The Center for

State Child Welfare Data

Revisiting the Impact of Youth Villages' Intercept® Program on Permanency Outcomes

Scott Huhr and Fred Wulczyn

February 2021

CHAPIN HALL
AT THE UNIVERSITY OF CHICAGO

1313 E. 60th Street
Chicago, IL 60637
(773) 256.5100
fcda.chapinhall.org

Table of Contents

Introduction

In this third study of Intercept, we examine the impact of Intercept® on whether a child placed in out-of-home care achieves permanency. In the two previous studies, we assessed the impact of Youth Villages' Intercept® program on important child welfare outcomes including prevention of placement in state custody and exit to permanency from state custody placements (Huhr & Wulczyn, 2019, 2020). The first study examined the impact of Intercept on placement prevention; the second study considered whether Intercept had an effect on permanency rates. In both studies, we found that Intercept had significant effects in the intended direction. Placement rates among the Intercept treated group were lower and permanency rates among young people placed in out-of-home care were higher.

Compared to the prior permanency study, this study considers only those young people who had no prior pre-placement exposure to Intercept. That is, among children served in the prevention program, there was a small number of children who, upon placement, were referred again to Intercept. In the first permanency study, we included young people who were referred to Intercept prior to their initial placement. In this study, young people referred to Intercept prior to placement were excluded.

As this new study replicates the two prior studies methodologically, we refer readers to the references 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). We also describe the statistical framework we used to manage the problem of censored observations. With specific reference to the permanency study, we followed identical procedures: exact matching, definitions of independent and dependent variables, multiple imputation of missing values, and statistical modeling. Given the foregoing, we present these findings in reduced form. We start with a program description and then describe the study population, the independent/dependent variables, the methods, and the results.

Program Background and Implementation

The overarching goal of the Intercept program is to reduce the utilization of out-of-home care by preventing entry into care, reduce the time spent in care, or reduce the risk of re-entry, depending on the child's circumstances and the timing of the Intercept referral. If the child has yet to be in out-of-home care, preventing placement is the goal. If the child is in foster care when referred to Intercept, the goal is permanency. If the child has a history of placement but was not in placement at the time of referral, the goal is preventing a return to out-of-home care.

The Tennessee Department of Children's Services (DCS) allocates Intercept slots to counties based on trends in the number of children in custody at the county level. Only children who have permanency goals of reunification, guardianship, or exit to relatives are considered appropriate for the program. There are two referral pathways. Youth who are in placement with Youth Villages, either in residential treatment, a group home, or foster home, are referred to Intercept by DCS staff or by Youth Villages' staff in the residential or foster care program. Once a youth is referred, Intercept staff contact the family to begin Intercept services. For youth in out-of-home placements with other agencies, DCS submits referrals for Intercept by phone, email, or online to Youth Villages' placement staff.

Regarding the program itself, Intercept employs Bachelor's- and Master's-level family intervention specialists who are trained to engage families. Key features of the program include program intensity (meeting with families an average of three times weekly), low staff caseloads of four to five families, active 24/7 on-call structure, and structured weekly supervision and consultation from a licensed clinician who is an expert in the Intercept treatment model. Program fidelity is monitored in each location through a comprehensive process that includes case review, family and staff surveys, and examination of operational data. Every month program leadership reviews key performance indicators, including clinical and operational metrics. Post-discharge outcomes are gathered from youth and families for one year.

At its core, Intercept draws on a diverse set of evidence-based and research-informed interventions. In keeping with the use of evidence- and research-informed programs, Intercept is manualized. There are specific principles that undergird the Intercept intervention and, in accordance with best practices, model adherence is measured routinely using information derived from multiple sources including youth, parents, staff, program leadership, case record review, and an assessment of long-term outcomes.[1]

Study Sample

Our sample universe encompasses Tennessee children who were placed in out-of-home care for the very first time between 1/1/2013 and 6/30/2018. There were 24,596 young people in that group. From that universe of children, we identified a study sample as follows.

To begin, we had to identify eligible children based on when their placement began. For each child placed during this window, the data provided by DCS contains the date when the very *first* placement in out-of-home care started (the placement start date). Given how the system is structured in Tennessee, a child will be placed in a DCS-supervised foster home or in a placement setting supervised by a social sector (private) agency. Placements with social sector agencies may include a foster home, group home, or residential treatment. By definition, the date of initial placement also marks the start of that child's time with the initial agency, whether that agency is DCS or one of the social sector agencies with which DCS contracts.

To manage the movement of children between agencies and movement into and out of care, we constructed two types of placement spells. The *child spell* is based on the date of entry into out-of-home care and exit from out-of-home care. When the child spell ends, the child is no longer in the legal custody of DCS. A child may have one or more unique child spells. The *agency spell* marks the time when a child is placed with a specific agency, whether that placement is with a DCS home or a placement supervised by a social sector agency. An initial agency spell always begins when a child spell begins. When an agency spells ends, the child spell may or may not end. When the child spell ends, the then-current agency spell also ends. There may be one or more agency spells within a child spell. If a child changes placement but does not change the agency supervising the placement (e.g., moves from one foster home to another foster home within an agency), these are referred to as intra-agency moves; the agency spell will capture all time spent in any placement supervised by a particular agency. Inter-agency moves refer to transfers between separate and distinct social sector agencies and are captured as separate agency spells.

The agency spell may end in one of several ways. Permanent exits (reunification, adoption, exit to relatives, and guardianship) are the typical pathways out of placement; non-permanent exits include reaching the age of 18 without having left care, runaway, or transfer. When the child leaves care through a permanent exit, the agency spell (the time in care with a specific agency) ends simultaneously with the child spell. In the case of transfers, a child will have at least one additional agency spell within that child spell.

With the placement history established, we excluded children and youth from the sample universe based on the following reasons:

[1] Additional information about the program is available at: www.youthvillages.org/intercept

1) Children under the age of 5. By DCS regulation, children under the age of 5 are not assessed using the Child and Adolescent Needs and Strengths assessment (CANS). Because the CANS was used in our approach to exact matching, we dropped these young people from consideration.

2) Children and youth who were placed initially in detention, hospitals, or emergency shelters were also excluded.

3) Children and youth who were discharged from care within 60 days were dropped from the study sample. Placements that lasted less than 60 days were excluded from the study because:

    i) a majority of inter-agency transfers happened within 60 days of the entry into care
    ii) children who exited permanently within 60 days were highly likely to exit without having been referred to services, and
    iii) the average length of Intercept service was 120 days and therefore at least 60 days are required to implement basic components of the program.

4) All children and youth who were referred to Intercept before placement were *excluded*.

The Treatment Group. From the residual group of 7,087 children, after the exclusions were applied, an Intercept referral was identified using linked administrative records received from Youth Villages and DCS. Following standard referral processes (as described), the Intercept treatment group includes children who were referred to the Intercept program during the first agency spell embedded within the first child spell.

Regardless of their level of program participation, all children who were referred to Intercept were included as part of the treatment group in the analysis. Thus, the analysis is an intent-to-treat (ITT) design. As already noted, given the referral and encounter data provided to us, we were able to identify Intercept referrals made during the first agency spell within the first child spell. Other referrals, during either a second or third child spell or during a second or third agency spell within the initial child spell, were dropped from this analysis.

Dependent Variable

Children in foster care typically leave placement by following one of several pathways. Reunification, adoption, exit to relatives, and guardianship are referred to as exits to permanency. Children may also age-out or runaway or they may be transferred to another child serving system (e.g., juvenile justice). Among those exit reasons, we focus our analysis on the likelihood of exit to permanency from the first agency spell. We expect children who are referred to Intercept will have a higher likelihood of exiting to permanency than similar children because of the services offered through Intercept.

As an outcome, permanency can be measured in one of two ways. First, the probability of permanency measures the likelihood of permanency, i.e., for every 100 children admitted what proportion exits to permanency. Interventions often seek to increase the probability of permanency. Second, permanency takes time and some interventions seek to decrease the time required to accomplish permanency (i.e., timeliness or the average time to permanency). It is possible to address the probability of permanency without addressing timeliness and vice versa. We manage this issue by using a discrete time hazard model (DTHM). The DTHM considers the likelihood of leaving care per unit of time, with time divided into intervals. The likelihood of exit to permanency is captured by considering whether a young person exits to permanency during the next interval of time, given they were in care at the start of the interval. Timeliness is accommodated by tracking how soon after placement an exit to permanency occurs.

There were two other consideration to bear in mind when assessing the impact of Intercept on permanency. First, a referral to Intercept can happen at any time after the start of the initial agency spell. The referral times are important because the likelihood of permanency itself varies with the passage of time. However, we did not differentiate and measure early referrals and later referrals separately in the impact analysis, but rather measured the average treatment effect regardless of referral dates. Second, we measured rates of permanency separately, depending on time intervals because we assume that the rates of permanency change as time passes after entry into care.

Exits from each interval involved either permanency or some other exit reason. Only the children who did not leave care in the prior interval (i.e., they are still in care) were included in the risk set at the start of the next interval. Structured this way the results provide us with the probability of exit to permanency per unit of time (i.e., the interval).

To summarize, starting from the date of the initial placement (the start date of the child spell), children were observed over the following time periods: (1) 900 days from the start date, (2) the start date to censor date (6/30/2018), (3) the start date to the date the child turned 18 years old, or (4) the start date to end the first agency spell, whichever came first. As such, the observation period for any individual youth is 900 days from placement or less (due to either censoring, reaching age 18, or leaving the first agency spell for reasons having to do with permanency, transfer, or some other exit reason).

Independent Variables

The independent variables are clustered into two basic categories: a set that describes demographics such as age, race, and gender and a second category that summarizes the clinical characteristics of the child and their family. For the latter, the Child and Adolescent Needs and Strengths (CANS) assessment was included. We used these variables to carry out the exact match (our approach to constructing the comparison group) and as covariates in the statistical model.

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. To identify a suitable comparison group, we followed closely the recommendations of the Title IV-E Prevention Services Clearinghouse Handbook for establishing a matched comparison group (Wilson et al., 2019). The second issue relates to the problem of censoring. The opportunity to observe outcomes within the study sample varies by admission date. Finally, we had to contend with the cross-classification of case workers and counties. Without careful controls for the agency and 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 our prior reports.

Approach to 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. Compared to other matching methods, exact matching has some important advantages. As opposed to propensity score matching, exact matching includes only exactly matched children, a feature of the approach 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; E A Stuart et al., 2013; Elizabeth A Stuart, 2010).

Table 1 shows how many children were included in the study after exact matching. Ninety percent of the children referred to Intercept were included in the final analysis sample as were 52 percent of the children who were not referred to Intercept. As such, 10 percent of the children in the treatment group did not have a comparison group counterpart and 48 percent of children in the comparison group were not matched to any treated child.

Table 1: Sample Size Before and After Exact Matching

	Before Exact Matching	After Exact Matching	Matching proportion
Intercept Group	331	299	90%
Comparison Group	6,756	3,528	52%

Baseline Equivalence

The goal of matching is to establish baseline equivalence. Baseline equivalence establishes the extent to which the treatment and control groups are equivalent or balanced. According to the Clearinghouse Handbook, a direct pre-test outcome variable must be used to assess baseline equivalence. Alternatively, if using direct pre-test data is not possible or 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 child age for studies of programs for children and youth.

None of the children experienced an exit to permanency previously because they were in their first agency spells; therefore direct pre-test data are not possible and also an alternative pre-test does not exist. As a result, baseline equivalence was established using exact matching with the covariates listed in Table 2: race/ethnicity, age, placement reasons, permanency goal, residential stability, and social resources.

Composition of the pre- and post-match treatment and comparison groups is provided in Table 2. Before exact matching, older children (10 to 13 age group and 14 to 17 age group) were more likely to be referred to Intercept (5.5% and 5.6%, respectively) than younger children (3.5%), as were male children (5.2% versus 4.2%). The likelihood of referral was higher among African American children (6.0%); referrals were lower among White children and children of other races/ethnicities (4.4% and 4.0 %, respectively). Children in congregate care were substantially more likely to be referred to Intercept than children in other care types (congregate care: 8.6%, foster care: 4.4%, and kinship care: 3.9%).

Regarding CANS assessments (Anderson et al., 2003), the children who were referred to Intercept had a lower percentage of residential stability and social resources issues than members of the comparison group (4.0% versus. 5.2 and 4.5% versus 4.9%). Children who were placed due to child behavior issues were more likely to be referred to Intercept than other placement reasons (child behavior: 8.1%, abandonment/neglect: 4.9, abuse: 4.0%, and other: 4.2%). Regarding primary goal setting, 5.4% of children whose primary goal is exit to relatives were referred to the Intercept and children whose goal is reunification were 4.6%. For missing data, we used multiple imputation.[2]

[2] Multiple imputations (20 imputations here) for missing residential stability/social resources, missing placement reasons, and missing primary goal were conducted using SAS PROC MI and SAS PROC MIANALYZE (The MI procedure, SAS Institute).

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

	Before Matching					After Matching				
	Treatment		Comparison			Treatment		Comparison		
Characteristic	Number	Percent	Number	Percent	Percent Referred	Number	Percent	Number	Percent	Percent w/ weight
Age										
5 to 9	106	32.0%	2,912	43.1%	3.5%	94	31.4%	1,666	47.2%	31.4%
10 to 13	97	29.3%	1,677	24.8%	5.5%	88	29.4%	821	23.3%	29.4%
14 to 17	128	38.7%	2,167	32.1%	5.6%	117	39.1%	1,041	29.5%	39.1%
Gender										
Male	176	53.2%	3,209	47.5%	5.2%	155	51.8%	1,578	44.7%	51.8%
Female	155	46.8%	3,547	52.5%	4.2%	144	48.2%	1,950	55.3%	48.2%
Race/Ethnicity										
African American	78	23.6%	1,229	18.2%	6.0%	67	22.4%	386	10.9%	22.4%
White	226	68.3%	4,874	72.1%	4.4%	212	70.9%	3,023	85.7%	70.9%
Other	27	8.2%	653	9.7%	4.0%	20	6.7%	119	3.4%	6.7%
Care Type										
Congregate Care	56	16.9%	595	8.8%	8.6%	43	14.4%	289	8.2%	14.4%
Foster Care	203	61.3%	4,368	64.7%	4.4%	190	63.5%	2,520	71.4%	63.5%
Kinship Care	72	21.8%	1,793	26.5%	3.9%	66	22.1%	719	20.4%	22.1%
Residential Stability										
Missing	3	0.9%	143	2.1%	2.1%	3	1.0%	71	2.0%	1.0%
No	215	65.0%	3,918	58.0%	5.2%	196	65.6%	2,087	59.2%	65.6%
Yes	113	34.1%	2,695	39.9%	4.0%	100	33.4%	1,370	38.8%	33.4%
Social Resources										
Missing	3	0.9%	143	2.1%	2.1%	3	1.0%	71	2.0%	1.0%
No	199	60.1%	3,874	57.3%	4.9%	180	60.2%	2,077	58.9%	60.2%
Yes	129	39.0%	2,739	40.5%	4.5%	116	38.8%	1,380	39.1%	38.8%
Placement Reasons										
Child Behavior	59	17.8%	665	9.8%	8.1%	47	15.7%	329	9.3%	15.7%
Abandon/Neglect	98	29.6%	1,904	28.2%	4.9%	87	29.1%	885	25.1%	29.1%
Abuse	93	28.1%	2,206	32.7%	4.0%	85	28.4%	1,315	37.3%	28.4%
Other	62	18.7%	1,414	20.9%	4.2%	61	20.4%	665	18.8%	20.4%
Missing	19	5.7%	567	8.4%	3.2%	19	6.4%	334	9.5%	6.4%
Primary Permanency Goal										
Exit to Relative	36	10.9%	630	9.3%	5.4%	27	9.0%	87	2.5%	9.0%
Reunification	264	79.8%	5,414	80.1%	4.6%	253	84.6%	3,353	95.0%	84.6%
Missing	31	9.4%	712	10.5%	4.2%	19	6.4%	88	2.5%	6.4%
Total	331	100.0%	6,756	100.0%	4.7%	299	100.0%	3,528	100.0%	100.0%

Table 2 also shows the post-match treatment and comparison group composition. After exact matching, the final sample consists of 299 children referred to Intercept and 3,528 comparison children. In order to show who was included in the final treatment sample, the characteristics of the treatment and comparison samples are included in Table 2. The first comparison group frequencies and percents are unweighted. The weighted percents for the comparison group are identical to the percents for the treatment group due to exact matching. As noted, in order to establish baseline equivalence, we used exact matching. Specifically, our approach is a multi-dimensional exact match, which sets stricter conditions than typical baseline equivalence conditions. No comparison children are matched twice due to the nature of exact matching. Even though the unweighted percents 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 match. As such, weighted percentages are identical in both groups as shown in the last column.

Censoring

Even if we have matched samples, the placement outcome also depends on how much time has passed from the beginning of the agency spell (i.e., the start of the initial placement). For the DTHM, time from placement until the agency spell ended (the stop date) was divided into three-month time intervals with one record per interval of time through the end of the observation window. The person-periods, as they are called, divide the total time of exposure into discrete intervals. For this research, three-month periods (90 days) were used as shown in Table 3. 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).

Table 3: Person-Periods by Treatment / Comparison Group

Interval	Person-Period	Final Comparison	Final Treatment
1-90 days[3]	P-1	3,528	299
91-180 days	P-2	3,104	275
181-270 days	P-3	2,290	206
271-360 days	P-4	1,703	148
361-450 days	P-5	1,256	98
451-540 days	P-6	903	70
541-630 days	P-7	674	53
631-720 days	P-8	485	40
721-810 days	P-9	360	28
811-900 days	P-10	272	21

All children are included in first person-period (P-1) with the number of children decreasing in subsequent person-periods as children leave the agency spell, 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). If a child was in care for more than 811 days, then the record for that child contains 10 person-period records (P-1 through P-10).

By way of example, if a child exits to permanency at 150 days, the outcome for P-1 is coded as zero and the outcome for P-2 is coded as one, indicating that permanency occurred during this particular person-period. As such, until a child exits to permanency, the outcomes for all prior person-periods are coded as zero. The permanency outcome becomes a person-period specific outcome, which means we are measuring the likelihood of permanent exit during specific intervals. In the analysis, each interval has its own permanency likelihood, which means the impact of Intercept is assessed after accounting for how much time has passed since the start of the initial placement.

Cross-Classified Random Effects Model

The likelihood of permanency is affected by three sources of variation in the statistical model: (1) agency variation

[3] As mentioned in the sample population section, placements that lasted less than 60 days were excluded for the study. As a result, any permanency outcomes in P-1 occurred between 61-90 days.

(2) county variation, and (3) child and family variation. Agency variation refers to the fact that the probability of permanency depends on the agency with which the child was placed (in the same way that schools affect student outcomes). County variation arises for similar reasons. Child welfare systems, even though governed by federal and state regulations, are distinctly local in their operations. In particular, county courts are an important partner because judges must agree with the permanency plan. Finally, the prospects of permanency depend on who is served given the unique child and family dynamics.

In order to build statistical models that involve different levels or sources of variation, the structure of the underlying variation should be reflected in the models. Technically, the Intercept data can be described as a cross-classified structure. Children are nested within agencies *and* counties. As mentioned, an agency can contract with multiple counties or one county and a county may contract with more than one agency. To account for the cross-classification, we identify the agency/county combination for each child in the sample.

The DTHM model, with cross-classification, is illustrated below using a hierarchical form with separate equations for the person- and agency/county-levels. This follows the standard exposition on multi-level models (Raudenbush & Bryk, 2001).

Child Level (level one): $\eta_{ijkt} = \ln(h_{ijkt} / (1 - h_{ijkt})) = \beta_{0jk} + \beta_1 X_{ijk} + \beta_2 D_{ijk} + \Sigma T_t P_{ijkt}$

η_{ijkt} is the log of the odds of the outcome (permanency = 1) for child *i* in county *j* and agency *k* at discrete time *t*, h_{ijkt} is the hazard of the outcome for child *i* in county *j* and agency *k* at discrete time *t*, D_{ijk} represents the Intercept indicator for child *i* in county *j* and agency *k* and X_{ijk} represents child-level covariates for child *i* in county *j* and agency *k.* P_{ijkt} is an indicator variable of discrete person-periods. T_t [*t* from 1 to 10] represents statistical model Intercepts for different discrete time intervals, which form the baseline hazard rate.

County and Agency Level (level two): $\beta_{0jk} = \beta_{00} + \beta_{01} C_j + \beta_{02} C_k + \gamma_{0j} + \gamma_{0k}$

For the level-two model (counties and agencies), β_{0jk} has a subscript *j* and *k*, which means each county and each agency has a unique Intercept. For exposition purposes, β_{0jk} includes county-level fixed variables, C_j, and agency-level fixed variables, C_k, so that β_{0jk} becomes the adjusted Intercept for children in county *j* and agency *k.* β_{01} is the adjusted difference in the permanency rate associated with county variable C_j and β_{02} is the adjusted difference in the permanency rate associated with agency variable C_k.

β_{00} refers to the overall Intercept; however, when person-periods (P_{ijkt}) are included, the Intercept refers to the permanency rate for the omitted person-period. For example, if *t* in T_t indicates from 2 to 10, β_{00} refers to the permanency rate (model Intercept) for person period 1 (P-1). Then, the person-period estimates are relative to β_{00}. Alternatively, for a no-Intercept DTHM, which includes all *t* estimates (from 1 to 10), $T_1, T_2 \ldots T_{10}$ form the baseline hazard rate. We used the no-Intercept version of the DTHM with the cross-classified random effects.

In this model, γ_{0j} is a level-2 county random variable and represents the adjusted average permanency rate in county *j* and γ_{0k} is a level-2 agency random variable and represents the adjusted average permanency rate in agency *k.* The presence of γ_{0j} and γ_{0j} changes the model to a random effects model. Also, having the two random variables (γ_{0j} and γ_{0j}) in the same level results in a cross-classified random effects model. In terms of distributions, both the county and the agency Intercepts are assumed to be normally distributed with an expected value of zero. Therefore, the individual county and agency permanency Intercepts are deviations from zero.

Combined Model (levels 1 and 2 together):

$$\eta_{ijkt} = \ln(h_{ijkt} / (1 - h_{ijkt})) = \beta_{00} + \Sigma T_t P_{ijkt} + \beta_1 X_{ijk} + \beta_2 D_{ijk} + \beta_{01} C_j + \beta_{02} C_k + \gamma_{0j} + \gamma_{0k}$$

The mixed or combined model is formed by algebraic substitution. As shown, the model contains fixed components (overall Intercept, person-period Intercepts, level 1 covariates, the program variable, and two level 2 covariates) and two random components (γ_{0j} and γ_{0k}). The model used for the final analysis (Model 2 in Table 5) is below.

$$\eta_{ijt} = \ln(h_{ijt} / (1- h_{ijt})) = \Sigma T_t P_{ijkt} + \beta_1 X_{ijk} + \beta_2 D_{ijk} + \gamma_{0j} + \gamma_{0k}$$

Note that $\beta_{0jk} = \gamma_{0j} + \gamma_{0k}$ in this case, due to the use of a DTHM without an Intercept and county/agency fixed variables. SAS proc glimmix with two random commands was used to conduct the analysis.

Results

Descriptive Statistics

Within the observation period, 7,087 children were placed for the first time. Among them, 331 children (4.7 %) were referred to Intercept; the remaining 6,756 children make up the potential comparison group. Among those children, the number of children exited to permanency from the treatment group and the potential comparison group were 243 (73.4 %) and 4,378 (64.8%), respectively (see Table 4). After exact matching, the permanency rate for the Intercept group is 73.2% and the comparison group is 65.1%.

Table 4: Sample Size and Permanency before Matching

		Total Children	Number of Children Exited Permanently	Percent Exited Permanently (Rate)
Before Matching	Comparison	6,756	4,378	64.8%
	Intercept	331	243	73.4%
After Matching	Comparison	3,528	2,295	65.1%
	Intercept	299	219	73.2%

Average Treatment Effect

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

Estimates from two different models are shown in Table 5 to highlight how the treatment effect changes after the covariates are added to the model. In both models, treatment effect estimates, labeled Treatment-Intercept in Table 5, represent the effect of referral to Intercept program on the rate of permanency.[4] Both models include person-period specific estimates, which form the baseline hazard rate. Model 1 shows the parameter estimates before including covariates other than the Intercept treatment and person period dummy variables. Model 2 shows findings after including other covariates. The treatment effect size does not change much after including other covariates.

The treatment parameter indicates that Intercept increased the likelihood of permanency among children referred

[4] The models in Table 5 show the Treatment – *Intercept* estimates. This is the treatment effect attributable to *Intercept* and not statistical intercept.

to the program compared to a similar group of children who were not referred to Intercept. The Intercept treatment effects in Models 1 and 2 are all statistically significant. Based on the Model 2 results, the likelihood of permanency for children who were referred to Intercept is 22 percent higher than those who were not referred to Intercept.

Because agency spells often end with a transfer to another agency, we checked whether the positive impact of the Intercept intervention derives from some agencies having higher transfer rates. Approximately, 20% of children were transferred to other agencies as an exit reason among children in the first agency spell. This is a typical case of competing exit events (otherwise known as competing risks) and we investigated whether members of the treatment group showed a higher transfer likelihood. We used the same model structure and data that we used for permanency but used transfer from the agency as the dependent variable. Although not statistically significant, the results of this analysis showed that the transfer likelihood for the treatment group was lower than that of the comparison group. Therefore we are able to exclude transfer rates as an alternative explanation for the treatment effect.

Regarding the other covariates in the model, relative to children in the 5 to 9 age group, the between age group differences are not statistically significant. Gender differences are also not statistically significant. African American children are marginally less likely to leave care to permanency, but the finding is not statistically significant (p-value: 0.067). Compared to children in foster care, children in congregate care and kinship care have a higher likelihood of an exit to permanency. Regarding assessment variables, residential stability issues decrease the likelihood of permanency, as might be anticipated. The impact of social resources on the likelihood of permanent exit is not statistically significant.

Compared to children who were placed for child behavior issues, children who were placed for abandonment/neglect, abuse, and other placement reasons were less likely to be exited to permanency. However, only abandonment/neglect shows statistically significant negative impact. There is no statistically different impact tied to the permanency planning goal (e.g., reunification versus exit to relatives).

In order to capture any potential influence associated with the DCS caseworker responsible for case management, we included the Empirical Bayes residual in our final models. The Empirical Bayes residual measures permanency rates for the cases assigned to each caseworker. That is, of the cases assigned to a caseworker, there is a permanency rate. After adjusting the placement rate for characteristics of the cases assigned, the worker permanency rates vary around the average permanency rate. We used this variation as a covariate in the model of treatment effects so that the results are adjusted for the worker's influence. In Table 5, the EB parameter detects whether the permanency rate for children and youth in the treatment and control rises as the worker's permanency rate (EB estimate) rises. The positive estimate suggests that children whose cases are managed by caseworkers with higher EB estimates are associated with higher permanency rate, as expected. The EB*EB term assesses whether the caseworker effect is strictly linear or not. Because the EB*EB is statistically significant, this result suggests that the caseworker effect on outcomes does not increase at a constant rate.

Table 5: Treatment Effect Estimates

	Model 1 Before exact matching			Model 2 After exact matching w/ Person Periods			
Effect	Estimate	Standard Error	Pr > |t|	Estimate	Standard Error	Pr > |t|	O.R.
Treatment - Intercept	0.201	0.084	0.016	0.199	0.094	0.034	1.22
Person-Periods							
P-1	-2.630	0.129	<.0001	-2.681	0.233	<.0001	
P-2	-1.553	0.125	<.0001	-1.575	0.228	<.0001	
P-3	-1.532	0.127	<.0001	-1.447	0.230	<.0001	
P-4	-1.398	0.129	<.0001	-1.362	0.233	<.0001	
P-5	-1.321	0.132	<.0001	-1.340	0.236	<.0001	
P-6	-1.347	0.136	<.0001	-1.313	0.239	<.0001	
P-7	-1.235	0.140	<.0001	-1.152	0.242	<.0001	
P-8	-1.422	0.150	<.0001	-1.430	0.254	<.0001	
P-9	-1.380	0.159	<.0001	-1.308	0.262	<.0001	
P-10	-1.146	0.164	<.0001	-1.093	0.269	<.0001	
Age							
5 to 9				Reference			
10 to 13				-0.084	0.059	0.152	0.92
14 to 17				0.017	0.066	0.796	1.02
Gender							
Females				Reference			
Male				-0.018	0.049	0.714	0.98
Race/ Ethnicity							
Whites				Reference			
African Am.				-0.169	0.095	0.075	0.84
Other				0.068	0.124	0.580	1.07
Care Type							
Foster Care				Reference			
Congregate Care				0.483	0.130	0.000	1.62
Kinship Care				0.425	0.061	<.0001	1.53
CANS							
Residential Stability				-0.229	0.058	<.0001	0.80
Social Resources				-0.072	0.056	0.199	0.93
Placement Reasons							
Child Behavior				Reference			
Abandon/Neglect				-0.267	0.111	0.016	0.77
Abuse				-0.144	0.113	0.203	0.87
Other				-0.184	0.116	0.112	0.83
Primary Goal							
Exit to Relatives				Reference			
Reunification				-0.038	0.159	0.809	0.96
Caseworker							
EB				1.113	0.076	<.0001	
EB*EB				-0.694	0.161	<.0001	

Conclusion

In this third study of Intercept, we examined the effect of Intercept on permanency rates for children placed in Foster care. Foster care is designed to be a temporary solution used to help families during times when the parents are unable provide a safe and stable environment for their children. All things being equal, shorter placements are preferred provided issues with child safety have been addressed.

As in our prior work, this study shows positive effects that are statistically significant. The treated group, which consists of young people placed away from home for the first time who were not previously referred to Intercept, left their placement at rates that exceeded those reported for a group of exactly matched children drawn from a population of children not previously referred to Intercept.

To make that determination, 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 child protective services. Nevertheless, it is an observational study. To address non-random assignment to the treatment condition, we exactly match the treatment and control groups and then added two important methodological innovations: 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. Few studies 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. Conceptually, these are two significant adjustments to the way QED evaluations of permanency programs are done. The result is, we believe, a substantial gain in validity.

Although we did not consider Intercept from an implementation perspective, we do know from meeting with and interviewing staff that Intercept is grounded in theories that have been linked previously to positive program impact, the agency tracks the families it serves at an exceptional level of detail, and that Youth Villages' staff uses the evidence generated from that data to manage the program. Interactions with youth and families are manualized and there is regular feedback between program managers, clinicians, and families. As an approach to service delivery, these are features that fit within the common elements framework (Chorpita et al., 2005; M W Lipsey, 2009; Mark W Lipsey & Wilson, 1993) that is sometimes used to distinguish high quality programs from others. In sum, quality matters in ways that can be measured in terms of placement prevention on the one hand and increased rates on the other.

References

Anderson, R. L., Lyons, J. S., Giles, D. M., Price, J. A., & Estle, G. (2003). Reliability of the Child and Adolescent Needs and Strengths-Mental Health (CANS-MH) scale. *Journal of Child and Family Studies*, *12*(3), 279–289. https://doi.org/10.1023/a:1023935726541

Chorpita, B. F., Daleiden, E. L., & Weisz, J. R. (2005). Identifying and Selecting the Common Elements of Evidence Based Interventions: A Distillation and Matching Model. *Mental Health Services Research*, *7*(1), 5–20. https://doi.org/10.1007/s11020-005-1962-6

DiPrete, T., & Forristal, J. (1994). Multilevel Models: Methods and Substance. *Annual Review of Sociology*, *20*, 331–357. http://arjournals.annualreviews.org/doi/full/10.1146/annurev.so.20.080194.001555

Hollinshead, D. M., Kim, S., Fluke, J. D., & Merkel-Holguin, L. (2015). Influence of Family, Agency, and Caseworker Dynamics on Caregivers' Satisfaction With Their Child Protective Services Experience. *Journal of Public Child Welfare*, *9*(5), 463–486. https://doi.org/10.1080/15548732.2015.1091762

Huhr, S., & Wulczyn, F. (2019). *Do Intensive In-Home Services Prevent Placement?: A Case Study of Youth Villages' Intercept® Program* (pp. 1–24). Center for State Child Welfare Data, Chapin Hall, University of Chicago.

Huhr, S., & Wulczyn, F. (2020). *Do Intensive In-Home Services Promote Permanency?: A Case Study of Youth Villages' Intercept® Program* (pp. 1–19). Center for State Child Welfare Data, Chapin Hall, University of Chicago. https://fcda.chapinhall.org/uncategorized/do-intensive-in-home-services-promote-permanency/

Jorm, L. R., Randall, D. A., Falster, M. O., & Leyland, A. H. (2013). Using Linked Administrative Data and Multilevel Modelling to Identify Targets for Interventions to Tackle Health Disparities. *Journal of Epidemiology & Community Health*, *67*(Suppl 1), A7–A7. https://doi.org/10.1136/jech-2013-203126.8

Lipsey, M W. (2009). The primary factors that characterize effective interventions with juvenile offenders: A meta-analytic overview. *Victims and Offenders*, *4*(2), 124–147.

Lipsey, Mark W, & Wilson, D. B. (1993). The efficacy of psychological, educational, and behavioral treatment: Confirmation from meta-analysis. *American Psychologist*, *48*(12), 1181–1209. https://doi.org/10.1037/0003-066x.48.12.1181

Raudenbush, S., & Bryk, A. (2001). *Hierarchical Linear Models: Applications and Data Analysis Methods* (2nd ed.). Sage.

Reardon, S., Brennan, R., & Buka, S. (2002). Estimating Multi-Level Discrete-Time Hazard Models Using Cross-Sectional Data: Neighborhood Effects on the Onset of Adolescent Cigarette Use. *Multivariate Behavioral Research*, *37*(3), 297–330. http://www.leaonline.com/doi/pdf/10.1207/S15327906MBR3703_1

SAS Institute. *The MI procedure*. Retrieved from: https://support.sas.com/documentation/onlinedoc/stat/141/mi.pdf

Shadish, W. R., Clark, M. H., & Steiner, P. M. (2008). Can Nonrandomized Experiments Yield Accurate Answers? A Randomized Experiment Comparing Random and Nonrandom Assignments. *Journal of the American Statistical Association*, *103*(484), 1334–1344. https://doi.org/10.1198/016214508000000733

Singer, J. D., & Willet, J. B. (1993). It's About Time: Using Discrete-Time Survival Analysis to Study Duration and the Timing of Events. *Journal of Educational Statistics*, *18*(2), 155–195.

Stuart, E A, DuGoff, E., Abrams, M., Salkever, D., & Steinwachs, D. (2013). Estimating Causal Effects in Observational Studies Using Electronic Health Data: Challenges and (Some) Solutions. *EGEMs (Estimating Evidence Methods to Improve Patient Outcomes)*, *1*(3/4), 1–10.

Stuart, Elizabeth A. (2010). Matching Methods for Causal Inference: A Review and a Look Forward. *Statistical Science*, *25*(1), 1–21. https://doi.org/10.1214/09-sts313

Wilson, S. J., Price, C. S., Kerns, S. E. U., Dastrup, S. R., & Brown, S. R. (2019). *Title IV-E Prevention Services Clearinghouse Handbook of Standards and Procedures, Version 1.0* (OPRE Report # 2019-56; pp. 1–60).