Working Paper

IMPACTS OF HOME VISITING DURING THE PANDEMIC

Evidence from a Randomized Controlled Trial of Child First

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ABSTRACT

Existing research has found that home visiting programs for families with young children can improve children's development and strengthen caregivers' and families' well-being. However, the pandemic created numerous challenges for home visiting programs, forcing them to deliver services online or in a hybrid format and to adapt the content of their program models to respond to pandemic-related challenges. Questions remain about the impacts of these programs when delivered at scale during this uniquely challenging time. The current study reports 12-month impacts from a randomized controlled trial of Child First—an evidence-based home visiting program that provides a psychotherapeutic, parent-child intervention embedded in a coordinated system of care—implemented across two states. After randomly assigning a racially and ethnically diverse sample of families (N = 226) from predominantly low-income backgrounds to receive the Child First services or typical community services, the research team surveyed caregivers (N = 183) about a year after program enrollment. Results from regression models with site fixed effects revealed that Child First reduced caregivers' job losses, residential mobility, and self-reported substance abuse, and increased receipt of virtual services during the pandemic. Child First also reduced parenting dysfunction (the name of the subscale within the Parenting Stress Index) among caregivers who reported clinical depression at enrollment. There were null impacts on caregivers' psychological well-being, families' involvement with the child welfare system, children's behaviors, and other indicators of economic well-being. Implications for future research and policy are discussed.

Keywords: home visiting, telehealth, pandemic, economic stability, substance abuse

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INTRODUCTION

Due in part to structural socioeconomic inequality, children from families with lower incomes may be at particularly high risk of abuse, neglect (Ondersma, 2002), and behavioral problems during infancy, toddlerhood, and early childhood (DeCarlo Santiago et al., 2011). Additionally, involvement in the child welfare system results in high societal costs (Doyle & Aizer, 2018). Policymakers have supported and invested in scaled interventions to address these issues. The federal Family First Prevention Services Act was passed in 2018 and stipulated that evidence-based programs—like home visiting services with experimental evidence behind them—receive the majority of federal funding to prevent involvement in the child welfare system, among other outcomes (Family First Prevention Services Act, 2017–2018).

The Child First home visiting program is one intervention (Lowell et al., 2011) that seeks to accomplish these goals. The program targets families with children from birth through age 5 experiencing challenges with caregiver mental health or child development and behavior problems. It provides home-based therapeutic intervention embedded in a coordinated system of care. Child First operates on a wide scale, serving thousands of families each year across five states and growing.[1] An earlier experimental study conducted at one site found that Child First had improved children's language development and social-emotional outcomes as well as mothers' psychological well-being a year after study enrollment. The program also reduced families' involvement with the child welfare system three years after study enrollment (Lowell et al., 2011). Effects were moderate to large in magnitude, indicating substantial benefits for participants over and above existing community resources at the time. However, research is still needed to examine the impacts of Child First when implemented *at scale* and to determine for whom the intervention is most effective. Additionally, given the significant disruptions caused by COVID-19 (Fong & Iarocci, 2020), questions remain about the benefits of Child First when services are administered virtually during a time when families experienced unprecedented challenges with finances, childcare, and mental health (Patrick et al., 2020). Because technology provides flexibility that facilitates engagement and participation (Self-Brown et al., 2022), home visiting programs may continue to integrate some virtual services into their models moving forward. As such, it is critical to generate rigorous evidence on the efficacy of this type of service delivery approach.

To this end, the current study adds to the literature by estimating the impacts of the Child First program implemented across nine sites in Connecticut and one site in North Carolina before and during the start of the COVID-19 pandemic. We used a randomized controlled trial to conduct this study, allowing us to estimate the *causal impact* of Child First implemented through in-person and virtual delivery relative to business-as-usual services in the community. We also examine how impacts may have varied depending on challenges families faced at study entry, such as caregiver depression or involvement with the child welfare system. This study aims to

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[1] As of January 2023, Child First operates in sites located in Colorado, Connecticut, Florida, North Carolina, and Pennsylvania.

strengthen the evidence base for Child First when implemented at scale during a uniquely challenging period, and to provide needed research on the populations most likely to benefit from these targeted services and supports.

The Impact of Home Visiting on Family, Caregiver, and Child Outcomes

There are many well-studied home visiting models, including deployment of nurses to promote infant and child health (Combs-Orme et al., 1985; Olds et al., 2004), expansion of Head Start via home visits to promote child development (Bilukha et al., 2005), and provision of home-based family support to promote positive parenting behaviors and prevent child maltreatment (Harding et al., 2007). Taken together, these programs seek to identify the strengths and needs of families with young children, provide education and support during home visits and through referrals to community services, and promote positive child development and caregiver and family well-being (Michalopoulos et al., 2019a).

Due in part to resource constraints, home visiting programs typically target families experiencing poverty (Minkovitz et al., 2016). The Family Stress Model (Conger & Conger, 2002) posits that economic disadvantage predicts greater economic pressure on families, creating emotional distress and depression for parents and leading to conflict and harsh parenting practices. Children exposed to such contexts are at heightened risk for poor social-emotional, internalizing, externalizing, and cognitive outcomes (DeCarlo Santiago et al., 2011). Building off the bioecological model of development (Bronfenbrenner & Morris, 1998), this framework posits that children are nested within a family microsystem where caregivers and immediate family members are the most powerful direct influences on their development. Home visiting programs aim to strengthen the family microsystem to promote a more emotionally supportive, healthy, and cognitively stimulating developmental context (Minkovitz et al., 2016), thereby resulting in improved outcomes for the child and family. The family microsystem is nested within a broader mesosystem that includes interactions between the family and other contexts, such as childcare providers and the caregiver's job. Home visitors also provide referrals to services and supports that promote positive engagement in broader mesosystemic contexts.

There is substantial evidence that preventive home visiting interventions can be effective in strengthening a range of caregiver, child, and family outcomes. For example, programs involving families with low incomes have improved caregivers' mental health (Duggan et al., 2004), parenting practices (Chazan-Cohen et al., 2013), and economic self-sufficiency (Olds, Kitzman, et al., 2004). As for young children, evidence from randomized controlled trials shows that home visiting can promote better child language (Olds et al., 2002) and behavioral outcomes (Caldera et al., 2007), while reducing child maltreatment (Kitzman et al., 1997). However, much of the literature on the efficacy of home visiting models focuses on a single geographic location or program site, in contexts that do not reflect recent implementation or service provision. Evidence-based home visiting programs are increasingly being implemented on a wider scale and in new settings, a trend supported by the Family First Prevention Services Act in 2018 (Family First Prevention Services Act, 2017–2018). There is thus further need for research to better understand the efficacy of evidence-based models when implemented at scale across diverse and contemporary settings and populations.

The Child First Home Visiting Program

Child First is a home visiting program with two components, each administered by one member of a two-person team. The first component is care coordination, which connects families to needed resources such as comprehensive, integrated services and supports to decrease psychosocial stress and promote positive outcomes. Extant work has shown that linking families to community services promotes better economic and psychological outcomes for caregivers (Olds et al., 1999). In addition to connecting families to needed resources, Child First care coordinators also support caregiver and child executive functioning as well as language-rich interactions that strengthen parent-child engagement through the Abecedarian approach (Sparling & Meunier, 2019). The second component of Child First is a trauma-informed dyadic relational intervention that promotes caregivers' executive functioning, responsive nurturing, and high-quality parent-child relationships—typically Child-Parent Psychotherapy (CPP) but including other interventions, such as Circle of Security, where appropriate—that is implemented in the family's home by a licensed clinician. Studies of CPP in other settings have found that the approach improved children's emotional regulation, cognitive development, attachment security and organization, and quality of parent-infant interactions, and reduced maternal depressive symptoms (Cicchetti et al., 2000; Field et al., 2000; Heinicke et al., 2001; Lieberman et al., 2006; Toth et al., 2006).

Lowell et al. (2011) conducted a randomized controlled trial of Child First at one site in Connecticut serving mostly Hispanic and Black mothers with low incomes and children ages 6 months to 36 months. The study found that Child First caregivers were three times less likely to report clinical levels of parenting stress 6 months after enrollment and four times less likely to report clinical depression 12 months after enrollment. There were also large reductions (OR = 4.7) in children's parent-reported behavior problems 12 months after study enrollment and families' involvement with the child welfare system 3 years after enrollment (OR = 2.1). Impressively, Child First halved the likelihood of eventual child welfare involvement for families who had yet to interact with that system.

Despite this evidence, questions remain about the impacts of Child First when implemented at scale across multiple sites and about the effects of the program on the broader range of outcomes targeted by Child First's service delivery model. Additionally, the earlier study of Child First did not examine impacts for the full age range of children—ages 0 to 6 years—served by the program. Finally, the earlier study lacked a sufficiently large sample size to examine how impacts may have varied for subgroups of families. To address these limitations, our team launched a randomized controlled trial across 10 sites in two states in the spring of 2019, aiming to collect data on 600 total families (split between a Child First treatment group and a business-as-usual control group). A little more than a third of the target sample enrolled in the study prior to March 2020, when COVID-19 forced Child First to switch to a telehealth model. The team halted study enrollment and shifted the research design to estimate the impacts of Child First delivered largely via telehealth services during the first year of the COVID-19 pandemic.

Effects of Home Visiting During the Pandemic

Despite the obvious problems that the pandemic caused for the overarching study, this shift offered the opportunity to study the impacts of Child First delivered in a hybrid mode. There is a growing body of evidence that telehealth or hybrid services—involving both in-person and virtual supports—can be effective at supporting participation and engagement. Rybińska et al. (2022) examined program participation in an evidence-based postpartum home visiting intervention a year before and after the onset of the pandemic. While participation declined one year after the pandemic, the drop was less than 10 percentage points, leading researchers to conclude that hybrid services could be an effective approach for maximizing participation in times of public health uncertainty. Mogil et al. (2022) conducted a randomized controlled trial of a trauma-informed and family-centered virtual home visiting program targeted at military families with children aged 3 years old to 6 years old. The study detected moderately large positive impacts on child affect and behavior (ES = 0.40) and parent affect and behavior for both mothers and fathers (ES = 0.43 and 0.34, respectively) 12 months after enrollment, and reductions in parents' post-traumatic stress symptoms (ES = 0.44) at 6 months after baseline, showing promise for telehealth services.

The Current Study

The pandemic caused significant disruptions to our original study design but provided an opportunity to examine the causal impact of Child First services administered via hybrid mode. This is an important contribution given the need for evidence on the effects of telehealth implementation, which can inform how and whether to use this mode of service delivery in the future. As such, the current study seeks to answer the following research questions:

1. Did Child First increase families' receipt of telehealth and other support services 12 months after study enrollment?

2. What are the impacts of Child First on families' economic well-being and caregivers' psychological well-being, substance abuse, and parenting stress 12 months after study enrollment?

3. What are the impacts of Child First on children's externalizing behaviors 12 months after study enrollment?

We then use exploratory analyses to examine whether caregiver and family outcomes varied by baseline depression, substance abuse, and involvement in the child welfare system, and whether child outcomes varied by caregiver-reported baseline behavior problems. Findings will provide information on the impacts of Child First when implemented at scale largely via telehealth services during the pandemic.

METHOD

Participants

The team recruited 10 Child First sites to participate in the study—nine smaller sites across Connecticut and one larger site in southeastern North Carolina. A total of 226 families consisting of a caregiver and a focal child enrolled between June 2019 and March 2020 (*N* = 148 in Connecticut; *N* = 78 in North Carolina). The study team randomly assigned 60% of families (*N* = 136) to receive Child First and 40% (*N* = 90) to a business-as-usual control group.[2] At the 12-month follow-up, 81% completed the caregiver-reported web survey (*N* = 111 program, *N* = 72 control), with similar levels of attrition across the Child First and control groups.

Table 1 presents the demographic characteristics of the 183 families with data at baseline and the 12-month follow-up, or the analytic sample for the study. Approximately two thirds of children enrolled in the study were male and the average age of the child sample was 3.66 years (SD = 1.38). The average age of caregivers at enrollment was 34.48 years (SD = 10.63) and 73% of caregivers were the birth mothers of the focal child. Caregivers were diverse with respect to race and ethnicity, marital status, and education. About three quarters of families were in households with low incomes. Almost 60% of families had current or prior child welfare involvement. The study sample was representative of families enrolled in Child First in Connecticut and North Carolina from 2019 to the start of the pandemic.[3]

Procedure

The Institutional Review Board (IRB) at the lead organization for this study approved the human subjects plan prior to the commencement of study activities.

Site recruitment. Child First sites are affiliate agencies of the National Service Office for Nurse-Family Partnership and Child First. As such, their participation in the study was voluntary. To recruit sites, the study team met with affiliate agencies to discuss the goals of the study, study procedures, and roles and responsibilities. To be eligible for the study, sites were required to have a waitlist of families wanting Child First services, ensuring that demand for services exceeded supply and that random assignment could be implemented ethically. Sites that were eligible and agreed to participate received a study payment ($1,300 – $10,875) that was commensurate with the number of families they aimed to enroll. The study team provided sites with iPads to facilitate random assignment and study enrollment. Staff were told that they could keep the iPad at the

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[2] We used a 60/40 random assignment ratio to address sites' concerns about needing to serve enough families to meet the program's funding requirements. The effects on statistical power were minimal when we used the 60/40 split rather than a 50/50 split.

[3] Among the broader population of families served by Child First during this time, 68% of children were male and 67% of caregivers were biological mothers of focal children. Caregivers were 26% Hispanic, 46% non-Hispanic White, and 25% non-Hispanic Black. Some 68% of households identified as low-income households.

TABLE 1

SOCIODEMOGRAPHIC CHARACTERISTICS OF THE ANALYTIC SAMPLE AT BASELINE

CHARACTERISTIC	OVERALL	PROGRAM	CONTROL
Child			
Age in years	3.66	3.71	3.57
Female (%)	35.52	33.33	38.89
Caregiver			
Age in years	34.48	34.93	33.79
Birth mother (%)	72.68	70.27	76.39
Race/ethnicity (%)			
Hispanic	31.87	33.64	29.17
Non-Hispanic White	43.96	44.55	43.06
Non-Hispanic Black	19.78	18.18	22.22
Non-Hispanic Other	4.40	3.64	5.56
Marital status (%)			
Married or living with partner	40.66	38.18	44.44
Divorced or separated	19.23	23.64	12.50*
Single, never married	39.56	37.27	43.06
Widowed	0.55	0.91	0.00
Work status (%)			
Unemployed	36.81	35.14	39.44
Part-time employment	29.67	30.63	28.17
Full-time employment	33.52	34.23	32.39
Highest level of education attained (%)			
Less than high school degree	12.78	13.64	11.43
High school degree or GED	27.22	22.73	34.29
Some college	45.56	46.36	44.29
Bachelor’s degree or higher	14.44	17.27	10.00
Household (%)			
Has low income	73.68	74.76	72.06
Receives public assistance	74.59	73.64	76.06
Ever experienced homeless	17.71	13.33	24.29*
Any reported substance abuse	18.86	17.14	21.43
Any prior or current CPS involvement	58.33	59.63	56.34
Number of families	183	111	72

Note: Sample has limited missing data. GED = General Education Development test, CPS = child protective services. *** p < .01, ** p < .05, * p < .10.

end of enrollment and the team used this as another incentive for site participation. Sites located in areas with community services like Child First (i.e., offering in-home CPP) were excluded to maximize the service contrast between the treatment and control groups.

Families' enrollment into the study. To be eligible for the study, families needed to be eligible for Child First but could not have already participated in the program. They also needed to speak English or Spanish. They were ineligible if they exhibited suicidality or required immediate medical intervention for psychosis.[4] As illustrated in Figure 1, 240 families were eligible to enroll in the study before the pandemic began. Of those, 226 agreed to participate in research activities and signed an informed consent. Families that did not agree to the research still went through the random assignment process (approved by sites and the IRB) to ensure that the sample was representative and there were no external incentives to refuse study participation.

Random assignment. Site supervisors screened new applications and referrals to the program to determine eligibility based on the agency and study criteria. Families eligible for the study were informed that the site was participating in a randomized controlled trial and that they would have a 60% chance of receiving Child First. Random assignment was conducted during the initial visit in the home or shortly thereafter. Families assigned to Child First began receiving services immediately. Families assigned to the control group received a list of alternative services available to them in the community and were subject to an 18-month embargo during which time they could not receive Child First services.

Baseline caregiver survey. Caregivers that provided consent were asked to complete a baseline survey during the first home visit prior to random assignment. Child First staff administered the first half of the survey, which included topics such as demographics and family structure. Caregivers self-administered the remaining questions, which were more sensitive, including items about substance abuse, psychological well-being, and the child's behavior. Caregivers that completed the survey received a $50 gift card to thank them for their time.

Follow-up caregiver survey. The study team tracked caregivers' email addresses, phone numbers, and mailing addresses 3 months and 6 months after enrollment. We used baseline contact information, information from the tracking effort, and additional information provided by sites to locate caregivers for a 30-minute survey about 12 months after they enrolled. The team administered the survey from August 24, 2020 through March 13, 2021. Caregivers reported on their economic and psychological well-being, children's behaviors, and receipt of services during the previous year. Most caregivers completed the survey online via Qualtrics while a small group (5%) completed the survey over the phone. The average amount of time between enrollment and follow-up was 12.45 months (SD = 0.95, min = 10.72, max = 15.87). Caregivers that completed the follow-up survey received a $30 gift card to thank them for their time. Differential attrition

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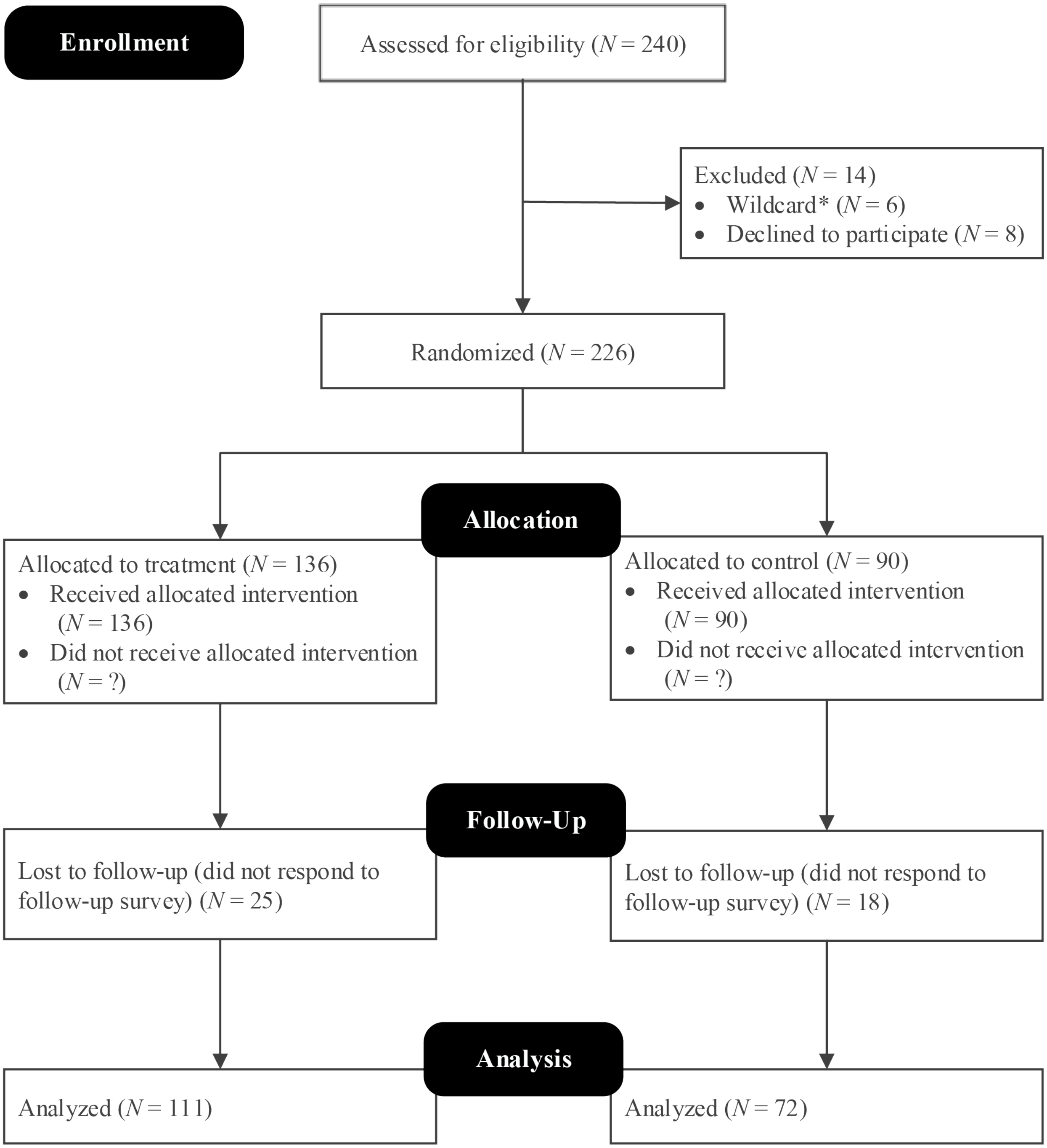
[4] If caregivers or children were eligible for Child First but not the study, sites continued to enroll them in services as normal. Teams were allotted a small number of "wildcards" (six were used across all sites during enrollment) to exempt families from random assignment if they determined that potentially denying services would be unethical.

FIGURE 1

CONSORT DIAGRAM



Note: *Sites were allotted a limited number of “wildcards” that allowed them to exclude eligible, high-risk families from the study and random assignment. Sites were allowed to use their own discretion for determining when to use a wildcard.

between the Child First and control groups (1.6%) was minimal. As seen in Table 2, follow-up survey respondents were representative of all initial study participants.

Programmatic data to assess dosage and fidelity. Child First's electronic health record tracks services received by families, including the number and length of visits and whether visits happened virtually or in person. We used data from this system to assess fidelity to the model for the 135 families assigned to the intervention. During the pandemic, Child First also started collecting data about the family's primary mode of contact, problems that may have begun or escalated during the pandemic, and assistance provided to families by staff.

Measures

The team aimed to collect similar measures at baseline and follow-up where possible (exceptions are noted below).

Treatment assignment. We coded treatment assignment at the family level, differentiating between families assigned to Child First (1) or the control group (0).

Families' economic stability. At enrollment, caregivers reported on several measures of economic stability. We created indicator variables coded as 1 if the category described the caregiver or family situation and 0 otherwise. Indicators included employment status (employed full time, employed part time, or unemployed), whether the family had received any public assistance (Supplemental Nutrition Assistance Program or SNAP, unemployment insurance or UI, and Temporary Assistance for Needy Families or TANF) in the past month, and whether the family had ever experienced homelessness. We also collected information on the number of jobs held, number of hours worked, and total earnings in the past month. The team collected similar measures at follow-up and then assessed whether the caregiver had lost a job in the previous year, whether the caregiver lost their job due to the pandemic, the number of times the family moved, whether the family was ever evicted, and whether the caregiver and child ever lived apart.

Food insecurity. We measured food insecurity at baseline with two items from the United States Department of Agriculture's index of food insecurity: "I worried whether our food would run out before we got money to buy more" and "The food I bought just didn't last and we didn't have money to get more" (adapted in Michalopoulos et al., 2019b). If the caregiver indicated worry about either item, then the household was considered food insecure (coded as 1). Due to time constraints, only the latter question was used to assess food insecurity at follow-up.

Substance abuse. We created a binary measure of substance abuse based on caregiver reports of heavy alcohol use and drug use at baseline and follow-up. We defined alcohol abuse as drinking an average of seven or more alcoholic beverages in a week in the past three months. We coded drug use based on reported use of any illicit drugs, as well as any prescription drugs (including opioids) and marijuana used without a prescription, in larger amounts than prescribed or for a longer period than prescribed. At baseline, we also included caregivers that had received help for substance abuse in the past year within this group. This is similar to how substance abuse has been captured in similar scaled studies of home visiting (Michalopoulos et al., 2019b).

TABLE 2

SOCIODEMOGRAPHIC CHARACTERISTICS OF RANDOMLY ASSIGNED FAMILIES AND ANALYTIC SAMPLE AT BASELINE

CHARACTERISTIC	ALL FAMILIES	ANALYSIS SAMPLE
Child		
Age in years	3.63	3.66
Female (%)	37.61	35.52
Caregiver		
Age in years	34.08	34.48
Birth mother (%)	73.45	72.68
Race/ethnicity (%)		
Hispanic	33.48	31.87
Non-Hispanic White	44.20	43.96
Non-Hispanic Black	18.30	19.78
Non-Hispanic Other	4.02	4.40
Marital status (%)		
Married or living with partner	40.89	40.66
Divorced or separated	17.78	19.23
Single, never married	40.44	39.56
Widowed	0.89	0.55
Work status (%)		
Unemployed	38.22	36.81
Part-time employment	28.44	29.67
Full-time employment	33.33	33.52
Highest level of education attained (%)		
Less than high school degree	15.70	12.78
High school degree or GED	27.35	27.22
Some college	44.39	45.56
Bachelor's degree or higher	12.56	14.44
Household (%)		
Has low income	75.00	73.68
Receives public assistance	75.45	74.59
Ever experienced homeless	16.97	17.71
Any reported substance abuse	20.56	18.86
Any prior or current CPS involvement	58.18	58.33
Number of families	226	183

Note: N = Sample has limited missing data. GED = General Education Development test, CPS = child protective services.

Caregiver psychological well-being. Caregivers completed the Center for Epidemiological Studies Depression Scale-Revised version (CESD-R; Eaton et al., 2004) at baseline and follow-up. The measure has demonstrated good psychometric properties in prior work (Van Dam & Earleywine, 2011). Items are rated on a 0 to 4 scale with responses ranging from *not at all or less than one day* to *nearly every day for two weeks*. The team dropped one item ("I wished I were dead") out of a total of 20 due to the sensitivity of the question. Despite this, the internal validity of the scale was high at both baseline (α = 0.94) and follow-up (α = 0.95). Following established procedures (Van Dam & Earleywine, 2011), the team created an indicator of clinical depression for anyone with a sum score over 16.

At follow-up, caregiver psychological well-being was also measured using the caregiver-reported Brief Symptom Inventory (BSI; Derogatis, 1993). The team excluded one item ("Thoughts about ending your life") out of a total of 18 due to sensitivity within the population. This measure was included because it was used in the original randomized controlled trial of Child First (Lowell et al., 2011). Items were rated on a scale from 0 to 4 with responses ranging from *not at all* to *extremely*. We then calculated one global score, and scores for three subscales (somatization, depression, and anxiety) by averaging the items within the target domain. All the scales demonstrated good evidence of internal reliability (global scale: α = 0.94; depression: α = 0.92; anxiety: α = 0.90; somatization: α = 0.80). Prior work on this measure has also found that it has good evidence for concurrent and predictive validity (Boulet & Boss, 1991).

Child behavior problems. Caregivers of children younger than 3 years old at baseline completed the Brief Infant-Toddler Social and Emotional Assessment (BITSEA; Briggs-Gowan et al., 2004), a 42-item standardized screener consisting of five subscales: externalizing behaviors, internalizing behaviors, dysregulation, problem behavior, and competence. Prior work examining the assessment in similarly aged populations has found evidence of acceptable reliability and validity (Karabekiroglu et al., 2010). Caregivers rated items on a scale from 0 to 2 with responses ranging from *not true/rarely* to *very true/often*. We used only the externalizing (α = 0.85) and dysfunctional (α = 0.77) behaviors subscales at baseline as covariates in analyses estimating impacts on those same behaviors for children in this younger age group at follow-up (externalizing α = 0.80, dysfunctional behaviors α = 0.75). These were the domains hypothesized to be most affected by Child First (Lowell et al., 2011). Prior work evaluating Child First used a similar approach with children younger than 4 years old at the 12-month follow-up (Lowell et al., 2011).

Caregivers with focal children older than 3 years old at baseline completed the Preschool and Kindergarten Behavior Scales-Second Edition (PKBS-2; Merrell, 2003) at study enrollment and at follow-up. This assessment contains 76 items and consists of two subscales: social skills and behavior problems. Items are rated from 0 to 3 with responses ranging from *never* to *often*. On the 12-month follow-up survey caregivers were only asked to report on 24 total items from the behavior problems subscale to reduce the burden and limit redundant questions. The team used those 24 items to create baseline and follow-up measures, which had excellent internal reliability at both time points (baseline: α = 0.93; follow-up: α = 0.96).

Parenting stress. At the 12-month follow-up (and not at baseline), caregivers completed two out of three subscales from the short form of the Parenting Stress Index (PSI; Whiteside-Mansell et al., 2007): parental distress and parent-child dysfunctional interaction. In response to these questions, caregivers rated items on a scale from 1 to 4 with responses ranging from *strongly agree* to *strongly disagree*. Both domains had evidence of internal consistency (parental distress: $\alpha = 0.85$; parent-child dysfunctional interaction: $\alpha = 0.78$).

Involvement in child welfare. At baseline caregivers reported on whether they were currently involved in the child welfare system (1 = yes, 0 = no) or had been in the past (1 = any involvement past or current, 0 = no involvement). Caregivers then reported at follow-up whether they had been involved with the child welfare system since study enrollment (1 = yes, 0 = no).

Receipt of services. At the 12-month follow-up caregivers reported on services they received since study enrollment, including any telehealth services via video or phone call (1 = yes, 0 = no); any telehealth support during COVID-19 via video or phone call (1 = yes, 0 = no); food assistance (1 = yes, 0 = no); any financial assistance which included anything related to technology, household, food, or income (1 = yes, 0 = no); services for child behavior (1 = yes, 0 = no); and services for domestic violence or counseling (1 = yes, 0 = no).

Child, caregiver, and family demographic characteristics. We measured several child and caregiver characteristics at study enrollment and used the information to describe the sample, complete baseline equivalence checks, and create covariates for impact analyses. We assessed caregiver age; child age; child gender; race/ethnicity of the caregiver (Hispanic, non-Hispanic Black, non-Hispanic other, and non-Hispanic White); whether the caregiver was married or partnered (1 = yes, 0 = no); employment (working part time, working full time, or not employed); caregiver education (less than high school degree, high school degree or GED, some college, or bachelor's degree or higher); whether the family had received any public assistance in the month prior to study entry; any reported substance use at enrollment; the number of individuals living in the household; and whether the caregiver spoke a language other than English.

Analytic Approach

Baseline equivalence. We used independent samples t-tests to compare child, caregiver, and family characteristics between the treatment and control groups at study enrollment and determined whether there were any observable pretreatment differences between the groups.

Descriptive analysis. We calculated means and frequencies to describe assessment scores and caregiver and family experiences at baseline (where available) and at the 12-month follow-up. We used independent samples t-tests to determine whether differences between the treatment and control groups at follow-up were statistically significant.

Service delivery analysis. We calculated descriptive statistics to understand the experiences of families assigned to Child First. The team limited the examination of general service delivery (e.g., duration, number of visits, and average length of visits) to the 12 months immediately following study enrollment. Child First has an established schedule to determine dosage. Home visits with both the clinician and care coordinator should occur twice per week during the first month of enrollment. After that families should receive at minimum weekly visits lasting 60 to 90 minutes. Families should be served for a period of 6 months to 12 months, depending on need and the speed at which they complete program activities. We compared treatment group families' receipt of services to these guidelines to evaluate implementation fidelity.

Impact analysis. The team used an intent-to-treat approach to estimate the impact of Child First on outcomes. As such, all families enrolled in the study were retained in the analysis and included in their original random assignment group regardless of service receipt. This approach is considered the gold standard in evaluation research and, assuming random assignment succeeded in producing equivalent groups, allows us to estimate *causal impacts* of Child First compared to business-as-usual services in the community.

We estimated impacts using OLS regressions with fixed effects for site. We regressed each outcome on an indicator for assignment to Child First (1 = yes, 0 = no), the level of the outcome measured at baseline or its proxy, and fixed effects for the Child First site. All models were adjusted for a suite of characteristics collected at study entry: child and caregiver age, child gender, caregiver race/ethnicity, caregiver married or living with partner, caregiver employment, caregiver education, family receipt of public assistance, caregiver self-reported substance abuse, family previous or current involvement with the child welfare system, household size, and English as the primary language spoken by the caregiver. We also controlled for family homelessness experience in the residential mobility impact model. In analyses predicting binary outcomes, the team estimated impacts with linear probability models, which are essentially OLS regressions but allow coefficients to be interpreted as percentage point differences between the treatment and control groups. This approach is widely used for binary outcomes with appropriate sample sizes as it improves transparency and the interpretation of impact estimates (Gomila, 2021).

We operationalized the child behavior outcome in three ways. To combine measures across age ranges, we standardized scores (i.e., created z-scores) within age groups. This allowed us to estimate impacts regardless of the assessment completed. We also created scores based on the same five items mapped across assessments. Finally, we fit separate impact models within each age group using raw scores to calculate impacts on assessments separately.

The variable of interest in the impact model is the coefficient on the treatment indicator and the corresponding standard error and p-value. This coefficient represents the difference in the outcome between the treatment and the control group. We set alpha equal to 0.10 as a threshold for statistical significance in the study. The team justified this decision given the small sample size, the lack of evidence on how Child First might benefit families during the pandemic, and the exploratory nature of the study (Kyriacou, 2016).

Subgroup analysis. We fit a series of additional exploratory models to examine variation in impacts for conceptually important subgroups—caregivers' evidence of depression at baseline, child behavior problems at baseline, and prior involvement in child welfare. We added interaction terms between the treatment indicator and subgroup characteristic to each impact model and tested each subgroup impact separately.

RESULTS

Descriptive Statistics

Evidence of baseline equivalence. As illustrated in Table 2, we found that families assigned to Child First were similar to families assigned to the control condition across most characteristics (91%). There were two statistically significant (p <.10) differences—whether the caregiver was divorced or separated and whether the family ever experienced homelessness. However, given the large number of characteristics observed (N = 23), it was likely that some differences would occur by chance. To address instances of non-equivalence and to improve the precision of estimates, we controlled for a robust set of covariates (described above) in impact models, including caregiver marital status and history of homelessness.

Descriptive statistics and change in outcomes over time. Table 3 presents proportions for binary outcomes and means for continuous outcomes at baseline and follow-up by treatment status. We did not observe any statistically significant differences between the treatment and control groups on measures captured at baseline. At the 12-month follow-up there were five statistically significant (p <.10) differences between the groups—whether the family received any telehealth services, whether the family received telehealth services during COVID, whether the family received help or support for child behavior, whether the family ever moved during random assignment, and whether the caregiver self-reported any substance abuse.

Among non-statistically significant results there were some interesting trends in the data. At follow-up there were much lower levels of receipt of domestic violence services, slightly higher receipt of SNAP, and lower reports of food insecurity compared to baseline reports. Depression among treatment group caregivers stayed the same but increased among control group caregivers by over 5 percentage points at follow-up. Child behavioral problems decreased for both groups at follow-up compared to baseline.

Intervention fidelity and dosage. Table 4 presents service delivery findings for families (N = 135) assigned to the intervention one year after enrollment. Generally, families received services at a level aligned with the program model. Treatment families participated in Child First for 7.74 months (SD = 3.92) and received 30.44 visits (SD = 21.42). Visits lasted about 55 minutes on average (SD = 15.75). Additionally, most families (75.76%) received at least one telehealth visit, with families receiving 15.70 telehealth visits on average (SD = 16.25). About 44% of the sample received over half of their visits via telehealth. After the start of the pandemic, the primary mode of contact between Child First and families was video/telehealth (69.47%), followed by voice-only (28.42%) contact. Parenting stress was the most frequently reported problem to develop or increase during the pandemic followed by child behavioral problems and caregiver depression. Material goods made up the three most common types of assistance provided through care coordination. These types of assistance included gift cards (25.95%), toys and books (24.43%), and COVID-19 testing (21.37%).

TABLE 3

UNADJUSTED SAMPLE STATISTICS AT BASELINE AND FOLLOW-UP BY TREATMENT STATUS

	BASELINE		FOLLOW-UP	
	PROGRAM	CONTROL	PROGRAM	CONTROL
Assistance/support receipt (%)				
Any telehealth services	--	--	60.19	42.03**
Telehealth support during COVID-19	--	--	28.16	8.57***
Food assistance	--	--	42.72	42.86
Any financial assistance	--	--	61.90	56.34
Help or support for child behavior	--	--	54.90	38.57**
Domestic violence services or counseling	22.86	19.44	5.66	4.23
Socioeconomic (%)				
Currently working for pay or self-employed	64.86	61.11	53.21	48.57
Lost job since random assignment	--	--	22.02	30.56
Received unemployment insurance	--	--	21.70	20.00
Received SNAP	55.45	45.07	56.19	57.14
Experienced food insecurity	37.38	45.07	35.85	38.57
Housing stability (%)				
Ever moved since random assignment	--	--	24.76	37.14*
Caregiver lived apart from child since random assignment	--	--	8.18	11.11
Involvement with the child welfare system (%)	59.63	56.34	29.81	30.88
Caregiver psychosocial well-being				
Any substance abuse (%)	17.14	21.43	2.91	12.86**
Clinical depression (CESD-R) (%)	25.23	18.31	24.55	23.94
Brief Symptom Inventory				
Global severity average (0-4)	--	--	0.49	0.47
Somatic subscale average (0-4)	--	--	0.38	0.31
Depression subscale average (0-4)	--	--	0.58	0.61
Anxiety subscale average (0-4)	--	--	0.53	0.51
Parenting Stress Index Short Form				
Dysfunctional subscale average score (1-4)	--	--	1.43	1.46
Distress subscale average score (1-4)	--	--	1.94	1.85
Child outcomes				
BITSEA				
Externalizing subscale (0-2)	0.78	0.60	0.59	0.33
Dysfunction subscale (0-2)	0.70	0.54	0.50	0.34
PKBS-2				
Externalizing subscale (0-3)	1.84	1.84	1.65	1.61
Number of families	111	72		

Notes: Sample has limited missing data. CESD-R = Center for Epidemiological Studies Depression Scale. BITSEA = Brief Infant-Toddler Social and Emotional Assessment. PKBS-2 = Preschool and Kindergarten Behavior Scales - 2nd Edition. *** p < .01, ** p < .05, * p < .10.

TABLE 4

CHILD FIRST SERVICE DELIVERY

CHARACTERISTIC	MEAN OR PERCENTAGE	STANDARD DEVIATION
Duration of service in months	7.74	3.92
Number of visits	30.44	21.42
Length of visits in minutes	54.99	15.75
Telehealth		
Ever received telehealth visit (%)	75.76	-
Number of telehealth visits	15.70	16.25
Received majority of visits via telehealth (%)	43.94	-
COVID-19 services (%)		
Primary mode of contact		
Video/telehealth	69.47	-
Voice only	28.42	-
Problems that began, escalated, or increased during the pandemic		-
Parenting stress	39.69	-
None	26.72	-
Child behavioral problems	17.56	-
Caregiver depression	17.56	-
Did Child First help with the following problems?		-
Parenting stress	96.15	-
Child behavioral problems	78.26	-
Caregiver depression	82.61	-
Assistance accessed or provided through care coordination		
Gift cards	25.95	-
Toys and books	24.43	-
COVID-19 testing	21.37	-
Number of program families	135	

Note: Sample has limited missing data. Duration of service is limited to the first 12 months post random assignment. The items related to the help provided by Child First only represent the opinion of caregivers that reported that problem.

Impact Analysis

Research Question 1: Impacts of Child First on families' receipt of services and support during the pandemic. As illustrated in Table 5, Child First increased the percentage of families who were able to access virtual services during the pandemic ($\gamma = .16$, SE = .07, $p = < .01$). Coefficients can be interpreted as percentage point differences between the treatment and control groups. Impacts

TABLE 5

IMPACTS AT 12-MONTH FOLLOW-UP

OUTCOME	PROGRAM	CONTROL	DIFFERENCE/ IMPACT	STANDARD ERROR
Assistance/support receipt (%)				
Any telehealth services since random assignment	55.63	42.36	13.28	0.08
Telehealth support during COVID-19	24.39	8.44	15.95	0.07**
Food assistance since random assignment	43.07	42.09	0.98	0.09
Any financial assistance since random assignment	59.27	55.78	3.49	0.08
Help or support for child behavior since random assignment	51.94	39.19	12.75	0.08
Domestic violence services or counseling since random assignment	4.43	4.50	-0.06	0.03
Socioeconomic (%)				
Currently working for pay or self-employed	51.09	49.87	1.22	0.07
Lost job since random assignment	19.79	33.24	-13.45	0.07*
Received unemployment insurance since random assignment	22.65	21.40	1.25	0.07
Received SNAP since random assignment	52.69	59.48	-6.79	0.06
Experienced food insecurity since random assignment	35.06	33.46	1.59	0.07
Housing stability (%)				
Ever moved since random assignment	23.64	36.65	-13.01	0.08*
Caregiver lived apart from child since random assignment	9.09	12.30	-3.21	0.05
Involvement with the child welfare system (%)	27.77	32.16	-4.39	0.07
Caregiver psychosocial well-being				
Any substance abuse (%)	4.05	11.67	-7.62	0.04*
CESD-R				
Depression cutoff (%)	25.26	25.38	-0.12	0.07
Brief Symptom Inventory				
Global severity average (0-4)	0.44	0.49	-0.05	0.10
Somatic subscale average (0-4)	0.34	0.31	0.03	0.08
Depression subscale average (0-4)	0.52	0.66	-0.14	0.13
Anxiety subscale average (0-4)	0.47	0.55	-0.07	0.12
Parenting Stress Index Short Form				
Dysfunctional subscale average score (1-4)	1.40	1.48	-0.07	0.10
Distress subscale average score (1-4)	1.94	1.92	0.02	0.14

(Continued)

TABLE 5 (CONTINUED)

OUTCOME	PROGRAM	CONTROL	DIFFERENCE/ IMPACT	STANDARD ERROR
Child behavior				
Standardized externalizing/dsyfunctional subscale (units)	0.00	-0.05	0.05	0.14
5-item cross domain subscale average (0-3)	1.41	1.40	0.01	0.12
BITSEA				
Externalizing/dysfunctional subscale average (0-2)	0.53	0.32	0.20	0.15
PKBS-2				
Externalizing subscale average (0-3)	1.62	1.64	-0.01	0.12
Number of families	111	72		

Notes: CESD-R = Center for Epidemiological Studies Depression Scale. BITSEA = Brief Infant-Toddler Social and Emotional Assessment. PKBS-2 = Preschool and Kindergarten Behavior Scales - 2nd Edition. At follow up, 43 children completed the BITSEA and 135 completed the PKBS-2. Three caregivers reported not living with the focal child at follow up or living apart for more than 10 months since baseline and did not complete the child assessment. Sample has limited missing data. *** p < .01, ** p < .05, * p < .10.

Baseline controls include: caregiver age, child age, child gender, caregiver race/ethnicity, caregiver married or partnered, employment status, caregiver education level, whether the family received any assistance [Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI), Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)], any substance use, past or current child welfare involvement, number of individuals in the household, caregiver speaks a language other than English, site indicators, and outcome control measures, if available.

on food assistance, financial assistance, help or support for child behavior, and domestic violence services or counseling were not statistically significant. The team multiplied impacts for binary outcomes by 100 so that impacts would be more easily interpreted as percentage point differences. The standard errors in Table 5, however, correspond to the form of the impact divided by 100.

Research Question 2: Impacts of Child First on families' economic well-being and caregiver's psychological well-being, substance abuse, and parenting stress. As summarized in Table 5, Child First reduced the likelihood of a caregiver losing a job (γ = -.13, SE = .07, *p* = <.10), reduced caregivers' self-reported substance abuse (γ = -.08, SE = .04, *p* = <.10), and reduced families' residential mobility (γ = -.13, SE = .08, *p* = <.10) in the 12 months after random assignment. These impacts were all sizeable, ranging from an 8 to 13 percentage point difference in the outcome between the treatment and control groups.

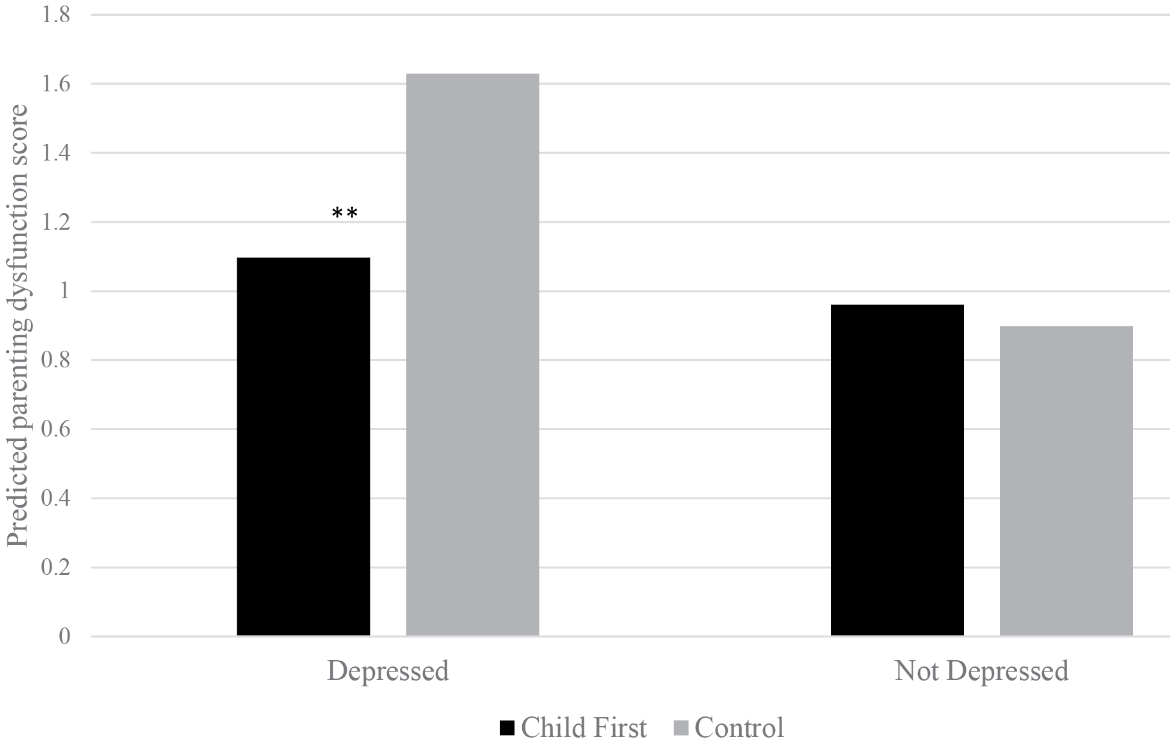
Importantly, however, there were no statistically significant impacts on caregivers' depressive symptoms, parenting stress, or self-reported involvement in the child welfare system. Given limited statistical power, we examined the *direction* of the impacts within this domain. We found that 11 out of 16 tests were in a direction favoring Child First, with the remaining effects close to 0. This pattern suggests a need for further study with a larger sample to generate the power needed to detect statistically significant differences between groups on these outcomes.

Research Question 3: Impact of Child First on children's externalizing behavior problems. Table 5 summarizes the results from models examining child behavior problems. We did not find evidence that Child First reduced children's caregiver-reported behavior problems across multiple checks. All three approaches resulted in a pattern of null—and close to 0—impacts on child behaviors. Notably, the bulk of the child-level sample was made up of children older than 3 years at enrollment (*N* = 135); only 43 children were in the younger group.

Exploratory subgroup analyses. We found a statistically significant interaction between assignment to Child First and the indicator for clinical depression at baseline (coefficient = -3.43, SE = 1.41, p = <.05) in the model predicting parenting dysfunction, a dimension of parenting stress. As seen in Figure 2, among parents who reported clinical depression at baseline, Child First reduced parenting dysfunction by 0.76 SD. However, we did not find that impacts on other outcomes varied by clinical depression, involvement in child welfare, or child behavior.

FIGURE 2

VARIATION IN PARENTING DYSFUNCTION AT 12-MONTH FOLLOW-UP BY TREATMENT STATUS



Note: *** p < .01, ** p < .05, * p < .10.

Sensitivity checks. We examined the robustness of the results to measurement decisions, model structure, and the number of impact models that we fit. We found that our substantive results were consistent across checks (see more in Table 6).

BOX 1

SUMMARY OF SENSITIVITY CHECKS

The team conducted several robustness checks to evaluate the consistency of our results. Below we summarize findings from this additional analytic work. Findings across our robustness checks were generally consistent with the results reported in the main text.

Measurement approaches. In cases where we operationalized scales using the average of the non-missing items in the scale, like caregiver and child well-being assessments, we also used summed scores to examine whether results were consistent across approaches. We ran models using these alternate measures and found that the magnitude of coefficients was the same and did not find any new statistically significant results.

Controlling for state effects. We fit a set of impact models with a state fixed effect for North Carolina rather than fixed effects for individual sites. All four of the caregiver and family outcomes reported in the main text were still statistically significant in the robustness checks. Whether families received telehealth support during the pandemic was still statistically significant, which we had concluded to be a major driver of findings in the main report and so we remain confident in our findings. Our key economic well-being and self-report of substance abuse findings remained statistically significant. In fact, the self-report of substance abuse finding increased in significance to $p < .05$. There were no differences between state fixed effect and site fixed effects child impact models. Results are presented in Table 6.

We also added interaction terms between the treatment indicator and state indicator for North Carolina to each impact model. We did not see any evidence that the effect of Child First varied across states.

Number of impact models. We used a series of omnibus tests to examine the general pattern of results and to consider the likelihood that overall, the program had a positive effect on parents and children. We felt that this was important given concerns about multiple comparisons (despite the largely exploratory nature of the current study). As noted in the impact tables, the general pattern of results was positive, and the omnibus test suggests that that was not likely due to chance. These results are encouraging because they suggest that with a better powered study—and a more intensive treatment—we could observe positive impacts of Child First at scale across a broader range of target outcomes, including caregivers’ psychological well-being.

TABLE 6

IMPACTS AT 12-MONTH FOLLOW-UP CONTROLLING FOR STATE

OUTCOME	PROGRAM	CONTROL	DIFFERENCE	STANDARD
Assistance/support receipt (%)				
Any telehealth services since random assignment	54.88	43.48	11.40	0.08
Telehealth support during COVID-19	24.18	8.74	15.44	0.07**
Food assistance since random assignment	42.67	42.68	-0.01	0.08
Any financial assistance since random assignment	58.75	56.54	2.21	0.08
Help or support for child behavior since random assignment	51.44	39.94	11.50	0.08
Domestic violence services or counseling since random assignment	3.91	5.25	-1.34	0.03
Socioeconomic (%)				
Currently working for pay or self-employed	51.14	49.80	1.33	0.07
Lost job since random assignment	20.30	32.48	-12.18	0.07*
Received unemployment insurance since random assignment	21.96	22.44	-0.49	0.07
Received SNAP since random assignment	52.63	59.56	-6.93	0.06
Experienced food insecurity since random assignment	35.26	33.17	2.09	0.07
Housing stability (%)				
Ever moved since random assignment	23.46	36.92	-13.46	0.07*
Caregiver lived apart from child since random assignment	9.12	12.26	-3.13	0.05
Involvement with the child welfare system (%)	27.18	33.08	-5.91	0.07
Caregiver psychosocial well-being				
Any substance abuse (%)	3.74	12.13	-8.39	0.04**
CESD-R				
Depression cutoff (%)	25.05	25.72	-0.67	0.07
Brief Symptom Inventory				
Global severity average (0-4)	0.44	0.50	-0.06	0.09
Somatic subscale average (0-4)	0.34	0.31	0.02	0.08
Depression subscale average (0-4)	0.52	0.66	-0.15	0.13
Anxiety subscale average (0-4)	0.47	0.55	-0.07	0.11
Parenting Stress Index Short Form				
Dysfunctional subscale average score (1-4)	1.40	1.48	-0.08	0.10
Distress subscale average score (1-4)	1.94	1.93	0.01	0.14

(continued)

TABLE 6 (CONTINUED)

OUTCOME	PROGRAM	CONTROL	DIFFERENCE	STANDARD
Child behavior				
Standardized externalizing/dsyfunctional subscale (units)	-0.01	-0.04	0.04	0.14
5-item cross domain subscale average (0-3)	1.41	1.40	0.01	0.11
BITSEA				
Externalizing/dysfunctional subscale average (0-2)	0.47	0.39	0.08	0.13
PKBS-2				
Externalizing subscale average (0-3)	1.62	1.63	-0.01	0.12
Number of families	111	72		

Notes: CESD-R = Center for Epidemiological Studies Depression Scale. BITSEA = Brief Infant-Toddler Social and Emotional Assessment. PKBS-2 = Preschool and Kindergarten Behavior Scales - 2nd Edition. At follow up, 43 children completed the BITSEA and 135 completed the PKBS-2. Three caregivers reported not living with the focal child at follow up or living apart for more than 10 months since baseline and did not complete the child assessment. Sample has limited missing data. *** p < .01, ** p < .05, * p < .10.

Baseline controls include: caregiver age, child age, child gender, caregiver race/ethnicity, caregiver married or partnered, employment status, caregiver education level, whether the family received any assistance [Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI), Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)] any substance use, past or current child welfare involvement, number of individuals in the household, caregiver speaks a language other than English, state indicator, and outcome control measures, if available.

DISCUSSION

This study sought to add important evidence to the existing literature on the impacts of home visiting. The timing of our research coincided with the COVID-19 pandemic, presenting a unique opportunity to study the causal effects of Child First when implemented using a combination of in-person and telehealth services during the pandemic. On average, families received a high dosage of Child First during the study, and a large proportion of the sample (44%) received over half their visits via telehealth services. We found positive effects on some key outcomes for caregivers and families during a challenging time, suggesting that Child First was successful in connecting with families via telehealth and tailoring services during the pandemic.

Specifically, Child First demonstrated benefits in two key areas of family economic self-sufficiency and well-being: caregivers in the treatment group were less likely to lose their jobs and families were less likely to move. The moderately sized effects demonstrate that Child First made a meaningful difference in families' ability to navigate the economic challenges caused by the pandemic. These effects were somewhat unexpected and may be due in part to the unique context of the study. Findings may reflect Child First staff members refocusing their work to address essential and immediate economic needs during the pandemic, which also supports long-term economic stability for families. Decreasing the likelihood of job loss—and thus strengthening economic stability—not only benefits the family's overall economic well-being, but also has the potential to positively shape the quality of the environment in which the child grows up (Morris & Gennetian, 2003). Pandemic-related job loss has also been found to be a key risk factor for child maltreatment (Lawson et al., 2020) and emotional or physical abuse (Ma et al., 2022).

The impact on job loss could partially explain why Child First reduced residential instability. That, and Child First helping families access needed financial support or providing gift cards that improved economic stability, could have made it possible for families to stay in their current homes. This impact is important in this context given linkages between greater residential mobility in early childhood and a higher frequency of behavior problems in children from families with low incomes (Ziol-Guest & McKenna, 2014).

The reduction in reported substance abuse among the treatment group is a policy-relevant and potentially multigenerational benefit for families participating in Child First. The rates of substance abuse were low at follow-up (4% program, 12% control), which may reflect some social desirability bias in self-reporting. Even so, this approach for gathering information on substance abuse is standard and has shown to be reliable in the literature (Murphy et al., 2010). A reduction of 8 percentage points in substance abuse is significant given the high societal cost of treatment and the negative implications of substance abuse on employment (Terza & Vechnak, 2007), self-psychological well-being (Volkow, 2001), and children's well-being (Kuppens et al., 2019). Additionally, during the pandemic there was a significant increase in drinking and drug use among adults in the United States (National Institute of Drug Abuse, 2022) and parents who drank alcohol both monthly and weekly had higher levels of parental stress and greater odds of punitive parenting (Wolf et al., 2021). Further research is needed to test whether this impact can be replicated in a post-pandemic context.

While we did find some encouraging results—particularly given that this study examined Child First when implemented at scale—our study failed to detect statistically significant impacts on several target outcomes, including child behaviors, families' involvement in child welfare, and caregivers' depression and parenting stress. Prior work had found impacts of the program on these outcomes captured at various follow-up time points (Lowell et al., 2011). One challenge our study faced, however, was that the enrolled sample was only about one third of the planned sample size needed for a fully powered study of a scaled model. Most impact estimates on caregivers were in the hypothesized direction but failed to reach statistical significance.

In addition, the dosage of Child First delivered virtually may not have been sufficient to impact outcomes that may require intensive, in-person clinical services. Research done early in the pandemic found that it was challenging for young children to participate in any type of virtual home visiting service (Korfmacher et al., 2021b), meaning that children in particular may have experienced little direct intervention via the virtual supports. Nationally, home visitors reported difficulty keeping children engaged and in view during virtual visits, and about 60% of home visitors agreed that they needed to use more strategic coaching of caregiver-child interaction during visits (Korfmacher et al., 2021a). It is possible that children enrolled in Child First were not as receptive to virtual therapeutic interventions, like CPP, as they would have been if provided via in-person services, and it may have taken time for staff to adapt to this mode of administration. Child behavior problems were not high at enrollment and decreased over time, suggesting that these challenges may not have been as urgent for home visitors to address as employment and income needs, especially during the pandemic. After the first year of the pandemic, home visitors participating in another national study reported that they spent most of their time interacting with just the caregiver, and spent the majority of this time providing information, asking for information, or listening. They reported spending very little time observing the caregiver and child or modeling parent-child behaviors (Korfmacher et al., 2021a). In this virtual setting, it is possible that the Child First home visiting teams were simply not able to spend as much time on the relationship-based aspect of the intervention, which may have affected the pattern of impacts.

In addition, despite not receiving Child First, we observed that control group families accessed a wide range of services in the community. It is possible that the difference in services received between the treatment and control groups—perhaps due in part to the expansion of services and supports (spurred on by unprecedented federal funding) during the pandemic—were not sufficient to detect statistically significant impacts between groups on some outcomes. Finally, it is possible that our follow-up time point might have missed some positive effects that faded out by the time the data were collected. For example, in the original trial, the team observed reductions in parenting stress at 6 months after enrollment but not at 12 months (Lowell et al., 2011). Conversely, it might also take a longer time for the benefits of the program to emerge on outcomes like involvement in the child welfare system. Again, the original study found reductions in involvement in child welfare at 36 months after enrollment but did not detect those effects at earlier time points (Lowell et al., 2011).

We did find reductions in parent-child dysfunctional interaction (the name of the subscale measured within the Parenting Stress Index) in families where caregivers reported clinical depression at study entry. As seen in the service delivery data, parenting stress was a new or increasing issue for a large proportion of families during the pandemic, which the program was seemingly able to address. This finding aligns with risk and resilience frameworks highlighting the importance of providing key supports to those who have demonstrated critical risk characteristics (Cicchetti et al., 2000)—in this context, caregivers with clinical depression. Reducing a dimension of parenting stress during this time may have been particularly salient because changes brought about by the pandemic placed additional stress upon parents and increased the risk of parental burnout, a key risk factor for future child abuse and neglect (Griffith, 2022). The inability to detect other statistically significant interactions is likely due to limited statistical power, but also may be due to all families experiencing multiple challenges during the study period, regardless of risk factors at enrollment. This limited our ability to discern which groups would benefit most from Child First under normal circumstances.

Limitations

There are a few limitations that the team will seek to address in future work. The smaller than expected sample size limited our ability to detect statistically significant interactions in subgroup analyses. We are restarting this randomized controlled trial in a post-pandemic context and aim to enroll 600 families so that analyses will be powered to detect impacts of less than .15 standard deviations. Another potential limitation is that control group families may have accessed similar services as treatment group families during the pandemic. Many services switched to virtual during the pandemic, which may have made more resources accessible and possibly minimized the service differential between groups. Additionally, we observed two statistically significant differences between the treatment and control groups at baseline. While we controlled for these characteristics in our models, a larger sample size will help avoid any non-equivalence in future work. Finally, our outcomes are based on caregiver self-reports. Future work will aim to include direct assessments of children and administrative data on families to triangulate findings across follow-up data sources. It will also include implementation research that will provide more detail on the experiences of the home visitors and caregivers—a key aspect that can further contextualize our findings.

Implications for Policy and Practice

This study observed some beneficial impacts of Child First when implemented at scale using a hybrid service delivery model immediately before and at the onset of the pandemic. Although many home visiting services have resumed in-person operations, telehealth services will likely continue to be used for the foreseeable future, as they provide a convenient way to serve families who have limited availability or do not want visitors in their home. Even during a uniquely challenging time, the Child First intervention administered in a hybrid format demonstrated improved employment, residential stability, substance abuse, and service receipt outcomes when the program was examined at scale. The ability of Child First to quickly pivot services to meet the immediate needs of families is also promising. However, findings suggest that telehealth is a helpful mode of care coordination and not psychotherapy, given the lack of findings in key domains. The null effects on caregivers' psychological well-being, parenting stress, and children's behavior problems suggests that telehealth may not be able to generate the same benefits as in-person supports. Thus, further research—with larger samples—is necessary to determine whether clinical psychotherapy may require in-person connection with the family or major adaptations to successfully administer remotely.

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ABOUT MDRC

MDRC, A NONPROFIT, NONPARTISAN SOCIAL AND EDUCATION POLICY RESEARCH ORGANIZATION, IS COMMITTED TO finding solutions to some of the most difficult problems facing the nation. We aim to reduce poverty and bolster economic mobility; improve early child development, public education, and pathways from high school to college completion and careers; and reduce inequities in the criminal justice system. Our partners include public agencies and school systems, nonprofit and community-based organizations, private philanthropies, and others who are creating opportunity for individuals, families, and communities.

Founded in 1974, MDRC builds and applies evidence about changes in policy and practice that can improve the well-being of people who are economically disadvantaged. In service of this goal, we work alongside our programmatic partners and the people they serve to identify and design more effective and equitable approaches. We work with them to strengthen the impact of those approaches. And we work with them to evaluate policies or practices using the highest research standards. Our staff members have an unusual combination of research and organizational experience, with expertise in the latest qualitative and quantitative research methods, data science, behavioral science, culturally responsive practices, and collaborative design and program improvement processes. To disseminate what we learn, we actively engage with policymakers, practitioners, public and private funders, and others to apply the best evidence available to the decisions they are making.

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