
By Child and Investigator	
Foster Care	-0.281***
	(0.094)
By Child and Rotation	
Foster Care	-0.281***
	(0.096)

Notes. Our preferred 2SLS specification throughout the paper clusters standard errors at the child level. This table shows that the main 2SLS results in Table 4 are robust to alternative levels of clustering standard errors.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Robustness to Out-of-State Migration

	(1)	(2)	(3)	(4)	(5)
			Convicted by Age 19		
	Ever Left State in K-12	Ever Attended College Outside the State	Excluding K-12 Leavers	Excluding Out-of-State College	Excluding High Migration Counties
Foster Care	-0.003	-0.072	-0.300***	-0.304***	-0.217**
	(0.112)	(0.076)	(0.105)	(0.098)	(0.103)
	{0.141}	{0.118}	{0.377}	{0.373}	{0.288}
Observations	118,273	118,273	105,709	112,746	99,310

Notes. Columns 1 and 2 report point estimates from 2SLS regressions of the dependent variable on foster care. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors are clustered at the child level and control complier means are reported in curly brackets. The dependent variable in Column 1 is an indicator variable equal to one if the student ever left the state while in grades K–12. We measure this outcome using exit codes that are assigned to students who leave the Michigan public school system. The dependent variable in Column 2 is an indicator variable equal to one if the student ever enrolled in postsecondary education outside of Michigan according to the National Student Clearinghouse. The remaining columns report the 2SLS effects of foster care on whether a child is convicted by age 19 using a sample excluding: children who ever left the state in K-12 (Column 3), children who ever enrolled in postsecondary education outside Michigan (Column 4), and children from counties with relatively high out-of-state migration rates (Column 5)—defined as being in the top decile of county-level migration rates. We calculate these figures using county-level out-of-state migration rates from the 2005-2009 American Community Survey.

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

Table A13: Effects of Foster Care on Adult Crime for Sample Comparable to Doyle (2008)

	(1) Arrested by Age 19	(2) Convicted by Age 19	(3) Incarcerated by Age 19
Foster Care	-0.263*	-0.277***	-0.192**
	(0.134)	(0.101)	(0.089)
	{0.378}	{0.338}	{0.241}
Observations	95,541	95,541	95,541

Notes. This table reports the results from 2SLS regressions of the dependent variable on foster care. The analysis sample is restricted to mirror the sample in Doyle (2008) and includes only children who were eligible for free or reduced-price lunch in any year before the investigation. All regressions include zip code by investigation year fixed effects and the covariates listed in Table A10. Standard errors are clustered at the child level and control complier means are reported in curly brackets.

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