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EFFECTS OF ENHANCED LEGAL AID IN CHILD WELFARE: EVIDENCE FROM A RANDOMIZED TRIAL OF MI ABOGADO

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ABSTRACT

Children spend years in foster care, and there are concerns that bureaucratic hurdles contribute to unnecessarily long stays. In a novel approach to policy making, the Chilean government randomized the introduction of a program aimed at reducing these delays in order to evaluate its effects on child well-being. Mi Abogado (My Lawyer) provides legal aid and social services to foster children living in institutions. Using administrative data linked across government registries, we find the program reduced the length of stay in foster care with no increase in subsequent placement, resulting in savings that are substantially greater than the cost of the program. The program also led to a reduction in criminal justice involvement and an improvement in school attendance. The results demonstrate that investment in the quality of foster care services can improve child well-being.

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A data appendix is available at
http://www.nber.org/data-appendix/w30974
A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/4160

1 Introduction

Family courts determine whether children should be placed in foster care for their protection, and this practice is remarkably common. In the US, 37% of children will be investigated for child abuse or neglect during their childhood, with maltreatment substantiated for 12% (Yi et al., 2020; Kim et al., 2017). In both high- and low-income countries, roughly 5% of youth spend some time in foster care during their childhood (Fallesen et al., 2014; Rouland and Vaithianathan, 2018; Yi et al., 2020; García and Hamilton-Giachritsis, 2014). In addition to being common, child protective services provide far-reaching interventions to children and families that are particularly vulnerable. For example, foster children experience 2-3 times higher childhood mortality rates, and 7 times higher rates of depression and anxiety, compared to children with similar observable characteristics (Johnson-Reid et al., 2007; Turney and Wildeman, 2016).

Once children are in foster care, the primary aim of family courts is to rehabilitate and reunify families or secure an adoptive home, an outcome known as "permanency" (Becker et al., 2007; Ryan and Gomez, 2016; Konijn et al., 2019). The rehabilitation interventions can include drug treatment and mental health services. The act of child removal combined with these rehabilitative services means that foster care likely has a larger impact on children and families than most public policy interventions (Bald et al., 2022; Grimon, 2021).

These activities typically take two years to complete, and there are serious concerns that bureaucratic requirements, including prolonged legal proceedings, lead to unnecessarily long stays in care that harm child well-being (Farber et al., 2009; Miller, 2004; Miller et al., 2020). Despite this interest, there is little evidence on the causal effects of length-of-stay or interventions aimed at speeding child-protection processes on child outcomes (Blome and Steib, 2008; Hunter et al., 2014). While a large literature investigates the correlation between length of stay in foster care and child outcomes, the results are mixed, possibly due to selection bias (Font et al., 2021; Bender et al., 2015; Okpych and Courtney, 2014; Dworsky et al., 2013).

In this paper, we analyze a randomized controlled trial of an intervention that aimed to improve the legal representation of children in foster care to reduce bureaucratic frictions. The program, Mi Abogado (My Lawyer), was introduced in Chile in 2017. It provides foster children living in institutions access to a lawyer with a much smaller caseload compared to children not in the program. The program also provides each case with a psychologist and a social worker who work together with the appointed lawyer to connect children and families with services. At the time of Mi Abogado's introduction, it was recognized that the program could not serve all eligible children. In a novel form of policymaking, the rollout was structured with evaluation in mind. Together with the Experimental Policy Initiative of the Chilean Budget Office, the Ministry of Justice randomized access to the program. Another advantage of this setting is that high-quality administrative data in Chile provide a low-cost way to track child outcomes.

By design, the treatment group was recommended to the family court for entry into the program, resulting in a 60% increase in program exposure compared to the control group over the following two years. Intent-to-treat estimates show that this greater exposure results in more days living with their biological or adoptive family, an average increase of 6 days per quarter, or 26% more than the control group mean.

An innovation in this paper is that we can test whether a program that enhances case management to reduce bureaucratic frictions and length of stay improves additional barometers of child well-being: measures of child safety, criminal justice involvement and school attendance. Child safety does not appear to be compromised across three related measures: foster care re-entry, subsequent child protection investigations, and criminal victimization. Meanwhile, the treatment group experienced a 30% reduction in criminal justice involvement over the two years following randomization compared to the control group, including a reduction in reports of violent crimes. We also find some evidence for improvement in school attendance. Across all three types of outcomes, the estimated effects are larger for boys. More generally, the living-with-family results are remarkably similar across a range of other child and group-home characteristics, while the crime results are concentrated among children and group homes that are associated with a higher propensity to commit crimes.

We also find that the reduction in criminal justice involvement is unlikely to be driven by a change in surveillance when exiting foster care. In particular, we do not observe a change in crime reports at the moment children exit foster care, and our findings are stable when controlling for foster care placement in an exploratory mediation exercise.

In terms of spending, the treatment group accrues substantially lower child welfare costs compared to the control group: the reduction in length of stay in state custody results in savings that are greater than the cost of the Mi Abogado program itself. If we included the cost of criminal justice involvement, the cost-benefit comparison would be even stronger. While we do not observe every welfare-relevant outcome of interest, the results demonstrate that improving the quality of case management for children in residential care can improve child well-being.

The remainder of the paper is organized as follows. Section 2 provides background information on legal aid in child welfare, foster care placement in Chile, and the intervention. Section 3 details the randomization and the empirical strategy we use to analyze it. Section 4 describes the data, Section 5 reports the results, and Section 6 concludes.

2 Background

2.1 Legal Aid in Child Protection

Child protection has parallels to criminal justice, involving allegations reported to authorities (often by reports that are mandatory for physicians and educators), investigations by child protective services, and a family court that holds hearings to oversee the process. In particular, family courts decide on whether the child should be removed from home and placed in foster care. Once a child is in care, the goal of the case is typically family rehabilitation and reunification. If the court determines this is unlikely, then there is a process to terminate parental rights and seek an adoptive home. The average length of stay in foster care in high-income countries is two years (Bald et al., 2022).

Legal support for the child in this process varies across jurisdictions, but children are often represented by a lawyer, a court-appointed special advocate (CASA), or a guardian ad litem (Sexton, 2018; Miller et al., 2020). Their role is to represent the "best interests" of the child. Although such representation is increasingly common, there is little empirical work investigating the effects of different forms of legal aid on child outcomes (Cooley et al., 2019; Pilkay and Lee, 2015). Orlebeke et al. (2016) implemented a randomized evaluation of additional training for 264 lawyers in Washington and Georgia. The training improved adherence to best practices, although no difference in family reunification or adoption were detected. For a subset (older children in Washington), the time to these outcomes was reduced. Osborne et al. (2020) used propensity score methods and found that appointment of a CASA was associated with delays in family reunification, although CASAs are typically assigned to cases that are the most complex, which can confound comparisons (Cooley et al., 2019). Rashid and Waddell (2019) studied the staggered rollout of mandates for representation by a lawyer across states in the U.S., and they found that such a mandate increased the likelihood of adoption within one year of foster care entry by 14%.

Meanwhile, parents are rarely represented, but evidence from matched comparisons in New York City and Washington State suggests that such representation can reduce the time in foster care (Courtney and Hook, 2012; Gerber et al., 2019). Given the ubiquity of family courts and the large variation in how children and families navigate them, more research is needed to guide policy that can improve the functioning of this system.

2.2 Child Protection in Chile

In Chile, at the time of the intervention we are studying, the child protection system was administered by the Servicio Nacional de Menores (SENAME).[1] The allegations that lead to foster care involved some form of neglect in 84% of cases, while 28% involved physical abuse, and 18% were related to sexual abuse.[2]

Family courts not only determine placement into foster care and case disposition but also play a role in determining the type of placement. Currently, the most common placement type is with a foster family, often the child's own extended family (kinship foster care), and residential care is also common (Muñoz-Guzmán et al., 2015). Residential care is supervised by public and private non-profit agencies. In our data,

[1]The child welfare system is currently administered by Mejor Niñez, Servicio Nacional de Protección Especializado a la Niñez y Adolescencia.

[2]Authors' calculations based on SENAME and judiciary data; categories are not mutually exclusive.

residences vary from fewer than 10 to over 200 children, with the average (median) child living in an institution with 30 (48) other children (Appendix Figure A.1). The average length of stay in Chile is relatively long by international standards at three years (De Iruarrizaga, 2016).

In 2016, SENAME was the subject of a high-profile condemnation of the care and supervision provided within residences due to a large number of unexplained deaths over the prior decade. This included an investigation by a SENAME commission and by the United Nations Committee on the Rights of the Child (USDoS, 2019). The scrutiny led to a number of policy changes. First, there was a push to reduce the reliance on residential care. In 2010, there were 15,497 children in substitute care, including 12,350 (80%) in residential care. By 2021, there were 10,865 children in substitute care, including 4,451 (41%) in residential care (SENAME, 2021). Second, funding levels for residential care increased. Subsidies to residences had been US$300 per child per month, which was criticized as far lower than the estimated US$1,000 deemed necessary for high-quality supervision. In 2019, at the start of the intervention we are studying, the per-child subsidies had increased to approximately US$700 per child per month.

Third, there were calls for improved legal representation to protect the rights of children. While all children were nominally assigned a lawyer historically, we find that 16% of children did not have an official lawyer assignment in 2019. Even for those with lawyers, there were concerns that high caseloads prevented them from providing high-quality representation. In order to explore ways of protecting the rights of foster children, the Ministry of Justice started a pilot that in 2017 was formally initiated as the Mi Abogado program.

2.3 Mi Abogado Program

The Mi Abogado program delivers legal aid to children who are in foster care, with priority to children in group homes. Each child is assigned a triad composed of a lawyer, a psychologist, and a social worker with the goals of protecting the rights of children, promoting their return to family life (whether with their family of origin, with extended family, or through an adoption process), and providing access to services aimed at improving child well-being.

The program's intervention begins when the child is assigned to the program by a family-court judge. The program team then reviews the child's legal file and typically visits the residence to speak with the child and staff. Within the first 30 days of program initiation, the team is tasked with devising an interdisciplinary plan that involves a mental health evaluation, a diagnosis of social needs, and a legal strategy to overcome procedural hurdles. During the next three to six months, the team continues to meet with the child on a monthly basis, as well as the residence staff and the family, in an effort to speed reunification. Once a child leaves residence and is reunited with family, the Mi Abogado program continues to monitor the child's welfare for at least 90 days to verify the quality of the family reestablishment.

Compliance with the objectives of the program is monitored by the family court. Figure 1 reports the average number of processes carried out over the first year of participation for each child. Documentary work is the most common, averaging 18 processes, followed by interacting with the group home staff (13). The team or the lawyer meets with the child 9 times over the first year and meets with the family 4 times. Despite this being a legal aid program, court appearances are rare.

The nominal caseload of the lawyers is limited to 80, with a goal of fewer than 60. The nominal caseload of the psychologist and the social worker is limited to 240, with a goal of fewer than 180. Data on caseloads can be difficult to interpret, as cases often remain open even when they are dormant. In our investigation of the data, lawyers in Mi Abogado were assigned 109 cases on average in the last 12 months of our observation period. Over the same period, non-Mi Abogado lawyers averaged 309 case assignments (Appendix Figure B.1). Moreover, the lawyers' salary is higher, so the total amount that the program spends on lawyers is 5 to 6 times more than what it spends on psychologists or social workers.

In summary, while the strategy of the program is based on the work of the team, the program is called "Mi Abogado" because it is largely focused on legal aid carried out by the lawyer. This includes connecting families with rehabilitative services, as well as improved preparation for hearings and attention to the timely achievement of case goals. While relatively little time is actually spent in court, documentary work and meetings with the child, the residence, and the family constitute the bulk of the intervention. For more detailed information, Appendix C describes the tasks associated with each member of the team.

3 Empirical Strategy

3.1 A Randomized Controlled Trial

The Mi Abogado program was introduced in the four most populous regions in Chile in 2019: Maule, Biobío, Valparaíso, and Metropolitan, which includes the capital city, Santiago. The assignment of children to the program was overseen by the Ministry of Justice and facilitated by an evaluation team of the Experimental Policy Initiative at the Chilean Budget Office. To allocate the capacity-constrained number of openings, the team implemented a pragmatic randomized controlled trial. This method of introduction was chosen in order to evaluate the program and allocate slots in an equitable manner.

The eligible population was defined as all children between 6 and 18 years old who lived in a SENAME group home at some point during January and February of 2019 in these four regions, a total of 1,871 children. The randomization of the program occurred on March 30, the last day of the first quarter of 2019. Out of the 1,871 children, 581 were selected to enter the program. The randomization was stratified according to age group (older than and under 12 years), sex, and region. The number of available slots in the program, and the number of eligible children, varied by region. As a result, the share randomized to the treatment group varied markedly across regions: 32% in the largest region, Santiago, 92% in Maule, 10% in Valparaiso, and 7% in Bíobío (Appendix Table D.1 shows the sample sizes). We discuss the empirical implications of this varying propensity of treatment across strata below.

The program petitioned the court to enroll children assigned to the treatment group. The family-court judge then needed to accept the new lawyer for the case. As we show later, approximately 60% of the requests among the treatment group were granted soon after the randomization. The lack of compliance with the program meant that new slots became available, and in May 2019, the program randomly selected 51 children who were in the control group to be eligible for the program. We include these children as part of our treatment group, although results are not affected by how these children are included in the analysis. In addition, there was noncompliance among the control group, as they began entering the program over time as well. The main analysis considers intent-to-treat models for program engagement and child outcomes, and we explore the dynamics of estimated effects over time as well.

3.2 Empirical Model

Our goal is to test whether the Mi Abogado program was successful in increasing time living with a family outside the foster-care system and measures of child well-being. Given that we have longitudinal data on outcomes, we compare the treatment and control groups over time in event studies.[3] In particular, for child i in calendar quarter t and event time q,

$$Y_{iq} = \alpha + \mathbf{X_i}\boldsymbol{\beta} + \kappa T_i + \sum_{q \neq 0} \gamma_q \mathbb{1}\{Q_t = q\} + \sum_{q \neq 0} \theta_q \mathbb{1}\{Q_t = q\} \times T_i + \varepsilon_{iq} \tag{1}$$

where q is normalized as the number of quarters from the first quarter of 2019 (recall that the randomization occurred on the last day of the first quarter). X_i includes the strata indicators. We report estimates with a broader set of controls as well. The summation terms are indicators for each quarter in event time, and we are interested in the estimates of θ, the difference between the treatment and control groups in each quarter. The panel is balanced, and including individual fixed effects yields the same estimates.

Given our event-study findings, a more parsimonious model that pools the data into two periods, pre-randomization and post-randomization, provides a useful summary of the results along with more statistical power. For these models, we estimate:

$$Y_{iq} = \alpha + \mathbf{X_i}\boldsymbol{\beta} + \gamma T_i + \delta Post_q + \psi T_i Post_q + \varepsilon_{iq} \tag{2}$$

where $Post$ is a variable that takes the value of 1 in all periods after randomization and 0 otherwise. ψ is our main parameter of interest, which represents the average

[3] We report results for alternative estimation strategies. Our preferred approach allows a transparent inspection of intent-to-treat effects over time and provides more precision compared to other approaches we considered.

difference across the groups in the post-period relative to the average difference in the pre-period. For the event-study and difference-in-differences models, standard errors are clustered at the child level, which is the level of the randomization.[4]

There are concerns when estimating event studies and difference-in-difference models when the treatment evolves over time (De Chaisemartin and d'Haultfoeuille, 2022). The event in question is the time of the randomization rather than the time of program entry, which avoids issues related to staggered treatments. Instead, the estimates will provide a view of the evolution of intent-to-treat effects on program engagement and child outcomes over time, which can then be used to compare the costs and benefits of offering the program over a two-year period (de Chaisemartin and D'Haultfoeuille, 2022). We also investigate how the program exposure impacts outcomes over time. Caution is warranted in interpreting these dynamics, however, as they are potentially complicated by changing complier characteristics, and, most notably, the dramatic change in environment one year after the randomization in the form of the COVID-19 pandemic. Nevertheless, we will explore both complier characteristics and whether the effects of the program grow or decline over time.

4 Data Description

4.1 Data Sources

The analysis benefits from a wide range of outcomes visible longitudinally in registry data. The data are linked across administrative agencies in Chile using the child's social security number. Appendix Table E.1 reports the time periods for the data sources.

First, we have child protection data from SENAME from January 2017 to February 2021. This includes the dates of reports and their allegations. SENAME also oversees foster care, so we can observe the dates when children enter and exit different care settings, including an ID for each institution. For children who exit substitute care, we

[4]The randomization was carried out at the child level, although siblings may receive attention from the program. This contributes to noncompliance. We explored using family-level models, but the family identifiers contain measurement error that we do not want to incorporate. Instead, we use intent-to-treat models of engagement and child outcomes to yield unbiased estimates of both the costs and benefits of offering the program.

observe the disposition, including returning home or placement in an adoptive home. These data allow us to track whether children who exit the system subsequently reenter care as a measure of child safety. These data include demographics, including sex, age, and a measure of school delay defined as the difference between age and the age expected for the child's grade.

Second, the Judiciary Registry data described above allow us to observe criminal justice involvement from 2006 to August 2021. This includes reports to the courts when a child is suspected of committing a crime. We restrict the sample to begin in January 2014, as crime is rare prior to 2014 when the average age of children in the trial is under ten years old.[5]

We can also use the Judiciary Registry to observe victimization. This includes two main categories: children reported missing, which may be more likely for children in institutions, as residence staff are required to report children as missing if they are not in the residence at night; and children being reported as victims of a crime, which we use as a complementary measure of child safety along with the child protection reports. When SENAME data have missing allegation data and there is an open case involving child victimization at the time of the foster care placement, we use the victimization data to clarify the nature of the allegation.

We observe the associated lawyers for all children. Using these data, we can compute the number of cases assigned to lawyers as a proxy of their caseload. We do not estimate the caseload directly, however, because cases usually stay "open" even after they become inactive. These data also include information on family-court hearings, which we use to measure court activity in the case. For those participating in Mi Abogado, we observe program information, including dates of participation and processes carried out.

Finally, the Ministry of Education registry allows us to investigate schooling outcomes between March 2017 and December 2019. We have monthly school attendance data and annual school performance data, coded as the average performance across all subjects in a given year. The COVID pandemic severely impacted most school

[5]Other common criminal justice outcomes such as conviction and incarceration are more difficult to observe in the Justice Registry, as some fields appear to be incomplete. For example, most reports are not accompanied by a guilty sentence in our data, in part because many cases are not closed. We note these results in the cost-benefit analysis below.

activities beginning in March 2020, and it is not possible to obtain outcomes for 2020.

4.2 Program Engagement Measures

Using the Mi Abogado program data, we measure engagement in a few ways. First, we measure when a child enters the program. Given that the program can influence outcomes after initiation, and the end of the program is affected by the endogenous exit from foster care, our preferred engagement measure is one of exposure: days since first entry into the program. Further, we do not observe a program end date, so when we measure time in the program, we rely on the program rules that the Mi Abogado team oversees a case for up to 90 days after exit from foster care. An advantage of the detailed program data is that we observe the processes performed for each case, including visits with the child, the residence staff, and the family.

4.3 Child Outcome Measures

The first child outcome we consider is whether children are living with family instead of living in foster care, which is a focus of child welfare agencies and courts. For a given calendar quarter, we use the registry data to construct a proxy for the number of days when children are living with their family if they meet the following criteria: (1) they have yet to enter foster care or (2) they exited foster care to live with a biological or adoptive family and have not re-entered foster care. Children who exit foster care as adults (known as "aging out") are not recorded as having achieved this outcome of living with family.[6] We also examine the number of days children are in foster care as a complementary outcome.

One question is whether reducing the time before children are living with family improves child well-being, and, in particular, whether the family is a safe environment. We measure child safety by observing whether a child returns to foster care, whether there is a new investigation for child maltreatment, and whether the child is observed as a victim of crime, which is typically a form of child abuse. To the extent that the Mi

[6] In the jargon of child protection, these children have not achieved "permanency" by the time they leave foster care, which means they have not been adopted nor reunified with their biological family. In a small number of cases, children exit foster care alone or transition to another supervisory residence. These cases are also not coded as living with family. That is, children who exit foster care to live with family are considered to be living with their family unless they re-enter foster care.

Abogado program speeds the return home, program participants will have more "time at risk" for these outcomes. In addition, recall that the program provides services to children for 90 days after exit from foster care. To the extent that this greater surveillance leads to a nominal increase in child maltreatment *reporting*, the estimated effects of the program would be biased upward relative to effects of the program on actual child maltreatment.

Another set of well-being measures cover criminal justice involvement. In particular, we measure whether the child is suspected of committing a crime that has been reported to criminal justice authorities each quarter. In the main results, we consider the number of crimes reported, as this measures the intensity of criminal justice involvement. We also discuss other related measures, such as whether a crime is reported and the types of crimes reported.[7]

For educational outcomes, we focus on attendance in a given quarter. This is measured as the share of school days that the child attended school. We report effects on school performance as well, although these outcomes were too imprecisely estimated to yield insights into the effects of the program.

5 Results

5.1 Balance

Before exploring differences in outcomes across the treatment and control groups, we first report comparisons across the two groups in terms of observable characteristics at the time of the randomization. This set of baseline comparisons provides context for the system and the children involved. The comparisons also serve as a check that the prescribed randomization was carried out faithfully, which would result in similar baseline characteristics across the two groups conditional on the randomization strata. When comparing the means, we employ a regression of each characteristic regressed on an indicator that the child was in the treatment group and strata controls for region, sex, and age group.

Table 1 reports the results. The first two rows describe family-court activity that

[7] To the extent that the Mi Abogado program leads to more surveillance of children through legal-aid and other services, this would tend to bias our results toward zero.

we observe in our data back to 2010. Both groups are similar, with around 2.8 writs filed per quarter and 0.2 hearings per quarter, including quarters with no hearings when a case was not active. The next row shows that children spent approximately 26 days with their families and 62 days in residential care per quarter in the pre-period. Recall that all of the subjects were in a residence in early 2019 to be eligible for the program and the study.[8]

The criminal justice data show that the groups are comparable in terms of the number of times suspected of a crime per quarter (0.03), reported missing (0.07), and reported as a victim of abuse (6 per thousand) during the pre-period. The education data show that the subjects are disadvantaged, and the treatment and control groups are comparable. The share of days attending school in 2017–2018 is 66% according to official records. The children are in the 27th percentile among those with grades available in 2018.

The remaining rows show balance in other observable characteristics that we use as control variables in robustness checks. They average 1.4 siblings, and their school delay is large for this group at 0.8 years. The child maltreatment allegations that led to the placement in early 2019 are similar to those in the child welfare system as a whole: over 80% involve some form of neglect, approximately 30% involve physical abuse, and 17% involve sexual abuse. In terms of demographics, the average age is 14 at the time of the randomization. This is somewhat older than the full set of children in care, in part because participation in the evaluation was restricted to children at least age 6. Their first entry into any residence was at 11 years old, so many families have a long history with child protection. Fifty-seven percent of the sample are girls. The comparisons in this table confirm that the randomization resulted in treatment and control groups that are highly similar to one another as designed.

5.2 Program Engagement

The treatment group was randomized to have access to the program, but participation depended on approval from a family-court judge. There was also noncompliance as the control group gained access over time. Nevertheless, the randomization of court

[8] The remaining days are transitions between programs or family foster care.

petitions generated substantial variation in exposure to the program that we can use to evaluate its effectiveness.

For a first look at the difference in exposure to the program across the treatment and control groups, we report differences using daily data. To do so, we residualize the data by regressing an indicator for having entered the program on the set of strata controls and a treatment indicator. We then compare the average residuals for the treatment and comparison groups on each date, which describes the shares of the treatment and control groups that had ever entered the Mi Abogado program controlling for the randomization strata. We also report event-study estimates where the data are binned by calendar quarter.

Figure 2 shows that a few weeks after the randomization, there is a sharp rise in program participation among the treatment group relative to the control group. In the first quarter after randomization, the treatment group is 40 percentage points more likely to be participating in the program, and this difference falls over time. Members of the control group gradually enter the program over time until roughly 70% of the treatment group and 60% of the control group have participated in the program by mid-2021. The difference in cumulative exposure is shown in Figure 2b. This difference is increasing and concave in the time since randomization. After one year, the treatment group has 100 more days since first exposure to the program, and this increases to 150 days at two years.

The program processes follow a similar pattern, with the treatment group experiencing 4 more program interactions in the first quarter after randomization, increasing to 6 in the second quarter, and the difference falls afterward. Appendix Figure F.1 shows that this is distributed across documentary work, with over two more court filings each quarter during 2019, about one more child interaction, and two more interactions with residence staff. In addition, we see that in the quarter after randomization, the treatment group has one more writ entered into the system compared to the control group, a difference that is short-lived (Appendix Figure F.2).

These differences are summarized using the two-period difference-in-differences specification, which is simply the average difference across the treatment and control groups over the post-randomization period compared to the difference in the pre-period.[9] Ta-

[9]Recall that pre-period participation is not strictly zero because a small number of children were able to

ble 2 shows that the treatment group had 20 more days of exposure per quarter, approximately 60% higher compared to the control group's mean of 32 days over the post-randomization period. Column (2) reports results for days actually participating in the program each quarter, which reflects both program entry and exit. On average, the treatment group has 13.5 more days of participation in the program each quarter in the post-period.

5.3 Living with Family

The primary goal of child protection cases is to secure a permanent family relationship that will continue for life. The family court typically oversees efforts to rehabilitate the family. Less commonly, the court will proceed with termination of parental rights and seek an adoption: among children who exited care to live with a family in 2019, 5% went to an adoptive home. Mi Abogado aimed to improve legal representation and case management to overcome unnecessary delays in these processes. A first question when evaluating its effectiveness is whether the program achieved its goal of having children return to living with family.

Figure 3 reports the event-study estimates for the main child-welfare outcomes: living in a SENAME residence and living with family outside the foster care system. At the time of randomization, the groups are similar by construction: they are all in a residence in early 2019, with the treatment group having slightly more days in a residence. Following the randomization, the treatment group is less likely to be living in a residence, approximately 5 fewer days per quarter compared to the pre-period.

Similarly, in the three quarters after the treatment group was recommended for the program, the measures of living with family increase relative to the control group: the difference in ever living with a family each quarter rises to 8 percentage points higher, which remains stable over time at approximately 7 percentage points. Similarly, children spend an additional 5 days per quarter living with family on average rather than living in a SENAME residence or with a foster family.

Table 3 summarizes the impacts of the program on living with family and in residences. In terms of days living with a family, the treatment group averages 5.6 more

join the program during its pilot phase.

days per quarter, or 26% more compared to an average of 19 days for the control group during the post-randomization period. Column (2) shows that the likelihood of ever living with family during a calendar quarter is 6.6 percentage points higher for the treatment group, which is 25% higher than the control group's mean. The estimate is somewhat larger for boys, with a coefficient of 0.09 vs. 0.04 for girls who have a higher rate of leaving foster care to live with a family in the control group: 29% compared to 21% for boys.

Children are more likely to be living with family because they are leaving the SENAME residences faster. Column (5) shows that the treatment group is 4.6 percentage points less likely to be living in a SENAME residence each quarter, or 7% higher compared to the control group's mean of 63%. Similar results are found when examining days in a residence (Column (6)) and when we add additional controls (Table H.1). With so much attention devoted in the child welfare literature to time in care, it is noteworthy that a legal aid intervention can have a substantial effect on speeding children through the system toward the goal of family reunification.

5.4 Child Safety

One question that arises for programs aimed at speeding family reunification is whether the effort results in premature exits and child-safety concerns. Figure 4 shows event-study results for the three related measures of child safety. They do not suggest an increase in new investigations, crime victimization, or foster care re-entry. These null effects are despite the fact that the treatment group is more likely to have returned home, where these outcomes can occur. Further, the treatment group is more likely to participate in the Mi Abogado program, which provides some assistance after the child returns home that could result in greater surveillance.

Table 4 reports the difference-in-differences estimates for these outcomes. For each, we find a null effect, although the standard errors do not rule out sizeable increases compared to the (relatively rare) means.[10] That said, the point estimates are small, none are statistically significant, any increase is not sustained according to the event studies, and they are inconsistent in sign. These findings suggests that child safety

[10]The upper bound of the 95% confidence interval for protection case is 0.010, for child victim of a crime is 0.004, and for foster care re-entry is 0.006.

does not worsen for those in the treatment group.

5.5 Criminal Justice Involvement

There is a close link between child welfare and juvenile delinquency (Cho et al., 2019; Hirsch et al., 2018). As a result, criminal justice involvement can be used as one barometer for whether the intervention is successful in improving child well-being.

Figure 5 shows how the difference in the number of crime reports between the treatment and control groups changes with time. At the beginning of the period, the treatment and control groups are similar, although crime is relatively rare, as the children are younger. The difference across the groups remains close to zero prior to the randomization and then falls to approximately 0.05 fewer crime reports in the second quarter after randomization. The difference remains lower until two years later, when the difference narrows to -0.03.

Table 5 summarizes the result, showing that children in the treatment group are suspected of 0.037 fewer crimes per quarter in the post-period with a standard error of 0.013.[11] The rate of criminal justice involvement each quarter is relatively high, at 0.12 crime reports per quarter for the control group in the post-randomization period, and the intent-to-treat estimate suggests a fall of 30% relative to this mean. The point estimate is not statistically significant for girls, although the point estimate of 0.013 fewer reports is meaningful relative to the control group's mean of 0.06. Meanwhile, Column (3) shows that there is a negative and statistically significant effect of the treatment for boys: a reduction of 0.066 crimes per quarter, or 32% of the control-group mean in the post-period. A related measure is whether the child was ever reported for committing a crime each quarter. This provides an extensive-margin measure of whether children are getting involved with criminal justice at all. Column (4) shows that this is reduced by 2.3 percentage points, which is again large compared to the control group's mean of 8.8%.

The main crime results apply to reports for all types of crime. Table 6 presents the results when the dependent variable is the number of crime reports for three cate-

[11] When we estimate a cross-sectional regression of number of crimes over the post period, controlling for number of crimes by the time of randomization and strata controls, the point estimate is -0.256 (s.e. = 0.13). This is similar to the reduction of 0.037 crime reports times 8 quarters.

gories. The estimates suggest a large reduction in violent crime reports, with a smaller, statistically insignificant decrease for property crimes. There is also a drop in "other crimes," which include a range of offenses from vandalism to weapons possession.[12] The results show that crime reports fall for a range of crimes, including serious ones with greater welfare implications.

5.6 School Attendance

Another well-being measure is whether children are attending school. Figure 6 reports the monthly event study for the attendance rate across the two groups. The difference prior to the randomization is close to zero but somewhat lower for the treatment group compared to the control group on average. We see a positive spike in the difference in attendance rates in June 2019. This was a month when attendance was low across all students, as represented by the black diamonds in the figure. The low attendance was due, in part, to the national teacher strike on June 3rd that lasted until July 9th. We also observe a positive difference in the last two months, which were also periods of lower-than-usual attendance due to social unrest at the time. Overall, the difference is larger in the period after randomization, particularly when the decision to attend school is more discretionary.

Table 7 shows the difference-in-difference results for school attendance. The difference in attendance rate rises by 3 percentage points in the period after the randomization, or approximately 5% of the control group mean. This is partly due to a 4.7 percentage-point increase in June 2019 (s.e. = 0.026), or 11% of the mean. Again, the increase is concentrated among boys, which is consistent with the larger effects on living with family and crime reports found above. We also considered effects on school performance in 2019. These results suggest an improvement for the treatment group, but the results are relatively imprecise.[13]

[12]Drug crimes are very rare in our data due to how these offenses are treated by the Ministry of Justice (we observe a control group mean of 0.003).

[13]Using the difference-in-differences model and controlling for performance in 2018, we find a the treatment group experiences a 0.048 standard deviation increase in overall school grades, with a 95% confidence interval of (-0.071 to 0.167).

5.7 Heterogeneous Treatment Effects and Mechanisms

5.7.1 Heterogeneous Treatment Effects

The effects of the program may differ across children and across residences. The program distributes a potentially scarce resource, legal teams, so understanding heterogeneous treatment effects would be useful to inform efforts to target the program. In addition, if the program improves outcomes for those at the highest or lowest risk of the outcomes, then we learn about the types of cases that have more malleable outcomes, which can help inform other programs aimed at improving child welfare. Third, by comparing living-with-family and criminal justice outcomes, we can learn whether these improvements typically go together or whether they are relatively independent. If they move together, this provides suggestive evidence that the types of improvements that lead to family reunification are likely to have the co-benefit of improving criminal justice involvement outcomes. That is, we begin to learn about mechanisms for the crime-report results.

To summarize the cases, we first predicted which children had a higher likelihood of returning home within one year of randomization and which were more likely to be reported for crimes over the same period. Specifically, we regressed each outcome on the demographic and allegation characteristics (see Appendix G). We then divided the sample into categories based on the median of the predicted outcomes. Table 8 shows that the group with a high predicted rate of living-with-family within one year is indeed more likely to be living with a family in the post-period, with a mean of 0.32 vs. 0.20 for the group with a low likelihood of reunification. We find that the program was effective at improving the likelihood of living with family for both groups, with coefficients of 0.048 and 0.044. The table also shows that the effects on crime reports are concentrated in the group that is less likely to return home within a year.

We can conduct a similar exercise but compare children with high vs. low predicted criminal justice involvement. Here, we find that crime reports are much more likely in the above-median group, and this is the group that experiences the reduction in crime. Both groups experience improvements in days living with family, with coefficients of 0.08 and 0.05.

Another question is whether there are types of residences where the program is more effective. We again categorized children based on predicted criminal justice involvement and living-with-family, but this time we used the averages for other children in each child's residence. When we look at the outcome of living with family, we find improvements regardless of whether residences have high or low crime rates, as well as high or low living-with-family rates. For crime, some residences have higher crime report rates than others, and the effects on criminal justice involvement are found in the residences with high crime rates. In addition, we find the program lowers criminal justice involvement in residences where children are expected to remain in care longer, despite similar crime rates across long-stay and short-stay residences.

Another characteristic of residences is the wide variety of sizes.[14] The program improves the likelihood of living with family in both large and small residences, while the reduction in criminal justice involvement is found in the larger residences, which are also the types of residences that have a high crime-report rate among the control group.

We also explored heterogeneity across our control variables. We divided the sample based on the median of each control and regressed our main outcomes on a model that included an interaction between the treatment group and an indicator that the child had an above-median measure of the control variable. This provides eight tests for each outcome, and some caution is warranted in the interpretation as we do not adjust the standard errors for multiple hypothesis testing.

Similar to the above results, we do not find a statistically significant difference in returning to a family across any of these comparisons. The largest coefficient was found for the comparison across boys and girls, with boys being more likely to return home, as shown in Table 3. In terms of the point estimates, when we compare Santiago to other regions, children in Santiago have a somewhat lower treatment effect on returning home. Meanwhile, those in a residence for a longer time at the date of randomization have a somewhat larger impact.

For crime reports, we do detect differences in the program effects across different types of children. This echoes the earlier finding that crime is reduced for children who are at greater risk of crime reports, such as boys. We find that the crime reduction is larger for those with a larger school delay, relatively fewer siblings, fewer days in a

[14] Recall Appendix Figure A.1.

residence, and those who were older when first in a residence.

In summary, family reunification results are found for a wide range of case and group characteristics, while the crime report reductions are more prominent for groups with higher rates of criminal justice involvement.

5.7.2 Robustness Checks

The results are robust to a number checks. We find that the results are not sensitive to adding controls for child characteristics (Appendix Table H.1). This is consistent with the balance of characteristics across the two groups (Table 1).

Another robustness check considers the timing of the randomization. The main randomization occurred at the end of March 2019. Later, it was found that more openings in the program could be accommodated, and 51 children from the control group were randomized into treatment. We have coded these children as part of the treatment group in the main analysis, as the group is too small to have precise estimates when analyzed separately. We find the results are very similar when we do not include these 51 children in our analysis (Appendix Table H.2).

One concern when the probability of treatment varies across strata is that the pooled regression with strata fixed effects places more weight on areas with higher variance in treatment. If there are heterogeneous treatment effects across areas, such weighting can lead to bias (Gibbons et al., 2019), as there is little reason to weight the estimates based on the capacity of the program in the different areas. In our context, this concern appears to be unwarranted. When we re-weight the data so that the areas with higher variance do not receive additional weight, the results are very similar (Appendix Table H.3).[15]

5.8 Mechanisms

The Mi Abogado program could affect living-arrangement and crime outcomes directly: living arrangements by reducing bureaucratic delays as designed and crime through access to services. One question is whether the crime reports are reduced because

[15] We can also estimate effects separately by region, but they are relatively imprecise as expected given the sample sizes. The effect on crime reports is stronger in Santiago compared to the other regions, but the difference is not statistically significant (Appendix Tables G.2 and G.4).

time in a residence is reduced. This could occur if residential stays result in crime or residential stays are accompanied by greater surveillance by staff or police.

To begin to consider this surveillance channel, we can investigate whether crimes rise or fall when children enter or exit residences. While entry and exit times are endogenous, a sudden increase or decrease in crime reports would be consistent with living in a residence being related to crime reports. Appendix Figure I.1 considers our analysis sample and shows that in the quarter prior to residence entry, crime reports increase by 0.03 crimes, and they remain at an elevated level of 0.05 crimes per quarter higher than in the pre-entry period. However, when children exit residences, crime reports barely change (Appendix Figure I.2). The lack of a discontinuous drop in crime at the time of exit suggests that greater surveillance in residences is not driving the estimated effects of the program on crime reports.

To complement this time-series exploration, we also estimate a model of crime reports on treatment status while controlling for (endogenous) time in a residence in a given quarter as a mediation analysis. Our estimated effect of the program on crime reports is not affected by controlling for time in a residence (Appendix Table I.1). This again suggests that time in a residence, and the potential for greater surveillance, is not driving the main crime results. The heterogeneous treatment effect estimates also point to mechanisms other than time in a residence. A leading alternative explanation for the reduction in crime reports stems from stronger child and family rehabilitation services received by children as part of the program.

5.9 Exploring Dynamics

5.9.1 Effects over Time

For both living-with-family and crime outcomes, we observe an improvement shortly after the randomization followed by a relatively sustained improvement. Meanwhile, when we examined differences in exposure to the program, we found that cumulative days since first joining the Mi Abogado program for the treatment group relative to the control group rises over time at a decreasing rate. This suggests that the program has a large effect for relatively little exposure and that effect continues over time.

These intent-to-treat estimates are relatively straightforward to interpret as the effect of offering the program and provide a useful comparison of its overall costs and (measured) benefits. Understanding how the effects evolve with program exposure, however, is more difficult because participation in both groups changes over time. Still, the question is policy-relevant and has theoretical interest. It would also provide a better understanding of the sources of the intent-to-treat differences.

We can make progress if we assume that the environment and effectiveness of the program do not change over time. In the first year, this could be violated by any seasonality in these outcomes. In the second year of the study period, when effect sizes stabilize, this is most likely violated by the global pandemic. So, we view our analysis of dynamics as speculative and perhaps more reliable over the first year after randomization, before the COVID-19 response began.

It helps to consider a simpler context. Suppose (i) all treated children entered the program at the same time, (ii) no control children participated, and (iii) there were no time shocks to the effects of the program, so we can make comparisons across calendar time; then we could trivially identify how treatment effects change with time since program initiation by observing the difference between the treatment and control groups over time.

Those conditions are not met in practice, so we need to impose some structure that relies on additional assumptions. In particular, we assume that (i) the treatment effects are homogeneous across cohorts (so, no difference between the treatment effects of compliers entering early and those entering late) and (ii) calendar time does not interact with treatment effects. Under those assumptions, our strategy calculates the difference in outcomes between the entire treatment and control groups period by period. This implies that the estimates are always made across comparable groups to minimize the risk of endogeneity. The idea behind the approach is that in the first observation quarter after randomization, we can identify the *effect of being exposed to the program for one quarter* by comparing outcomes and program exposure across the treatment and control groups. We can then use this estimate in the second observation quarter to predict the effect on children who are exposed for one quarter because they are entering the program at that time. The remainder of the difference across treatment and control identifies the *effect of being exposed to the program for two quarters.* Using this method recursively, we obtain identification of the dynamic effects of the program, which can depend nonlinearly on the number of periods since program exposure.

More specifically, let the outcome in a given quarter for a given individual depend on the total time in the program. Let e^j_{iq} be an indicator which takes the value of 1 if an individual i has spent j quarters in the program up to quarter q (inclusive) and 0 otherwise. The quarters are defined such that the second quarter of 2019 is Quarter 1. In any given calendar quarter, each treated individual only has one such indicator taking a non-zero value. For example, a given individual i entering the program in Quarter 1 will have $e^1_{i1} = 1$ when $q = 1$, $e^2_{i2} = 1$ when $q = 2$, and so on. People entering in Quarter 2 (third quarter of 2019) will have $e^1_{i1} = 0$ when q=1, $e^1_{i2} = 1$ when $q = 2$, $e^2_{i3} = 1$ when $q = 3$, and so on (given that exposure to the program is an absorbing state). This definition of e^j_{iq} is simply equivalent to indicating program cohorts, with newer cohorts having participated in the program for less time.

We can let the effects of the program vary in a non-parametric way using the following specification, where β^j will capture the cumulative effect of having been exposed to the program for j quarters:

$$Y_{iq} = \alpha_q + \sum_{j \in \{1 \dots q\}} \beta^j e^j_{iq} + v_{iq} \tag{3}$$

To save notation, let $\Delta X_q \equiv E[X_q|T = 1] - E[X_q|T = 0]$, the simple difference of means between the treatment and the control group in quarter q, where T is an indicator of having been randomly assigned to the program.

From Equation 3, a simple difference in means of the observed outcomes in Quarter 1 implies:

$$\Delta Y_1 = \beta^1 \Delta e^1_1 \tag{4}$$

Then, the impact of one quarter in the program can be identified by the usual IV estimator, taking differences across the treatment and control groups: $\beta^1 = \frac{\Delta Y_1}{\Delta e^1_1}$.

Taking the difference in means in the second calendar quarter, we obtain:

$$\Delta Y_2 = \beta^2 \Delta e^2_2 + \beta^1 \Delta e^1_2 \tag{5}$$

This implies that the difference between the treatment and control groups in the second calendar quarter is given by (i) the effect of two periods in the program, experienced by those who entered in the first quarter, and (ii) the effect of a single quarter in the program, experienced by those who entered in the second quarter. As the latter effect is already identified, we can plug in our estimate of β^1, solve for β^2, and identify it from the data:

$$\Delta Y_2 = \beta^2 \Delta e_2^2 + \beta^1 \Delta e_2^1 \tag{6}$$

$$\hat{\beta^2} = \frac{\Delta Y_2 - \hat{\beta^1} \Delta e_2^1}{\Delta e_2^2} \tag{7}$$

Using this method recursively, we can estimate the dynamics of the treatment effects for all periods. Standard errors are bootstrapped and incorporate the variance stemming from the calculation of the plug-in estimates.

The results are presented in Figure 7. The figure shows that the effects of the program on both living-with-family and crime reports are almost linear, with a minor degree of concavity which is especially visible for crimes. The results suggest that the effects of program exposure grow over the first two years of exposure to the program.

As noted, however, caution is warranted in interpreting the results due to the assumptions required. In particular, the approach assumes that the effects of program exposure are unrelated to calendar time. In this case, the effects appear to level off around the fourth quarter after randomization and then begin to grow in magnitude again. That increase is in the second quarter of 2020, when the COVID-19 pandemic began. To the extent that the quarters prior to the pandemic are more informative, then the results suggest that the effects are long-lasting up to at least one year after being exposed to the program.

5.9.2 Compliers over Time

Another complication when estimating the effects of the program over time is that the nature of the participants can change over time as well. We can also learn more about how judges adopted the program by tracking changes in compliers—those who were induced into the program due to their treatment status.

Over the first year after the randomization, we find that compliers have more siblings, more time in a residence, and smaller delays in schooling. They are also more likely to be younger and female (Appendix Table J.1). To summarize these results, we can compare those above and below the median in terms of predicted living-with-family and predicted crime reports. Compliers are twice as likely to be above-median in terms of predicted living-with-family and 14% more likely to be below-median in terms of predicted crimes. This suggests that compliers are relatively "easy" cases in terms of reunification.

We find the same patterns when we consider the types of children who are compliers over the first two years after randomization rather than just one year (Appendix Table J.2). This suggests that changing complier characteristics does not complicate the interpretation in our context.

5.10 Cost-Effectiveness

Investing in quality-improvement programs may face budgetary hurdles, and evidence of a return on that investment can spur adoption. The intent-to-treat estimates measure benefits and costs of assignment to the treatment group, which offers a straightforward way to make these comparisons. In this section, we first consider the benefits and costs to SENAME in the form of Mi Abogado program costs and the costs of foster care. To place the crime-report reduction in context, we also provide estimates of the reductions in these costs as well. We consider the effects over the entire time period we observe, a total of nearly two years (721 days). All costs are in 2022 US dollars.

5.10.1 Cost-Effectiveness within Child Protection

Table 12 summarizes the costs and benefits to SENAME. First, children in the treatment group participated in Mi Abogado for 106 more days, on average, compared to the control group. Conversely, children in the treatment group were assigned to non-Mi Abogado lawyers for 90 fewer days. Mi Abogado has a cost of $4.99 per child per day, while non-MA lawyers cost $2.73 per child per day.[16] Overall, offering the program increased legal-aid costs by $283 per child during the two-year observation period.

Meanwhile, treated children spent 5.6 fewer days in government residences than the control group and 29 fewer days in private residences. The former cost $67.27 per

[16] Costs for the MA program and non-MA lawyers were calculated by the Interagency Roundtable according to the Ministry of Justice.

child per day, while the latter cost $28.35 per child per day.[17] We also observe a small increase in days in family foster care as children leave residences for this setting.[18] In total, SENAME saved over $850 per child by offering them the program, which takes into account any non-compliance with the treatment assignment. The estimates imply that for every dollar of additional legal aid, SENAME saved $3.[19]

While these savings are relevant to the budget of SENAME and incentives to make such investments by other child welfare agencies, from a societal perspective there are other costs and benefits to consider. A limitation of the analysis is that we do not observe the social services that may increase due to participation in Mi Abogado. However, our findings for criminal justice and schooling outcomes suggest that additional benefits are likely substantial. The next section considers the potential benefits of a reduction in crime.

5.10.2 Cost-Effectiveness Incorporating Criminal Justice Outcomes

To place the magnitude of the criminal justice outcomes in context, we can also consider the costs associated with different types of crimes (Appendix K). This analysis is more speculative, as we observe suspected involvement in crime, similar to arrest data, rather than actual convictions. This implies that we could be overestimating the number of crimes committed. On the other hand, many crimes go unreported, and the incidence of many types of crimes is much higher than the number of arrests made. For example, Heckman et al. (2010) correct for the ratio between victimization and arrests on the different crimes they consider by using an inflation factor, which can be large for some crimes, on the directly observed change in arrests. This implies that we could be underestimating the total number of crimes. A final concern is that there is measurement error because we consider broad categories of crimes, including property,

[17] Costs for public (Centros de Reparación Especializada de Administración Directa, or CREAD) residence were obtained from program monitoring documents in 2020 and nonprofit (Organismos Colaboradores, or OCAS) residence and family foster care costs as established in Law 21140.

[18] We excluded the category of directly administered family foster care because it is rare. The difference in the number of days in SENAME care across these categories sums to 35 days over these two years, as expected based on the results in Table 3. The days with a lawyer differ from days in SENAME in part because the Mi Abogado program continues to aid children for 90 days after exit from SENAME.

[19] From Table 12, -854/(528-244) = 3.01.

violent, and substance-related offenses.[20]

We obtain crime costs from Miller et al. (2021), and we apply a deflation factor equal to the ratio of Chile's per capita GDP to the United States' (0.34). The estimated reduction in societal crime costs totaled nearly $1,800. After including crimes, the total benefits from the crime reduction and savings to SENAME is $2,600 per child from offering the program.[21]

6 Conclusion

Child protection involves far-reaching interventions into the lives of children and families, and more rigorous evidence is needed to inform efforts to increase the quality of foster care services. This study demonstrates that as new programs are introduced, the rollout can be structured in a way that provides useful variation to evaluate their effects. Coupled with administrative data, we can examine the effects on a primary goal of the program—stable family reunification or adoption—and additional welfare-relevant outcomes for broader, though incomplete, measures of well-being: criminal justice and schooling outcomes.

We find that the randomly assigned treatment group had 60% greater exposure to the program over the two years after the program's introduction. This additional treatment resulted in substantial increases in family reunification or adoption, no detectable decline in child safety, a decline in criminal justice involvement, and evidence of improvement in school attendance. For all of these outcomes, the results were larger for boys. Along other dimensions, results were similar for living-with-family outcomes across a wide range of children, while reductions in criminal justice involvement were concentrated among groups with higher crime report rates.

The results suggest that expanded legal aid is a reform that can increase the likeli-

[20] We considered the individual types of crimes, but the estimates were imprecise. In particular, we had a statistically insignificant difference in the very rare category of homicide, which suggested the program raised crime costs due to the high value of statistical life. Estimates based on the average cost of broader categories allow us to use more precise estimates of any differences.

[21] We do not include guilty sentences or victimization because we are concerned about data quality and our estimated effects are (relatively imprecise) null effects. For boys, where there is some mass with a control group mean of 0.021, our estimated effect of offering MA is -0.001 (s.e.= 0.006). For the cost-benefit analysis, when we incorporate guilty sentences the treatment group accrues only 3 fewer dollars over the two year period; if we include victimization where we also find a null effect, the estimated savings in societal cost increases by $141 over the two years.

hood of children living with family and improve child well-being more generally. Efforts to scale the program to be even larger would need to consider effects on the quality of legal aid as more lawyers are recruited, along with the opportunity cost of the productive capacity of the legal team if employed elsewhere. Nevertheless, the results should add urgency to policy and practice that attempts to improve the quality of foster care.

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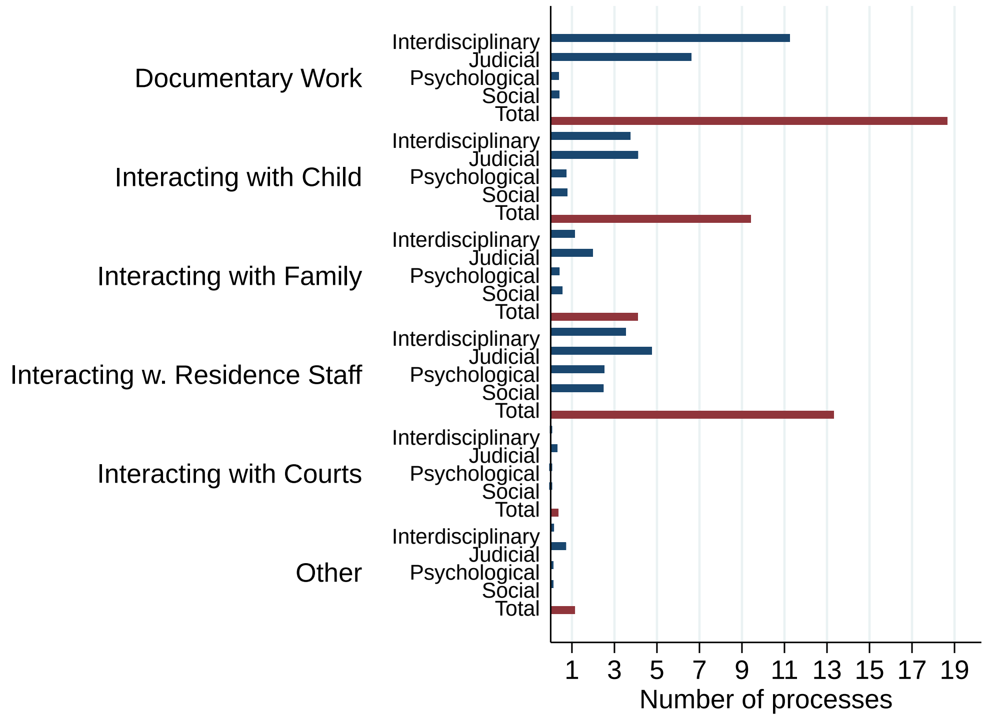
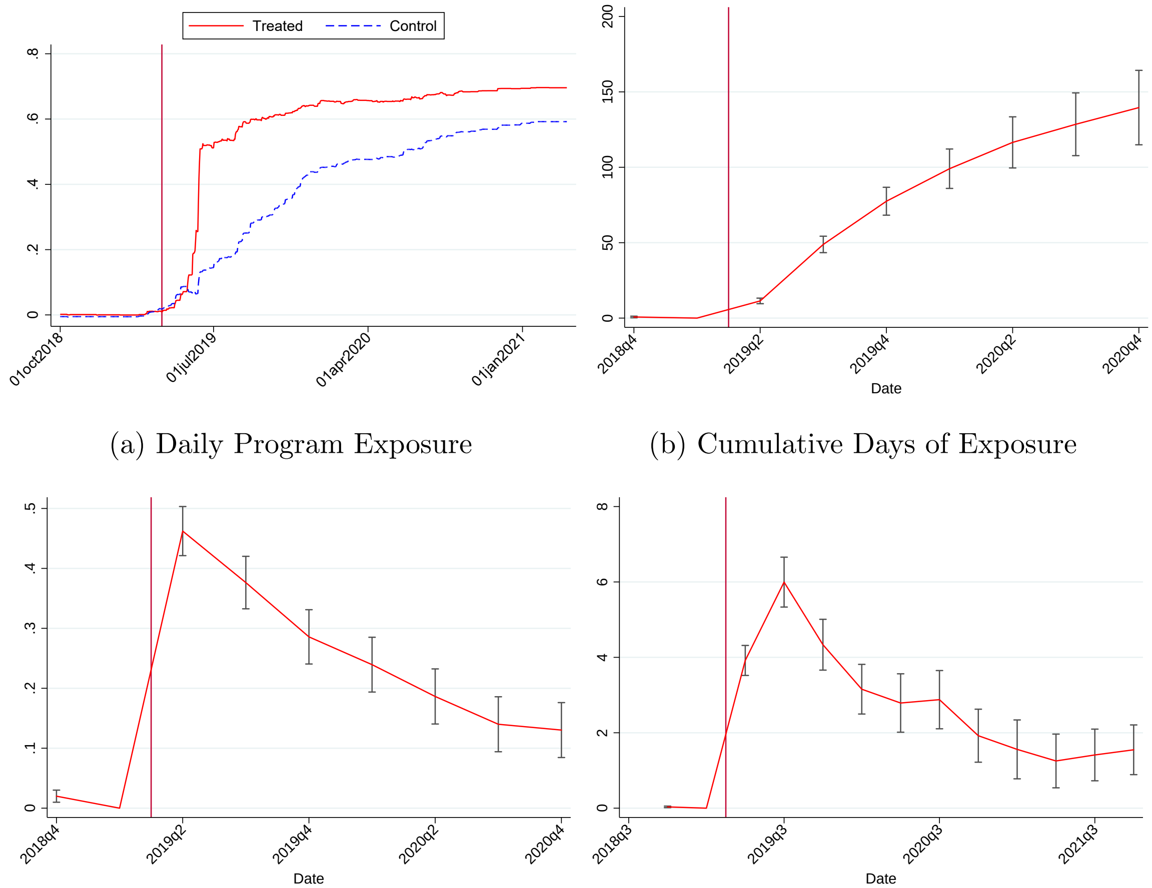
Figure 1: Mi Abogado Processes per Child



Note: This figure presents the average number of processes per child in their year after program initiation. The sample is uncensored, as it includes all Mi Abogado participants observed in the program for at least one year.

Figure 2: Mi Abogado Exposure and Engagement



(a) Daily Program Exposure

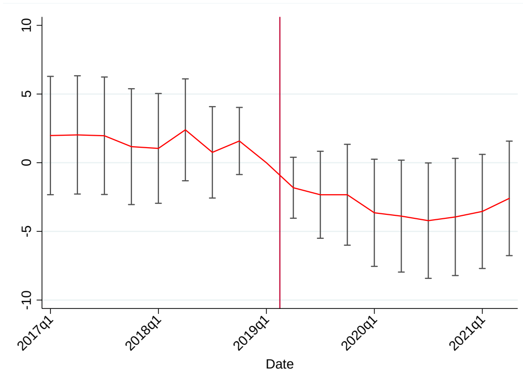
(b) Cumulative Days of Exposure

(c) Program Participation

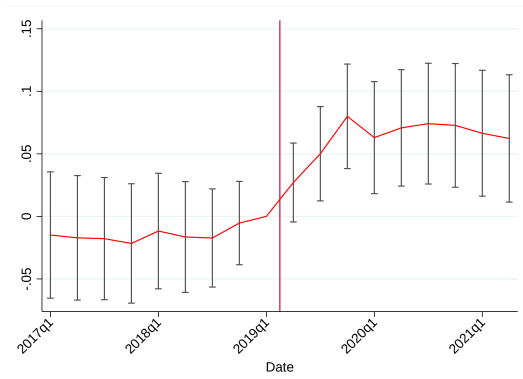
(d) Program Processes

Note: All estimates come from models that control for randomization strata: sex, region and age group. Figure 2a displays residualized program exposure rates (a measure for ever having enrolled in the program by that date) for the treatment and the control groups at the daily level. Figures 2b-2d report event-study estimates binned at the calendar quarter level. Cumulative days of exposure measures the number of days since a child first enrolled in the program. Participation and processes measure whether the child participated that quarter. Confidence intervals are calculated using standard errors clustered at the child level. The vertical line shows the time of randomization.

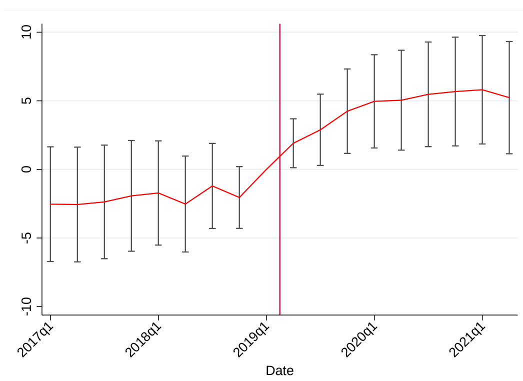
Figure 3: Living Arrangements



(a) Days in SENAME Residence



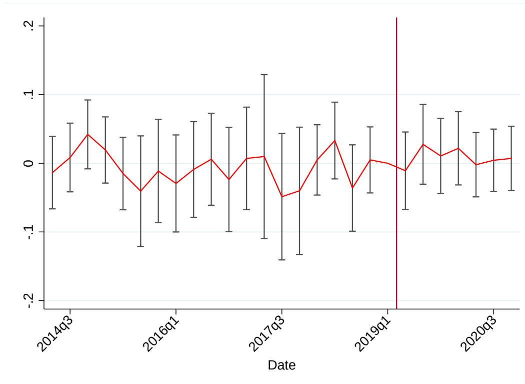
(b) Ever Living with Family



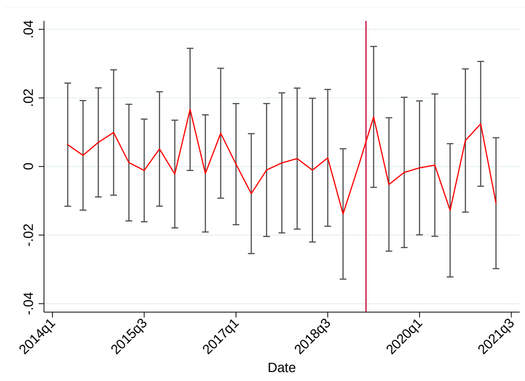
(c) Days Living with Family

Note: These figures report event-study estimates of differences between the treatment and control groups for measures of living in a SENAME residence and living with a permanent (biological or adoptive) family. Models include controls for randomization strata: sex, region and age group. Confidence intervals are calculated using standard errors clustered at the child level. The vertical line shows the time of randomization.

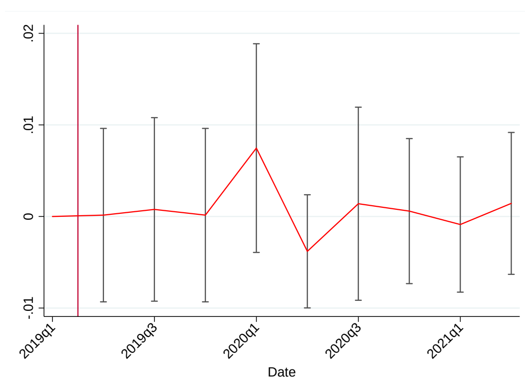
Figure 4: Child Safety Measures



(a) Child Protection Case Opening



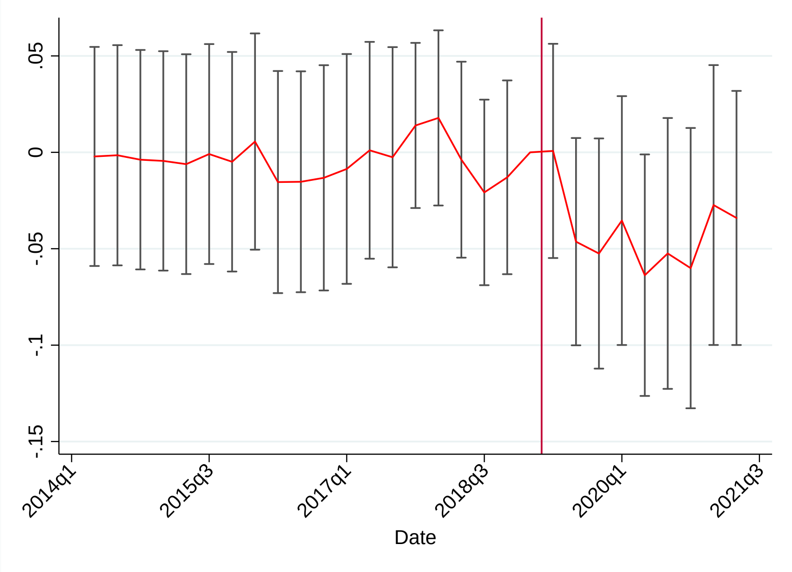
(b) Crime Report of Child Victimization



(c) Foster Care Re-entry

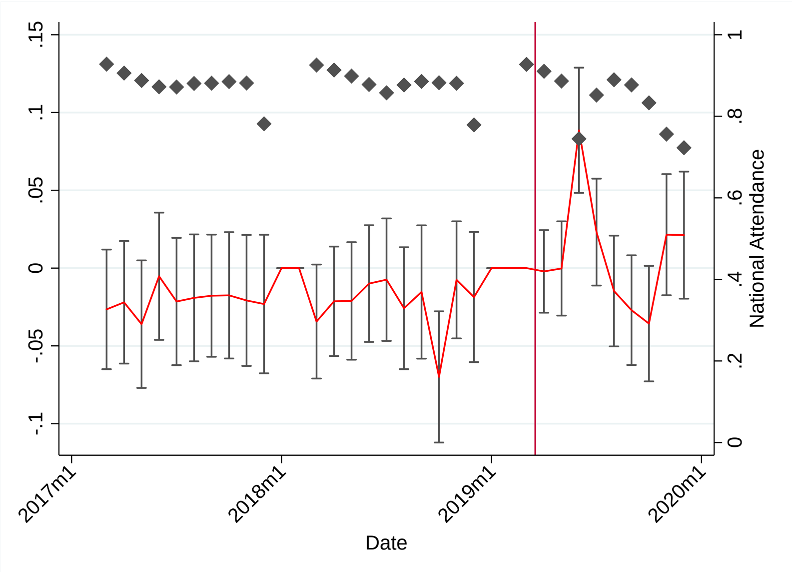
Note: These figures report event-study estimates of differences between the treatment and control groups for child protection cases being opened, criminal reports where the child is a victim, and foster care re-entry in the post period, as all children are in foster care in Jan/Feb 2019. Models include controls for randomization strata: sex, region and age group. Confidence intervals are calculated using standard errors clustered at the child level. The vertical line shows the time of randomization.

Figure 5: Crime Reports



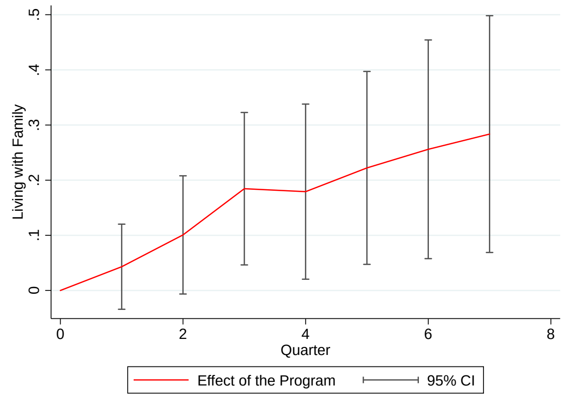
Note: This figure reports event-study estimates of differences between the treatment and control groups for the number of crime reports in a given quarter. Models include controls for randomization strata: sex, region and age group. Confidence intervals are calculated using standard errors clustered at the child level. The vertical line shows the time of randomization.

Figure 6: School Attendance

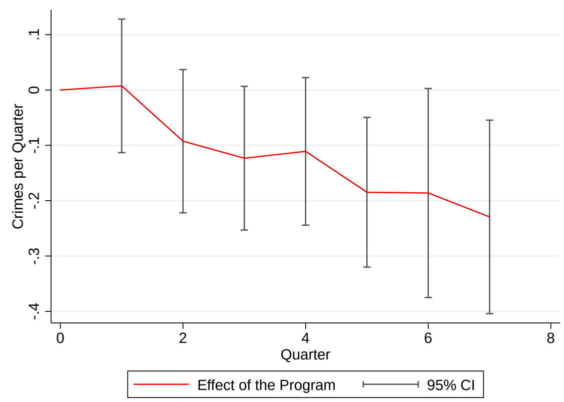


Note: This figure reports event-study estimates of differences between the treatment and control groups for the school attendance rate in each month along with average attendance rates for all students each month (black diamonds). Summer months are represented as zeros with no confidence intervals. Models include controls for randomization strata: sex, region and age group. Confidence intervals are calculated using standard errors clustered at the child level. The vertical line shows the time of randomization.

Figure 7: Treatment Effect Dynamics



(a) Living with Family



(b) Crime Reports

Note: These figures show effects for each quarter of exposure to the program. The first quarter is estimated using a Wald estimator. Subsequent quarters use the full sample and estimates from prior quarters as described in the text. Standard errors are calculated using bootstrap.

Table 1: Balance in Baseline Measures

	Mean C	Mean T	SD	Dif	p
Writs/Qtr	2.73	2.95	2.42	0.22	0.16
Hearings/Qtr	0.19	0.20	0.17	0.01	0.25
Days Living with a Family/Qtr	26.20	24.27	31.06	-1.93	0.34
Days Living In a Residence/Qtr	61.57	63.99	31.66	2.42	0.24
Times Suspect Crimes/Qtr	0.03	0.04	0.13	0.01	0.28
Times Missing/Qtr	0.07	0.08	0.19	0.01	0.46
Times Victim of Abuse/Qtr	0.01	0.01	0.01	-0.00	0.57
School Attendance Rate	0.66	0.66	0.27	-0.01	0.76
Grades Percentile	26.87	28.73	24.28	1.86	0.34
Grades Percentile Missing	0.37	0.36	0.48	-0.01	0.86
Number of Siblings	1.50	1.35	2.06	-0.15	0.26
Delay in Schooling (Years)	0.75	0.92	1.73	0.17	0.12
Allegation: Sex Abuse	0.17	0.18	0.39	0.01	0.80
Allegation: Physical Abuse	0.26	0.30	0.45	0.04	0.23
Allegation: Neglect	0.85	0.83	0.36	-0.02	0.42
Age First Entry in Residence	10.83	10.73	3.69	-0.09	0.62
Age at Randomization	13.68	13.81	3.26	0.13	0.24
Female	0.56	0.57	0.49	0.01	0.86
Observations	1871				

Note: Each row of the table presents the sample values in the pre-treatment period until March 30, 2019. The beginning date for each measure varies depending on data availability: writs and hearings from 2010, days in residence from 2017, days with family from 2017, criminal justice measures from 2014, and schooling for 2017-2018. The grades percentile measure is from 2018 and has a sample size of 1,222. Mean C is the mean for the control group. Mean T is calculated from a regression of the characteristic on a treatment indicator and strata indicators, where the coefficient on treatment is added to the control-group mean. SD is the control group standard deviation, Dif is the coefficient on the treatment indicator, and p-value is from the t-test for this coefficient. The comparison for Female comes from a model that includes the age and regional strata.

Table 2: Mi Abogado Participation and Exposure

Dependent Variable:	(1) Days exposed to Mi Abogado/Qtr.	(2) Days participating in Mi Abogado/Qtr.	(3) Days exposed to any Lawyer/Qtr.
Treatment x Post	20.3	13.5	4.96
	(1.78)***	(1.71)***	(2.05)**
Treatment Group	-2.15	-.416	-4.93
	(1.35)	(1.24)	(2.07)**
Post Randomization	31.5	28.9	-14.5
	(.967)***	(.927)***	(1.13)***
N	16,839	16,839	24,323
N of children	1,871	1,871	1,871
N Control Group	1,188	1,188	1,188
Control Group Mean	32.266	29.639	56.312

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: This table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Sample sizes vary due to different time periods for data availability. Control Group Mean indicates the mean in the post-period.

Table 3: Living Arrangements

Dependent Variable:	(1) Days Living w/ Family/Qtr.	(2) Ever Living w/ Family/Qtr.	(3) Ever Living w/ Family/Qtr. Females	(4) Ever Living w/ Family/Qtr. Males	(5) Ever Living in Residence/Qtr.	(6) Days Living in Residence/Qtr.
Treatment x Post	5.60	.0656	.0431	.0945	-.0462	-4.58
	(1.66)***	(.0208)***	(.0285)	(.0301)***	(.0235)**	(2.2)**
Treatment Group	-2.07	-.0247	.00703	-.0693	.0181	1.77
	(1.56)	(.0206)	(.0282)	(.0302)**	(.0193)	(1.88)
Post Randomization	13.4	.111	.135	.0785	-.0895	-7.22
	(.986)***	(.0126)***	(.0178)***	(.0175)***	(.014)***	(1.32)***
N	20,581	20,581	11,781	8,800	33,678	33,678
N of children	1,871	1,871	1,071	800	1,871	1,871
N Control Group	1,188	1,188	670	518	1,188	1,188
Control Group Mean	21.667	0.259	0.294	0.214	0.630	54.351

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: This table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Sample sizes vary in columns (5) and (6) due to different time periods for data availability. Control Group Mean indicates the mean in the post-period.

Table 4: Child Safety Measures

Dependent Variable:	(1) Protection Case this Quarter	(2) Child Victim this Qtr.	(3) Re-entered Foster Care this Quarter
Treatment x Post	-.000584	-.00112	
	(.00529)	(.0027)	
Treatment Group	-.00188	.000426	.00177
	(.0024)	(.00158)	(.00191)
Post Randomization	-.00521	.00782	
	(.00343)	(.00173)***	
N	114,009	54,259	18,710
N of children	1,869	1,871	1,871
N Control Group	1,187	1,188	1,188
Control Group Mean	0.063	0.021	0.008

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: This table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Sample sizes vary due to different time periods for data availability. Control Group Mean indicates the mean in the post-period. Child victimization largely includes child abuse, as well as other crimes such as assault and robbery.

Table 5: Crime Reports

Dependent Variable:	(1) Crime Reports/Qtr.	(2) Crime Reports/Qtr. Females	(3) Crime Reports/Qtr. Males	(4) Any Crime Reports this Qtr
Treatment x Post	-0.037	-0.013	-0.066	-0.023
	(0.013)***	(0.010)	(0.028)**	(0.008)***
Treatment Group	0.010	0.028	-0.013	0.002
	(0.012)	(0.015)*	(0.019)	(0.006)
Post Randomization	0.093	0.046	0.154	0.065
	(0.010)***	(0.007)***	(0.021)***	(0.006)***
N	54,259	31,059	23,200	54,259
N of children	1,871	1,071	800	1,871
N Control Group	1,188	670	518	1,188
Control Group Mean	0.124	0.064	0.202	0.088

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: This table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Control Group Mean indicates the mean in the post-period.

Table 6: Crimes by Type

Dependent Variable:	(1) Property Crimes Reports/Qtr.	(2) Violent Crimes Reports/Qtr.	(3) Other Crimes Reports/Qtr.
Treatment x Post	-.0100	-.0138	-.0136
	(.00538)*	(.00645)**	(.00658)**
Treatment Group	.00657	.00278	.000767
	(.00825)	(.00361)	(.00271)
Post Randomization	.0162	.0368	.04
	(.00431)***	(.00438)***	(.0046)***
N	54,259	54,259	54,259
N of children	1,871	1,871	1,871
N Control Group	1,188	1,188	1,188
Control Group Mean	0.028	0.050	0.047

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: This table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Control Group Mean indicates the mean in the post-period. Property crime includes theft, robbery, burglary, and arson. Violent crime includes homicide, rape, sexual assaults, robbery with violence, injuries, domestic violence, child abuse, prostitution, threats, and kidnapping. Other crimes include vandalism, carrying a weapon, disorderly conduct, missing, public health, fraud, driving and crashing under the influence of alcohol and possession and sale of drugs.

Table 7: School Attendance

Dependent Variable:	(1) School Attendance	(2) School Attendance Females	(3) School Attendance Males
Treatment x Post	0.029	0.018	0.046
	(0.013)**	(0.018)	(0.019)**
Treatment Group	-0.006	0.001	-0.015
	(0.017)	(0.022)	(0.027)
Post Randomization	-0.085	-0.086	-0.083
	(0.008)***	(0.011)***	(0.012)***
N	56,130	32,130	24,000
N of children	1,871	1,071	800
N Control Group	1,188	670	518
Control Group Mean	0.580	0.576	0.585

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Attendance is the share of school days attended in a given month. This table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Control Group Mean indicates the mean in the post-period.

Table 8: Heterogeneity by Predicted Living With Family

Dependent Variable:	(1) Crime Reports/Qtr. Low Predicted Permanency Group	(2) Crime Reports/Qtr. High Predicted Permanency Group	(3) Ever Living w/ Family/Qtr. Low Predicted Permanency Group	(4) Ever Living w/ Family/Qtr. High Predicted Permanency Group
Treatment x Post	-0.046	-0.025	0.021	0.062
	(0.017)***	(0.018)	(0.024)	(0.028)**
Treatment Group	-0.009	0.034	-0.023	0.014
	(0.008)	(0.021)	(0.020)	(0.025)
Post Randomization	0.092	0.096	0.173	0.292
	(0.014)***	(0.013)***	(0.013)***	(0.018)***
N	27,144	27,115	11,199	11,124
N of children	936	935	936	935
N Control Group	657	531	657	531
Control Group Mean	0.111	0.134	0.198	0.324

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Predicted permanency, meaning living with a permanent family, is estimated using a linear regression of an indicator that the child is living with family on March 30, 2020 against the full set of controls. Subgroups are defined based on the median of this predicted likelihood of reunification or adoption. The table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Control Group Mean indicates the mean in the post-period.

Table 9: Heterogeneity by Predicted Crime Reports

Dependent Variable:	(1) Crime Reports/Qtr. Low Predicted Crimes Group	(2) Crime Reports/Qtr. High Predicted Crimes Group	(3) Ever Living w/ Family/Qtr. Low Predicted Crimes Group	(4) Ever Living w/ Family/Qtr. High Predicted Crimes Group
Treatment x Post	-0.004	-0.047	0.045	0.085
	(0.006)	(0.025)*	(0.025)*	(0.028)***
Treatment Group	0.003	0.014	-0.037	-0.021
	(0.002)	(0.023)	(0.022)*	(0.024)
Post Randomization	0.022	0.158	0.216	0.234
	(0.004)***	(0.017)***	(0.016)***	(0.016)***
N	27,144	27,115	11,186	11,137
N of children	936	935	936	935
N Control Group	564	624	564	624
Control Group Mean	0.025	0.209	0.242	0.265

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Predicted crime reports is estimated using a linear regression of the number of crime reports in the first year after randomization against the full set of controls. Subgroups are defined based on the median of this predicted crime reports. The table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Control Group Mean indicates the mean in the post-period.

Table 10: Heterogeneity by Type of Residence: Living With Family

Dependent Variable:	(1) Permanency Larger Residences	(2) Permanency Smaller Residences	(3) Permanency High Crime Residences	(4) Permanency Low Crime Residences	(5) Permanency Long Stay Residences	(6) Permanency Short Stay Residences
Treatment x Post	0.068	0.071	0.065	0.079	.0827	0.054
	(0.028)**	(0.026)***	(0.026)**	(0.028)***	(.0246)***	(0.028)*
Treatment Group	-0.014	-0.030	-0.023	-0.022	-.0375	0.006
	(0.015)	(0.022)	(0.017)	(0.019)	(.0149)**	(0.021)
Post Randomization	0.236	0.214	0.238	0.203	.184	0.266
	(0.016)***	(0.016)***	(0.014)***	(0.018)***	(.015)***	(0.017)***
N	16,176	15,403	18,968	12,611	16,660	14,919
N of children	959	912	1,131	740	952	919
N Control Group	657	531	769	419	597	591
Control Group Mean	0.282	0.259	0.293	0.233	0.219	0.325

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Subgroups are defined based on measures of the residences using leave-out means of the analysis sample and then divided at the median. This includes the number of children in the residence (columns 1 and 2), the number of crime reports within one-year of entry (columns 3 and 4), and the share of children living at each residence who exited to live with family within one year of entry (columns 5 and 6). The table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Control Group Mean indicates the mean in the post-period.

Table 11: Heterogeneity by Type of Residence: Crime Reports

Dependent Variable:	(1) Crimes Larger Residences	(2) Crimes Smaller Residences	(3) Crimes High Crime Residences	(4) Crimes Low Crime Residences	(5) Crimes Long Stay Residences	(6) Crimes Short Stay Residences
Treatment x Post	-0.041	-0.014	-0.040	-0.005	-0.055	-0.016
	(0.023)*	(0.009)*	(0.021)*	(0.005)	(0.015)***	(0.020)
Treatment Group	0.010	0.004	0.008	0.003	0.018	0.007
	(0.018)	(0.004)	(0.019)	(0.003)	(0.013)	(0.020)
Post Randomization	0.128	0.038	0.126	0.018	0.089	0.087
	(0.016)***	(0.007)***	(0.014)***	(0.004)***	(0.012)***	(0.015)***
N	29,729	28,272	35,061	22,940	29,512	28,489
N of children	959	912	1,131	740	952	919
N Control Group	657	531	769	419	597	591
Control Group Mean	0.173	0.049	0.170	0.022	0.119	0.117

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Subgroups are defined based on the crime-report rate of the child's residence at baseline, measured by the average number of crime reports among children living at that residence within one year of their entry. The table presents linear regression results. Standard errors are clustered at the child level. All models include strata indicators. Control Group Mean indicates the mean in the post-period.

Table 12: Cost Benefit Analysis

	Mean T	Mean C	Dif	P-Value	Costs	DIF*Costs
A. Lawyer Costs						
Days w/ MA Lawyer	340.63	234.89	105.74	0.00	4.99	527.57
Days w/ Non-MA Lawyer	92.42	182.00	-89.57	0.00	2.73	-244.41
B. Residence Costs						
Days in residence (public)	126.37	131.98	-5.61	0.46	67.27	-377.62
Days in residence (nonprofit)	321.83	351.29	-29.46	0.07	28.35	-835.18
C. Family Foster Care Costs						
Days in care (nonprofit)	13.06	7.66	5.41	0.28	13.94	75.42
Total						-854.22

Note: Estimates are on a per-child basis. The means report days in the program, residence, or family foster care over our entire observation period after randomization, 721 days. Costs are calculated in 2022 US dollars.