The Center for

State Child Welfare Data

Do Intensive In-Home Services Promote Permanency?: A Case Study of Youth Villages' Intercept® Program

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Introduction

In our previous paper (*Do Intensive In-Home Services Prevent Placement?: A Case Study of Youth Villages' Intercept® Program*), we assessed the impact of Youth Village's Intercept program (previously known as YVIntercept) on the likelihood of out-of-home placement. As a continuation of that research, we next investigated the impact of Youth Villages' Intercept program on the likelihood of permanency for children who were placed in out-of-home placement. We measured permanency as the likelihood of exiting out-of-home care through reunification, adoption, exit to relatives, or guardianship. Children in placement are referred to Youth Villages' Intercept program (Intercept) by the Tennessee Department of Children's Services (DCS) staff or by Youth Villages' staff in the residential or foster care program. Intercept program staff then work with families in order to increase the rate of permanency.

To study the effects of Intercept on permanency, we designed a quasi-experimental study using administrative data provided to us by the state of Tennessee and Youth Villages. We assumed responsibility for linking the data and designing the study. As described more fully below, with data from the linked file, we used exact matching based on child- and family-related covariates to establish baseline equivalence. To manage the confounds of distinct provider performance and county variation in permanency rates, we used multi-level discrete time hazard models as the foundation of our analysis. Also, to capture caseworker bias, the extent to which caseworkers influence permanency rates was also taken into account.

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, reducing the time spent in care, or reducing the risk of re-entry. The specific goal depends on the status of the child at the time of 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.

DCS allocates Intercept slots to counties based on recent 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 4 – 5 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]

Data

The study uses administrative records within a quasi-experimental design. As opposed to a well-designed randomized clinical trial (RCT), quasi-experimental designs have to confront the problem of selection bias into treatment programs. To overcome the selection bias, we used multiple empirical strategies. Details of the approach adopted follows.

Overview

To conduct the study, we relied exclusively on administrative records provided to us by DCS and Youth Villages. DCS provided us with 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:

- Child-level characteristics including race/ethnicity, agency spell age, and gender
- Placement data that track when a young person enters care, how long they were in care, their reason for leaving (e.g., reunification, adoption, exit to relatives, runaway, transfer to other agencies), and care type (kinship care, foster care, and congregate care)
- We also noted the agency with physical custody at the time of referral to Intercept services. There are several reasons for noting the agency with custody at the time of referral. Among them, the most important reason has to do with agency-specific rates of permanency. Simply stated, agencies are a source of permanency rate variation independent of child and family characteristics.
- We pulled from TFACTS information on counties where a youth and his or her family was living at the time the young person entered foster care for the first time. Counties are a potentially significant source of outcome variation because each county has different policies and practices for adjudicating a youth's placement status.
- The Child and Adolescent Needs and Strengths (CANS), which is an assessment that measures the needs and strengths of children, adolescents, and their families. The CANS is used during the time a young person is in placement and measures several different functional domains. Among them, the caregiver resources domain was used to capture socioeconomic status (SES). The CANS is administered by DCS staff.
- Caseworker assignments to a child. Caseworkers have a unique ID, assignment start and stop date, and a link to the ID attached to the child.

From Youth Villages, we received Intercept referral and enrollment data, which captures the date of referral to Intercept, onset of Intercept services, and a stop date that indicates when services ended. The referral data also include the TFACTS ID for each child so that the link with the agency spell data is unambiguous. These data were linked together using the TFACTS ID. Among child's agency spell data within the time span of the underlying data, only first placement experiences (first agency spells) were included in the analysis. In this particular case, we have the complete placement history from 2003 forward, so there is very little difficulty in identifying a child's first ever placement in out-of-home care.

With these data, we were able to identify precisely when referral to Intercept took place relative to the start and stop of placement. The event histories gave us a refined way to define placement prevention, permanency, and re-entry populations given what had already happened when the referral to Intercept took place. When Intercept

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

referral date is in-between the start and stop dates of first agency spells, children were defined as part of the treatment group for this study. Therefore, regardless of whether the children actually received Intercept services, all children who were referred to Intercept were considered part of the treatment group provided the referral dates fell within the time period covered.

Study Sample

Sample Universe. 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 private (social sector) 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 to many 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 to many 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) Children under the age 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 (see below), 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 with 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.

The Treatment Group. From the residual group of 7,214 children, after the exclusions were applied, an Intercept referral was identified using linked administrative records received from Youth Villages and the Tennessee Department of Children's Services. 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 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. In our study, we focused on both. We did this by using a discrete time hazard model. The discrete time model 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 permanency starts to happen.

There were two other consideration to bear in mind when assessing the impact of Intercept on permanency. First, we were mindful of the fact that 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.

The Child and Adolescent Needs and Strengths (CANS). The CANS is used to summarize a wide range of child and family strengths and needs (i.e., domains). Among them, the following two items out of the Caregiver Resources and Needs domain were used in the analysis: social resources and residential stability. Residential stability refers to the housing stability of the caregivers and social resources refer to the social assets and resources of the caregivers. These items were selected because they capture family's socioeconomic status (SES). CANS uses a 4-point scale (0, 1, 2, or 3) with zero indicating no needs in that domain and a three indicating a need that should be addressed immediately. A child with a 2 or 3 rating for those items were coded as 1. Otherwise, it was coded as 0. The descriptions and values of those two variables are shown in Table 3 and Table 4 (Epstein & Lyons, 2016).

Table 3: CANS Data Field and Description

Data Field	Description
Social Resources	This item rates the social assets (extended family) and resources that the caregiver can bring to bear in addressing the multiple needs of the youth and family.
Residential Stability	This item rates the housing stability of the caregiver and does not include the likelihood that the child or youth will be removed from the household.

Table 4: CANS Variable Value and Description

Value	Description
0	No current need; no need for action or intervention. Caregiver has significant social and family networks that actively help with caregiving.
1	Identified need requires monitoring, watchful waiting, or preventive activities. Caregiver has some family or friend or social network that actively helps with caregiving.
2	Action or intervention is required to ensure that the identified need is addressed; need is interfering with functioning. Work needs to be done to engage family, friends or social network in helping with caregiving.
3	Problems are dangerous or disabling; requires immediate and/or intensive action. Caregiver has no family or social network to help with caregiving.

Agency and County. We noted the county where a child was living at the time they entered foster care for the first time. We also noted the agency responsible for the first agency spell. These are important characteristics to consider in the model structure because agencies and counties are potentially significant sources of outcome variation that is independent of the children served and the intervention. It is also important to note that any given social sector agency can serve children from different counties and the counties are served by multiple agencies. The many-to-many structure (many agencies paired with many counties and vice versa) is often referred to as a cross-classified structure. Our analytical file provided the means needed to account for the cross-classified structure. We describe our specific approach in the next section.

Caseworker. Caseworkers make decisions on service referrals and also have a potential influence on permanency. To capture their potential influence, the extent to which caseworkers have influence on permanency was included in the model as follows. Because each child can be attached to multiple caseworkers, we had to link one child to one caseworker. The linking process was conducted based on the date that caseworkers were assigned to a child (assignment start date) and placement date. The first caseworker was assigned right after placement was linked to the child. If no casework was found after placement, the first caseworker that was assigned right before placement was linked to the child.

Using this link, we then developed a separate random effects logistic regression model in which the rate of permanency as allowed to vary between caseworkers.[2] From the model, we computed the Empirical Bayes (EB) residual which tells us the extent to which a worker's permanency rate deviates from the adjusted average permanency rate. When children share the same caseworker, they will have the same EB residual. The worker EB residuals were then added to the impact analysis as a way to adjust for the worker confound.

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. 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 problem mentioned in the prior section. Without careful controls for the agency and county context, there may be otherwise unobserved influences that confound our interpretation of the outcome. Our specific solution to each of these issues is described below.

Matching Methodology[3]

In studies of treatment effects, the people who seek or are offered treatment may differ from people who do not seek or receive treatment in ways that are correlated with the outcome. For example, in a program that offers voluntary enrollment, the people who sign up may be more motivated. In the case of permanency, motivated parents may be more willing to do the work needed to bring their child home. As a consequence their intrinsic

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[2] The specifics of the model used to derive the EB residuals are available from the authors. That said, one additional detail is worth noting in this context. In building the model we had to contend with the fact that although children are nested within counties, workers are not, strictly speaking, nested within counties. That is, workers may serve children from more than one county. This creates a cross-classified data structure because children are, potentially, nested within two different entities - workers and counties. We tried to account for the cross-classified structure statistically, but the models did not converge. Therefore, we adopted the two-step strategy to overcome this issue.

[3] A more detailed description of our matching approach and rationale is provided in our prior report, which we incorporate here by reference. https://fcda.chapinhall.org/wp-content/uploads/2019/10/YV-Intercept-Results-1-8-2020-final.pdf

motivation, rather than the program they attend, may account for why their children are more likely to exit to permanency. This is the situation random assignment addresses. Random assignment balances the treatment and control group in terms of both observables and non-observables (e.g. intrinsic motivation) so that any treatment effect can be more easily attributed to the treatment rather than differences in groups of parents who do and do not receive the intervention. Quasi-experimental designs balance the treatment and control group by other means.

In the case of Intercept, we have observational data from an at-scale intervention for which referral was non-random. To counteract the selection effects, we followed conventional methods to manage the problem of non-random assignment to the treatment program. As a general matter, the goal of these methods is to match the treatment and comparison groups as closely as possible on a set of observable characteristics so that we can say with reasonable certainty that the treatment and comparison groups are similar before the onset of treatment. Given those similarities, it is then possible to conclude that a positive treatment effect is because the treatment group received the service provided.

Baseline Equivalence

Baseline equivalence deals with whether, based on observed characteristics, the intervention group and the comparison group are similar enough to each other to allow a meaningful evaluation of treatment effects. The matching method, then, is designed to maximize equivalence. According to the Title IV-E Prevention Services Clearinghouse Handbook of Standards and Procedures (Handbook; Wilson et al., 2019), the methods used to establish baseline equivalence are those used by the What Works Clearinghouse. The Handbook, which provides guidance to states pertaining to evidence-based interventions, establishes the thresholds for baseline equivalence as follows:

> *"Specifically, baseline equivalence is assessed by examining baseline differences expressed in effect size (ES) units. Baseline effect sizes less than 0.05 are considered equivalent and no further covariate adjustments are required. Baseline effect sizes between 0.05 and 0.25 indicate that statistical adjustments in the impact models may be required. These baseline effect sizes are said to be in the adjustment range. Evidence of large differences (ES > 0.25) in demographic or socioeconomic characteristics can be evidence that the individuals in the intervention and comparison conditions were drawn from very different settings and are not sufficiently comparable for the review. Such cases may be considered to have substantially different characteristic confounds."*

According to the Handbook, if an effect size unit is less than 0.05, statistical adjustments are not required to examine program impacts; however, if the effect size is in the adjustment range (between 0.05 and 0.25), statistical adjustments may be required to account for potential confounding effects. Effect size quantifies the difference between two groups. Effect size can be used to judge either covariate differences or impact size. Effect size in this context, as explained in the Handbook, is not about treatment impact, but refers instead to a standardized mean difference between the treatment group and the comparison group. In other words, the effect size units determine the extent to which the intervention group and the comparison group are similar at baseline. Technically, this is calculated as the mean difference between the groups, divided by their pooled standard deviation.

Approach to Matching

Matching treatment group members to comparison group members is actually a family of approaches that involve using observable attributes of the study subjects to find someone in the comparison group that looks as much like a counterpart in the treatment group as possible. Through the matching process, the problem of selection bias is minimized if not eliminated altogether so that the observed outcome differences can be attributed to the intervention with more confidence.

A key feature of the matching algorithms is the notion of closeness (Stuart, 2010). As the name suggests, closeness refers to the degree to which a comparison group member resembles the matching treatment group member. For obvious reasons, the match should be as close as possible. Matching approaches range from exact matching to propensity score matching (PSM). The former involves matches where each member of the treatment group matches with a control group member on each observable, exactly. Propensity scoring matching provides a way to evaluate the closeness of a match when exact matching is not possible, which happens when, for example, the number of variables for matching is relatively large and the number of comparison subjects is relatively small.

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. Even so, exact matching is not always possible. When the number of matching variables is large, the exact match requirement may lead to many unmatched cases (i.e., treatment group members without a match). We are, again, able to avoid this issue because we have a relatively large number of children as potential candidates for the comparison group.

Table 5 shows how many children were included in the study after exact matching. Ninety-one percent of the children referred to Intercept were included in the final analysis sample as were 54 percent of the children who were not referred to Intercept. As such, 9 percent of the children in the treatment group do not have a comparison group counterpart and 46 percent of children in the comparison group were not matched to any treated child.

Table 5: Sample Size Before and After Exact Matching

	Before Exact Matching	After Exact Matching	Matching proportion
Intercept Group	362	328	91%
Comparison Group	6,852	3,701	54%

Covariates for 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 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 described in the data section: gender, race/ethnicity, age, placement reasons, permanency goal, residential stability, and social resources (see Table 6).

Composition of the pre- and post-match treatment and comparison groups is provided in Table 6. Before exact matching, older children (10 to 13 age group and 14 to 17 age group) were more likely to be referred to Intercept (6.6% and 6.2%, respectively) than younger children (3.9%), as were male children (6.0% vs. 4.6%). The likelihood of referral was higher among African American children (6.8%); referrals were lower among White children and children of other races/ethnicities (5.1% and 4.1 %, respectively). Children in congregate care were substantially more likely to be referred to Intercept than children in other care types (congregate care: 10.6%, foster care: 4.9%, and kinship care: 4.4%).

Regarding CANS assessments, the children who were referred to Intercept had a lower percentage of residential stability and social resources issues than members of the comparison group (4.4% vs. 6.0% and 5.0% vs. 5.6%). Children who were placed due to child behavior issues were more likely to be referred to Intercept than other placement reasons (child behavior: 10.6%, abandonment/neglect: 5.5%, abuse: 4.5%, and other: 4.4%). Regarding primary goal setting, 6.1% of children whose primary goal is exit to relatives were referred to the Intercept and children whose goal is reunification were 5.2%. For missing data, we used multiple imputation.[4]

Table 6: 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	113	31.2%	2,928	42.7%	3.9%	101	30.8%	1,679	45.4%	30.8%
10 to 13	112	30.9%	1,709	24.9%	6.6%	102	31.1%	933	25.2%	31.1%
14 to 17	137	37.8%	2,215	32.3%	6.2%	125	38.1%	1,089	29.4%	38.1%
Gender										
Male	195	53.9%	3,255	47.5%	6.0%	173	52.7%	1,629	44.0%	52.7%
Female	167	46.1%	3,597	52.5%	4.6%	155	47.3%	2,072	56.0%	47.3%
Race/Ethnicity										
African American	84	23.2%	1,240	18.1%	6.8%	72	22.0%	410	11.1%	22.0%
White	251	69.3%	4,951	72.3%	5.1%	236	72.0%	3,169	85.6%	72.0%
Other	27	7.5%	661	9.6%	4.1%	20	6.1%	122	3.3%	6.1%
Care Type										
Congregate Care	66	18.2%	621	9.1%	10.6%	51	15.5%	307	8.3%	15.5%
Foster Care	217	59.9%	4,419	64.5%	4.9%	204	62.2%	2,652	71.7%	62.2%
Kinship Care	79	21.8%	1,812	26.4%	4.4%	73	22.3%	742	20.0%	22.3%
Residential Stability										
Missing	3	0.8%	146	2.1%	2.1%	3	0.9%	72	1.9%	0.9%
No	240	66.3%	3,984	58.1%	6.0%	219	66.8%	2,228	60.2%	66.8%
Yes	119	32.9%	2,722	39.7%	4.4%	106	32.3%	1,401	37.9%	32.3%
Social Resources										
Missing	3	0.8%	146	2.1%	2.1%	3	0.9%	72	1.9%	0.9%
No	219	60.5%	3,931	57.4%	5.6%	199	60.7%	2,206	59.6%	60.7%
Yes	140	38.7%	2,775	40.5%	5.0%	126	38.4%	1,423	38.4%	38.4%
Placement Reasons										
Child Behavior	73	20.2%	691	10.1%	10.6%	61	18.6%	371	10.0%	18.6%
Abandon/Neglect	107	29.6%	1,930	28.2%	5.5%	94	28.7%	934	25.2%	28.7%
Abuse	100	27.6%	2,227	32.5%	4.5%	92	28.0%	1,385	37.4%	28.0%
Other	63	17.4%	1,431	20.9%	4.4%	62	18.9%	676	18.3%	18.9%
Primary Permanency Goal										
Exit to Relative	39	10.8%	639	9.3%	6.1%	30	9.1%	100	2.7%	9.1%
Reunification	287	79.3%	5,491	80.1%	5.2%	276	84.1%	3,509	94.8%	84.1%
Missing	36	9.9%	722	10.5%	5.0%	22	6.7%	92	2.5%	6.7%
Total	362	100.0%	6,852	100.0%	5.3%	328	100.0%	3,701	100.0%	100.0%

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

Table 6 also shows the post-match treatment and comparison group composition. After exact matching, the final sample consists of 328 children referred to Intercept and 3,701 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 6. 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 discrete time model, 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 7. 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.

Table 7: Person-Periods by Treatment / Comparison Group

Interval	Person-Period	Final Comparison	Final Treatment
1-90 days[5]	P-1	3,701	328
91-180 days	P-2	3,254	299
181-270 days	P-3	2,397	221
271-360 days	P-4	1,768	160
361-450 days	P-5	1,298	103
451-540 days	P-6	928	74
541-630 days	P-7	697	55
631-720 days	P-8	502	41
721-810 days	P-9	370	28
811-900 days	P-10	279	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

[5] 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.

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.

Time-Sensitive vs. Time-Invariant Treatment

Referral to Intercept can happen at any time after the beginning of the agency spell. For example, in Table 7, the referral can occur during the second person-period (P-2), rather than the first (P-1). In that case, treatment exposure is defined based on the timing of the referral: P-2 and all subsequent person periods are included as person-periods during which there was either exposure to the treatment or the person-periods were post-treatment. This time-sensitive approach defines exposure to treatment at the point of referral and thereafter.

A challenge with this approach arises within the comparison group because there is no comparable date of referral. To solve this problem, the treatment group was defined based on whether there was a referral rather than on the timing of the referral. This approach means that all person-periods prior to referral (when the dummy variable indicating a permanent exit is set to zero) are included as periods when there was exposure to treatment even though there was no treatment exposure prior to referral. Although the intent-to-treat approach dilutes the treatment effect (if there is a treatment effect), we adopted this more conservative approach as one other way to avoid potential bias. Moreover, if, in the face of this choice, we find a statistically significant program effect, then we can be more confident in the conclusions reached.

Discrete-time Hazard Model

When faced with the censoring issue (i.e., the window of observation is longer for some study participants than it is for others), there are two major approaches for solving the problem: the Cox proportional hazard model and the discrete-time hazard model (Singer and Willet, 2003). In contrast to the Cox proportional hazard model, which uses one record per child, discrete time models divide time into intervals (three-month time intervals in this case), with one record per interval of time through the end of observation for a given child (i.e., permanency). If censoring occurs, the outcomes of all person-periods are recorded as zero. Between the two methods, we opted for the discrete-time hazard model (DTHM) because it offers a number of advantages. First, the DTHM calculates the likelihood of permanency for each person-period. Second, when testing for interaction effects that involve time, the DTHM offers more flexibility and transparency to test specific interactions than the Cox proportional hazard model. Third, in the event there are between-agency and between-county differences in permanency rates, the DTHM addresses the nested/cross-classified data structure in a straightforward manner.

Cross-Classified Random Effects Model

The likelihood of permanency is affected by three sources of variation in the statistical model: (1) agency variation (2) county variation, and (3) child 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. 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 and Bryk, 2002).

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 9) 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, 7,214 children were placed for the first time. Among them, 362 children (5.0 %) were assigned to Intercept; the remaining 6,852 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 267 (77.8 %) and 4,435 (64.7%), respectively (see Table 8).

After exact matching, the permanency rate for the Intercept is 73.5% and the comparison group is 64.9%. Therefore, after exacting matching, permanency exit rate is similar to before exact matching; however, the treatment group slightly decreased from 77.8% to 73.5%.

Table 8: Sample Size and Permanency before Matching

		Total Children	Number of Children Exited Permanently	Percent Exited Permanently (Rate)
Before Matching	Comparison	6,852	4,435	64.7%
	Intercept	362	267	77.8%
After Matching	Comparison	3,701	2,403	64.9%
	Intercept	328	241	73.5%

Average Treatment Effect

The results of the cross-classified random effects model are found in Table 9, 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 9 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 9, represent the effect of referral to Intercept program on the rate of permanency.[6] 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

[6] The models in Table 9 show the Treatment – Intercept estimates. This is the treatment effect attributable to Intercept and not statistical intercept..

other covariates.

The treatment parameter indicates that Intercept increased the likelihood of permanency among children referred 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 of children who were referred to Intercept is 24 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 less likely to leave care to permanency.[7] 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 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. As described earlier, 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 results 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 9, the EB parameter detects whether the permanency rate for children and youth in the treatment and control rises as the worker's permanency rate rises. The positive estimate (1.229) suggests that children whose cases are managed by caseworkers with above average permanency rates achieve permanency at rates that are above the rate achieved by caseworkers with average permanency rates. In general, this is what one would expect. The EB*EB and EB*EB*EB terms assess whether the EB effect is strictly linear – for each unit of change in the caseworkers' permanency rate, is the change in the permanency rate for treatment and the control group the same? In this case, the EB*EB parameter is statistically significant, which indicates the change in permanency rates for caseworkers with permanency rates that are closer to the average (the EB residual is smaller), the permanency

[7] Further analysis has been completed related to the differential effects of race on likelihood of exit to permanency. We will report the results from those later. We can say here that Intercept had a positive effect on permanency rates for Black children.

rate differences tend to be smaller. The EB*EB*EB parameter is not significant.

Table 9: 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	0.251	0.089	0.005	0.218	0.090	0.015	1.24
Person-Periods							
P-1	-2.764	0.132	<.0001	-2.675	0.216	<.0001	
P-2	-1.673	0.124	<.0001	-1.564	0.211	<.0001	
P-3	-1.559	0.127	<.0001	-1.422	0.214	<.0001	
P-4	-1.479	0.131	<.0001	-1.342	0.216	<.0001	
P-5	-1.468	0.136	<.0001	-1.315	0.219	<.0001	
P-6	-1.487	0.143	<.0001	-1.331	0.224	<.0001	
P-7	-1.321	0.148	<.0001	-1.169	0.227	<.0001	
P-8	-1.529	0.164	<.0001	-1.389	0.237	<.0001	
P-9	-1.485	0.177	<.0001	-1.324	0.247	<.0001	
P-10	-1.235	0.184	<.0001	-1.063	0.253	<.0001	
Age							
5 to 9				Reference			
10 to 13				-0.055	0.057	0.332	0.95
14 to 17				0.011	0.065	0.864	1.01
Gender							
Females				Reference			
Male				-0.002	0.048	0.965	1.00
Race/ Ethnicity							
Whites				Reference			
African Am.				-0.176	0.093	0.057	0.84
Other				0.071	0.123	0.561	1.07
Care Type							
Foster Care				Reference			
Congregate Care				0.458	0.123	0.000	1.58
Kinship Care				0.427	0.060	<.0001	1.53
CANS							
Residential Stability				-0.226	0.056	<.0001	0.80
Social Resources				-0.086	0.055	0.119	0.92
Placement Reasons							
Child Behavior				Reference			
Abandon/Neglect				-0.268	0.106	0.012	0.77
Abuse				-0.156	0.105	0.139	0.86
Other				-0.188	0.112	0.092	0.83
Primary Goal							
Exit to Relatives				Reference			
Reunification				-0.042	0.146	0.773	0.96
Caseworker							
EB				1.229	0.123	<.0001	
EB*EB				-0.631	0.154	<.0001	
EB*EB*EB				-0.373	0.296	0.207	

Conclusion

Foster care is designed to be a temporary solution used to help families during times when the parents are either unable or unwilling to care for their children. All things being equal, shorter placements are preferred provided issues with child safety have been addressed. When developing an initial case plan, reunification with parents is the goal most often assigned. In time, as the likelihood of reunification wanes, adoption and guardianship move to the top of the priority list. Nevertheless, for the benefit of the children involved, the preference for short stays in foster care is a durable one, regardless of the permanency option ultimately chosen.

In the long history of work that examines whether the time to permanency is amenable to intervention, few programs have reached the well-supported level. For example, according to the California Evidence-Based Clearinghouse, there are 26 programs that target reunification and 26 programs that target adoptions (with some overlap). Of those programs, only a small handful have reached the well-supported level. More importantly, at a time when a premium is placed on interventions that demonstrate efficacy, the majority of programs could not be rated at all, an indication that the available scientific evidence was insufficient. Of the 9 guardianship programs, 6 were not rated; the other 3 are listed as promising practices.

Against this backdrop, the results of our Intercept evaluation indicate that Intercept does affect permanency rates. Compared to a matched comparison group, after controlling for how long they were in care, the odds of achieving permanency were about 24 percent higher for the Intercept group. Given how important timely permanency is, these findings are a reason for substantial optimism. First and foremost, as an intervention, the Intercept model follows the path other effective interventions have followed. The intervention is linked to a theory of change that is itself grounded in theories of behavioral change. Second, the intervention as implemented demands a level of fidelity to the model that is essential. To support those expectations, there is routine tracking of outcomes along with fidelity and report-back to program staff and managers. Staff are trained and supported. There is a manual that guides the work with families.

Although we acknowledge that our research design is quasi-experimental, we do think the design has a number of important strengths. First, our matching strategy, which uses demographic characteristics, placement history (i.e., placement reason and placement type), and clinical acuity at both the child-and family-levels reinforces our confidence in the findings. Second, as implemented, the program operates at scale. That is, there were no adjustments to the program in the field to accommodate the evaluation, a feature that raises the likelihood that replication of the intervention in other contexts will yield similar results. Finally, we were able to account for three sources of variation that affect permanency rates that are often side-stepped in randomized clinical trials: county variation in permanency rates, private agency variation in permanency rates, and caseworker variation in permanency rates. Child welfare interventions are deployed in contexts where there are multiple influences on the time to permanency other than the intervention itself. Our ability to adjust for these unobserved county, agency, and worker effects substantially improves the validity of our findings.

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