# Legal Aid in Child Welfare: Evidence from a Randomized Trial of Mi Abogado

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This paper is a working paper; Comments welcome.

### 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 wellbeing. Mi Abogado (My Lawyer) provides legal aid and social services to children living in institutions in Chile. Using administrative data linked across government registries, we find the program substantially reduced length of stay in foster care with no increase in subsequent placement, with resulting savings greater than the cost of the program. The program also led to a large reduction in criminal-justice involvement, and we find suggestive evidence of improvements in school attendance. Effects are stronger for boys across these outcomes.

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# 1 Introduction

Family courts determine whether children should be placed in foster care for their protection, and this oversight 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 that affect children who are particularly vulnerable. For example, foster children experience 2-3 times higher childhood mortality rates, and have 7 times higher rates of depression and anxiety, compared to children with similar observable characteristics (Bald et al., 2022; Johnson-Reid et al., 2007; Turney and Wildeman, 2016).

Once in foster care, the primary aim of family courts is to find a long-term, stable home for children, an outcome known as ”permanency” (Becker et al., 2007; Ryan and Gomez, 2016; Konijn et al., 2019). This process takes approximately two years before children are reunified with their family or, less commonly, adopted. A large literature investigates the determinants and consequences of time spent in foster care and tends to report correlations between length of stay 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).

One result is that there are serious concerns that bureaucratic requirements, including prolonged legal proceedings, lead to unnecessarily long stays in care that harm child wellbeing (Farber, Julie and Laurie Bensky and Lily Alpert, 2009; Miller, 2004; Miller et al., 2020). Nevertheless, there is little evidence on the causal effects of interventions aimed at speeding family-court processes on child outcomes (Blome and Steib, 2008; Hunter et al., 2014). Orlebeke et al. (2016) implemented a randomized evaluation of additional training to 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.

In this paper, we analyze a randomized trial of an intervention that aimed to improve legal representation for children in foster care. We test whether the program succeeded in its goal of speeding the exit from foster care and into a more-permanent family. We are also able to measure broader measures to examine whether speeding exit improved child wellbeing.

The program, Mi Abogado (My Lawyer), was introduced in Chile in 2017. It provides foster children living in institutions access to an attorney with a much smaller caseload compared to children not in the program. The program also provides each case a psychologist and a social worker who work together with the appointed lawyer to connect children with services. At the time of the introduction of Mi Abogado, it was recognized that the program could not serve all eligible children. In a novel form of policy making, the roll out 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 across eligible children. Another advantage of this setting is that high-quality administrative data in Chile provides a low-cost and comprehensive way to track child outcomes.

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

An innovation in this paper is that we can test whether a program designed to reduce bureaucratic frictions and reduce length of stay improves additional barometers of child wellbeing: measures of child safety, criminal justice involvement and school attendance. Child safety does not appear to be compromised across three measures: foster care re-entry, subsequent child protection investigations, and criminal victimization. The treatment group experienced a 30% reduction in criminal-justice involvement over the two years following randomization compared to the control group. This reduction is more likely to come from violent crimes relative to property crimes. We also find a suggestive improvement in school attendance. Across all three types of outcomes, the findings are stronger for boys than girls. 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 more on children and 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 to controlling for foster care placement in an exploratory mediation exercise.

In terms of public spending, the program appears to lower child-welfare costs. The reduction in length of stay in state custody results in savings that are substantially greater than the cost of the Mi Abogado program itself. If we include 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 suggest that improving the quality of case management for children in residential care can substantially improve child wellbeing.

The remainder of the paper is organized as follows. Section 2 provide background information on legal aid in child welfare, foster care placement in Chile and the intervention. Section 3 details the pragmatic randomization and empirical strategy we use to analyze it. Section 4 describes the data, and Section 5 reports the results, Section **??** discusses the cost-benefit analysis, 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 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. Across developed countries, the average length of stay in foster care in the US is on the order of two years, which is similar to other high-income countries. (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 aid is increasingly common, there is little empirical work investigating the effects of different forms of legal aid to children in child protection, (Cooley et al., 2019; Pilkay and Lee, 2015). Osborne et al. (2020) use propensity score methods and find that appointment of a CASA is 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) study the staggered roll out of mandates for representation by a lawyer across states in the U.S., and they find that such a mandate increases the likelihood of adoption by 14% within one year of foster care entry. Meanwhile, parents are rarely represented, but evidence from matched comparisons in New York City and Washington State suggest that such representation can substantially reduce the time in foster care (Courtney and Hook, 2012; Gerber et al., 2019). Given the ubiquity of family courts and large variation in how children and families navigate them, more experimentation and empirical work is needed to guide policy that can improve the functioning of this system and potentially improve child outcomes.

## 2.2 Child Protection in Chile

In Chile, at the time of the intervention we study, the child protection system was administered by the Servicio Nacional de Menores (SENAME).[1] The allegations that lead to foster care involve some form of neglect in 84% of cases, while 28% involve physical abuse and 18% are 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. The most common placement type is with a 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

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[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.

supervised by public and private, non-profit agencies. In our data, residences vary from less than 10 to over 200 children, with the average (median) child living in an institution with 30 (48) other children (Appendix Figure B.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$1000 deemed necessary for high-quality supervision. In 2019, at the start of the intervention we study, the per-child subsidies had increased substantially, 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 20% of children did not have an official lawyer assignment in 2019. Even for those with lawyers, there were concerns that high caseloads prevented lawyers from providing high-quality representation. In order to explore ways of protecting foster children's rights, the Ministry of Justice started a pilot that in 2018 was formally initiated as the Mi Abogado program.

## 2.3 Mi Abogado Program

### 2.3.1 Program Overview

The Mi Abogado program delivers legal aid to children who are in foster care, with a priority toward 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 a family life (whether of origin, extended family or through an adoption process), and providing access to services aimed at improving child wellbeing.

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 3 to 6 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 staff of the group home (13). The team or the lawyer meets with the child 9 times over the first year and meets with the family 4 times. Despite being a legal-aid program, court appearances are rare.

The nominal caseload of the lawyers is limited to 80 with a goal of less than 60. The nominal caseload of the psychologist and the social worker is limited to 240 with a goal of less than 180. Data on caseloads can be difficult to interpret, as cases often remain open even when the case is 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 were each assigned 309 cases (Appendix Figure D.1). Moreover, the salary of the lawyers is higher, so the total amount of money 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. 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 A describes the tasks associated with each member of the team.

# 3 Empirical Strategy

## 3.1 A Pragmatic 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 that had lived in a SENAME residence at some point during January and February of 2019 in these four regions, a total of 1871 children. The randomization of the program occurred on March 30, the last day of the first quarter of 2019. Out of the 1871 children, 581 were selected to enter the program. The randomization was stratified according to age group (older 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 C.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 the children assigned to the treatment group to join the program. 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 consider those children as part of our treatment group, although results are not affected by how these children are included in the analysis, as shown in Appendix H.1. In addition, there was non-compliance 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 dynamic effects of program participation below.

## 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 wellbeing. Given that we have longitudinal data on outcomes, we compare the treatment and control groups over time in event studies. 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). $X_i$ include the strata indicators. We report estimates with a broader set of controls as well in Appendix Table H.3. The summation terms are indicators for each quarter in event time, and we are interested in the estimates of $\theta$, the difference between the treatment and control groups in each quarter. The panel is balanced, and including individual fixed effects yield 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 1 in all periods after randomization and 0 in all other cases. $\psi$ is our main parameter of interest, which represents the average 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.[3]

The estimates will provide a view on the evolution of effects over time, and the summary intent-to-treat estimates are relatively straightforward to interpret when comparing the costs and benefits of the first two years of the program. One question is whether we can use these time patterns to investigate how the program impacts outcomes. Caution is warranted in interpreting these dynamics, however, as they are potentially complicated by the potential for 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 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, 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 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 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 re-enter care, a measure of child safety and a check on whether children are returning to an unsafe home. 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 grade.

Second, the Judiciary Registry allows us to observe criminal justice involvement

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[3] The randomization was carried out at the child level, although siblings may receive attention from the program. This contributes to the non-compliance. 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 at the child level to yield unbiased estimates of the costs and benefits of offering the program.

from 2006 to August 2021. This includes reports to the courts where 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 is under ten years old.[4]

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 has 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 in the administrative data cases usually stay open even after they become inactive. These data also include information on family-court hearings, which we use to measure of court activity in the case. For those participating in Mi Abogado, we observe program information, including dates of participation and processes carried out as described in Figure 1.

Finally, the Ministry of Education registry allows us to consider schooling outcomes between March 2017 to December 2019. We have monthly school attendance data and annual school performance data, coded as the percentile of the average performance in all subjects. The COVID pandemic severely impacted most school activities beginning in March 2020, and it is not possible to obtain outcomes for this period.

## 4.2 Program Engagement and Child Outcome 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

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[4] Other common criminal-justice outcomes such as conviction and incarceration are more difficult to observe the Justice Registry. Most reports are not accompanied by a guilty sentence in our data, in part because many cases are not closed. Incarceration is rare in the data we observe as well.

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 up to 90 days after exit from foster care. Second, we measure the processes performed for each child, including visits with the child, with the residence staff, and with the family.

The first child outcome we consider is the focus of child welfare agencies and family courts: having children return home or adopted so they can live in a more-permanent family. We use the registry data to observe when children are living with their family, either because they had yet to enter foster care, or they exited foster care to live with a family. Children who exit foster care as an adult (known as aging out), do not achieve this measure of "permanency" by definition.[5] Children who exit care to live with a family are considered to be living with their family unless they re-enter foster care.

One question is whether a return home improves child wellbeing, in particular whether the family is a safe environment. We measure child safety in three ways: (1) whether a child returns to foster care, (2) whether there is a new investigation for child maltreatment, and (3) whether the child is observed as a victim of crime each quarter. To the extent that the Mi Abogado program speeds the return home, and provides services to children for 90 days after exit from foster care which increases the surveillance of the home, children in the program may be at higher risk for these outcomes both in terms of actual safety and in a higher likelihood of safety concerns to be reported to authorities.

Another set of outcomes measures 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.

Last, 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 have access to school performance as well, although these outcomes were too imprecisely estimated

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[5] For 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.

to yield insights into the effects of the program.

# 5 Results

## 5.1 Balance

Before exploring the difference between the treatment and control group outcomes, we report comparisons across the two groups in terms of observable characteristics at the time of the randomization. This set of comparisons provides context for the system and the children involved, and serves as a check that the prescribed randomization was carried out faithfully, which would result in similar baseline characteristics across the two groups. In Table 1 the treatment and control groups are compared based upon these characteristics as of February 1, 2019. 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.

The first two rows describe family-court activity that 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 the case was not active. The next row shows that children in the treatment and control groups 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. The remaining days are transitions between programs or family foster care.

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 on other observable characteristics that we use as control variables in some of the analyses below. They average 1.5 siblings, their school delay is large for this group at almost a year.

The child maltreatment allegations that led to the placement in early 2019 for the analysis sample are similar to 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 a residence was at 11 years old, so many families have a long history with child protection. 57% of the sample are girls.[6] 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 non-compliance as the control group gained access over time. Nevertheless, the trial 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. We residualize the data by regressing an indicator for entering the program on the set of strata controls and a treatment indicator. Figure 2a shows the average residuals for the treatment and comparison group on each date, which describes the shares of the treatment and control group who ever entered the Mi Abogado program controlling for the strata. The red vertical line represents the date of randomization.

The figure 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. Members of the control group gradually enter the program over time until roughly 70% of the treatment group and 60% of the control group participate in the program by the middle of 2021.

The remaining results in Figure 2 report event study estimates where the data are binned by calendar quarter. In the first quarter after randomization, the treat-

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[6] We do not test the difference in sex because the sample the model used to estimate differences includes strata indicators.

ment group is 40 percentage points more likely to have entered the program, and this difference falls gradually over time.

Figure 2c shows that 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.2 shows that this is distributed across documentary work, with over 2 more court filings each quarter during 2019, about 1 more child interaction, and 2 more interactions with residence staff. In addition, we see that in the quarter after randomization, the treatment group has one more writ entered in to the system compared to the control group, a difference that is short lived (Appendix Figure F.3).

An equivalent summary of the difference in engagement across the two groups considers cumulative exposure: the difference in days since the children were first exposed to the program. Figure 2d shows how this exposure difference is increasing and concave in time since randomization. After one year, the treatment group has 100 more days since first exposed to the program, and this increases to 150 days at two years.

These differences can be summarized using the two-period difference-in-differences specification, which simply averages the difference across the treatment and control groups over the post-randomization period compared to difference in the pre-period.[7] Table 2 shows that the treatment group had 20 more days of exposure per quarter, approximately 60% higher compared to the control group mean of 32 days over the post-randomization period. This represents days since first joining the program. 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.

The table also shows that the treatment group is more likely to have a lawyer at all (5 more days per quarter compared to a control group mean of 56). The presence of a lawyer is higher for the Mi Abogado program especially in the first quarter and then falls due to the control group gaining more access to the program and any differences in time in foster care. The next section considers time in foster care as the first child

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[7] (Recall that the a small number of children were able to join the program in the pre-period during its pilot phase.

outcome of interest.

## 5.3 Living with a Family

The primary goal of child protection cases is to establish 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 is whether the program improved the likelihood of children living with a biological or adoptive family.

Figure 3 reports the event study estimates for the main living-with-family outcomes: living in a SENAME residence and living with a permanent family outside the foster care system. At the time of randomization the groups are similar by construction: they are all in residence in early 2019, with the treatment group having slightly more days in residence. Following the randomization, the treatment group is less likely to be living in residence, approximately 4 fewer days per quarter compared to the pre-period. In the three quarters after the treatment group was recommended for the program, the measures of living with a permanent family increase: 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, the child spends an additional 5 days per quarter living with a permanent 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 a family. Column (1) shows that returning and continuing to live with family is 6.6 percentage points higher for the treatment group each quarter, which is 25% higher than the control group mean during the post-randomization period. The estimate is somewhat larger for boys with a coefficient of 0.09 vs 0.04 for girls, with girls having a higher rate of leaving foster care to live with a family in the control group: 29% compared to 21% for boys. In terms of days living with a family, the treatment group averages 5.6 more days per quarter, or 26% higher compared to 19 days for the control group. 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 mean of 63%. Similar results are found when examining days in residence (Column (6)), and when we add additional controls (Table H.3.

With so much attention devoted in the child welfare literature to time in care, these results suggest that procedural barriers contribute to longer stays, and a legal-aid intervention can have a substantial effect on speeding children through the system toward the goal of achieving a return to a permanent family.

## 5.4 Child Safety

One question that arises for programs aimed at speeding family reunification is whether the effort results in premature exits, resulting in child-safety concerns. Figure 4 shows event-study results for three measures of child safety. First, we consider whether a subsequent child-protection case was opened, followed by an examination of whether children were more likely to re-enter foster care. Second, we study whether children leaving the residences re-enter foster care later during the observation period. Third, the Justice data provides a measure of criminal victimization, typically a form of child abuse. The event study does not suggest an increase in this measure either. These measures could be mechanically higher in the treatment group because they are more likely to have returned home where these outcomes can occur. Further, the Mi Abogado program provides some assistance after the child returns home and could result in greater maltreatment reporting due to greater surveillance. Regardless of these caveats, we do not observe a change in these child-safety measures following randomization.

Table 4 reports the difference-in-differences estimates for these outcomes. For each, the point estimates are small relative to the control-group mean, and none are statistically significant. For protection-case opening and crime victimization, the point estimates are actually negative. That said, the confidence intervals do not rule out a 10% increase in these measures. Nevertheless, the consistency of the results across the event studies suggest that child safety 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, one measure of whether a child welfare intervention is successful in improving child wellbeing can be measured by whether the program reduces criminal justice involvement.

Figure 5 shows how the difference in the number of crime reports between the treatment and control group changes with time. At the beginning of the time period we study, the treatment and control groups are similar, although crime is relatively rare as children are younger. The difference 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. 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 a 0.013 fewer reports is substantial relative to the control-group 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, compared to the control-group mean of 8.8%.

The crime reports are common for this group of children, and one question is whether the results differ for more- or less-serious crimes. Table 6 shows the results when the dependent variable is the number of criminal-justice reports for three categories of crimes. The estimates suggest a large reduction in violent crime reports, with a smaller, statistically insignificant decrease for property crimes. There is a substantial drop in "other crimes", which includes a range of offenses from vandalism to weapons possession.[8] The results show that crime reports fall for a range of crimes, including serious ones.

## 5.6 School Attendance

Another wellbeing 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 national teacher strike on the 3rd of June that lasted until the 9th of July. While speculative, the result is consistent with the program having an effect when the decision to attend school is more discretionary.

For completeness, Table 7 shows the difference-in-difference results for school attendance. The attendance rate rises by 3 percentage points in the period after the randomization, or approximately 5% of the control group mean. This is due to the 5 percentage-point increase in June 2019, 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.[9]

## 5.7 Heterogeneous Treatment Effects and Mechanisms

### 5.7.1 Heterogeneous Treatment Effects

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 at targeting the program. In addition, if the program improves outcomes for those at the highest- or lowest-risks 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,

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[8] 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).

[9] We also considered effects on test scores, which we observe in 2017-2019. These results are imprecise, however, and we do not detect a difference across the treatment and control groups.

by comparing living-arrangement 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 or adoption are likely to have the co-benefit of improving criminal justice involvement outcomes. That is, we begin to learn about mechanisms.

To summarize the cases, we first predicted which children had higher likelihoods of returning home within one year of randomization, and which were more likely to be reported for crimes over the same time period. Specifically, we regressed each outcome on the control variables used in the main analysis. We then divided the sample into categories based on the median of the predicted outcomes. Table 8 shows that the group with high predicted days living with family are indeed more likely to be living with a family in the post period with a mean of 0.32 vs 0.20 in the group with a low living-with-family score. 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 on the group that is less likely to return home within a year, which is the group that is much more likely to commit crimes.

We can view the same exercise but comparing children with high- vs. low-predicted criminal justice involvement. Here, we find that crimes 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 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 residence averages for other children rather the child characteristics. When we look at living-with-family, we find substantial 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 high-crime residences. 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. The program substantially improves living-with-family outcomes 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 higher numbers of crime reports among the control group.

We also explored heterogeneity across our control variables (see Appendix G). 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 above-median measure of the control variable. This provides eight tests for each outcome, and 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 effect of the program on returning home; those in residence for a longer time at the date of randomization have a somewhat larger impact.

For crime, we do detect differences in the program across different types of children. This echos the earlier findings 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 residence, and who were older when first in residence. In summary, the living-with-family outcomes 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

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 because there is little reason to weight the estimates based on the capacity of the program in the different areas (Gibbons et al., 2019). In our context, this concern appears to be unwarranted. First, we can re-weight the data so that the areas with higher variance do not receive additional weight, and results are very similar when we do (Appendix Table H.2). Second, when we estimate the model where we interact the treatment indicator with regional indicators, we do not detect a difference in the estimates across regions for returning to a family (Appendix Table Q.4). For crime, we find that Valparaiso has a larger reduction in crime compared to Santiago, and the difference is marginally statistically significant (Appendix Table Q.6). We do not emphasize these comparisons, however, as the separate estimates by region are less precise.

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 original control group were randomized to 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 by estimating them separately. We find the results are very similar when we do not include these 51 children in our analysis (Appendix Table H.1).

Table 1 shows that the treatment and control group are comparable due to the randomization. As a result, we find that the results are not sensitive to adding controls for child characteristics (Appendix Table H.3).

### 5.7.3 Mechanisms

The Mi Abogado program could affect living arrangements and crime outcomes directly: living-with-family by reducing bureaucratic delays as designed and crime through access to services. One question is whether the crime reports are reduced because time in 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 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 would be consistent with living in a residence being related to crime reports. Appendix Figure I.1 shows that in the quarter prior to residence entry, crime reports increase by 0.03 crimes per quarter, they remain at an elevated level of 0.05 crimes per quarter higher than the pre-entry period. However, when children exit residences, crime reports barely change at the time of exit (Appendix Figure I.2). The lack of a discontinuous drop in crime at the time of exit suggests that greater surveillance in residence 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 on treatment status while controlling for (endogenous) time in residence in a given quarter as a mediation analysis. Our estimated effect of the program on crime is not affected by controlling for time in residence (Appendix Table I.1). This again suggests that time in residence, and the potential for greater surveillance, is not driving the main crime results. The estimates also point to mechanisms other than time in residence, such as stronger child and family rehabilitation stemming from services received by children as part of the program.

## 5.8 Dynamics

### 5.8.1 Effects over Time

For both living-with-family and crime, we observe an improvement shortly after the program began, followed by a relatively sustained improvement. Meanwhile, when we examined differences in exposure to the program above, we found that cumulative days since first joining the Mi Abogado program for the treatment group relative to the control group rises through 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 provides a useful comparison of overall costs and (measured) benefits of that offer. Understanding how the effects evolve with program exposure, however, is more difficult because participation in both groups is changing 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 is not changing through 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 are 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 in the program just by observing the difference between the treated and the control group. Those conditions are not met in practice, so we need to impose some structure and make some 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.

Our strategy works by calculating 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. We let the effect of the program depend non-linearly on the number of periods participating and assume that the effect of the program is homogeneous across program cohorts. Thus, in the first observation quarter we can identify the *effect of being exposed to the program for one quarter*, then use it in the second observation quarter to predict the effect on children entering the program then. The rest of the difference between the treated and control children is then the *effect of being exposed to the program for two quarters*, which is then identified. Using this method recursively, we obtain identification of the dynamic effects of the program.

Let the outcome on a given quarter for a given individual depend on the total time in the program. Let $e_{iq}^{j}$ be an indicator which takes the value of one 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. At 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 $e_{i1}^{1} = 1$ when $q = 1$, $e_{i2}^{2} = 1$ when $q = 2$, and so on. People entering on quarter 2 (third quarter of 2019) will have $e_{i1}^{1} = 0$ when $q$=1, $e_{i2}^{1} = 1$ on $q = 2$, $e_{i3}^{2} = 1$ on $q = 3$, and so on (given that exposure to the program is an absorbing state [*staggered* in the fixed effects language], this definition of $e_{iq}^{j}$ is simply equivalent to indicating program cohorts, with newer cohorts having participated less time in the program).

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

$$Y_{iq} = \alpha_q + \sum_{j \in \{1 \ldots q\}} \beta^{j} e_{iq}^{j} + v_{iq} \tag{3}$$

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:

$$\Delta Y_2 = \beta^{2} \Delta e_2^{2} + \beta^{1} \Delta e_2^{1} \tag{5}$$

This implies that the difference between the treated and the control group 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 already identified, we can plug in our estimate of $\beta^{1}$, solve for $\beta^{2}$, and identify it from the data:

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

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

Using this method recursively, we can estimate the dynamics of the treatment effects for all periods. Standard errors are calculated using bootstrap. The results are presented in Figure 7. The figures show that the effects of the program on both living-with-family and crime reports are almost linear, with a a minor degree of concavity which is especially visible for crimes. The results suggest that the effects of the program grow over the first two years of exposure to the program.

Caution is warranted in interpreting the results. In particular, the approach assumes that the effects of program exposure are unrelated to calendar time. In this case, the effects appear to be levelling 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 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.

### 5.8.2 Compliers over Time

Another complication when estimating the effects of the program through time is that the nature of the participants can change over time as well. To focus on the identifying variation and the nature of how judges reacted to the program availability, we can describe the observable characteristics of the compliers of the experiment through time–those who were induced into the program due to their treatment status. We find that compliers over the first year after randomization have more siblings, more time in residence, lower delays in schooling. They are also more likely to be younger and female (Appendix Table J.1). We find the same patterns when we consider the types of children who are compliers over the first two years after randomization (Appendix Table J.2).

To summarize these results, we can compare those above and below median in terms of predicted living-with-family and predicted crime reports. Here we find that compliers are twice as likely to be above median in terms of predicted living-with-family and 14 percent more likely to be below-median in terms of predicted crimes. This suggests that compliers are relatively "easy" cases in terms of reunification or adoption. We do not see evidence that these patterns change substantially over time.

## 5.9 Cost Effectiveness

### 5.9.1 Cost-Effectiveness within Child Protection

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 of the measurable benefits and costs of the program provide a straightforward way to make these comparisons. In this section, we first consider 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 estimated reductions in these costs as well. We consider the effects over the entire timeframe we observe these outcomes, a period of of nearly two years (721 days). All costs are in 2022 US dollars.

Table 12 summarizes these estimates. First, the treatment group children participated, on average, 89 more days in Mi Abogado compared to the control group.[10] Conversely, children in the treatment group were assigned to non-Mi Abogado lawyers 89 fewer days. Mi Abogado has a cost of 4.99 dollars per child per day, while non-MA lawyers cost 2.7 dollars per child per day.[11] Overall, the cost of legal aid is more costly through the Mi Abogado program, but the difference is relatively small, at around 200 dollars per child per day during the observation period.

Meanwhile, treated children spent 4.7 fewer days in government-run residences than the control group, and 29.9 fewer days in privately-run residences. The former cost 67.2 dollars per child/day, while the latter cost around 28.3 dollars per child/day.[12] The total savings for less time living in public and subsidized residences is substantial at almost US$1200 in savings per child. We also observe a small increase in days in family foster care as children leave residences for this setting.[13] In total, the program easily pays for itself within the budget of SENAME, saving over $1000 per child by offering them the program.

There could be other societal costs and benefits of the program. A limitation is that

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[10] The construction of this variable includes 90 days of program engagement after returning home.

[11] Costs for the MA program and non-MA attorneys were calculated by the Interagency Roundtable cited by the Judiciary.

[12] Costs for Cread residencies were obtained from program monitoring documents in 2020 and Ocas residency costs as established in law 21140.

[13] The number of days in SENAME care across these categores sums to 30 days over these two years, as expected based on the results in Table 2. Days in Mi Abogado include the 90 days after exit when the program is expected to continue to aid the child.

we do not observe any increase in services that the children may receive as a result of the Mi Abogado program. Our 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 Cost-Effectiveness Incorporating Criminal Justice Outcomes

To place the magnitude of the criminal-justice outcomes in context, we can also consider costs associated with different types of crimes. 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 amount of crimes committed. On the other hand, many crimes go unreported, and the incidence for many types of crimes is much higher than the number of arrests made. For example, Heckman et al. (2010) corrects for the ratio between victimization and arrests on the different crimes they consider by using an inflation factor, that can be substantial for some crimes, to 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, violent and substance-related offenses.[14]

Costs for the sentences were obtained from the Ministry of Justice. We obtain crime costs from Miller et al. (2021), and we apply a deflation factor equal to the ratio of the US per capita GDP to Chile's (0.34). Our estimates show that the program reduced the number of crimes committed by the children in the sample, and the number of crimes where children in the sample were the victims (Appendix Table K.1). The first effect is large in magnitude, representing cost savings of more than USD$1500 per child over the observation period, roughly equivalent to three times the median wage in Chile. After including crimes, the total savings adding all dimensions that we can measure amount to more than US$2500 per child.

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[14] 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 cost 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.

# 6 Conclusion

Child protection involves far-reaching interventions into the lives of children and families, and more rigorous evidence is needed to inform reforms aimed at increasing the quality of foster care services. This study demonstrates that as new programs are introduced, the roll out can be staggered in a way that provides useful variation to evaluate their effects. Coupled with administrative data, we can examine effects on a primary goal of the program: the stable placement of children back home with family or in an adoptive home. We can also examine criminal justice and schooling outcomes for broader, though incomplete, measures of wellbeing.

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 time living with family, no detectable decline in child safety, a decline in criminal justice involvement, and suggestive evidence of improvement in school attendance. For all of these outcomes, 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 likelihood that children are living in a permanent family and improve child wellbeing 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 reduce procedural hurdles and improve the quality of the foster care system.

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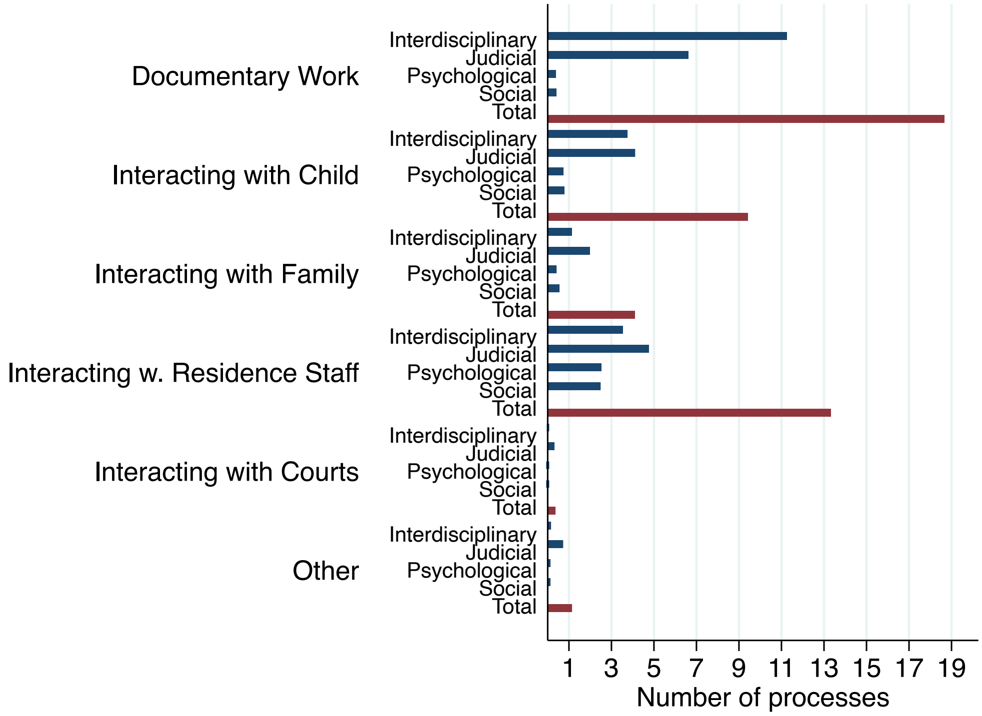
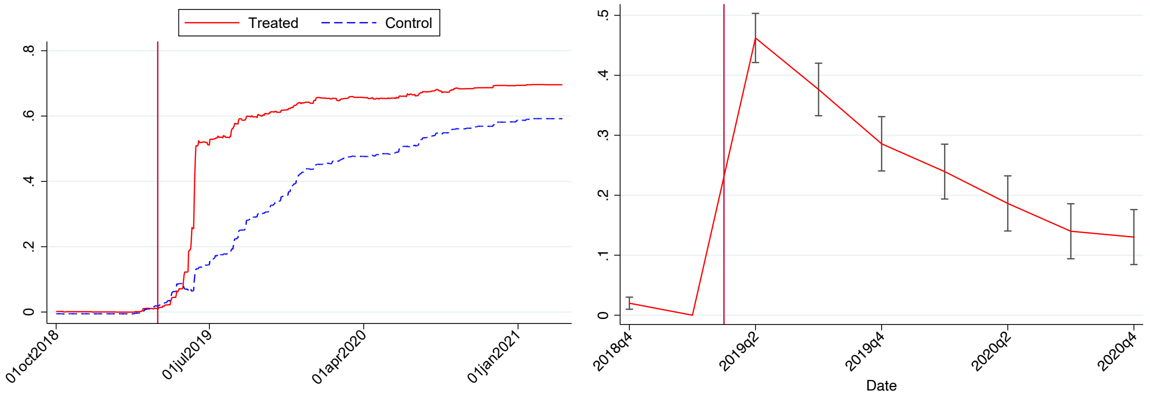
Figure 1: Mi Abogado Processes per Child

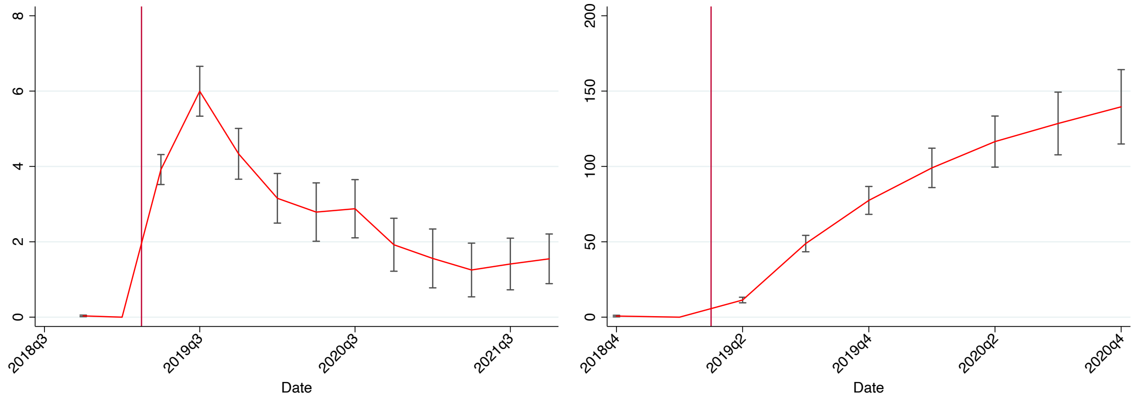


Note: This figure presents the average number of processes per child in their year after program initiation. The sample is uncensored, as it includes all Mi Abogado participants observed for that time period.

Figure 2: Mi Abogado Engagement



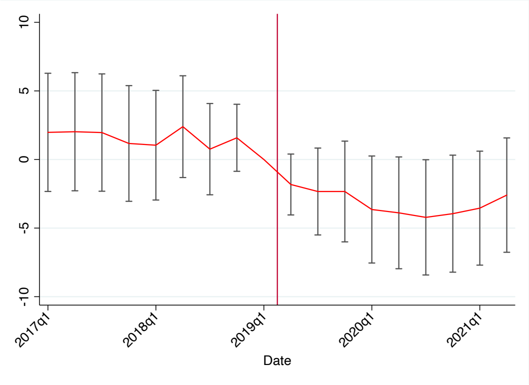
(a) Daily Program Participation (b) Program Participation



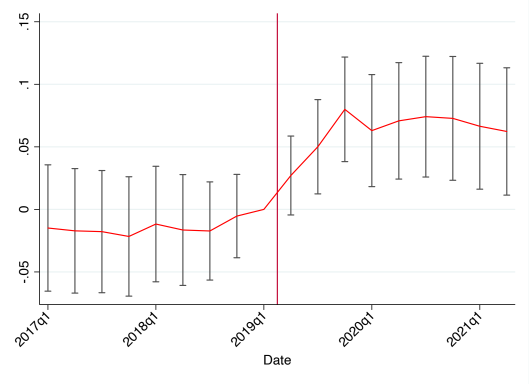
(c) Program Processes (d) Cumulative Days of Exposure

Note: All estimates come from models that control for randomization strata: sex, region and age group. Figure 2a displays residualized program participation rates for the treated and the control groups. Figures 2b-2d report event-study estimates of differences between the treatment and control groups for measures of program participation (an indicator the child participated that quarter), the number of Mi Abogado processes that quarter, and cumulative exposure (the number of days since the child first entered the program). The vertical line shows the time of randomization.

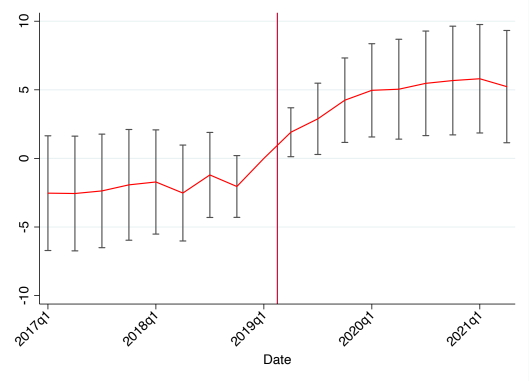
Figure 3: Living Arrangements



(a) Days in SENAME Residence



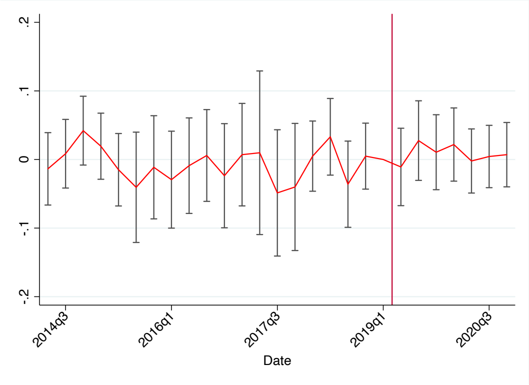
(b) Ever Living with Permanent Family



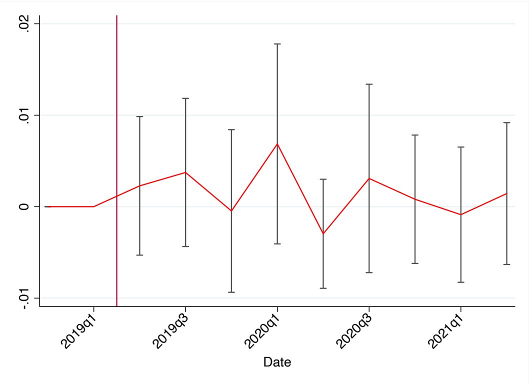
(c) Days Living with Permanent Family

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. The vertical line shows the time of randomization.

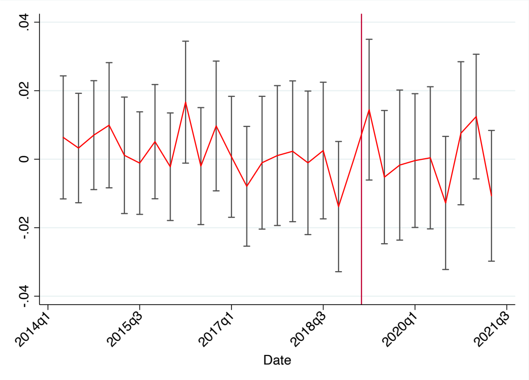
Figure 4: Child Safety Measures



(a) Child Protection Case Opening



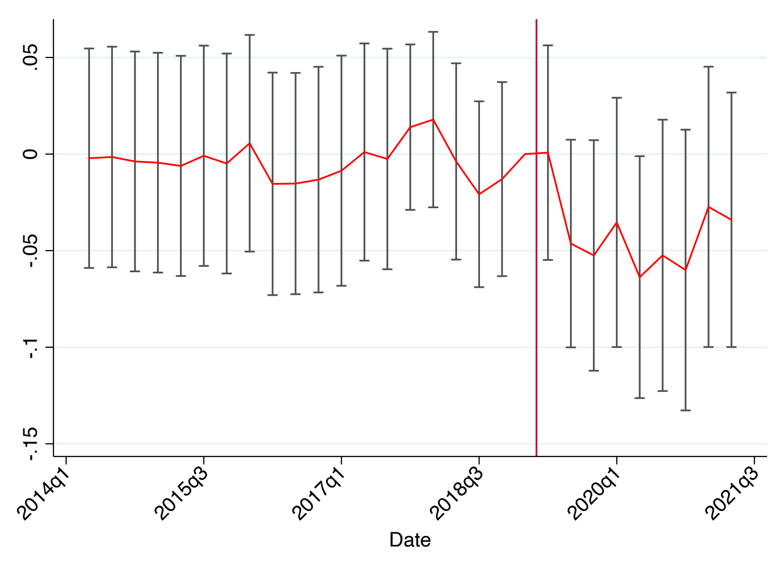
(b) Foster Care Re-entry



(c) Crime Report of Child Victimization

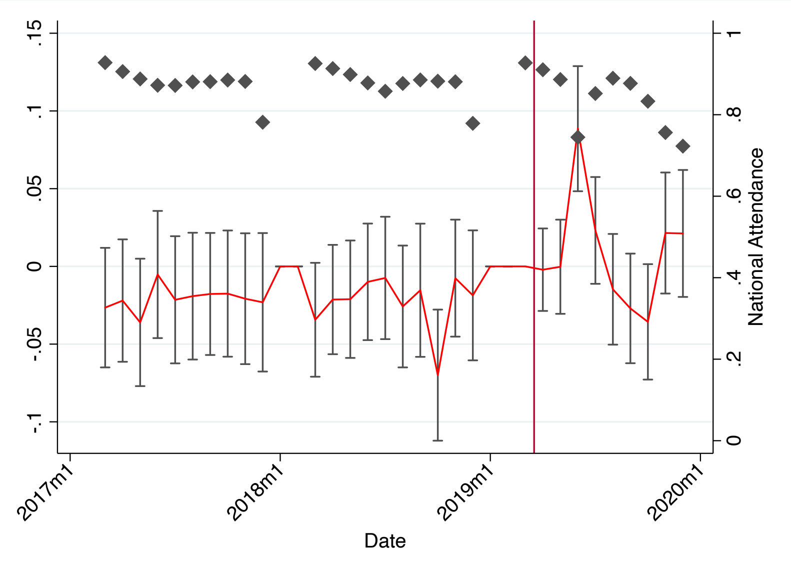
Note: These figures report event-study estimates of differences between the treatment and control groups for child protection cases being opened, foster care re-entry, and criminal reports where the child is a victim. Models include controls for randomization strata: sex, region and age group. The vertical line shows the time of randomization.

Figure 5: Crime Reports



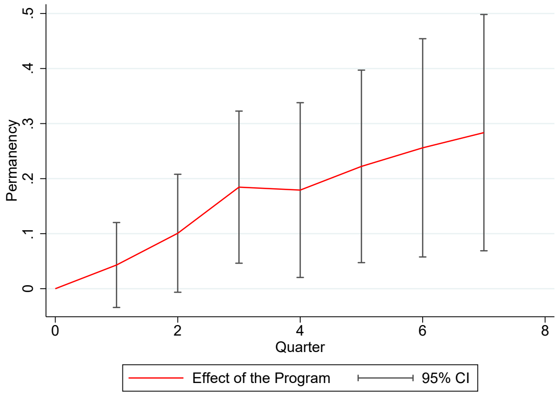
Note: This figure reports event-study estimates of differences between the treatment and control groups for the number of crime reports in a given quarter. Models include controls for randomization strata: sex, region and age group. The vertical line shows the time of randomization.

Figure 6: School Attendance

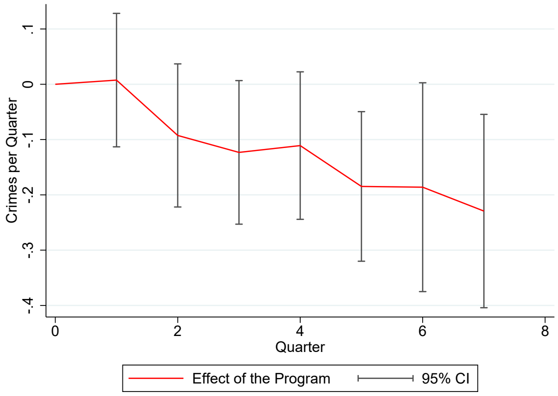


Note: This figure reports event-study estimates of differences between the treatment and control groups for the school attendance rate in each month along with average attendance rates for all students each month (black diamonds). Summer months are represented as zeros with no confidence intervals. Models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

Figure 7: Treatment Effect Dynamics



(a) Living with a Permanent 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 for prior quarters as described in the text. Standard errors are calculated using bootstrap.

Table 1: Balance in Baseline Measures

| | N | Mean C | Mean T | SD | Dif | p |
|---|---|---|---|---|---|---|
| Writs/Qtr | 1,871 | 2.731 | 2.951 | 2.423 | 0.220 | 0.161 |
| Hearings/Qtr | 1,871 | 0.191 | 0.203 | 0.165 | 0.012 | 0.251 |
| Days Living with a Family/Qtr | 1,871 | 26.202 | 24.268 | 31.062 | -1.934 | 0.337 |
| Days Living In a Residence/Qtr | 1,871 | 61.571 | 63.990 | 31.663 | 2.419 | 0.238 |
| Times Suspect Crimes/Qtr | 1,871 | 0.030 | 0.039 | 0.131 | 0.009 | 0.282 |
| Times Missing/Qtr | 1,871 | 0.068 | 0.077 | 0.194 | 0.009 | 0.463 |
| Times Victim of Abuse/Qtr | 1,871 | 0.006 | 0.006 | 0.012 | -0.000 | 0.574 |
| School Percentage of Attendance in 2017-2018 | 1,871 | 0.665 | 0.660 | 0.272 | -0.005 | 0.764 |
| Grades Percentile in 2018 | 1,222 | 26.867 | 28.729 | 24.281 | 1.862 | 0.340 |
| Grades Percentile Missing | 1,871 | 0.366 | 0.361 | 0.476 | -0.005 | 0.860 |
| Number of Siblings | 1,871 | 1.498 | 1.348 | 2.057 | -0.150 | 0.257 |
| Delay in Schooling (Years) | 1,871 | 0.746 | 0.918 | 1.735 | 0.172 | 0.115 |
| Allegation: Sex Abuse | 1,871 | 0.170 | 0.176 | 0.387 | 0.006 | 0.795 |
| Allegation: Physical Abuse | 1,871 | 0.261 | 0.296 | 0.454 | 0.035 | 0.225 |
| Allegation: Neglect | 1,871 | 0.845 | 0.826 | 0.357 | -0.019 | 0.417 |
| Age First Entry in Residence | 1,871 | 10.827 | 10.733 | 3.687 | -0.095 | 0.619 |
| Age at Randomization | 1,871 | 13.684 | 13.811 | 3.260 | 0.128 | 0.239 |
| Female | 1,871 | 0.564 | 0.570 | 0.495 | 0.006 | 0.862 |

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 2019, criminal justice measures from 2014, and schooling for 2017-2018. The grades percentile measure 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. We do not test for a difference in Female because sex is in the set of strata indicators.

## Table 2: 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 | 20.255 | 13.460 | 4.960 |
| | (1.784)*** | (1.705)*** | (2.052)** |
| Treatment Group | -2.151 | -0.416 | -4.932 |
| | (1.345) | (1.239) | (2.070)** |
| Post Randomization | 31.531 | 28.890 | -14.548 |
| | (0.967)*** | (0.927)*** | (1.128)*** |
| $N$ | 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. Control Group Mean indicates the mean in 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 | 5.604 | 0.066 | 0.043 | 0.094 | -0.046 | -4.582 |
| | (1.664)*** | (0.021)*** | (0.028) | (0.030)*** | (0.023)** | (2.200)** |
| Treatment Group | -2.067 | -0.025 | 0.007 | -0.069 | 0.018 | 1.771 |
| | (1.556) | (0.021) | (0.028) | (0.030)** | (0.019) | (1.881) |
| Post Randomization | 13.429 | 0.111 | 0.135 | 0.079 | -0.090 | -7.220 |
| | (0.986)*** | (0.013)*** | (0.018)*** | (0.018)*** | (0.014)*** | (1.319)*** |
| $N$ | 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. Control Group Mean indicates the mean in post period.

Table 4: Child Safety Measures

| Dependent Variable: | (1) Protection Case this Quarter | (2) Reentered Foster Care this Quarter | (3) Child Victim this Qtr. |
|---|---|---|---|
| Treatment x Post | -0.001 | 0.002 | -0.001 |
| | (0.005) | (0.001) | (0.003) |
| | [-0.011 - 0.010] | [-0.001 - 0.004] | [-0.006 - 0.004] |
| Treatment Group | -0.002 | 0.001 | 0.000 |
| | (0.002) | (0.001) | (0.002) |
| | [-0.007 - 0.003] | [-0.001 - 0.003] | [-0.003 - 0.004] |
| Post Randomization | -0.005 | 0.007 | 0.008 |
| | (0.003) | (0.001)*** | (0.002)*** |
| | [-0.012 - 0.002] | [0.005 - 0.009] | [0.004 - 0.011] |
| *N* | 114,009 | 20,559 | 54,259 |
| N of children | 1,869 | 1,869 | 1,871 |
| N Control Group | 1,187 | 1,187 | 1,188 |
| Control Group Mean | 0.063 | 0.007 | 0.021 |

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

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 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) 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)*** |
| o.fem | | 0.000 | 0.000 | |
| | | (0.000) | (0.000) | |
| *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 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 | -0.010 | -0.014 | -0.014 |
| | (0.005)* | (0.006)** | (0.007)** |
| Treatment Group | 0.007 | 0.003 | 0.001 |
| | (0.008) | (0.004) | (0.003) |
| Post Randomization | 0.016 | 0.037 | 0.040 |
| | (0.004)*** | (0.004)*** | (0.005)*** |
| *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 post period. Property crimes include theft, robbery, burglary, and arson. Violent crimes include 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 | (4) School Attendance (June 2019) Cross-Sectional Model |
|---|---|---|---|---|
| Treatment x Post | 0.029<br>(0.013)** | 0.018<br>(0.018) | 0.046<br>(0.019)** | |
| Treatment Group | -0.006<br>(0.017) | 0.001<br>(0.022) | -0.015<br>(0.027) | 0.047<br>(0.026)* |
| Post Randomization | -0.085<br>(0.008)*** | -0.086<br>(0.011)*** | -0.083<br>(0.012)*** | |
| $N$ | 56,130 | 32,130 | 24,000 | 1,871 |
| N of children | 1,871 | 1,071 | 800 | 1,871 |
| N Control Group | 1,188 | 670 | 518 | 1,188 |
| Control Group Mean | 0.580 | 0.576 | 0.585 | 0.454 |

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

Note: Attendance/mo is the attendance rate each 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 post period. Control Group Mean indicates the mean in 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<br>(0.017)*** | -0.025<br>(0.018) | 0.021<br>(0.024) | 0.062<br>(0.028)** |
| Treatment Group | -0.009<br>(0.008) | 0.034<br>(0.021) | -0.023<br>(0.020) | 0.014<br>(0.025) |
| Post Randomization | 0.092<br>(0.014)*** | 0.096<br>(0.013)*** | 0.173<br>(0.013)*** | 0.292<br>(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 living-with-family is estimated using a linear regression of an indicator that the child is living with a permanent 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 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 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 | 0.083 | 0.054 |
| | (0.028)** | (0.026)*** | (0.026)** | (0.028)*** | (0.025)*** | (0.028)* |
| Treatment Group | -0.014 | -0.030 | -0.023 | -0.022 | -0.037 | 0.006 |
| | (0.015) | (0.022) | (0.017) | (0.019) | (0.015)** | (0.021) |
| Post Randomization | 0.236 | 0.214 | 0.238 | 0.203 | 0.184 | 0.266 |
| | (0.016)*** | (0.016)*** | (0.014)*** | (0.018)*** | (0.015)*** | (0.017)*** |
| $N$ | 16,176 | 15,403 | 18,968 | 12,611 | 16,660 | 14,919 |
| 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 the share of children living at each residence who exited to live with a permanent family within one year of 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 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 |
| 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 post period.

## Table 12: Cost Benefit Analysis

| Program Costs | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
|---|---|---|---|---|---|---|
| MA | 329.80 | 240.17 | 89.62 | 0.00 | 4.99 | 447.14 |
| No MA | 92.42 | 182.00 | -89.57 | 0.00 | 2.73 | -244.41 |
| Residence Costs | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
| Cread | 113.36 | 118.07 | -4.71 | 0.53 | 67.27 | -316.87 |
| Ocas | 279.85 | 309.75 | -29.91 | 0.07 | 28.35 | -847.92 |
| Family Foster Care Costs | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
| Ocas | 14.98 | 8.64 | 6.34 | 0.21 | 13.94 | 88.45 |
| Direct Administratin | -1.06 | 0.45 | -1.51 | 0.15 | 23.46 | -35.52 |
| Total | | | | | | -909.13 |

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.

# ONLINE APPENDIX

# A Mi Abogado Detailed Processes

These are all the actions carried out by the interdisciplinary team of the Mi Abogado program granting specialized legal defense.

- Diagnosis of the situation of children: Each child that enters the program is diagnosed, determining the urgency and prioritization of the legal decisions to be made. The diagnosis is an interdisciplinary exercise agreed between the psychosocial-judiciary triplet of the program. For this diagnosis, the interview or observation of the child within the first month from the acceptance of the appointment of curatorship ad litem is fundamental.
  - The elaboration of the legal strategy includes the psychosocial aspects raised by the specialist professionals: from the diagnosis of the judicial situation of the child carried out in the previous stage, the teams develop a legal strategy to represent the interests of the children by appointment of a curator ad litem The elaboration of the legal strategy will include the following sub-processes:
    - Strategy scheme: The objectives of legal representation must be established according to each case, defining the particular actions to be developed before the courts of justice.
    - Feedback of the legal strategy: The strategy must be fed back with the observations and contributions of the actors, people, and institutions that relate directly to each child.
    - Registration of the legal strategy: Information and background information that accounts for the strategy implemented, including the contributions of other actors, must be incorporated into each child's folder.
- Visits to the family of the child: According to what the legal strategy defines, the appropriate actions must be established and executed, if applicable, with the family or significant adults of the child, namely:
  - Communication with the family or significant adults about the legal strategy to be adopted and permanent feedback regarding the status of the cause, if applicable.
  - Collaborate in monitoring the work of the residence, or another agency or

    program, in the strengthening of parental powers for decision-making regarding judicial actions.
  - Relationship with the community in which the child is inserted.

- Intersectoral coordination: Because situations of violation of rights generate effects beyond the strictly legal, teams must ensure that whoever is responsible for the child's care uses referral mechanisms to other relevant public services to cover the integrality of children's needs present. Similarly, suppose deficiencies are detected in this area. In that case, the Regional Coordination of the program will monitor that the child's representation team complies with the duty to represent the situation to SENAME or to whom it corresponds or make a presentation to the court, as required.

- Procedural processes: Corresponds to the execution of the legal strategy and essentially concentrates the set of actions that are carried out in a judicial process in the family courts, with jurisdiction in 10 criminal courts, superior courts of justice, and any other instance or headquarters in which the child is involved. As in the previous stages, all the actions carried out must be registered in each child's folder and be aimed at guaranteeing the exercise of children's rights recognized in the Convention.

- Follow-up of the child's situation once the situation of alternative care is over: The Technical Unit will verify that the regional teams of the program supervise the fulfillment of the sentences to guarantee adequate protection of the children. The duration of the follow-up must be extended for a minimum of three months until the practical completion of the sentence. The Social Worker will be in charge of the follow-up.

- Children exit the program: The triplet team evaluates if the objectives of the legal strategy have been met if the processing of the cases has been completed and if the follow-up period has been exhausted. Some causes of discharge are consistent with the end of alternative care, for example, successful adoption, return to the family of origin, completion of 18 years, etc.

- Referrals: The triplet in charge of the child's defense informs the residence of SENAME, the need for referral of the child, by findings made during the representation process, for example referrals to the health system or other programs of the SENAME Network.

## A.1 Lawyers

Lawyers are responsible for processing cases before courts of law, especially family courts, courts with jurisdiction in criminal matters, civil courts, and higher courts of justice, related exclusively to the execution of the ”My Lawyer” program and hired part-time (50%) in Charge of 60 children.

### A.1.1 Functions

- Develop the legal strategy for each child who accesses the service in conjunction with the psychosocial duo.
- Manage the appropriate legal actions in all the matters in which the represented child might be involved.
- Responsible for the complete processing of the cases of children him/her represents.
- Attend all hearings in which the law courts summon him.
- Conduct in-person interviews or observations with the children, family, or whoever is involved.
- Exhaust all procedural options to obtain a judicial decision favorable to the child’s interests he represents legally.
- Periodically inform, if appropriate, relatives or significant adults of the child’s procedural status of the cases he represents.
- Periodically inform the child of the procedural status of the cases in which he is represented, according to his stage of evolutionary development.
- Participate in case analysis meetings.
- Provide support to professionals of complementary projects regarding the orientation, care, and protection of a child who must appear at a hearing and, in general, during the processing and management of the case.
- Keep track of all the procedures carried out and incorporate required verifiers.

### A.1.2 Training and Experience

Qualified lawyer with desirable specialization in human rights, child and adolescent rights, criminal law, criminal procedural law, family law, or similar. With experience in litigation before the courts of the first instance of family, in ordinary and extraordinary procedures; before criminal courts of the first instance and before the superior courts of justice, with knowledge in prevention, promotion, protection, and restitution of rights, threat, and violation of rights and crimes committed against children. With experience in work, coordination, and articulation in the inter-institutional and intersectoral network. With skills for conflict resolution and interventions in crises. Desirable experience in interviews with children in situations of high complexity.

## A.2 Social worker

Professional social worker, with training and experience in family law, the law of childhood and adolescence, child abuse and intersectoral management, with skills to work and link with children violated in their rights, and work in multidisciplinary teams. In addition, experience and knowledge are required regarding the family courts' functioning, the health and education network, and the SENAME Network and hired full time, in charge of 200 children.

### A.2.1 Functions

- Responsible for delivering social support to the program team in problems associated with serious violations of rights.
- Socio-family care and follow-up, home visits, interviews, work in and with networks, as strictly required by the legal strategy, and in permanent coordination with professionals of complementary projects to the program, when appropriate.
- Conduct interviews or observations with the children, family, or others involved that correspond and must move if necessary. Permanent coordination with the network involved.
- Contribute to the elaboration of the diagnosis of the judicial situation and the development and execution of the legal strategy of each child. Record all the actions performed and incorporated required verifiers.

- Other functions specific to the work methodology and legal strategy adopted for the program's execution.

### A.2.2 Training and Experience

A qualified social worker with specialized training in family and childhood matters, desirable training in criminal law or child abuse, experience working with children in violation of rights, and health and education networks. Desirable experience in interviews with children in situations of high complexity.

## A.3 Psychologist

Professional psychologist with training and experience in matters of the law of family, the law of childhood, adolescence, and reparation of the damage, with skills to work and link with children whose rights have been violated, and work in interdisciplinary teams.

### A.3.1 Functions

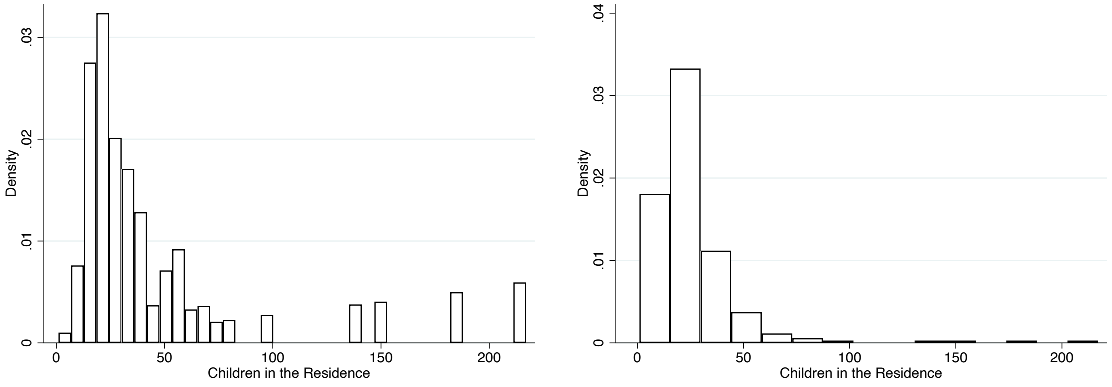
- Assess the child's mental health is entering the program by pre-existing reports.
- Assistance in emergencies or crises of the child in the context of the hearing, when appropriate.
- Contribute to elaborating the diagnosis of the judicial situation and legal strategy of each child.
- Permanent coordination with the network involved. Conduct interviews or observations with the children, family, or others involved that correspond and must move if necessary.
- Record all the actions performed and incorporated required verifiers.
- Other functions specific to the work methodology and legal strategy adopted by the programme.

### A.3.2 Training and Experience

Qualified psychologist with specialized training in family and childhood matters, desirable training in the field of criminal law to child abuse, and experience in working with children in situations of violation of rights.

# B Residences

Figure B.1: Number of Children in Residence



(a) By Child.

(b) By Residence.

Note: This figure shows the distribution of the number of children in residence grouped in two different ways. Panel (a) is built using children as the unit of interest. This way we build a data set that has the number of children in the residence for each child. Panel (b) is built using the residence as the unit of interest. This way we build a data set that has the number of children in the residence for each residence.

# C Randomization by Region

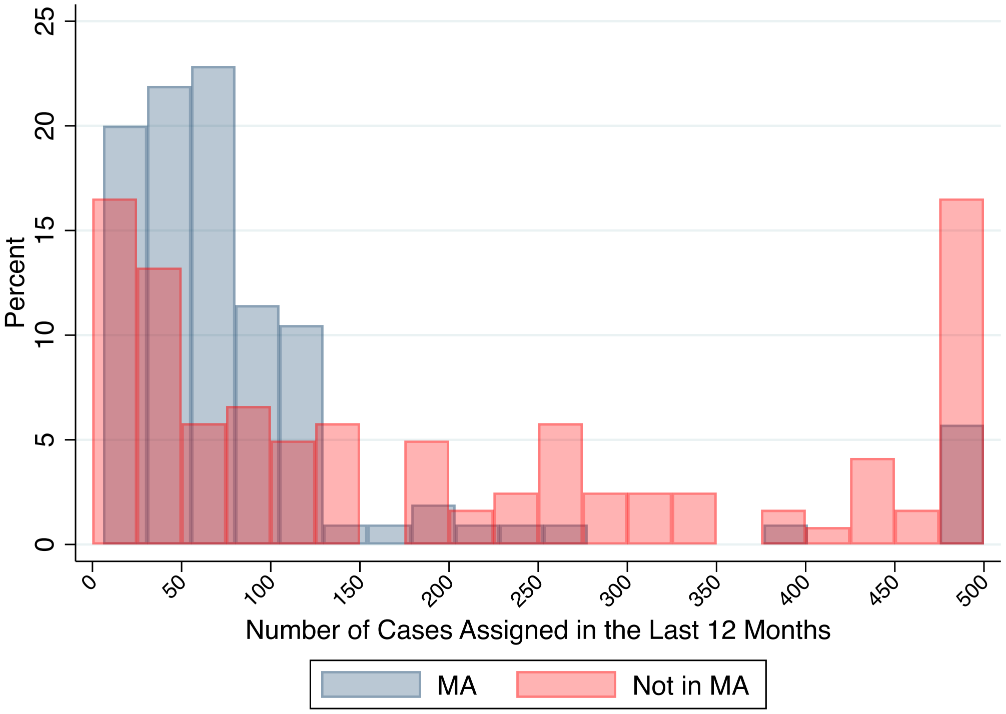
Table C.1: Randomization by Region

| Region | N Total | N Treated | Share Treated |
|---|---|---|---|
| Bío Bío | 378 | 28 | 0.07 |
| Santiago | 623 | 200 | 0.32 |
| Maule | 451 | 413 | 0.92 |
| Valparaíso | 419 | 42 | 0.10 |

Note: This table shows the number and share of children randomized to the treatment group across the four regions.

# D Lawyer Cases Assigned

Figure D.1: Number of Cases Assigned in the Last 12 Months at Endline, by Lawyer Institution



This figure shows the caseload distribution for lawyers from MA program and lawyers not in the MA program. Caseload is built with the number of new cases that lawyers have started working on in the past twelve months. This variable is truncated in 500 cases.

# E Data Sources

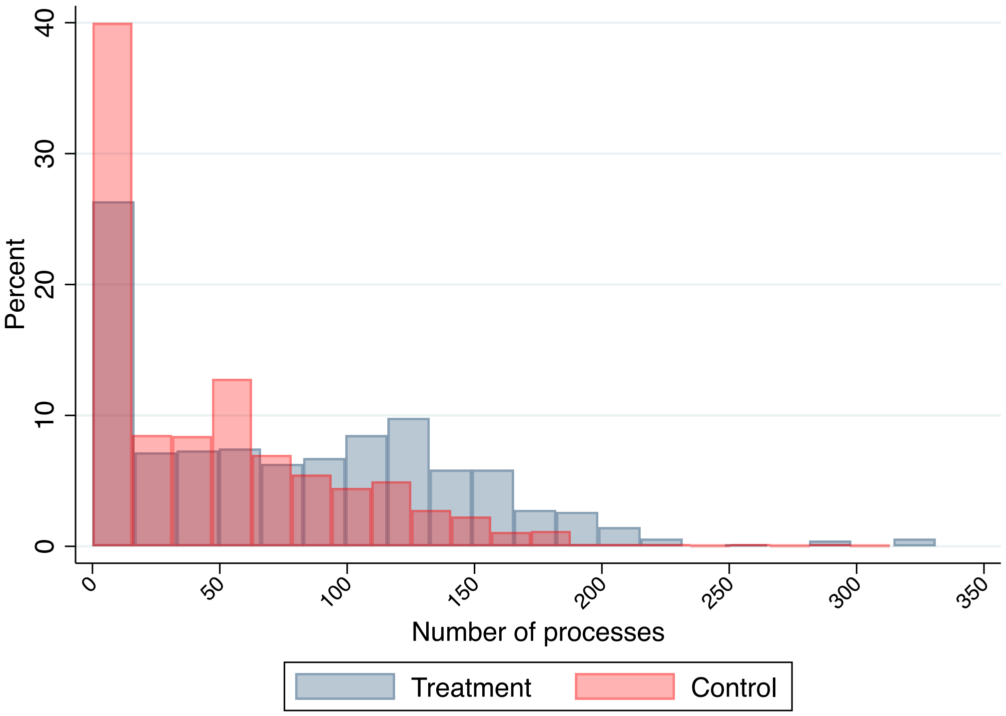
Table E.1: Data Description

| Source | Variable | Period use | Obs |
|---|---|---|---|
| Judiciary Registry | Crimes | January 2014 - August 2021 | 1871 |
| | Protection cases | January 2006 - February 2021 | 1871 |
| | Missing | January 2014 - August 2021 | 1871 |
| | Victimization | January 2014 - August 2021 | 1871 |
| | Allegations | January 2014 - August 2021 | 1871 |
| | Writs | January 2010 - February 2021 | 1871 |
| | Hearings | January 2010 - February 2021 | 1871 |
| SENAME (SENAINFO) | Days living with family | October 2018 - August 2021 | 1871 |
| | Days living in residence | January 2017 - August 2021 | 1871 |
| | Permanency | January 2017 - August 2021 | 1871 |
| | Age at entry in residence | January 2017 - August 2021 | 1871 |
| | Allegations | 2017 - 2021 | 1871 |
| | Dispositions | March 2019 - Dicember 2019 | 1871 |
| | Lengh of stay in residence | January 2017 - August 2021 | 1871 |
| | Delay in School | 2017 - 2019 | 1871 |
| Mi Abogado | Participation in Mi Abogado program | October 2018 - March 2021 | 1871 |
| | Days in Mi Abogado program | October 2018 - February 2021 | 1871 |
| | Days with non Mi Abogado Lawyer | October 2018 - February 2021 | 1871 |
| | Mi Abogado processes | January 2018 - January 2022 | 1871 |
| Ministry of Education | Grades | 2018 - 2019 | 1222 |
| | School Attendance | March 2017 - Dicember 2019 | 1871 |

Note: This table shows the sources of information used to construct each variable, the period available for each set of information, and the number of observations (children) that each source includes.

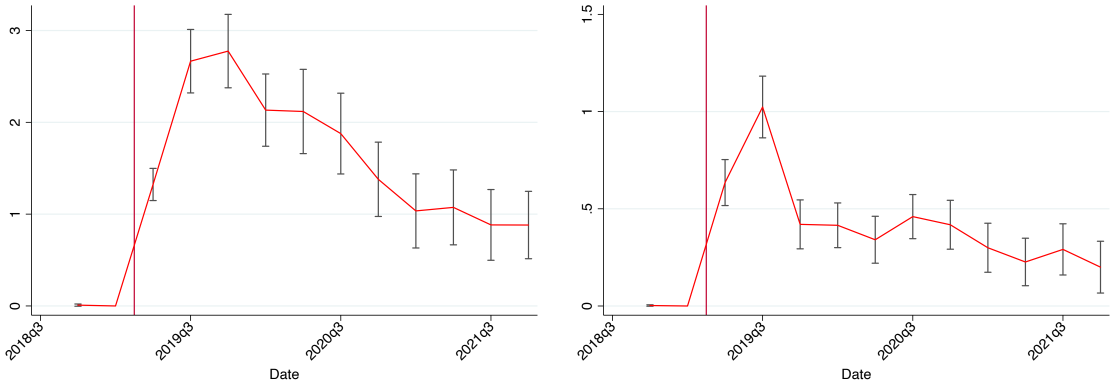
# F Process Analysis

Figure F.1: Distribution of the number of processes per child

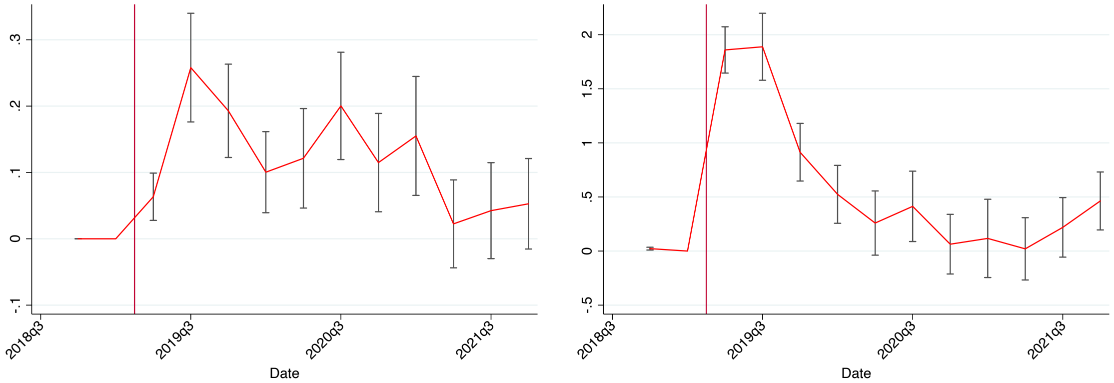


The figure depicts the total number of processes for children in our sample at the last observed date (may 27, 2022), by treatment status. The processes start in 2019, with minimal exceptions before then.

Figure F.2: Processes by type



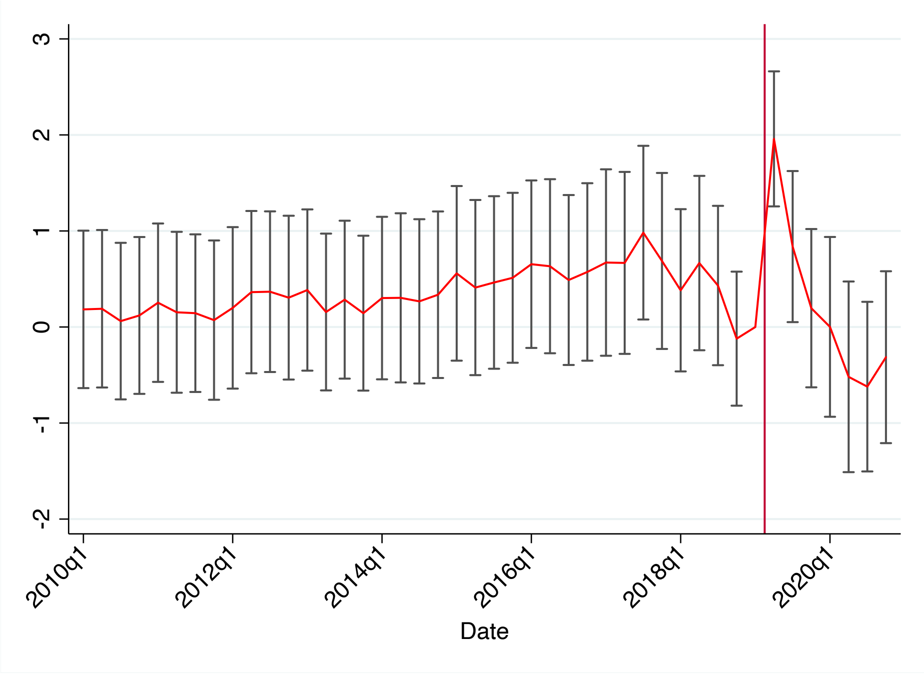
(a) Documentary Work (b) Interacting with Child



(c) Interacting with Family (d) Interacting with Residence Staff

Note: This figure shows, for each quarter, the effect of the Mi Abogado program on four different variables related to lawyer work with the children, estimated from daily data. Each regression includes one indicator for each period (minus the base, i.e., T-1 indicators) and an additional indicator for each period interacted with the treatment. We control for sex, region of residence and age group. The vertical line shows the time of randomization.

Figure F.3: Impacts on Quarterly Writs Submitted



Note: This figure shows, for each quarter, the effect of the Mi Abogado program in the number of writs sent, estimated from daily data. Each regression includes one indicator for each period (minus the base, i.e., T-1 indicators) and an additional indicator for each period interacted with the treatment. We control for sex, region of residence and age group. The vertical line shows the time of randomization.

# G Heterogeneous Treatment Effects

Table G.1: Treatment effects by subgroups

| Dependent Variable: | (1) Ever Living w/ Family/Qtr. | (2) Ever Living w/ Family/Qtr. | (3) Ever Living w/ Family/Qtr. | (4) Ever Living w/ Family/Qtr. |
|---|---|---|---|---|
| Heterogeneity Variable: | School Delay | Siblings | Residence | Age When First in Residence |
| Treatment x Post | 0.075 | 0.060 | 0.044 | 0.076 |
| | (0.026)*** | (0.028)** | (0.030) | (0.024)*** |
| Treatment Group | -0.020 | -0.008 | 0.037 | -0.040 |
| | (0.014) | (0.014) | (0.017)** | (0.013)*** |
| Post Randomization | 0.244 | 0.216 | 0.260 | 0.169 |
| | (0.015)*** | (0.015)*** | (0.016)*** | (0.015)*** |
| Heterogeneity Variable x Post | -0.038 | 0.022 | -0.069 | 0.107 |
| | (0.022)* | (0.022) | (0.022)*** | (0.022)*** |
| Heterogeneity Variable x Post x Treatment | -0.018 | 0.010 | 0.025 | -0.007 |
| | (0.037) | (0.037) | (0.038) | (0.038) |
| Treatment x Heterogeneity Variable | 0.004 | -0.023 | -0.085 | 0.042 |
| | (0.011) | (0.010)** | (0.014)*** | (0.013)*** |
| *N* | 31,579 | 31,579 | 31,579 | 31,579 |
| Control Group Mean for Hetero. Var. = 1 | 0.256 | 0.284 | 0.238 | 0.321 |

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

Note: This table shows the difference of MiAbogado effect on the chance of the children to go back to live with their family within different heterogeneous subgroups: school delay, amount of siblings, time in residence prior to randomization, and age when first in residence. Each estimation uses two groups formed from a heterogeneous variable that splits the sample in two. The cutoff point for the groups is the median within that variable. Control Group Mean Hetero. Var. = 1 indicates the mean in post period for the control group within the group that has heterogeneity variable equal to 1.

Table G.2: Treatment effects by subgroups

| Dependent Variable: | (1)<br>Ever Living w/ Family/Qtr. | (2)<br>Ever Living w/ Family/Qtr. | (3)<br>Ever Living w/ Family/Qtr. | (4)<br>Ever Living w/ Family/Qtr. |
|---|---|---|---|---|
| Heterogeneity Variable: | Region | Abuse | Gender | Younger than 12 |
| Treatment x Post | 0.042 | 0.074 | 0.097 | 0.067 |
| | (0.034) | (0.026)*** | (0.028)*** | (0.023)*** |
| Treatment Group | -0.008 | -0.011 | -0.021 | -0.014 |
| | (0.011) | (0.014) | (0.014) | (0.013) |
| Post Randomization | 0.223 | 0.245 | 0.184 | 0.239 |
| | (0.019)*** | (0.014)*** | (0.016)*** | (0.013)*** |
| Heterogeneity Variable x Post | 0.005 | -0.051 | 0.077 | -0.047 |
| | (0.024) | (0.022)** | (0.022)*** | (0.024)* |
| Heterogeneity Variable x Post x Treatment | 0.036 | -0.001 | -0.053 | 0.010 |
| | (0.041) | (0.037) | (0.038) | (0.039) |
| Treatment x Heterogeneity Variable | -0.014 | -0.024 | 0.002 | -0.025 |
| | (0.024) | (0.009)** | (0.013) | (0.014)* |
| $N$ | 31,579 | 31,579 | 31,579 | 31,579 |
| Control Group Mean for Hetero. Var. = 1 | 0.278 | 0.241 | 0.307 | 0.237 |

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

Note: This table shows the difference of MiAbogado effect on the chance of the children to return to live with their family within different heterogeneous subgroups: region, abuse, gender, and age. Each estimation uses two groups from a heterogeneous variable that splits the sample in two. For region, the heterogeneity variable equals zero if the child is from Region Metropolitana and one if the child is from any other region. For abuse, the heterogeneity variable equals one if the child has at least one abuse allegation that led to a SENAME stay. For gender, the heterogeneity variable equals one if the child is female. For "Younger than 12", the heterogeneity variable equals one if the child is 12 or younger at the randomization date.

# Table G.3: Treatment effects by subgroups

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent Variable: | Crimes Reports/Qtr. | Crimes Reports/Qtr. | Crimes Reports/Qtr. | Crimes Reports/Qtr. |
| Heterogeneity Variable: | School Delay | Siblings | Residence | Age When First in Residence |
| Treatment x Post | -0.001 | -0.050 | -0.067 | 0.019 |
| | (0.008) | (0.018)*** | (0.020)*** | (0.009)** |
| Treatment Group | -0.001 | 0.015 | 0.029 | -0.008 |
| | (0.009) | (0.013) | (0.015)** | (0.008) |
| Post Randomization | 0.024 | 0.112 | 0.128 | 0.009 |
| | (0.006)*** | (0.014)*** | (0.016)*** | (0.007) |
| Heterogeneity Variable x Post | 0.140 | -0.057 | -0.080 | 0.154 |
| | (0.020)*** | (0.018)*** | (0.020)*** | (0.019)*** |
| Heterogeneity Variable x Post x Treatment | -0.065 | 0.048 | 0.072 | -0.089 |
| | (0.027)** | (0.024)** | (0.026)*** | (0.025)*** |
| Treatment x Heterogeneity Variable | 0.017 | -0.012 | -0.037 | 0.035 |
| | (0.010)* | (0.009) | (0.011)*** | (0.011)*** |
| *N* | 58,001 | 58,001 | 58,001 | 58,001 |
| Control Group Mean for Hetero. Var. = 1 | 0.199 | 0.079 | 0.084 | 0.197 |

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

Note: This table shows the difference of MiAbogado effect on crime reports within different heterogeneous subgroups: school delay, amount of siblings, time in residence prior to randomization, and age when first in residence. Each estimation uses two groups formed from a heterogeneous variable that splits the sample in two. The cutoff point for the groups is the median within that variable. Control Group Mean Hetero. Var. = 1 indicates the mean in post period for the control group within the group that has heterogeneity variable equal to 1.

Table G.4: Treatment effects by subgroups

| Dependent Variable: | (1)<br>Crimes Reports/Qtr. | (2)<br>Crimes Reports/Qtr. | (3)<br>Crimes Reports/Qtr. | (4)<br>Crimes Reports/Qtr. |
|---|---|---|---|---|
| Heterogeneity Variable: | Region | Abuse | Gender | Younger than 12 |
| Treatment x Post | -0.004 | -0.031 | -0.058 | -0.038 |
| | (0.022) | (0.017)* | (0.026)** | (0.018)** |
| Treatment Group | 0.006 | 0.018 | 0.000 | 0.009 |
| | (0.014) | (0.014) | (0.013) | (0.013) |
| Post Randomization | 0.068 | 0.102 | 0.149 | 0.125 |
| | (0.017)*** | (0.011)*** | (0.019)*** | (0.013)*** |
| Heterogeneity Variable x Post | 0.034 | -0.034 | -0.106 | -0.122 |
| | (0.020)* | (0.021) | (0.020)*** | (0.013)*** |
| Heterogeneity Variable x Post x Treatment | -0.043 | 0.004 | 0.048 | 0.036 |
| | (0.027) | (0.026) | (0.027)* | (0.018)** |
| Treatment x Heterogeneity Variable | 0.001 | -0.021 | 0.016 | -0.004 |
| | (0.021) | (0.009)** | (0.013) | (0.009) |
| *N* | 58,001 | 58,001 | 58,001 | 58,001 |
| Control Group Mean for Hetero. Var. = 1 | 0.129 | 0.083 | 0.059 | 0.004 |

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

Note: This table shows the difference of MiAbogado effect on crime reports within different heterogeneous subgroups: region, abuse, gender, and age. Each estimation uses two groups from a heterogeneous variable that splits the sample in two. For region, the heterogeneity variable equals zero if the child is from Region Metropolitana and one if the child is from any other region. For abuse, the heterogeneity variable equals one if the child has at least one abuse allegation that led to a SENAME stay. For gender, the heterogeneity variable equals one if the child is female. For "Younger than 12", the heterogeneity variable equals one if the child is 12 or younger at the randomization date.

Table G.5: Treatment effects by predicted living-with-family and predicted crime

| Dependent Variable: | (1) Crime Reports/Qtr. | (2) Ever Living w/ Family/Qtr. | (3) Crime Reports/Qtr. | (4) Ever Living w/ Family/Qtr. |
|---|---|---|---|---|
| Heterogeneity Variable: | Predicted Crime | Predicted Crime | Predicted Permanency | Predicted Permanency |
| Treatment x Post | -0.004 | 0.067 | -0.033 | 0.028 |
| | (0.007) | (0.024)*** | (0.017)** | (0.023) |
| Treatment Group | 0.002 | -0.047 | -0.012 | -0.015 |
| | (0.008) | (0.013)*** | (0.008) | (0.012) |
| Post Randomization | 0.021 | 0.196 | 0.078 | 0.161 |
| | (0.005)*** | (0.016)*** | (0.014)*** | (0.013)*** |
| Heterogeneity Variable x Post | 0.131 | 0.034 | 0.025 | 0.119 |
| | (0.017)*** | (0.021) | (0.020) | (0.021)*** |
| Heterogeneity Variable x Post x Treatmeant | -0.043 | 0.002 | -0.005 | 0.033 |
| | (0.024)* | (0.037) | (0.025) | (0.035) |
| Treatment x Heterogeneity Variable | 0.010 | 0.048 | 0.046 | 0.013 |
| | (0.011) | (0.014)*** | (0.011)*** | (0.011) |
| Female | -0.044 | 0.050 | -0.067 | 0.011 |
| | (0.009)*** | (0.012)*** | (0.009)*** | (0.011) |
| $N$ | 58,001 | 31,579 | 58,001 | 31,579 |
| Control Group Mean | 0.116 | 0.254 | 0.116 | 0.254 |

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

Note: This table shows the difference of MiAbogado effect on crime reports and ever living with family within different heterogeneous subgroups: predicted crime and predicted living-with-family. Each estimation uses two groups from a heterogeneous variable that splits the sample in two. The cutoff point for the groups is the median within that variable. Control Group Mean indicates the mean in post period.

# H Additional Robustness Checks

## H.1 Second Randomization

Table H.1: Robustness Check: Not Using Replacements

| Dependent Variable: | (1) Participation /Qtr. Using replacements | (2) Participation /Qtr. Not using replacements | (3) Permanency /Qtr. Using replacements | (4) Permanency /Qtr. Not using replacements | (5) Crimes /Qtr. Using replacements | (6) Crimes /Qtr. Not using replacements |
|---|---|---|---|---|---|---|
| Treatment x Post | 18.545 | 18.749 | 0.058 | 0.057 | -0.036 | -0.038 |
| | (1.782)*** | (1.829)*** | (0.017)*** | (0.018)*** | (0.012)*** | (0.013)*** |
| Treatment Group | -1.220 | -1.303 | -0.016 | -0.016 | 0.010 | 0.011 |
| | (1.080) | (1.109) | (0.017) | (0.018) | (0.012) | (0.012) |
| Post Randomization | 33.328 | 33.328 | 0.218 | 0.218 | 0.088 | 0.088 |
| | (0.979)*** | (0.979)*** | (0.010)*** | (0.010)*** | (0.010)*** | (0.010)*** |
| *N* | 24,323 | 23,660 | 22,323 | 21,712 | 58,001 | 56,420 |
| Control Group Mean | 33.720 | 33.720 | 0.272 | 0.272 | 0.118 | 0.118 |
| N of children | 1,871 | 1,820 | 1,871 | 1,820 | 1,871 | 1,820 |
| N Control Group | 1,188 | 1,188 | 1,188 | 1,188 | 1,188 | 1,188 |

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

Note: This table compares two different estimations for three of our main results. Columns 1, 3, and 5 show the estimators or our main results. Columns 2, 4, and 6 show the estimators when we run the same regressions but without replacements. Replacements are a group of 51 children that were initially not drawn into the treated group on the randomization date but were later incorporated into this group because the program expanded. For all other estimations shown in the paper, replacements are treated as part of the treated group without any special consideration. Control Group Mean indicates the mean in post period.

## H.2 Weighting Regression Estimates by the Inverse of the Treatment Variance

Table H.2: Living-with-family and Crime Reports with region variance weights

| | (1) Dependent Variable: Ever Living w/ Family/Qtr. | (2) Dependent Variable: Ever Living w/ Family/Qtr. | (3) Dependent Variable: Crime Reports/Qtr. | (4) Dependent Variable: Crime Reports/Qtr. |
|---|---|---|---|---|
| | No Weights | Region Variance Weights | No Weights | Region Variance Weights |
| Treatment x Post | 0.066 | 0.069 | -0.032 | -0.038 |
| | (0.019)*** | (0.019)*** | (0.012)*** | (0.012)*** |
| Treatment Group | -0.020 | -0.019 | 0.009 | 0.011 |
| | (0.013) | (0.014) | (0.011) | (0.011) |
| Post Randomization | 0.194 | 0.194 | 0.089 | 0.094 |
| | (0.012)*** | (0.012)*** | (0.009)*** | (0.009)*** |
| $N$ | 31,579 | 31,579 | 58,001 | 58,001 |
| Control Group Mean | 0.254 | 0.254 | 0.116 | 0.116 |
| Controls | Yes | Yes | Yes | Yes |

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

Note: This table shows the effect of Mi Abogado program on living-with-family and crime reports. ITT regressions are estimated using a panel structure, clustered at the child level. Also, the estimates in columns (2) and (4) were calculated by weighting by the inverse of the variance of the treatment effect in each region. Crime Reports per quarter and days living-with-family per quarter are the main dependent variables. Control Group Mean indicates the mean in post period.

## H.3 Controls vs No Controls

Table H.3: Controls vs No Controls

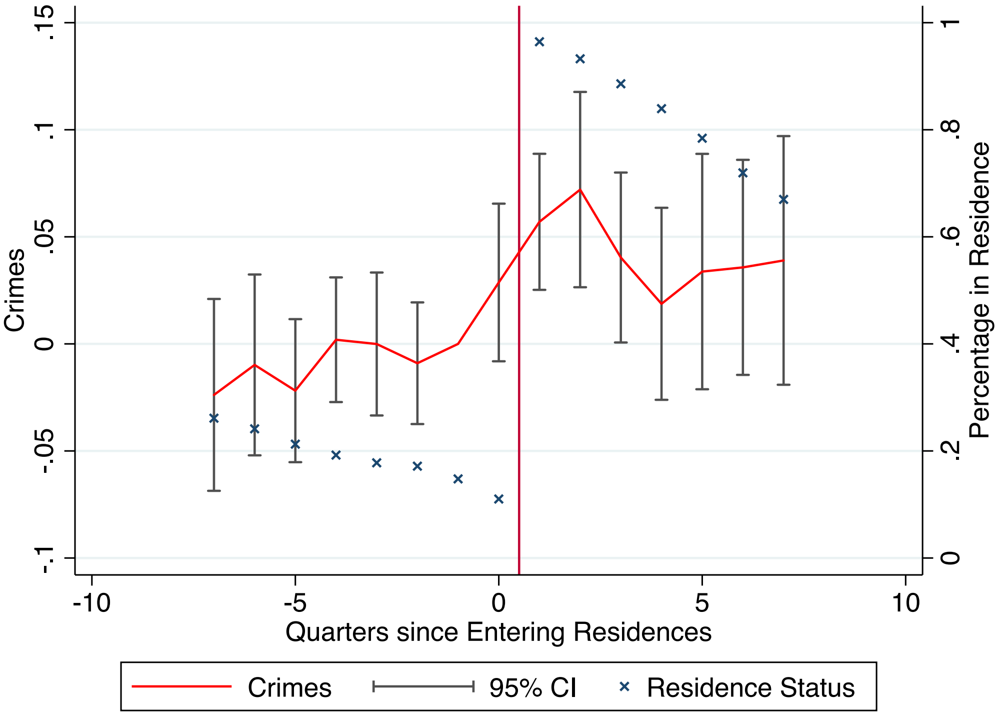
| Dependent Variable: | (1) Days exposed to Mi Abogado/Qtr. | (2) Days exposed to Mi Abogado/Qtr. | (3) Ever Living w/ Family/Qtr. | (4) Ever Living w/ Family/Qtr. | (5) Crime Reports/Qtr. | (6) Crime Reports/Qtr. |
|---|---|---|---|---|---|---|
| Treatment x Post | 18.545 | 18.545 | 0.066 | 0.066 | -0.036 | -0.036 |
|  | (1.782)*** | (1.783)*** | (0.021)*** | (0.021)*** | (0.012)*** | (0.012)*** |
| Treatment Group | -1.220 | -1.043 | -0.025 | -0.020 | 0.010 | 0.010 |
|  | (1.080) | (1.086) | (0.021) | (0.020) | (0.012) | (0.011) |
| Post Randomization | 33.328 | 33.328 | 0.124 | 0.124 | 0.088 | 0.088 |
|  | (0.979)*** | (0.980)*** | (0.013)*** | (0.013)*** | (0.010)*** | (0.010)*** |
| $N$ | 24,323 | 24,323 | 22,452 | 22,452 | 58,001 | 58,001 |
| N of children | 1,871 | 1,871 | 1,871 | 1,871 | 1,871 | 1,871 |
| N Control Group | 1,188 | 1,188 | 1,188 | 1,188 | 1,188 | 1,188 |
| Control Group Mean | 33.720 | 33.720 | 0.272 | 0.272 | 0.118 | 0.118 |
| Controls | No | Yes | No | Yes | No | Yes |

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

Hello

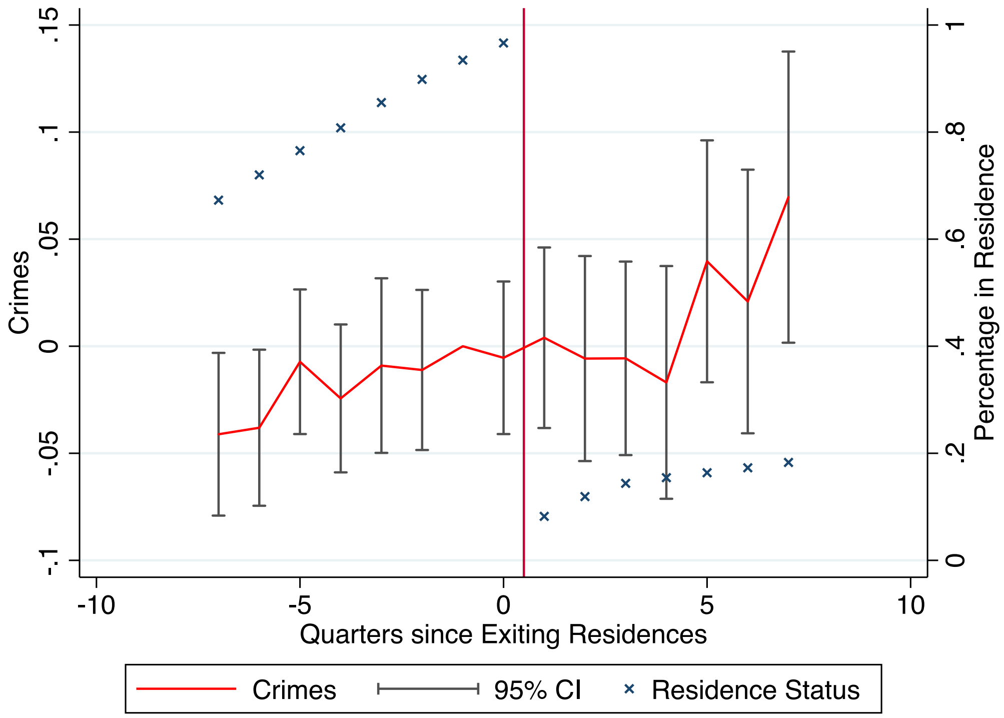
# I Exploring Mechanisms

Figure I.1: Event Study on Entering Residences



This chart shows an event study for crime reports before and after entry into a residence. The omitted period is two quarters before entering residence, to test whether crime reports precipitate entry. The vertical line is the moment of entering residences. The estimates are obtained from a regression of crime reports on the strata, period indicators, interactions between the periods and the treatment status, and age dummies (to control for the increase in crime that comes with age). The standard errors are clustered by child.

Figure I.2: Event Study on Exiting Residences



This chart shows an event study for crime reports before and after entry into a residence. The omitted period is two quarters before exiting residence, to test whether crime reports are changing just prior to exit. The vertical line is the moment of entering residences. The estimates are obtained from a regression of crime reports on the strata, period indicators, interactions between the periods and the treatment status, and age dummies (to control for the increase in crime that comes with age). The standard errors are clustered by child.

Table I.1: Residences as a Mediator

| Dependent Variable: | (1) Crime Reports/Qtr. Usual Estimate | (2) Crime Reports/Qtr. Control for Residences | (3) School Attendance/Qtr. Usual Estimate | (4) School Attendance/Qtr. Control for Residences |
|---|---|---|---|---|
| Treatment x Post | -0.032 | -0.035 | 0.030 | 0.034 |
| | (0.012)*** | (0.013)*** | (0.013)** | (0.013)*** |
| Treatment Group | 0.010 | 0.019 | -0.006 | -0.008 |
| | (0.011) | (0.021) | (0.017) | (0.016) |
| Post Randomization | 0.089 | 0.062 | -0.065 | -0.086 |
| | (0.009)*** | (0.009)*** | (0.008)*** | (0.008)*** |
| In Residence | | -0.024 | | 0.128 |
| | | (0.008)*** | | (0.010)*** |
| $N$ | 58,001 | 35,549 | 22,452 | 22,452 |
| N of children | 1,871 | 1,871 | 1,871 | 1,871 |
| N Control Group | 1,188 | 1,188 | 1,188 | 1,188 |
| Control Group Mean | 0.116 | 0.116 | 0.614 | 0.614 |

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

Note: This table shows the effect of Mi Abogado program on crime reports and school attendance. ITT regressions are estimated using a panel structure, clustered at the child level. Also, the estimation in columns (2) and (4) control for whether the child is in a residence. Crime reports per quarter and school attendance per quarter are the main dependent variables. Control Group Mean indicates the mean in post period.

# J Participation in the program

## J.1 Participation due to treatment status: Compliers

Table J.1: Compliers by control groups Q1 2020

| | Column 1 | Column 2 | Ratio | P[X=x] Col. 1 | Mean C1 | Mean C2 | Obs. |
|---|---|---|---|---|---|---|---|
| Number of Siblings | 65.291<br>(11.845) | 85.430<br>(12.455) | 0.764 | 0.49 | 0.00 | 3.06 | 1,871 |
| Delay in Schooling (Years) | 82.201<br>(12.130) | 68.383<br>(12.131) | 1.202 | 0.54 | -0.12 | 4.39 | 1,871 |
| Time in Residence | 66.729<br>(12.446) | 82.668<br>(11.707) | 0.807 | 0.50 | 0.85 | 2.22 | 1,871 |
| Age First Entry in Residence | 88.237<br>(12.154) | 58.414<br>(11.956) | 1.511 | 0.50 | 7.52 | 13.78 | 1,871 |
| Age at Randomization | 82.693<br>(12.585) | 68.330<br>(11.358) | 1.210 | 0.50 | 10.82 | 16.23 | 1,871 |
| Gender (Column 2 = Girl) | 58.837<br>(13.369) | 87.905<br>(11.105) | 0.669 | 0.43 | 0.00 | 1.00 | 1,871 |
| Predicted Permanency | 55.798<br>(12.643) | 92.525<br>(11.558) | 0.603 | 0.50 | 1.14 | 1.94 | 1,871 |
| Predicted Crimes | 85.694<br>(12.736) | 66.699<br>(11.361) | 1.285 | 0.50 | -0.05 | 1.29 | 1,871 |
| | | Beta | | Compliers | | | Obs. |
| Full Sample | | 75.223<br>( 8.570) | | 929 | | | 1871 |

Note: This table is built using the whole sample of children for Q1 2020. The sample is separated into two groups for each control variable. Column 1 is the coefficient for treatment when we regress "Days in the program" against treatment and strata variables using the children below the median in that control (or 0 if it is a dummy variable). Column 2 is the same coefficient but uses children above the median (or 1 if it is a dummy variable). P[X=x] Col. 1 is the result of the total number of children in the below group divided by the total sample of observations. Mean C1 is the mean of the heterogeneity variable for Column 1 individuals. Mean C2 is analogous for Column 2. The last row shows the coefficient for treatment when we use the whole sample and the number of compliers in the full sample

Table J.2: Compliers by control groups Q1 2021

| | Column 1 | Column 2 | Ratio | P[X=x] Col. 1 | Mean C1 | Mean C2 | Obs. |
|---|---|---|---|---|---|---|---|
| Number of Siblings | 96.565 | 134.963 | 0.715 | 0.49 | 0.00 | 3.06 | 1,871 |
| | (25.084) | (26.182) | | | | | |
| Delay in Schooling (Years) | 122.459 | 112.565 | 1.088 | 0.54 | -0.12 | 4.39 | 1,871 |
| | (25.197) | (26.062) | | | | | |
| Time in Residence | 100.874 | 129.333 | 0.780 | 0.50 | 0.85 | 2.22 | 1,871 |
| | (26.240) | (24.649) | | | | | |
| Age First Entry in Residence | 131.538 | 92.492 | 1.422 | 0.50 | 7.52 | 13.78 | 1,871 |
| | (24.425) | (26.469) | | | | | |
| Age at Randomization | 121.174 | 113.670 | 1.066 | 0.50 | 10.82 | 16.23 | 1,871 |
| | (25.084) | (25.360) | | | | | |
| Gender (Column 2 = Girl) | 76.217 | 147.395 | 0.517 | 0.43 | 0.00 | 1.00 | 1,871 |
| | (27.686) | (23.775) | | | | | |
| Predicted Permanency | 76.823 | 151.215 | 0.508 | 0.50 | 1.14 | 1.94 | 1,871 |
| | (26.626) | (24.318) | | | | | |
| Predicted Crimes | 126.044 | 110.934 | 1.136 | 0.50 | -0.05 | 1.29 | 1,871 |
| | (25.712) | (24.909) | | | | | |
| | | Beta | | Compliers | | | Obs. |
| Full Sample | | 116.368 | | 1100 | | | 1871 |
| | | ( 18.094) | | | | | |

Note: This table is built using the whole sample of children for Q1 2021. The sample is separated into two groups for each control variable. Column 1 is the coefficient for treatment when we regress ”Days in the program” against treatment and strata variables using the children below the median in that control (or 0 if it is a dummy variable). Column 2 is the same coefficient but uses children above the median (or 1 if it is a dummy variable). P[X=x] Col. 1 is the result of the total number of children in the below group divided by the total sample of observations. Mean C1 is the mean of the heterogeneity variable for Column 1 individuals. Mean C2 is analogous for Column 2. The last row shows the coefficient for treatment when we use the whole sample and the number of compliers in the full sample.

# K Cost-Benefit Analysis

Table K.1: Cost Benefit Analysis

| Legal Aid | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
|---|---|---|---|---|---|---|
| MA | 329.80 | 240.17 | 89.62 | 0.00 | 4.99 | 447.14 |
| No MA | 150.63 | 301.26 | -150.63 | 0.00 | 2.73 | -411.00 |
| Residence | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
| Cread | 113.36 | 118.07 | -4.71 | 0.53 | 67.27 | -316.87 |
| Ocas | 279.85 | 309.75 | -29.91 | 0.07 | 28.35 | -847.92 |
| Family Foster Care | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
| Ocas | 14.98 | 8.64 | 6.34 | 0.21 | 13.94 | 88.45 |
| Direct Administration | -1.06 | 0.45 | -1.51 | 0.15 | 23.46 | -35.52 |
| Total | | | | | | -1,075.72 |
| Sentencing | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
| Semi Closed | 3.73 | 1.74 | 1.99 | 0.14 | 38.50 | 76.56 |
| Closed | 0.24 | 0.81 | -0.57 | 0.28 | 136.21 | -77.07 |
| Partial | 1.03 | 0.53 | 0.50 | 0.41 | 36.68 | 18.36 |
| Full | 3.28 | 3.86 | -0.57 | 0.69 | 36.68 | -20.96 |
| Crime | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
| Property | 0.23 | 0.34 | -0.12 | 0.00 | 629.98 | -74.65 |
| Violent | 0.22 | 0.28 | -0.07 | 0.02 | 23,464.14 | -1,550.57 |
| Substance | 0.02 | 0.03 | -0.01 | 0.22 | 2,083.24 | -19.34 |
| Victimization | Mean T | Mean C | Dif | P-Value | Costs | DIF*Costs |
| Property | 0.00 | 0.01 | -0.01 | 0.05 | 975.74 | -6.19 |
| Violent | 0.17 | 0.18 | -0.01 | 0.59 | 8,912.69 | -112.07 |
| Substance | -0.00 | 0.00 | -0.00 | 0.08 | 12,156.26 | -21.24 |
| Total | | | | | | -2,571.18 |

Note: Estimates are on a per-child basis, and the observation period is 721 days. Costs are calculated in 2022 US dollars.