The Center for
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The Impact of Youth Villages' Intercept® Program on Placement Prevention: A Second Look

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Introduction

Youth Villages' Intercept® program is an evidence-based in-home service with the goals of (1) preventing entry into out-of-home care, (2) reducing time to reunification for children in care, and (3) preventing re-entry into care for children who exited out-of-home care. In this study, we examine again whether the Intercept program has an impact on the likelihood of out-of-home placement for children who are at risk of placement because they were the subject of maltreatment investigation filed with Tennessee Department of Children's Services (DCS). The aim of this second study is to establish whether, across a different period, Intercept has the effect on placement rates we saw in the first study

In our previous study, we assessed the placement prevention impact of Intercept using administrative data from TN DCS including demographic, assessment, caseworker, and placement data. Children included in the study had their *first* contact with TN DCS through a Child Protective Services (CPS) report between 1/1/2013 and 6/30/2018 (Huhr & Wulczyn, 2019, 2020). The first study found that Intercept had statistically significant effects on reducing the likelihood of placement. In this report, we reassess the impact of Intercept using the DCS administrative data to examine a more recent period. Children included in this study were the subject of an initial maltreatment report between 7/1/2018 to 12/31/2020. Only children with their first reports in this period are included in the analysis. As such, *none* of the youth from our prior studies (neither the treatment group nor the comparison group) were included in the new study. Even though the data in this study cover a shorter observation period, this study seeks to determine whether the initial results can be replicated with a non-overlapping population drawn from a more recent timeframe.

As this second prevention study replicates the first study methodologically, we refer readers to the previous report where we describe in substantial detail how we approached the problem of baseline equivalence and the challenges associated with other confounds (Huhr & Wulczyn, 2019, 2020). In summary, we used the same independent and dependent variables, the same person-period data structure for censoring, the same multiple imputation methods for missing values, and the same exact matching strategy for establishing baseline equivalence. We also incorporated caseworker's propensity to refer children to Intercept and the random effects of counties in the statistical model to account for these two important confounds.

Data

Study Sample

Overview. The study uses administrative records within a quasi-experimental (QED) design as was used in the first study (Jorm et al., 2013). Administrative records were provided to us by DCS and Youth Villages. DCS provided data from the Tennessee Family and Child Tracking System (TFACTS), the state's administrative data system (i.e., Statewide Automated Child Welfare Information System [SACWIS]). Data extracted from TFACTS included (1) maltreatment investigations and related information including child-level characteristics, the report date, perpetrator type, the caseworker responsible for managing the case, the county where the child was living at the time of the investigation, and assessment or investigation track, (2) placement data that track if and when a young person enters care, and (3) data from the Family Advocacy and Support Tool (FAST) that is completed by DCS caseworkers as part of the CPS process (Lyons & Fernando, 2020). DCS caseworkers use the FAST assessment to document their concerns pertaining to family safety, financial resources, and conflict, among other domains before making referrals. From Youth Villages, we received Intercept encounter data that captures the date of referral and a stop date that indicates when services ended. These two data sets were linked using the TFACTS ID. After the link was completed, we organized the data around the timing of the investigation, dates of subsequent investigations, if any, start and stop dates of Intercept, and the date of placement (for those youth who were placed during the study period).

Sample Period. This study includes only children whose first CPS maltreatment investigation started after 7/1/2018. From the date of the initial CPS report (the start date), children were observed over the following time periods: (1) 900 days from the start date, (2) the start date to the censor date (12/31/2020), (3) the start date to the date the child turned 18 years old, or (4) the start date to placement into out-of-home care, whichever came first. As such, the observation period for any individual youth is at most 900 days from the CPS report date or less because of censoring, reaching age 18, or placement. Children with FAST data (94% of all children with an initial CPS report) were included in the analysis.

The Treatment Group. From the main study population (initial CPS reports filed between July 1, 2018, and December 31, 2020), children referred to Intercept were regarded as the treatment population. Whether a child was referred to Intercept was established using the linked administrative records received from Youth Villages and DCS. Regardless of their level of participation, all children referred were included as part of the treatment group in the analysis. Thus, the analysis is an intent-to-treat (ITT) design. Among children referred, some children were referred after placement; however, only children who were referred before their first placement were included in the prevention sample. Referrals to Intercept come from the Tennessee Department of Children's Services.

Dependent and Independent Variables

Our analysis considers the likelihood of placement into out-of-home care given a report of child abuse and/or neglect. We expect children referred to Intercept will have a lower likelihood of placement than similar children because of the Intercept intervention.

The independent variables are clustered into a set of child and family characteristics, a set that describes the maltreatment investigation, and a set of case characteristics that describes the risk profile of the child and the family derived from the Family Advocacy and Support Tool (FAST) assessment. The independent variables are used for the exact matching of comparison group members and are also used as covariates in the statistical model.

Other Confounds

Although the approach we used to match children in the treatment group with children in the comparison group provides a recognized solution for the problem of selection bias, there are other confounds that interfere with sound study designs based on administrative records (Brookhart et al., 2010). Among them, differences in administrative structure and differences in the way workers approach their job are especially important in the child welfare context. Courts, which operate at the county level in Tennessee, have considerable discretion regarding whether a child will be placed into out-of-home care. To control for any county variation in placement rates, we use the county random effects (i.e., a hierarchical model) to allow the treatment effect in the model to vary at the county level. The random effects capture unmeasured differences in county characteristics (Merlo et al., 2016). For this purpose, we used the county where the child was living at the time the investigation was started.

Caseworkers appear to have different decision-making thresholds relative to whether a child will be placed out of their home or referred to services while remaining in the home with their family (Baumann et al., 2011; Hollinshead et al., 2015). Because each TFACTS ID may be attached to multiple caseworker IDs (i.e., a child may have more than one assigned worker), we had to match children to the worker responsible for making decisions about service referrals. To capture caseworker referral tendencies, we developed a separate statistical model in which we connected a worker to the children assigned to them. Technically, the referral to Intercept (yes/no) served as the dependent variable in a separate random effects logistic regression model with child and other covariates included. From those models, we computed the Empirical Bayes (EB) residual that tells us the extent to which the worker's referral rate deviates from the adjusted average referral rate. The adjustment for worker referral rate differences controls for unmeasured case characteristics that workers observe that may contribute to why some children are referred to Intercept and others are not. When the worker EB residuals are added as covariates to the county random effects model, both sources of heterogeneity are accounted for in the impact analysis.

Methods

The study design addresses a set of interrelated challenges associated with the use of observational data in studies of intervention effects. Our first task addressed the matching process used to establish baseline equivalence. The second issue relates to the problem of censoring because the opportunity to observe outcomes within the study sample varies by when the initial CPS report is filed within the study timeframe. Finally, we had to contend with the cross-classification of case workers and counties. Without careful controls for the county context, there may be otherwise unobserved influences that confound our interpretation of the outcome. Our solutions to each of these issues are described in detail in our prior report (Huhr & Wulczyn, 2019) and in brief here.

Exact Matching

In this study, because we have a large number of children in the control group compared to the treatment group, we used exact matching. As opposed to propensity score matching, exact matching includes only exactly matched children, a feature that reduces bias and renders a distance weight unnecessary. Exact matching uses all matched children (one-to-many matchings) which increases efficiency without causing bias (Shadish et al., 2008; Stuart, 2010; Stuart et al., 2013).

Table 1 shows how many children were included in the study before and after exact matching. Ninety-one percent of the children referred to Intercept were included in the final analysis sample as were 59 percent of the children who were not referred to Intercept.

Table 1: Sample Size Before and After Exact Matching

	Before Exact Matching	After Exact Matching	Matching proportion
Intercept Group	2,053	1,873	91%
Comparison Group	134,106	79,205	59%

Baseline Equivalence

The goal of matching is to establish baseline equivalence. Baseline equivalence refers to the extent to which the treatment and control groups are equivalent or balanced. According to the Title IV-E Prevention Services Clearinghouse Handbook of Standards and Procedures (Wilson et al., 2019), a direct pre-test outcome variable must be used to assess baseline equivalence. Alternatively, if using direct pre-test data is not feasible, or a suitable pre-test alternative is not available, baseline equivalence must be established on both race/ethnicity and socioeconomic status (SES). The Handbook also requires baseline equivalence to be demonstrated on age for studies of programs for children and youth.

Because none of the included children experienced placement prior to their study inclusion, direct pre-test data are not available. Alternative pre-test data also do not exist. As a result, baseline equivalence was established using exact matching with the covariates described in the data section: gender, race/ethnicity, age, perpetrator type, family finance, family safety, sexual/physical abuse, emotional abuse, neglect, education, and developmental/mental health/substance use (see Table 2). Regarding SES, family finance from the FAST was used;

family finance refers to whether income and other sources of money available to family members (particularly caregivers) were a concern at the time of assessment. Family safety refers to whether the family home environment/neighborhood is safe or poses danger or risk (Lyons & Fernando, 2020).

Composition of the pre- and post-match treatment and comparison groups is shown in Table 2. Before exact matching, older children (11 to 13 age group and 14 to 17 age group) were more likely to be referred to Intercept (3.3% and 3.7%, respectively) than younger children (1.1% for 6 to 10 age group and 0.2% for 0 to 5 age group). Male and female children were similar in the percent referred to the program (1.2% for male and 1.3% for female). Regarding race and ethnicity, referral to Intercept was the highest among African American children (3.2%) followed by White children (2.7%) and children of other races and ethnicities (2.0%). Additionally, children whose race/ethnicity was listed as unknown (in contrast to missing) were the least likely young people to be referred to (0.9%) whereas children whose race/ethnicity was missing were the young people with the highest referral rate (8.7%). Issues concerning missing and unknown data are addressed in the section on multiple imputation.

Children with parents as perpetrators are less likely to be referred to Intercept (1.1% for parent and 2.5% for other). Children with assessments resulting in services being required and/or accepted (ASR/ASA) were more likely to be referred to (2.4% versus 1.3%) and children with substantiated investigations were less likely to be referred to (1.2% versus 1.6%).

Regarding the FAST assessment, children referred to Intercept had a higher percentage of family finance and family safety issues than members of the comparison group (1.8% versus 1.5% and 2.0% versus 1.4%, respectively). Further, referral rates were also different between the treatment and comparison groups based on whether youth had issues related to sexual and physical abuse (2.9% versus 1.2%), emotional abuse (4.5% versus 1.3%), neglect (1.9% versus 1.5%), education (5.9% versus 1.1%), and developmental/mental health/substance use concerns (5.3% versus 0.7%). As the comparison of the FAST assessment indicates, the treatment group had more challenging and riskier individual and family circumstances, a fact that likely contributed to the higher baseline placement rate (see Table 4).

After exact matching, the final sample consisted of 1,873 children in the Intercept treatment group and 79,205 comparison children (see Table 2). The weighted percentages for the comparison group are identical to the percentages for the treatment group due to exact matching. Even though the unweighted percentages of the two groups look different in unidimensional, line-by-line comparisons, only identical children (i.e., matched on all covariates) were included following the multi-dimensional exact matching. As such, weighted percentages are identical in both groups as shown in the last column of Table 2.

Table 2: Composition of the Study Sample Before and After Matching

	Before Matching					After Matching				
	Treatment		Comparison			Treatment		Comparison		
Characteristic	Number	Percent	Number	Percent	Referred - Tx. Group	Number	Percent	Number	Percent	Percent w/ weight
Age										
0 to 5	120	5.8%	54,342	40.5%	0.2%	109	5.8%	32,835	41.5%	5.8%
6 to 10	350	17.0%	31,179	23.2%	1.1%	319	17.0%	19,746	24.9%	17.0%
11 to 13	594	28.9%	18,263	13.6%	3.3%	521	27.8%	11,536	14.6%	27.8%
14 to 17	767	37.4%	20,492	15.3%	3.7%	702	37.5%	12,540	15.8%	37.5%
Missing	222	10.8%	9,830	7.3%	2.3%	222	11.9%	2,548	3.2%	11.9%
Gender										
Male	726	35.4%	62,694	46.7%	1.2%	637	34.0%	36,593	46.2%	34.0%
Female	853	41.5%	64,424	48.0%	1.3%	767	41.0%	38,336	48.4%	41.0%
Missing	474	23.1%	6,988	5.2%	6.8%	469	25.0%	4,276	5.4%	25.0%
Race/Ethnicity										
African American	177	8.6%	5,450	4.1%	3.2%	146	7.8%	1,949	2.5%	7.8%
White	417	20.3%	15,466	11.5%	2.7%	358	19.1%	5,241	6.6%	19.1%
Other	90	4.4%	4,568	3.4%	2.0%	57	3.0%	831	1.0%	3.0%
Unknown	899	43.8%	103,197	77.0%	0.9%	847	45.2%	67,679	85.4%	45.2%
Missing	470	22.9%	5,425	4.0%	8.7%	465	24.8%	3,505	4.4%	24.8%
Perpetrator										
Parent	982	47.8%	91,442	68.2%	1.1%	892	47.6%	55,863	70.5%	47.6%
Other	1,071	52.2%	42,664	31.8%	2.5%	981	52.4%	23,342	29.5%	52.4%
Assessment: ASR/ASA										
No	1,328	64.7%	104,463	77.9%	1.3%	1,236	66.0%	63,572	80.3%	66.0%
Yes	725	35.3%	29,643	22.1%	2.4%	637	34.0%	15,633	19.7%	34.0%
Investigation: Substantiated										
No	1,914	93.2%	122,588	91.4%	1.6%	1,770	94.5%	76,397	96.5%	94.5%
Yes	139	6.8%	11,518	8.6%	1.2%	103	5.5%	2,808	3.5%	5.5%
Family Finance										
No	1,694	82.5%	113,935	85.0%	1.5%	1,601	85.5%	74,062	93.5%	85.5%
Yes	359	17.5%	20,171	15.0%	1.8%	272	14.5%	5,143	6.5%	14.5%
Family Safety										
No	1,564	76.2%	110,211	82.2%	1.4%	1,473	78.6%	74,735	94.4%	78.6%
Yes	489	23.8%	23,895	17.8%	2.0%	400	21.4%	4,470	5.6%	21.4%
Sexual/Physical Abuse										
No	1,324	64.5%	109,215	81.4%	1.2%	1,256	67.1%	68,330	86.3%	67.1%
Yes	729	35.5%	24,891	18.6%	2.9%	617	32.9%	10,875	13.7%	32.9%
Emotional Abuse										
No	1,662	81.0%	125,401	93.5%	1.3%	1,562	83.4%	77,728	98.1%	83.4%
Yes	391	19.0%	8,705	6.5%	4.5%	311	16.6%	1,477	1.9%	16.6%
Neglect										
No	1,670	81.3%	113,584	84.7%	1.5%	1,572	83.9%	74,915	94.6%	83.9%
Yes	383	18.7%	20,522	15.3%	1.9%	301	16.1%	4,290	5.4%	16.1%
Education										
No	1337	65.1%	121,892	90.9%	1.1%	1,253	66.9%	74,640	94.2%	66.9%
Yes	716	34.9%	12,214	9.1%	5.9%	620	33.1%	4,565	5.8%	33.1%
General Well-being*										
No	810	39.5%	110,704	82.5%	0.7%	768	41.0%	67,699	85.5%	41.0%
Yes	1,243	60.5%	23,402	17.5%	5.3%	1,105	59.0%	11,506	14.5%	59.0%
Total	2,053	100.0%	134,106	100.0%	1.5%	1,873	100.0%	79,205	100.0%	100.0%

* General Well-being refers to whether developmental delays, mental health, or substance use issues were identified as part of the FAST assessment.

Censoring

Even if we have matched samples, the placement outcome also depends on how much time has passed after the initial assessment or investigation. The time from the investigation date until the stop date was divided into three-month intervals with one record per interval of time through the end of the observation window. For this research, three-month periods (90 days) were used as shown in Table 3 because the observation period is shorter than the first study. P-1 stands for the first three months, P-2 stands for the next three-month interval, and so on. Person-periods were assessed until 900 days (P-10) at the maximum. Constructed this way, the approach allows us to use as much of the available data as possible without introducing a truncation bias (DiPrete & Forristal, 1994; Reardon et al., 2002; Singer & Willet, 1993). The discrete-hazard model (DTHM) was employed based on this person-period data structure.

Table 3: Person-Periods by Treatment / Comparison Group

Interval	Person-Period	Final Treatment	Final Comparison
1-90 days	P-1	1,873	79,205
91-180 days	P-2	1,616	70,926
181-270 days	P-3	1,465	63,643
271-360 days	P-4	1,341	57,725
361-450 days	P-5	1,189	50,194
451-540 days	P-6	1046	43,060
541-630 days	P-7	857	34,833
631-720 days	P-8	674	27,232
721-810 days	P-9	457	18,616
811-900 days	P-10	246	10,440

All children are included in first person-period (P-1) with the number of children decreasing in subsequent person-periods as children enter placement, reach age 18, or the observation period ends. If the length of observation from start date until the end of observation (the child leaves care, reaches maturity, or the window of observation closes – i.e., the observation is censored) is less than 90 days, then that child has one person-period record (P-1). If more than 90 days but less than 181 days elapse, then only two person-periods are available (P-1 and P-2) for that child. If more than 811 days passed since the initial investigation without the child entering placement or turning 18, then the record for that child contains 10 person-period records (P-1 through P-10). By way of example, if a child is placed at 150 days, the outcome for P-1 is coded as zero and the outcome for P-2 is coded as one, indicating that placement occurred during this person-period. As such, until a child is placed, the outcomes for all prior person-periods are coded as zero. The placement outcome becomes a person-period specific outcome, which means we are measuring the likelihood of placement during specific intervals.

Multiple Imputations

As shown in Table 2, there are missing data for race/ethnicity, age, and gender. In our previous report, missing demographic data were less common. With this more recent sample, missing data is more common for two reasons related to the time needed to first observe and then record case information. Because this study spans the period between July 1, 2018, and December 31, 2020, and the data used for the study were pulled in early 2021, children whose CPS report came into DCS toward the end of the period were more likely to have missing data. This reflects the fact that case record information is updated as information becomes known and as workers enter more data into the record. It is also important to note that workers may select 'Unknown' for race/ethnicity, or they may decline to record a choice.

To address the missing data, we used multiply imputed data (20 imputations here) for unknown race/ethnicity, missing race/ethnicity, age, and gender using SAS PROC MI. We used a fully conditional specification (FCS) method that assumes the existence of a joint distribution for all variables (The MI procedure, SAS Institute). Within FCS, the regression is for continuous variables and thus we used the discriminant function for the nominal categorical race variable. The parameter estimates from each data set were combined using SAS PROC MIANALYZE. For the category of unknown race, we imputed values for African American, White, and Other. However, to preserve the unique information linked to children whose race was originally listed as unknown, we retained the unknown dummy variable in the models even after imputation. We also included in the final models a dummy variable for age (1 = missing), again to preserve any unique information linked to children whose age was missing. In a separate series of models, we did include the other dummy variables (i.e., missing age, gender, and race/ethnicity), but those models failed to converge because missing gender, age, and race/ethnicity are highly correlated. Models with and without multiple imputations are show in Table 5.

Random Effects Model

The between-county differences pose important substantive and statistical concerns. For example, because counties operate in slightly different ways (e.g., the courts in Tennessee are organized at the county level), children with similar characteristics may have different placement rates because of administrative operating differences. Statistically, when the data are clustered in this way, standard errors are affected, which in turn affects the statistical significance of the parameter estimates. The random effects model applied to the DTHM provides a convenient way to manage this issue. The random effects model allows parameter estimates to vary (i.e., county differences in placement rates) and adjusts the standard errors accordingly.

Results

Descriptive Statistics

Within the 30-month observation period (7/1/18 – 12/31/20), 136,159 children were reported to DCS for the first time. Of those, 2,053 children (1.5%) were referred to Intercept; the remaining 134,106 children make up the potential comparison group. Among those children (prior to matching), the number placed from the treatment group and the potential comparison group was 183 (8.9%) and 7,948 (5.9%), respectively (see Table 4).

Table 4: Sample Size and Placement before Matching

	Total Children	Number of Children Placed	Percent Placed (Rate)
Comparison	134,106	7,948	5.9%
Intercept	2,053	183	8.9%

Average Treatment Effect

The results of the random effects model are found in Table 5, which displays model coefficients and their standard errors, p-values, and odds ratios (O.R.) for selected models. Odds ratios greater than one are associated with an increased likelihood of placement in out-of-home care. Odds ratios smaller than one are associated with a lower likelihood of placement.

Estimates from four different models are shown. Across all the models, the estimate of interest is the Intercept parameter (labeled as *Intercept Tx. Effect* in Table 5), which represents the treatment effect. Model 1 (all 2,053 treated and 134,106 comparison children) shows the parameter estimates before exact matching was employed. Models 2, 3 and 4 are based on exact matching (1,873 treated and 79,205 comparison children). Models 3 and 4 show the results without and with imputation, respectively. The *Intercept Tx. Effect* row in Table 5 measures the Intercept program's overall impact (i.e., the average treatment effect), which assumes that the baseline hazard rates for person-periods are all equal.

Regardless of model type, demographic characteristics of race/ethnicity are associated with statistically significant effects. African American children are more likely to be placed than White children (reference category). Children of other races/ethnicities (which includes Hispanics and Asians) have similar placement rates when compared with White children. The race Unknown category, which is different than missing race, was used as a dummy variable together with African American, White, and Other. Children for whom race/ethnicity was listed as unknown were less likely to be placed.

To determine age effects, infants, toddlers, and preschoolers (0 through 5 years old) serve as the reference category, which means the rate of placement for other ages is evaluated relative to the rate at which infants, toddlers, and preschoolers are placed. Except for children whose age was missing, babies, toddlers, and preschoolers are more likely to be placed than other, older children). Male children were more likely to be placed.

Regarding perpetrator type and assessment/investigation type, children whose parent(s) were the perpetrator were slightly more likely to be placed. Children whose assessment outcome was ASR or ASA (services are required or accepted), in contrast to other assessment outcomes, show a higher likelihood of placement. Also, children whose investigation was substantiated show a higher likelihood of placement. These findings are in the expected direction.

The caseworker's Empirical Bayes (EB) residual measures the worker's propensity to refer children to Intercept services as compared to the Intercept referral rate as observed for all workers in a county. The statistical significance of this parameter estimate suggests that a caseworker's tendency to refer to the Intercept program is associated with placement outcomes. The EB parameter detects the linear trend, whereas the EB * EB parameter says the relationship between a worker's referral propensity and placement is a bit more complicated. As a general matter, why worker referral tendencies are correlated with outcomes is a matter of considerable significance to the child welfare field, but beyond the scope of this analysis. Here, we are interested in the worker tendencies as source of worker differences and unmeasured aspects of the case. Adjusting the treatment effect with estimates of the worker's influence on the process improves the treatment effect estimate.

Regarding the Intercept treatment effect, the treatment parameter indicates that Intercept lowered placement rates among the children who were referred to the program as compared to a similar group of children who were not referred. The Intercept treatment effects are similar in Models 1, 2, 3, and 4 (a little lower in Model 3 and Model 4) and all of them are statistically significant. Compared to Model 1, Model 2 incorporates exact matching as the basis for constructing the comparison group; Models 3 and 4 both include the person-periods in addition to exact matching; Model 4 includes imputations. The comparison between Model 3 and Model 4 shows an almost identical treatment effect (both 0.63 odds-ratio), which indicates the imputations due to gender, age, and race/ethnicity missingness do not destabilize the treatment effect estimate. Based on the Model 4, after exact matching and imputations, and including person-periods, the odds of placement among treatment group members is 37 percent lower than those in the comparison group.

Table 5: Treatment Effect Estimates

	Model 1			Model 2			Model 3				Model 4			
	Before exact matching			After exact matching			After exact matching w/ Person-Periods (no imputation)				After exact matching w/ Person-Periods and Imputations			
Effect	Estimate	s.e.	Pr > |t|	Estimate	s.e.	Pr > |t|	Estimate	s.e.	Pr > |t|	O.R.	Estimate	s.e.	Pr > |t|	O.R.
Model Intercept	-2.705	0.082	<.0001	-2.747	0.140	<.0001								
Intercept Tx. Effect	-0.687	0.093	<.0001	-0.631	0.105	<.0001	-0.459	0.094	<.0001	0.63	-0.469	0.096	<.0001	0.63
Person-Periods														
P-1							-3.228	0.132	<.0001		-3.629	0.131	<.0001	
P-2							-4.953	0.146	<.0001		-5.346	0.145	<.0001	
P-3							-5.155	0.151	<.0001		-5.548	0.150	<.0001	
P-4							-5.429	0.159	<.0001		-5.822	0.158	<.0001	
P-5							-5.476	0.164	<.0001		-5.870	0.162	<.0001	
P-6							-5.924	0.184	<.0001		-6.318	0.182	<.0001	
P-7							-6.036	0.196	<.0001		-6.430	0.194	<.0001	
P-8							-5.774	0.193	<.0001		-6.168	0.192	<.0001	
P-9							-6.102	0.234	<.0001		-6.499	0.233	<.0001	
P-10							-6.362	0.319	<.0001		-6.761	0.318	<.0001	
Age														
0 to 5	Reference													
6 to 10	-0.951	0.049	<.0001	-0.965	0.112	<.0001	-0.956	0.105	<.0001	0.38	-0.679	0.112	<.0001	0.51
11 to 13	-1.209	0.053	<.0001	-0.899	0.113	<.0001	-0.869	0.106	<.0001	0.42	-0.561	0.103	<.0001	0.57
14 to 17	-1.046	0.048	<.0001	-0.643	0.106	<.0001	-0.608	0.098	<.0001	0.54	-0.204	0.102	0.049	0.82
Missing	1.096	0.058	<.0001	1.470	0.124	<.0001	1.401	0.113	<.0001	4.06	2.031	0.076	<.0001	7.62
Gender														
Females	Reference													
Male	0.198	0.034	<.0001	0.400	0.071	<.0001	0.379	0.067	<.0001	1.46	0.338	0.080	0.000	1.40
Race/ Ethnicity														
Whites	Reference													
African Am.	0.188	0.055	0.001	0.212	0.111	0.057	0.183	0.105	0.083	1.20	0.380	0.100	0.000	1.46
Other	-0.288	0.053	<.0001	-0.147	0.141	0.300	-0.142	0.134	0.289	0.87	-0.012	0.137	0.928	0.99
Unknown	-4.246	0.053	<.0001	-3.821	0.099	<.0001	-3.687	0.093	<.0001	0.03	-3.510	0.084	<.0001	0.03
Perpetrator														
Other perpetrators	Reference													
Parent	0.237	0.040	<.0001	0.125	0.085	0.143	0.160	0.081	0.048	1.17	0.247	0.081	0.002	1.28
Assessment														
Other	Reference													
ASR or ASA	1.535	0.042	<.0001	1.519	0.078	<.0001	1.457	0.075	<.0001	4.29	1.434	0.075	<.0001	4.20

Effect	Model 1 Before exact matching			Model 2 After exact matching			Model 3 After exact matching w/ Person-Periods (no imputation)				Model 4 After exact matching w/ Person-Periods and Imputations			
	Estimate	s.e.	Pr > |t|	Estimate	s.e.	Pr > |t|	Estimate	s.e.	Pr > |t|	O.R.	Estimate	s.e.	Pr > |t|	O.R.
Investigation														
Unsubstantiated	Reference													
Substantiated	2.875	0.047	<.0001	2.304	0.116	<.0001	2.212	0.107	<.0001	9.14	2.302	0.107	<.0001	10.00
FAST														
Finance	0.622	0.035	<.0001	0.648	0.080	<.0001	0.504	0.067	<.0001	1.65	0.529	0.070	<.0001	1.70
Safety	0.422	0.035	<.0001	0.571	0.076	<.0001	0.479	0.064	<.0001	1.61	0.530	0.066	<.0001	1.70
Sexual/Physical	-0.055	0.039	0.162	-0.029	0.073	0.689	-0.017	0.063	0.788	0.98	-0.026	0.065	0.692	0.97
Emotional	0.141	0.049	0.004	0.141	0.108	0.189	0.080	0.090	0.371	1.08	0.049	0.093	0.599	1.05
Neglect	0.475	0.035	<.0001	0.450	0.082	<.0001	0.379	0.070	<.0001	1.46	0.412	0.072	<.0001	1.51
Education	0.391	0.043	<.0001	0.144	0.071	0.044	0.036	0.062	0.559	1.04	0.013	0.064	0.838	1.01
General Well-being*	0.215	0.037	<.0001	-0.058	0.065	0.372	-0.095	0.057	0.099	0.91	-0.088	0.062	0.157	0.92
Caseworker														
EB Residual	-0.056	0.043	0.192	-0.042	0.074	0.574	-0.018	0.065	0.785		-0.016	0.066	0.811	
EB*EB	0.014	0.042	0.743	-0.207	0.072	0.004	-0.131	0.063	0.039		-0.134	0.064	0.036	

* General Well-being refers to whether developmental delays, mental health, or substance use issues were identified as part of the FAST assessment.

Conclusion

This is the second study of placement prevention that asks whether Intercept reduces placement in out-of-home care within the context of an at-scale, statewide implementation.[1] In both cases, the answer has been yes. The first study considered children at risk of entering foster care for the first time by virtue of having been the subject of a CPS report filed between January 1, 2013, and June 30, 2018. To control for the confound of case history, the population selected consisted of all children reported for the first time during that period. We split those children into two groups: the treatment group and an exactly matched comparison group. The treatment group was made up of children referred to the Intercept program. The odds of placement were significantly lower in the treatment group.

As before, with this second study, we set out to contribute to the evidence base for in-home services as an important, effective component of the child welfare service array. Again, working together with the state of Tennessee and Youth Villages, a social-sector agency based in Memphis, Tennessee, we were asked to determine whether the Intercept program, provided by Youth Villages in Tennessee, had a measurable impact on placement rates during a more recent timeframe.

To answer that question, we linked data from DCS with data from Youth Villages (Jorm et al., 2013). By most standards, when put together at the child-level, the two data sets provided us with an unparalleled opportunity to study the at-scale delivery of a particular service in the context of state-funded child protective services. Nevertheless, it is an observational study. To address non-random assignment to the treatment condition, we exactly matched the treatment and control groups on child and family characteristics, then replicated two important methodological innovations used in the first study: a control for county random effects and a control for worker referral patterns. Together with the exact matching we used, the controls for county and worker variation place similar families and the decisions affecting them in a similar context. When compared with previous evaluations of intensive home-based services, few studies have placed as much emphasis on the context in which decisions are made, even though context is an important element of decision-making (Hollinshead et al., 2015). The county effect picks up the idiosyncratic practices found in local child welfare offices and court systems; the worker effect does the same thing at the caseworker-level

In the current study we found that the Intercept program had its intended effect. Using an ITT design, children who were referred had a significantly lower placement rate than similar children who were not referred. The first and second study effect sizes were of comparable magnitude (.47 and .63, respectively).

What do the Intercept findings mean more broadly? Generally, the empirical news is good. A systematic review of *randomized control studies* by Bezeczky and colleagues (2020) found positive results for intensive family-based programs, generally speaking. To that conclusion, we can add the results from the studies of Intercept. In our experience, this sort of convergence in studies with both internal and external validity in child welfare outcomes is

[1] There have been 3 studies of Intercept. There are the two prevention studies: the original and this second prevention study. The third study, which was carried out between the two permanency studies, focused on whether Intercept has a positive impact on permanency rates among children who are placed. Children in the permanency study were placed in out-of-home care and then referred to Intercept (or not). Because the data available to us once a child is in placement, the control variables are somewhat different. Otherwise, we largely replicated the original permanency study in form: we compared the permanency rates for the Intercept treatment group and an exactly matched comparison group, all with the same attention to county and worker confounds. The findings show that Intercept-referred children achieved permanency at rates that significantly exceeded those of the comparison group.

both rare and welcome.

Across the two studies of placement prevention, we looked at *every* child investigated for maltreatment between 2013 and 2020. We know when the investigation started from a life course perspective (Wulczyn, 2020). Some of those children were referred to Intercept. We know when that happened relative to the start of the investigation. We also know who was placed, their level of risk, and how the risk of placement was influenced by Intercept alongside the contribution of place (the county where the work was being done) and worker (who was managing the case). From out of the middle of all of that, we demonstrated a statistically significant effect consistent with the programmatic intent.

Next to studies with stronger internal validity (Bezeczky et al., 2020), the findings here suggest that Intercept represents a good public investment provided the investment is well-targeted. Given that rates of placement vary between and within states, the expected return on investment, measured as fewer children needing foster care, will likely vary unless implementation includes identification of the areas that are most likely to benefit from an effective program that serves children at high risk of placement. The evidence suggests that when implemented with fidelity, prevention programs located in areas where the risk of entry is high are more likely to reduce placements than the same programs brought to scale elsewhere. Wise investors make such cost calculations routinely (Dowrick et al., 1998; Olsen, 1997; Ward & Holmes, 2008); those investing in prevention programs in the child welfare system now have better information on which make these important decisions.

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