Who Gets a Family? The Consequences of Family and Group Home Allocation for Child Outcomes

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Abstract

Hundreds of thousands of children grow up in the US foster care system every year and are at high risk of experiencing negative outcomes such as incarceration and homelessness. This paper documents how the placement of foster children into families rather than group homes improves their outcomes using the exits of other children from families as an instrument for their placement setting. Policies that change which children are matched to families can achieve a large percentage of the gains from policies that add families to the foster care system due to heterogeneity in treatment effects.

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Every year, child protective service agencies in the US spend $30 billion to protect the well-being of children and end up placing over 200,000 children into foster care (Children's Bureau, 2016; Child Trends, 2016). Foster children in the US are very disadvantaged: 1/3 of 17 year olds in foster care will end up homeless and 1/5 will end up incarcerated.[1] Moreover, these children make up a large proportion of the incarcerated young adult population: a 1997 survey found that 1/5 of US inmates under age 30 spent some time in foster care (Doyle, 2007b). Most previous research finds that entering foster care increases the chances of criminal behavior and reduces earnings (Doyle, 2007b, 2008; Bald, Chyn, Hastings and Machelett, 2019).[2] These discouraging results suggest the following question: How can foster care be improved?

One promising area identified by policymakers and researchers is the placement settings of children. Children can be placed with substitute care families or in larger group home settings with professional caretakers. Families are thought to be more beneficial due to a strong belief that children do better growing up in loving homes.[3] Placing more children with families may improve their outcomes, but foster families are scarce and hard to recruit, requiring extensive training and monthly subsidies.[4] If children benefit from families differently, then an alternative approach to improving outcomes consists of reallocating children to families to maximize the effectiveness of family placements. It is an empirical question whether reallocating family and group home placement settings can achieve similar gains to placing more children with families.

This paper studies how the allocation of families and group homes to children affect their outcomes at the individual and aggregate level. I identify a Local Average Treatment Effect (LATE) that shows that the marginal placement with a family improves an outcome index that includes incarceration and homelessness at age 21 by more than one

[1] Author's calculations from the 2011 and 2014 National Youth in Transition Database used in this paper. See Section 1.2 for more details on the data.

[2] Some exceptions have found positive effects for young girls in Rhode Island on test scores (Bald, Chyn, Hastings and Machelett, 2019) and positive effects for older children in Michigan on education and maltreatment outcomes (Gross and Baron, Forthcoming).

[3] The largest child welfare reform in recent years in California, the Continuum of Care Reform (CCR), places foster care placements as the center of its agenda. The CCR website states:"The Continuum of Care Reform draws together a series of existing and new reforms to our child welfare services program designed out of an understanding [foster children] do best when they are cared for in committed nurturing family homes." (California Department of Social Services, 2021). Academic research on family vs. institutional settings provides a similar perspective (Barth, 2002; Nelson III, Zeanah, Fox, Marshall, T. and Guthrie, 2007; Ryan, Marshall, Herz and Hernandez, 2008).

[4] Foster families in California can be paid up to $1,000 a month for a foster child and it takes parents 3 to 6 months to become approved to be foster families (California Department of Social Services, 2021).

standard deviation relative to placement in a group home. I extrapolate these estimates to non-compliers and allow for heterogeneity in treatment effects using a generalized Roy (1951) model. I examine how two types of policies differ in affecting children's outcomes. The first policy adds families to the foster care system allowing more children to be placed with families. The second policy changes which children are placed with families without adding families. Policies that reallocate children can only affect outcomes if children experience heterogeneous treatment effects. I find evidence for this heterogeneity in treatment effects. For instance, I find that boys benefit from families more than girls but are placed with them less often. Overall, I find that adding families to foster care improves children's well-being but well-tailored policies that change the set of children matched to families can achieve similar gains.

My analysis uses administrative foster care data on placements in 2010-2015 linked to outcomes obtained from surveys of children at age 21. To identify the effect of placement setting on outcomes I utilize the exits of other children from foster families as an instrumental variable (IV) for whether a child is placed with a foster family or in a group home. Exits of other foster children vary due to the timing of a child's reunification with their birth family or their emancipation when they age out of foster care. This instrument is similar to instruments in other papers that use exogenous market condition shifters to alter placements in a matching market context (Agarwal, Hodgson and Somaini, 2020). The main identifying assumption underlying my empirical strategy is that exits of other children are uncorrelated with factors that predict entries of children that are more likely to be placed or more likely to have good outcomes. I investigate whether this assumption is likely to be valid through a series of tests including a randomization test which confirms that the instrument appears quasi-randomly assigned to a rich set of observable child characteristics. I also provide evidence that suggests that the other assumptions required of an IV in a heterogeneous treatment effect setting, such as monotonicity, are satisfied (Imbens and Angrist, 1994).

The IV results show that foster families cause better outcomes than group homes for foster children at age 21. On an outcome index that combines employment, enrollment, incarceration, homelessness and substance abuse, foster children gain between 0.97 and 0.99 standard deviations improvement from being placed with families relative to group homes. Thes estimates I obtain for incarceration outcomes are similar to those found in the literature using propensity score matching techniques (Ryan, Marshall, Herz and Hernandez, 2008). I undertake a variety of robustness exercises including but not limited to examining robustness to non-random non-response bias and survey attrition and find similar results.

To study how different policies affect foster children's outcomes in this setting and incorporate heterogeneous treatment effects I build and estimate a generalized Roy (1951) model of child placement into families and group homes. In this model, children are placed with families if they are among the most preferred children. Markets are geographic and time specific and families have homogeneous preferences over their characteristics. This modeling setup adapts methods from the centralized matching market literature (e.g. Agarwal (2015); Agarwal, Hodgson and Somaini (2020)) to the decentralized foster care setting where families play a large role in determining placements. The model then predicts outcomes for children based on observable and unobservable characteristics following Heckman (1979), Kline and Walters (2016) and Walters (2018).[5] I estimate the model under parametric assumptions and control function techniques that follow the literature (Heckman, 1979; Kline and Walters, 2016). I find significant preferences for girls, younger children, and non-black children. I find that boys have larger treatment effects than girls, consistent with studies that find boys are more responsive to childhood interventions (Kling, Ludwig and Katz, 2005; Bertrand and Pan, 2013; Autor, Figlio, Karbownik, Roth and Wasserman, 2019). I also compute different model-based treatment effects and find evidence that treated children benefit less than non-treated children in general, and show that the model based LATE estimate and IV based LATE estimate are similar.

I use the model to compare policies that increase the availability of families to policies that reallocate children to families. These policies could be achieved in practice by changing the average subsidy rate and the relative subsidy rates of different children.[6] I compare a policy that adds a percentage of families to each foster care market to policies that increase the rate of placement of boys while decreasing the rate of placement of girls, and that optimize allocations based on observables and unobservables informed by the outcome model estimates. More families benefit children, but I also find that a large share of these benefits can be achieved by reallocating children to families. Allocating children to families based on observable demographics can achieve over 2/3 of the aggregate gains that come from adding 50% more families to the foster care system on the outcomes I study.

[5]This modeling exercise in this paper is related to papers that connect IV and model based treatment effect estimates (Vytlacil, 2002; Kline and Walters, 2019) and other applications of the Roy (1951) model in matching market contexts such as Walters (2018) who looks at an application to enrollment in charter schools and Abdulkadiroğlu, Pathak, Schellenberg and Walters (2020) who looks at an application to enrollment in New York City High Schools to understand if parents value school effectiveness.

[6]Doyle (2007a) shows that kin families change their care in response to foster care stipends.

This paper is related to several literatures. First, this paper is related to a broad literature studying how interventions for disadvantaged children can causally affect their outcomes (Almond, Doyle, Kowalski and Williams, 2010; Heckman, Pinto and Savelyev, 2013; Chetty, Hendren, Kline and Saez, 2014; Aizer, Eli, Ferrie and Lleras-Muney, 2016; Hoynes, Schanzenbach and Almond, 2016; Isen, Rossin-Slater and Walker, 2017; Chyn, 2018; Currie, Mueller-Smith and Rossin-Slater, 2019). This paper contributes to this literature by focusing on the comparison between institutionalization and family settings for older disadvantaged children. I show that institutionalization has a large negative impact on outcomes and that the allocation of children to families and institutions has important consequences for aggregate outcomes.

The results in this paper are also relevant to the literature that studies how a child's family circumstances affect their outcomes (Sacerdote, 2007; Fagereng, Mogstad and Rønning, 2021). While most of the studies in this literature measure treatment effects on children's later outcomes by parental characteristics such as parental wealth or education by studying adoptive parents, the results in this paper isolate causal effects of family settings relative to institutionalized settings by studying foster care. Close to one hundred thousand children in the US grow up in institutions every year.[7]

Finally, this paper is closely related to a smaller literature that examines how placement settings affect foster children's outcomes. While there is a large literature comparing kin and non-kin family placements[8] there is less work studying families and group homes. Existing work is limited and uses propensity score matching methods or focuses on cognitive outcomes of young children outside of the US (Ryan, Marshall, Herz and Hernandez, 2008; Nelson III, Zeanah, Fox, Marshall, T. and Guthrie, 2007). I provide new evidence that combines an instrumental variable method with new outcomes such as homelessness and a focus on teenage foster children in the US, a population at severe risk of poor outcomes. Perhaps the most novel contribution I make to this literature is in studying heterogeneous treatment effects of placement settings and their consequences for foster care policy design.[9]

The rest of this paper is organized as follows. Section 1 provides a description of foster

[7] Author's calculation from the AFCARS data.

[8] Berrick, Barth and Needell (1994); Berrick (1997); Ehrle and Geen (2002); Font (2014); Andersen and Fallesen (2015); Hayduk (2017)

[9] Robinson-Cortes (2019) also studies policy design in a structural model of foster care but focuses on different outcomes such as placement stability and different policies, including relaxing geographic constraints in placements.

care and the data used in the analysis. Section 2 describes the instrumental variable strategy and results. Section 3 describes the model setup. Section 4 describes the model estimation and results. Section 5 discusses the policy counterfactuals. Section 6 concludes.

1 Setting and Data

1.1 Overview of Foster Care and Foster Care Placement in the U.S.

Child protective services are administered at the county-level in the U.S. County officials receive reports of abuse or neglect. Social workers investigate over 4 million reports of abuse and neglect every year and determine whether a child should be removed from their current birth family or guardian (Children's Bureau, 2016). Children can be placed in three different placement options. The first is kin foster family placement which consists of placement with a relative. The second is non-kin foster family placement which consists of placement with a family or adult that volunteer their time and house.[10] The third is group home or institution. Group homes and institutions provide 24-hour care and are staffed with adults that care for children in a professional role. Some examples include residential treatment facilities and maternity homes. Non-kin placements are the most common compromising 46% of placements (compared to 32% for kin) (Children's Bureau, 2020).

When social workers are making placement decisions, they generally view group homes as an option of last resort (Barth, 2002; Ryan, Marshall, Herz and Hernandez, 2008). Group homes are known to be restrictive settings. Sixto Cancel, a former foster youth, says in a New York Times guest essay:

> "My next stop was to be a group home. My younger brother lived in a group home for five years. I saw how workers there restrained him, took away his visiting 'privileges' when he misbehaved and how he ate cafeteria food for every meal. I refused to go. I knew that no matter how difficult it had been for me to join foster families of total strangers, an institutional context would be worse." (Cancel, 2021)

Children exit foster care in three main ways. The first, and most common, is reunification with their parent or primary caretaker. When their child enters foster care, birth parents

[10] Foster families receive basic training and go through an approval process that varies by state. While caring for children they are given a stipend that ranges between \$500 and \$1000 a month. This stipend depends on the age of the child and other child characteristics. (WeHaveKids.com, 2020).

work with social workers on a plan for eventual reunification. For example, if a child is removed from their birth parents because the parents are abusing drugs, the social worker may ask the parents to undergo drug rehabilitation before the child reunites with them. The second is adoption, often their foster parents. The third is emancipation which occurs when a child is too old and loses eligibility for foster care funding.

1.2 Main Data and Sample

I link two datasets from the National Data Archive on Child Abuse and Neglect (NDACAN) for my analysis. The first is the Adoption and Foster Care Analysis and Reporting System (AFCARS) foster care file and the second is the National Youth in Transition Database (NYTD) outcomes file.

The AFCARS data is part of a federally mandated data collection system maintained to provide case specific information on all children covered by the protections of Title IV-B/E of the Social Security Act. This dataset covers all counties and states in the US, and all children in foster care for whom child welfare agencies have responsibility for care. The AFCARS data used in this paper contains placement data for every foster child in the US every 6 months between 2010 and 2015. The data includes the placement type (kin, non-kin, group home, institution), demographics (age, sex, and race) and reasons for removal for each child, including whether the child entered because their parents are in jail, they were abused, neglected, or had a behavioral problem.[11]

The NYTD data contains results of a survey administered to eligible children at the ages of 17, 19 and 21. This paper uses two NYTD cohorts, those 17 in 2011 and those 17 in 2014. Children are eligible for the NYTD survey if they turn 17 years old while in foster care and remain in foster care within the 45-day period following their birthday. The survey asks about outcomes such as incarceration, homelessness, and substance abuse in the past two years. Appendix A.2 contains more information on the outcome variables in this survey. The survey response rate is 60%.[12] My analyses account for the possibility of

[11]Some of the removal reasons are known to be noisy indicators of services provided, but are still useful proxies that can predict family placement and subsequent outcomes. To address this, Appendix Table A6 reproduces the main results including only demographic child controls. Waldfogel (2000) discusses the benefits of the new AFCARS data and how it should assist in understanding important issues in child welfare and foster care through data.

[12]This is the response rate for children eligible to take the survey at age 21. The survey is administered so that a child is eligible to take the survey at age 21 if they are (1) eligible to take the survey at age 17 (2) completed the survey at age 17. Furthermore the child must satisfy both (1) and (2) and must be randomly sampled by a state if the state elects to randomly sample from this subpopulation due to resource constraints.

non-random non-response bias and attrition in the survey.

I define the main placement setting variable as an indicator variable for whether a child is initially placed with a non-kin family, where a zero represents placement in a group home or institution.[13] My empirical strategy does not exogenously vary placement in kin homes, so I only focus on children placed in non-kin family homes or group homes/institutions. To maximize power, I create an index of my outcome variables. I follow Kling, Liebman and Katz (2007) in creating an index that combines whether a child is enrolled or employed, has been incarcerated at ages 20 or 21, has been homeless at ages 20 or 21, and has had a substance abuse referral at ages 20 or 21.[14] The main results also break the results out into individual outcomes with the caveats of inference under multiple hypothesis testing.

My analysis is conducted at the child entry level in the AFCARS data with outcomes measured at age 21 in the NYTD data. Each observation in the main analysis is a unique *child-entry and outcome at 21* pair. I use age 21 to focus on the longest term effects available. Because the survey is administered at age 17, I only consider entries of children that occur at age 14 or older to remove selection bias that might occur from considering children that enter at a younger age. I show that the results are robust to this age cutoff choice. Additional details related to sample definition are contained in Appendix A.2.

I consider three different samples in my reduced form analysis to test the robustness of the assumptions of my empirical strategy. The first sample is all foster children entering between ages 14 and 17 in the US between 2010 and 2015 ("old children sample"). The second is all children in the old children sample who are eligible for the NYTD survey ("eligible sample"). The final sample is all children in the eligible sample who complete at least one question of the NYTD survey that goes into the outcome index at age 21

Table 1 shows that the outcome sample is much less than 60% of the eligible sample. This is because of this sampling scheme that is used. The 60% accounts for the children that satisfy both (1) and (2) and are randomly sampled by the state.

[13]There are other ways one can measure a child's placement experience in foster care. I choose initial placement as as the primary measure for two reasons. First, the instrumental variable relies on market conditions when a foster child enters the system to exogenously shift their placements, and thus should have the most power for initial placements. Second, foster care placements are quite "sticky": a child initially placed in a non-kin family will spend over 80% of their time in a non-kin family, a child initially placed in a group home will spend 11% of their time in a non-kin familys. Robustness of this analysis to this choice of endogenous variable is assessed in the Table A19 where I repeat the main analysis using endogenous variables of the percentage of time in a non-kin placement and months in a non-kin placement.

[14]The summary index is defined to be the equally weighted average of z-scores of its components, where the sign of each component is set up so that more beneficial outcomes have higher scores (i.e. it is increasing in enrollment/employment, decreasing in incarceration). The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation.

("outcome sample").

Table 1 provides descriptive statistics of the three samples. Half of the children in the outcome sample are placed with non-kin families. This proportion is higher than the corresponding proportion in both the eligible sample and old children sample. The difference with the eligible sample highlights the potential importance of correcting for response bias and attrition in the survey.[15] I address this potential bias directly in the analysis. This table also shows the mean (1.01) and standard deviation (2.08) of the index.

2 Placement Instrument and Regression Analysis

2.1 Research Design: Children Exiting Non-Kin Families as a Placement Instrument

This section describes the research design used to identify the effect of being placed in a non-kin family vs. a group home on a child's outcomes. Suppose we want to estimate the effect of placement type $Place_i$ on criminal behavior Y_i. One potential strategy that has been used in the literature is to assume that a set of observable features X_i for each child i are sufficient for controlling for all factors that jointly determine placement into a family and a child's criminal behavior Y_i (Ryan, Marshall, Herz and Hernandez, 2008). I take a different approach and perform an instrumental variable (IV) analysis that allows for correlated unobservables.

My empirical strategy takes advantage of the fact that some non-kin foster families foster more than one child (Cherry and Orme, 2013) and the fact that there is a shortage of families in foster care (Doyle and Peters, 2007). In over 95% of the counties that I study, some older children are placed in group homes and institutions. This paper proposes that one shifter of family scarcity that is exogenous to a child's potential outcomes is the exits of other foster children from non-kin foster families. If children exit placements of families that continue to foster,[16] then those families can care for entering foster children. If these

[15]The difference with the old children sample is mainly driven by age differences between the samples. The old children sample is far more balanced on the age distribution, while most children in the outcome and eligible samples enter at age 16. Another notable comparison between the samples is that there are substantially less boys in the outcome sample than in the eligible sample or old children sample. This is likely because boys are more likely to be incarcerated or homeless and are harder to survey between ages 20 and 21.

[16]The literature has identified a set of foster mothers called the"Vital Few" (Cherry and Orme, 2013) that foster multiple children over their lives. Appendix A.1 provides more details on these families.

exits satisfy certain assumptions then they can serve as an IV to measure a Local Average Treatment Effect (LATE) of placement into a non-kin foster family relative to a group home or institution (Imbens and Angrist, 1994).

In order to measure these types of exits, for every county-month-year t, I count the number of exits from non-kin placements that end with a child being emancipated or reunified with their birth family $Exits_t$. I do not include adoptions and guardianship since they are less likely to represent true slots opening up in foster families. To account for average county differences and US-wide seasonal changes in foster care policy that may affect exit behavior, I residualize the exits variable on county and month-year fixed effects. The final consideration in creating this variable is that some counties are larger than others. Therefore the main specification normalizes the number of exits by the log population to account for the fact that some counties will have higher deviations of non-kin exits due to their population.[17]

This instrument is then utilized in a two-stage least squares (2SLS) framework:

$$Y_i = \beta \cdot Place_i + X_i\gamma + \delta_{c(i)} + \delta_{m(i)} + \epsilon_i \tag{1}$$

$$Place_i = \alpha \cdot Exits_{c(i),m(i)} + X_i\Gamma + \Delta_{c(i)} + \Delta_{m(i)} + \nu_i \tag{2}$$

where i is a child index, $c(i)$ is the county that child i enters into, $m(i)$ is the month-year (ex: December 2013) that child i enters. This framework includes child controls X_i, county fixed effects $\delta_{c(i)}$, $\Delta_{c(i)}$ and month-year fixed effects $\delta_{m(i)}$,$\Delta_{m(i)}$. The endogenous variable is the placement variable $Place_i$ which is 1 if child i is initially placed in a non-kin foster family and a 0 if child i is initially placed in a group home.[18] The first stage is estimated through a linear probability model. When the outcome is a binary variable, such as homelessness or incarceration, I estimate a linear probability model in the second stage. β is the LATE and the parameter of interest in this setup. Standard errors are clustered at the county level throughout.

[17]Alternative ways to account county size are explored in Table A1.

[18]I explore whether the results change if we consider the whole placement experience of children. Due to the nature of placements, initial placements are quite predictive of full placement experiences and Table A19 shows the results are robust to this consideration.

2.2 Identifying Variation of the Instrument

The identifying variation for the instrument comes from idiosyncratic variation in exits within counties. In principle, this variation can come from families finishing court-mandated rehabilitation or other subjective case plan action items that allow for reunification as determined by social workers and judges,[19] or children reaching their birthday and being emancipated. I show in Table A2 that reunification-based exits provide the main source of identification. Note that because I control for general month-year fixed effects, this variation cannot come from nationwide changes in foster care policy. I also show that this variation is not driven mechanically by entries in previous months by controlling for total entries that occur in the same month in Table A1.

Figures A1 and A2 in the Appendix provide visualizations of the raw variation in the instrument. The unit of measurement is total non-kin exits in the month to make the visualizations interpretable. Figure A1 plots the variation in residual non-kin exits after controlling for county and month by year fixed effects within four counties. Residual exits varies between 20 exits under predicted by the fixed effects and 20 exits over predicted by the fixed effects for these counties. The average standard deviation of the residualized instrument $\tilde{Exits}_{c,m}$ across counties weighted by county size is 7.75 exits. For comparison, the average standard deviation of the residualized number of entries across counties weighted by county size is 8.27 entries. Figure A2 plots exits against non-kin placements at the county-by-month-year level for four counties in my data. There is a strong positive correlation between exits and placements in a month within each county.

2.3 First Stage

Figure 1 plots a regression spline model of the first stage and a weighted density of the instrument in an aggregated county-month-year form. This figure shows a strong relationship between the (residualized) instrument and placement at the county-month level. Table 2 gives the corresponding coefficients for this county-month-year regression. The weighted F-statistic is 40.7. The 0.0033 coefficient in column (1) of Table 2 can be interpreted as saying that if there are 10 extra non-kin exits than predicted the percent of entering foster children that is matched with non-kin families increases by 3.3 percentage points.

Estimates of the coefficient α corresponding to the disaggregated first stage equation (2)

[19] More information on how families can expect to be reunified with a child placed in foster care can be found here: https://www.childwelfare.gov/pubPDFs/reunification.pdf (accessed August 3, 2021).

are provided in Appendix Table A1. This Table also provides alternative ways to account for county size in the first stage. Table A1 shows that non-kin exits in the same county and month as a child entry is strongly correlated with the placement of that child in a non-kin foster family with large F-statistics. The preferred specification in Panel B gives an F-statistic of 43.0.[20] Overall, the first stage of the instrument is strong and is well over the standard thresholds cited in the literature for weak instruments including Stock and Yogo (2005) and Olea and Pflueger (2013).

2.4 Instrument Validity

The main identifying assumption underlying my empirical strategy is that non-kin exits affects a child's outcomes only by changing the probability of placement with a family. In particular, non-kin exits must be uncorrelated with unobservable characteristics of entering children that affect those children's future outcomes, conditional on county and month-year fixed effects. While my specifications account for secular trends such as county-wide foster care policy and a rich set of child-level observables it is possible there are still county-specific trends in exits that are correlated with unobservables. For example, more exits may signify "good times" for a county if they are correlated with local economic conditions, and entering children may be more acceptable to families and more likely to have good outcomes.

To assess whether exits proxy for important child characteristics I test whether exits appear quasi-randomly assigned to observable characteristics of children, conditional on the fixed effects. This test regresses the instrument on these observables (and county and month-year fixed effects) and tests the null hypothesis that all the coefficients on the child observables are 0. The results for three different samples are contained in Table 3. The table also contrasts this test with regressions of the endogenous placement variable on the same observables. This table shows the p-values for the F-test testing the null hypothesis that all coefficients in the regression are zero are above 0.05. The F-statistics are orders of magnitude smaller than the corresponding F-statistics in the placement regressions in columns (4) - (6). Moreover, the coefficient sizes in columns (1) - (3) are very small

[20]This strong correlation is robust across changes in the instrument specification, and the samples in which the instrument is defined. I include specifications that use other methods to account for county size differences including controlling for the total entries of children in the same month, and that using the log of one plus the raw exits. Of these instrument specifications in the outcome and eligible samples, the only one that does not have a strong first stage is log non-kin exits. Table A1 shows though that this is due to county representation since the old children sample has a strong first stage.

compared to the coefficient sizes in columns (4) - (6) suggesting that there is very little correlation with any of the child observables. To further assess the identifying assumption using these child observables I show that adding child demographics and entry reasons do not change the main results in Section 2.5 below.

These child observables may not serve as perfect proxies for the unobservables that may threaten my strategy and so I provide complementary evidence by examining the correlation between exits and outcomes for children before age 17. Table 4 shows that the instrument is not strongly correlated with earlier outcomes which suggests it is unlikely that exits correlate with a children's ex-ante likelihood of experiencing good or bad outcomes.

Another way to assess whether exit shocks signify an underlying shock to the types of children entering is to examine the correlation between kin placements and exits. The logic of this test is as follows: if the unobservable characteristics of children entering when there is an exit shock are such that they are children that are easier to care for in general, then these children should be more likely to be accepted by kin families. Table 5 shows that kin placement is not correlated with the instrument. The economic magnitude of the coefficient is small and it is not statistically significant. Similar to this test, Table A2 performs a placebo test and shows that group home exits do not predict family placement. These results suggest non-kin exits only affect placement through non-kin placement changes.

While non-kin exits may be independent of child characteristics, non-kin exits may signify other changes in the foster care system such as the services available to children due to decreased stress on the system. Another challenge is that non-kin exits may shift both whether a child is placed with a foster family, and the number of children in an average foster family placement.[21] I test both of these possibilities in Tables A3 and A4. Table A3 shows that exits are uncorrelated with the services children receive. Table A4 shows that children placed when there are more exits end up in larger families, working against the intuition that these effects would be driven by better smaller families. This evidence suggests that exits only affect a child's future outcomes through their placement.

The final assumption required for the validity of the instrument with heterogeneous treatment effects is monotonicity. I follow the literature by computing the first stage in various subsamples in the data (Bhuller, Dahl, Løken and Mogstad, 2020; Dobbie, Goldin and

[21] Smaller families may lead to better outcomes for children in the theoretical literature (Becker and Lewis, 1973) though the empirical literature has generally found null effects (Black, Devereux and Salvanes, 2005; Angrist, Lavy and Schlosser, 2010).

Yang, 2018). Appendix Tables A7 - A14 include first stage coefficients, standard errors and cluster robust F-statistics for 32 different subsamples of the outcome sample based off child demographics and entry reasons.[22] In all subsamples except for 2, the estimated coefficient is positive. The negative coefficients in the 2 differing subsamples are not estimated to be statistically significant.

2.5 Effects of Non-Kin Foster Family Placement vs. Group Homes on Child Outcomes

Table 6 contains the LATE estimates of the effect of family placement for older foster children on outcomes measured at age 21. It also compares the LATE estimates to the OLS estimates. Columns (1) to (4) include OLS and IV estimates of family placement on the outcome index. They also include specifications with and without demographic and entry reason controls. Columns (5) and (6) compare OLS and IV estimates of the effects on current employment or enrollment with controls, columns (7) and (8) compare OLS and IV estimates of the effects on incarceration between ages 20-21, columns (9) and (10) compare OLS and IV estimates of the effects on homelessness between ages 20-21, and columns (11) and (12) compare OLS and IV estimates of the effects on substance abuse referrals between ages 20-21.

Columns (3) and (4) show that the IV estimate represents a statistically significant and substantially large effect of marginal placements on economic and social outcomes for children. Initial placement with a non-kin family relative to a group home or institution improves outcomes by 0.97 or 0.99 standard deviations of the index for complier children. When the index is broken out into individual indices in columns (6), (8), (10) and (12), a statistically significant effect is identified for both homelessness and substance abuse. The LATE on initial non-kin family versus group home for incarceration is marginally statistically significant (p = 0.069) and for employment or enrollment is not statistically significant.

As an additional piece of evidence supporting my main identifying assumptions I include IV results with and without child demographics and entry reason controls for the outcome index. The coefficient barely moves providing further evidence supporting the assumptions of the empirical strategy, as adding a large set of child-level controls does not

[22] When the sample sizes are too small, less than 250, for a subgroup, they are left out of this exercise.

alter the coefficient substantially.[23]

One way to interpret the magnitudes for each outcome is to use the regression model to get a predicted probability of the outcome of an average child when placed in a group home vs. a non-kin foster family. For homelessness, this method predicts that if half of the children are placed in non-kin foster families then placement in a group home almost quadruples the chance a child ends up homeless.[24] Similar calculations give that group homes triple the chance that a child ends up incarcerated and increase the chances that a child ends up with a substance abuse referral by more than 10 times. The results on incarceration are similar to those in Ryan, Marshall, Herz and Hernandez (2008) who find that the risk of delinquency associated with group homes is 2.5 times that associated with other foster care settings, though obtained with a different method and in a different sample.[25]

Table 6 shows that the estimated LATE is larger than OLS. In Appendix Section A.3 I explore the causes of the LATE and OLS difference. In summary, I find evidence that heterogeneous treatment effects and measurement error could explain these differences. Furthermore, the model results in Section 4 are also consistent with treatment effect heterogeneity which drives the LATE to be larger than the OLS which is based on the average treatment effect on the treated (ATT) and a selection bias.

2.6 Other Reduced Form Results

Appendix A.4 contains more reduced form results aimed at understanding why families make marginal children better off than group homes including understanding the potential for connections to an adult (Table A17), public welfare take-up (Table A17) and adoption or guardianship (Table A18) to all influence outcomes. Placement leads to large increases in a child having a connection with an adult, a decrease in social service takeup and an

[23] I only look at comparisons with and without controls for the outcome index since they represent the results with the highest power.

[24] Mathematically the method uses two equations:

$$\bar{y} = (0.5)(\bar{y}(1)) + (0.5)(\bar{y}(0))$$
$$\bar{y}(1) - \bar{y}(0) = \beta_{IV}$$

to solve for the two unknowns where $\bar{y}$ is the overall mean, $\bar{y}(1)$ is the predicted outcome for children receiving treatment and $\bar{y}(0)$ is the predicted outcome for children receiving the control. For simplicity I assume the complier mean and the population mean are the same for this exercise.

[25] The differences between the propensity score results from Ryan, Marshall, Herz and Hernandez (2008) and the OLS results in Table 6 are quite large. However, the raw differences in incarceration rates by placement type for children in my sample are similar to Ryan, Marshall, Herz and Hernandez (2008) suggesting that the difference is because they undertake a proportional hazards survival analysis.

increase in the probability of being adopted. Appendix A.5 makes an explicit comparison of the reduced form results to the results in Doyle (2008). My results suggest that family placement settings can explain over 80 percent of the increase in the probability of incarceration that occurs when a child is removed from their birth family or guardian and placed in foster care in Cook County.

2.7 Non-Response Bias and Attrition

Table 1 showed differences in the placement rate of children who respond to the survey and children who are eligible to take the survey. Non-response bias and non-random attrition could bias the estimates in Table 6. Intuitively, one might expect that children that end up homeless or incarcerated are probably less likely to respond to the survey. If children are more likely to be homeless or incarcerated when placed in group homes, then this would bias the results downward and the estimates would be lower bounds. I provide some evidence in the data that is consistent with this intuition. I also test for correlation between the instrument and attrition and provide conservative bounds on the OLS and intent-to-treat effects that account for attrition.

I assess non-random non-response bias on observables following Sacerdote (2007) and the method developed by Wooldridge (1999) by using inverse propensity score weighting that models the probability of responding to the survey as a logistic regression on observable characteristics. Panel A of Table A20 looks at inverse propensity score weighted versions of the IV estimate. This panel shows that the IV estimates are larger if weighted on child observables that predict whether we observe a child's outcome at age 21.

I also assess whether response rates are correlated with treatment. Panel B of Table A20 shows there is not a statistically significant difference in response rate by the value of the exits instrument, the assignment to treatment in the proposed natural experiment. The p-values are 0.308 when considering all children eligible for the survey at age 21 (column (2)) and 0.616 when considering all children eligible for the survey in states that do not randomly sample children to survey at age 21 from those eligible (column (4)). For completeness I also compute Lee (2009) bounds in each sample on the OLS and ITT effects. The Lee (2009) bounds in both samples for the OLS and ITT effects are positive. Thus, I find that the instrument is not correlated with attrition and the Lee (2009) bounds give the same qualitative results.

2.8 Other Robustness

One further worry is that initial placements are not a good proxy for the overall placement setting experience of a child in foster care. Table A19 looks at the robustness of the results to how the endogenous variable is measured showing the choice of initial placement is not consequential for the main conclusions. Another worry is that the age cutoff of 14 in defining the sample is arbitrary and could be driving the results. Table A21 presents results using different age cutoffs and shows similar results. Finally, one may worry about the general robustness of the results to the exact definition of the instrument, the outcome index, and small changes in the sample considered. Table A22 looks at robustness of the results to alternative instrument definitions, outcome index definitions and dropping outlier observations. All results are qualitatively similar.

3 A Model of Foster Care Placement and Child Outcomes

Having established the importance of family placement settings for complier children with the IV approach, I now build a model of foster care placement and child outcomes to study counterfactual policies that could improve foster child outcomes. So far the methods do not provide a way to predict which children will be placed under different policies, do not describe which allocations of children to families are feasible, and can only predict counterfactual outcomes for compliers.

3.1 Market Definition

I model the placement of foster children into foster families and group homes as occurring in distinct markets delineated by location and time.[26] Each market t is a county-month-year tuple (ex: Los Angeles County, December, 2011). In each market there is a set of foster children entering I_t that must be placed and a set of available families J_t. I assume that the set of entering children is exogenous, and placed in a one-shot style. I discuss dynamics below in Section 3.5. Each child $i \in I_t$ can either be placed with one of the available families, in which case their placement is denoted $Place_{it} = 1$, or in a group home, in

[26] This choice is made due to the institutional details of foster care. Social workers and other stakeholders involved treat foster children's placement on a case-by-case basis due to the time constraints they face in placing children. Social workers are constrained by the law to find a placement for a child within a reasonable time frame of that child entering. In California, for example, this time frame is 24 or 48 hours.

which case their placement is denoted $Place_{it} = 0$. Family availability $|J_t|$ is allowed to be endogenous.

3.2 Foster Family Preferences

Families are assumed to have preferences over child characteristics. Table A23 shows clear patterns in the types of children more likely to be placed with families: girls are predicted to be more likely, black children are predicted to be less likely, and older children are predicted to be less likely. To capture these patterns, I assume that family preferences are homogeneous and vertical over child characteristics (Lancaster, 1966; Berry and Pakes, 2007). I model the utility of family j for child i as

$$u_{ji}(X_{it}) = X_{it}\alpha + \xi_{it} \tag{3}$$

where X_{it} contains observable characteristics of children (i.e. demographics) and $\xi_{it} \sim N(0, 1)$ is an unobservable taste shock for child i common to all foster families.[27] The econometrician does not observe ξ_{it} but families observe ξ_{it}. Equation (3) describes how families "rank" children based on their characteristics.[28] I assume that all children are acceptable to families in every foster care market conditional on a family entering. However, family entries are allowed to depend on the average utility of foster children in a market.[29]

3.3 Market Equilibrium

This paper follows the empirical literature on two-sided matching markets (e.g. Agarwal (2015)) in assuming that a market equilibrium in market t consists of a stable match between available families and entering foster children. I assume that children do not have preferences.[30] Because children have no preferences, and families have identical vertical

[27] I leave out stipends and payments out of X_{it} since these are notoriously poorly measured in the AFCARS data.

[28] Can the model allow for heterogeneous preferences? In a matching market like this one, heterogeneous preferences complicates the analysis since I can no longer use the simple cutoff structure used below to estimate preferences with a probit model and simulate counterfactual matchings (Agarwal, 2015; Agarwal and Somaini, 2020). We would instead need a more complicated approach.

[29] I cannot identify both the number of families in each market and the outside option. However, the model does allow for families to consider outside options before entering the foster care market since I allow for arbitrarily endogenous entry of families into each market.

[30] Note that social workers are part of the matching process and may have preferences or objectives that affect the matching (Robinson-Cortes, 2019). I abstract from this issue and discuss the implications of this

preferences, stability in this case is equivalent to assigning children in each market to maximize family utility.

The equilibrium condition implies that

$$Place_{it} = 1 \Leftrightarrow u_{it} \geq \bar{u}_t \tag{4}$$

where $\bar{u}_t$ is a market threshold utility.[31] I allow for the possibility of endogenous family entry by allowing that $|J_t|$ may be correlated with ξ_{it} which implies that $\bar{u}_t$ may be correlated with ξ_{it}. This type of correlation could be present if, for example, children have higher average values of ξ_{it} across different markets which attracts more foster families. To address this issue I utilize the exits instrument. I assume that the exits instrument $Exits_t$ is independent of ξ_{it} conditional on county and month-year fixed effects and also affects the threshold utility $\bar{u}_t$ in (4). Evidence consistent with this independence assumption includes the randomization test done in Section 2.4. I assume that the conditional expectation of the threshold utility is linear in the exits instrument:

$$\mathbb{E}[\bar{u}_t | Exits_t, c(t), m(t)] = \lambda Exits_t + \eta_{c(t)} + \eta_{m(t)} \tag{5}$$

where $\eta_{c(t)}$ and $\eta_{m(t)}$ are county and month-by-year fixed effects. The relationship between $\bar{u}_t$ and $Exits_t$ can be microfounded by assuming that $Exits_t$ affects the number of available families $|J_t|$ in each market t. The first stage in the instrumental variable analysis in Section 2.3 suggests this is true. Then, the model implies a direct relationship between $|J_t|$ and $\bar{u}_t$. Intuitively, as more exits occur, more families are added to each market which then lowers the cutoff utility required for a child to be matched. Instead of relying on strict functional form assumptions imposed by the model, I approximate this relationship using (5) which can capture this same monotonic relationship when $\lambda < 0$.

3.4 Foster Child Outcomes

I follow Heckman (1979), Kline and Walters (2016) and Walters (2018) and model the mean potential outcomes of children as depending on the observables and unobservable

assumption more below in Section 3.5.

[31] Formally the model implies that if there are $\#fams_t = |J_t|$ families in market t, $\bar{u}_t$ is the $\#fams_t$ highest value of the set of values $\{u_{it}\}_{i \in I_t}$ in market t. The cutoff structure used here is similar to Gandhi (2019) who also models a decentralized assignment market and relies on hospitals selecting patients with a high enough profit.

child taste shock:

$$\mathbb{E}[Y_i(1)|X_{it}, \xi_{it}, Exits_t] = X_{it}\beta_1 + \gamma_1\xi_{it},$$
$$\mathbb{E}[Y_i(0)|X_{it}, \xi_{it}, Exits_t] = X_{it}\beta_0 + \gamma_0\xi_{it}. \tag{6}$$

This model of outcomes includes the common assumption of separability between the observables and unobservables in determining outcomes (conditional on treatment) (Brinch, Mogstad and Wiswall, 2017).[32] Here β_0 and β_1 allow for children with different characteristics X_{it} to vary in their average potential outcomes, and to vary in the average impact of the treatment of being placed with a family relative to a group home. γ_0 and γ_1 allow for unobservable selection on levels and unobservable selection on gains by families over children (Roy, 1951).

3.5 Model Discussion

This model emphasizes a few important aspects of the foster care market. The first is that families are scarce which implies that only some children can be placed with families. The second is that child characteristics affect placements and outcomes, allowing for average outcomes to depend on which children are allocated to families. Importantly, I do not restrict the relationship between the allocation and outcomes. In this way, it is possible that family preferences are not "aligned" with children's outcomes, and families prefer children that benefit the least from a family vs. a group home.

3.5.1 Limitations

The model abstracts from a few important features of the foster care market.

Dynamics and Timing: The model treats all children entering in the same month as being placed at the same time. This approximates a reality in which children are allocated dynamically based on their entry time. My assumption on the market structure discretizes this dynamic process into monthly time blocks. This may introduce measurement error but provides a tractable way to use the available data to model placement. Another potential issue is that social workers may be able to change children's placements over a longer time

[32] An important implication of this assumption is that selection on unobservables "works the same way" for all subgroups of the observables (Kline and Walters, 2016).

horizon. There are two reasons this should not greatly affect the results here. First, children placed with families initially spend more than 80% of their time with families (author calculation in AFCARS). Second, the enormous case loads many social workers face with entering children suggest that the ability to actively seek new placements after entry is not feasible.

Social Worker Discretion: Social workers play no role in the placement of children in this model.[33] Because social workers make offers to families of foster children, it is possible that their preferences affect the allocation. For example, an alternative interpretation of how children are assigned in the model is that social workers forecast children's outcomes and assign children to maximize average outcomes. The results from my main counterfactuals that change the allocation of children to families are not affected by this interpretation, but this would affect the interpretation of how the observed allocation is reached and the appropriate policy instruments required to implement new allocations.

There are institutional reasons to believe that social workers have limited scope for determining child placements. Under child welfare laws, social workers are generally expected to make the best possible effort to find a child "the least restrictive home possible", and in my talks with social workers, they emphasized more heavily how family preferences influence placement. Thus, the model approximates the reality that social workers are solely meant to facilitate family placements for all foster children.

4 Model Estimation, Identification and Results

4.1 Estimation

The model is fit in two steps. Equations (4) and (5) imply that we can write selection into placement as

$$Place_{it} = \mathbf{1}\{X_{it}\alpha + \xi_{it} \geq \lambda Exits_t + \eta_{c(t)} + \eta_{m(t)}\}. \tag{7}$$

Under the parametric assumptions, I estimate the preference parameters α and the threshold utility shifter λ in (7) using a probit model.[34] I estimate the parameters in this equation

[33] There are no social worker identifiers in the AFCARS data as it is currently circulated, so it is difficult to separate out the role of social worker and family preferences.

[34] Agarwal and Somaini (2020) show how to estimate preferences when both sides of the market have vertical preferences. This model is a special case of the two sided vertical preference case with one side having trivial preferences.

using all children entering between ages 14-17 in all markets t that have a child that has a valid outcome index in the survey.

Using the law of iterated expectations in (6) we can write

$$\begin{aligned}\mathbb{E}[Y_i|X_{it}, Place_{it}, Exits_t] =& X_{it}\beta_0 + \gamma_0\mathbb{E}[\xi_{it}|X_{it}, Exits_t, Place_{it}] \\ &+ Place_{it} \cdot (X_{it}\beta_1 + \gamma_1\mathbb{E}[\xi_{it}|X_{it}, Exits_t, Place_{it}]])\end{aligned} \tag{8}$$

I form control function estimates of $\mathbb{E}[\xi_{it}|X_{it}, Exits_t, Place_{it}]$, $\hat{\xi}_{it}(X_{it}, Exits_t, Place_{it})$, using the allocation model parameters and the parametric assumption on ξ_{it}. Appendix A.6 describes the closed form for these control function estimates. Using the estimates $\hat{\xi}_{it}(X_{it}, Exits_t, Place_{it})$ for each child I run a second step regression to obtain the outcome parameters:

$$\begin{aligned}Y_i =& X_{it}\beta_0 + \gamma_0\hat{\xi}_{it}(X_{it}, Exits_t, Place_{it}) \\ &+ Place_{it} \cdot (X_{it}\beta_1 + \gamma_1\hat{\xi}_{it}(X_{it}, Exits_t, Place_{it})) + \omega_i.\end{aligned} \tag{9}$$

I estimate the outcomes on all children that have a valid outcome index Y_i and for whom a valid estimate of $\hat{\xi}_{it}$ can be formed.[35] Following Kline and Walters (2016) I normalize the covariate vector to have unconditional mean 0 so that the intercept coefficient in the coefficient vector β_1 can be interpreted as the average treatment effect (ATE). To avoid overfitting and due to power issues I only include an intercept and the following demographics in X_{it}: sex, age, and race. To compute standard errors for the parameters estimated in the second step, I utilize a block bootstrap clustered at the county level, with 250 bootstrap replications.

4.2 Identification of Model Parameters and Treatment Effects

The preference parameters are identified by looking at how often children of certain observables X_{it} surpass the modeled threshold $\mathbb{E}[\bar{u}_t|Exits_t, c(t), m(t)]$. The control function estimates depend on the instrument $Exits_t$ and X_{it}. Intuitively, when $Exits_t$ is high and a child is not placed the estimation procedure infers that the child has a low ξ_{it}. When $Exits_t$ is low and a child is placed, the estimation procedure infers that the child has a high ξ_{it}.

[35]Note that due to the model definition some children will not have a valid $\hat{\xi}_{it}$ estimate if the market they enter in has no variation in placement. This removes about 600 children from the original IV sample estimated on in Table 6.

When X_{it} are such that a child is predicted to have low utility and they are placed with a family, then the model infers a high ξ_{it}. When X_{it} are such that a child is predicted to have high utility and they are not placed with a family, the model infers a low ξ_{it}.

The model allows me to extrapolate the LATE to different treatment effects of interest. Appendix A.7 derives the form for the model ATT, ATNT and LATE.

4.3 Parameter Estimates

Table 7 gives parameter estimates for the the utility threshold shifter parameter λ and the preference parameters α in equation (7). The coefficient on the exits instrument is statistically significant and is of the expected sign: exits translate to lower utility thresholds. The preference parameters, while not directly quantitatively interpretable, show a few important patterns. First, boys are preferred to girls by families. Second, younger children are preferred to older children. Third, black children are the least preferred children on race by families. These patterns are consistent with the descriptive patterns in Table A23, work on the types of foster children placed in group homes (Ryan, Marshall, Herz and Hernandez, 2008), and work on the types of children adopted (Baccara, Collard-Wexler, Felli and Yariv, 2014).

Table 8 provides the selection corrected estimates of parameters $(\beta_0, \beta_1, \gamma_0, \gamma_1)$ estimated from (9). The outcome variable in this is the previously defined outcome index. The implied ATE is 1.423. While the ATE itself is not statistically significant at the 10% level ($p = 0.12$) the counterfactuals run below in Section 5 do find statistically significant results from adding families. I compare the model LATE and IV LATE below.

The model estimates that there is negative selection on levels in column (1) and negative selection on gains in column (2). The standard errors are quite large and do not permit a statistically precise conclusion. On observables, boys have a statistically significant higher treatment effect on the outcome index than girls ($p = 0.05$) consistent with a literature on gender differences in child interventions (Kling, Ludwig and Katz, 2005; Bertrand and Pan, 2013; Autor, Figlio, Karbownik, Roth and Wasserman, 2019). All other observables do not have statistically precise results. While the model cannot identify statistically significant relationships for each individual observable or unobservable the counterfactuals in Section 5 do find statistically precise results from changing how the matching occurs on collectively on different subsets of the observables and unobservables.

4.4 Treatment Effect Estimates

Table 9 shows the model estimated treatment effects. Column (1) compares the model LATE and the IV for the subsample of children used to estimate the model. While the coefficient estimates are quantitatively different, the 90% confidence intervals contain 0 and suggest that we cannot reject that the model and IV estimated LATEs are different.[36]

The model estimate ATT in column (3) is smaller than the model estimated ATNT in column (4). However the 90% confidence interval for the difference between these estimates contains 0. While we do not have enough power to statistically distinguish the ATT and ATNT, the counterfactuals in Section 5 suggest that, on average, the children that are matched benefit less than children that are not matched, and that policies that change which children are matched, either by design or randomly, would improve child outcomes. These results stem from the differences in the ATT and ATNT measured here.

Finally, the ATT and LATE difference provides one more piece of evidence for understanding the difference between the LATE and OLS in Table 6. The ATT is smaller than the LATE further suggesting that the smaller OLS could be due to treatment effect heterogeneity and a smaller ATT.

5 Counterfactuals on Family and Group Home Allocation

This section studies counterfactuals aimed at improving children's outcomes through family allocation. I consider all children 14-17 years old entering in the markets defined for the probit estimation in (7). First, I establish a baseline average outcome for children on the outcome index in the observed equilibrium allocation. The first row in column (1) in Table 10 gives these children's average outcomes.

All counterfactual matchings must satisfy two constraints. The first is the matching constraint that $Place_{it} \in \{0, 1\}$.[37] The second is that, if we do not change the number of families in a market t, $\#fams_t$, then total placement cannot exceed this family capacity: $\sum_{i \in I_t} Place_{it} \leq \#fams_t$. In the counterfactuals I will assume that subsidies make families willing to care for children in any proposed allocation.[38] The subsidies required

[36] Part of the reason for this seems to be that the model estimated LATE has a large standard error.

[37] Fractional matchings can be allowed but will not be optimal since children will have strictly different treatment effects due to the unobservables.

[38] To justify this assumption, suppose there is a foster care subsidy s_{it} paid for each child. If utility is strictly increasing in s_{it} (Doyle, 2007a) then there exists some stipend vector s_t that can support any matching of children with $\sum_{i \in I_t} Place_{it} = \#fams_t$. Estimating these elasticities and the supporting stipends requires

to implement a certain allocation are an important consideration for this type of policy but their exact computation is outside of the scope of this paper. In all counterfactuals I bootstrap simulate 250 versions of the counterfactual comparing it to the baseline with both a 90% confidence interval and counting the proportion of simulations in which the counterfactual outcome leads to worse outcomes than the baseline.

A prominent policy discussed in foster care is the addition of more families to the system with the intent of placing more children with families. To predict the effect of such a policy requires predicting the marginal child that is placed with a family. The model predicts that when a family is added to market t the $\#fams_t + 1$ highest ranked u_{it} among all u_{it} in market t will now be placed. Counterfactual outcomes for these children can be predicted by the model. To simulate the effects of a policy that adds families, I consider changing $\#fams_t$ in each market while holding fixed u_{it}. Figure 2 presents results from different percentage increases in the number of families in each market.[39] There are large improvements in children's outcomes as more families are added. Table 10 row 1 column (1) shows that if $\#fams_t$ increases by 50% in each market t, this leads to a gain in average outcomes for children of 24%. This is a large gain and a large increase in the number of families. In 5.6% of the bootstrap simulations the addition of families leads to a decrease in average outcomes suggesting a marginal statistically significant improvement in outcomes.[40]

Now I consider policies that hold fixed the number of families but change the allocation of children to families. First, I consider a random allocation that satisfies the matching constraints. This counterfactual gives a sense of how, in general, family preferences affect children's outcomes in foster care. Table 10 row 3 column (1) shows that average outcomes increase to 1.065, or an 11.2% improvement in outcomes. Column (2) shows that the 90% confidence interval does not contain 0 and only 3.6% of bootstrap simulations has the random matching having a lower average outcome than the baseline giving a sta-

it's own exogenous variation in stipend and a separate empirical strategy and data, and is out of the scope of this paper.

[39] It seems more appropriate to consider proportional changes in the number of families in each market, as opposed to discrete changes since the market sizes vary by quite a large amount (1 family in LA is very different from 1 family in a very small county).

[40] An important limitation of this counterfactual is that while we let marginal children differ in their benefits we assume that marginal families provide the same treatment effects. One way to address this issue is to augment the model by allowing treatment effects to depend on placement rates in a county and extrapolate treatment effects at higher placement rates using this cross-sectional variation across counties. The model detects that inframarginal families are less beneficial but still estimates a large gain to additional families: 50% of families leads to an 18.3% increase in outcomes for children.

tistically significant improvement in outcomes. The current allocation of children leads to lower child outcomes than a random allocation of children. The model interprets this as coming from family preferences: the characteristics that families prefer are characteristics that make treatment effects smaller.

I now turn to counterfactuals in which social workers can purposefully change the allocation of children to families to improve child outcomes. I first consider a simple scenario in which social workers observe the model estimates and notice that boys get higher treatment effects than girls and that girls are placed twice as often as boys. The social workers prioritize the placement of boys so that they place boys twice as often as girls. In the model this is simulated by changing the α_{boy} preference parameter and resimulating the equilibrium until the percentage of boys placed is twice as much as the percentage of girls placed. How do outcomes change when boys are prioritized in this way? Table 10 row 4 column (2) shows that average outcomes increase to 1.064, or an 11.1% increase in outcomes. Column (2) shows that the 90% confidence interval does not contain 0 and 2% of bootstrap simulations has this policy performing worse than the baseline outcome. Thus, a simple policy that reprioritizes boys can achieve statistically significant and large gains for average outcomes. This could be achieved by raising the subsidy for boys relative to girls. This type of subsidy differentiation is already present on age. Because girls are placed less often with families, their average outcomes are lowered from 1.30 to 0.99, a 27% decline. This compares to an increase in average outcomes for boys from 0.65 to 1.13, a 55% increase.

We can generalize a policy that prioritizes boys to one that prioritizes children with high treatment effects on all observables. To simulate this type of policy, I look at the children with the highest predicted treatment effects on observables assuming that ξ_{it} is unobserved to social workers and is at the prior mean $\xi_{it} = 0$. The treatment effect prediction for child i is $X_{it}(\hat{\beta}_1 - \hat{\beta}_0)$. I assume social workers place the children with the highest predicted treatment effects with families up to the constraint that the number of families in the market remains at the observed equilibrium level. Table 10 row 5 column (1) shows that if social workers have access to the child demographics, they could increase average outcomes up to 1.126 in a feasible allocation, which represents an approximate 17.3% improvement in average outcomes. Column (2) shows the 90% confidence interval does not contain 0 and column (3) shows that less than 1% of bootstrap simulations give that optimizing the allocation on observables leads to a lower average outcome than the baseline outcome. Thus, allocations that optimize on the observables have a statistically significant increase in outcomes for children. This allocation achieves approximately 72% of the gain that occurs from adding 50% more families.

It is possible, however, that social workers observe a proxy for the unobservable taste shock ξ_{it}. I approximate this case by assuming that social workers can predict treatment effects as $X_{it}(\hat{\beta}_1 - \hat{\beta}_0) + (\hat{\gamma}_1 - \hat{\gamma}_0)\hat{\xi}_{it}$ and reallocating children so that the highest ranked children on predicted treatment effects are matched. Table 10 row 6 column (1) shows that if social workers see both X_{it} and ξ_{it} then they can allocate children to increase outcomes to 1.156, which represents an approximate 20.3% increase in average outcomes. Column (2) shows the 90% confidence interval does not contain 0. None of the bootstrap simulations that optimize on both observables and unobservables lead to worse average outcomes for children than the baseline by definition of how we compute predicted outcomes. I find substantial gains from optimizing the allocation on both the observable and unobservable characteristics of children. This allocation achieves approximately 84% of the gain that occurs from adding 50% more families.

6 Conclusion

Foster care is an important social service in the US affecting hundreds of thousands of abused and neglected children every year. This paper performs a reduced form analysis of family and group home placement settings that builds on the existing literature using instrumental variables. These reduced form results combined with a model of foster care placement show that placing more children with non-kin families could substantially improve children's later outcomes. However, the results also show that better aggregate outcomes can be produced for foster children without more families by changing the allocation of children to families. This could be achieved by altering the existing subsidies paid to families for different children.

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7 Figures and Tables

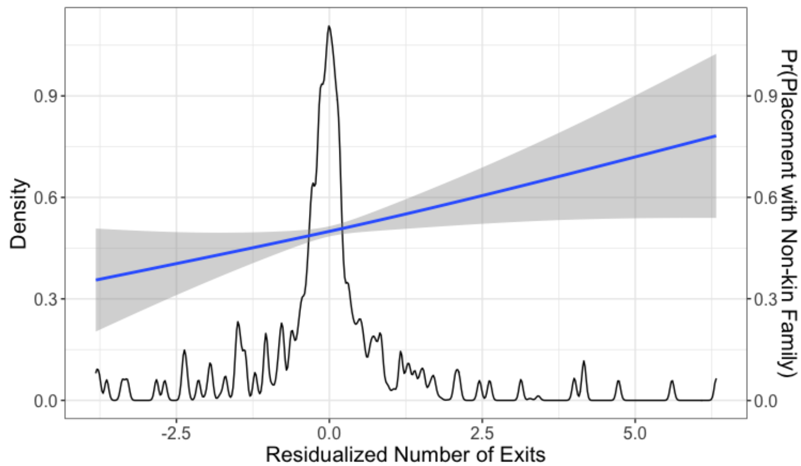

Figure 1: First Stage Variation

Notes: This figure shows the first-stage of non-kin family placement (vs. group home placement) on exits from non-kin families on the aggregated county-month-year sample (4,129 total observations). The x-axis plots the residualized number of exits divided by log population, residualized on county and month by year fixed effects. The y-axis on the right gives the probability of placement in a non-kin family. A generalized additive model with penalized regression splines is plotted along with 95% confidence bands. The density plot with y-axis on the left is a weighted density of the residualized number of exits divided by log population, where weights are given by the number of children in the corresponding county.

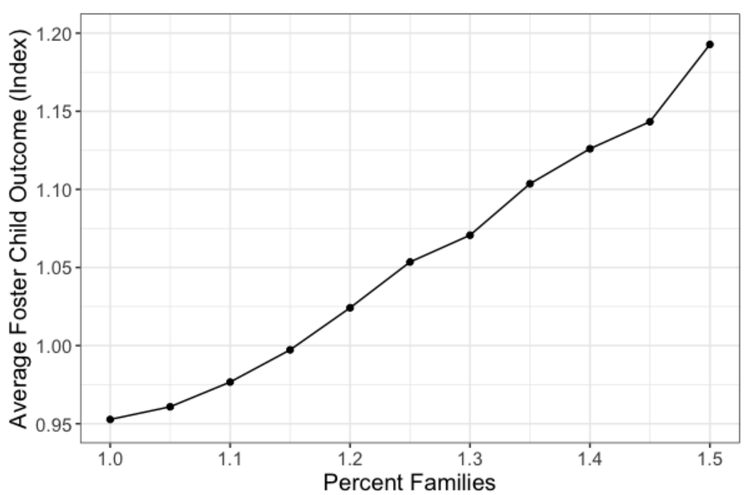

Figure 2: Child Outcomes from Adding Families

Notes: This figure shows average foster child outcomes on the outcome index simulated using the model. The x-axis measures the percent of families in each market t where 1.0 measure the current rate of families (100% capacity).

Table 1: Summary Statistics: Sample Means

	Outcome Sample (1)	Eligible Sample (2)	Old Children Sample (3)
Initial placement with non-kin family	0.500	0.432	0.379
Sex: male	0.420	0.507	0.524
Race: black	0.300	0.324	0.321
Race: white	0.443	0.426	0.418
Race/ethnicity: hispanic	0.206	0.200	0.198
Age at entry: 14	0.120	0.0980	0.215
Age at entry: 15	0.284	0.247	0.268
Age at entry: 16	0.532	0.556	0.295
Age at entry: 17	0.0630	0.0986	0.222
Economic and social outcome index	1.01 (SD = 2.08)	-	-
Currently employed or enrolled	0.687	-	-
Incarceration ages 20-21	0.225	-	-
Homeless ages 20-21	0.321	-	-
Substance abuse referral ages 20-21	0.127	-	-
Number observations	5,113	18,461	209,075

Notes: This table provides means of variables across three different samples. The sample definitions are provided in the main text. The outcome sample is defined as children that have a valid outcome index in the survey at age 21, are placed in a group home or non-kin family home for their first placement, and have their latest entry between ages 14 and 17. The eligible sample is defined as all children that were eligible for the survey at age 17, are placed in a group home or non-kin family for their first placement, and have their latest entry between ages 14 and 17. The difference in the number of observations of the outcome and eligible sample does not reflect true attrition, since children surveyed at age 21 must have responded at age 17. The old children sample is all foster children that are placed in a group home or non-kin family home for their first placement and entering between ages 14 and 17.

Table 2: First Stage Coefficients and F-Statistics

	% Children Placed in Non-Kin Families	
	(1)	(2)
Number non-kin exits	0.0033	0.0031
	(0.0008)	(0.0005)
Cluster robust F-statistic	16.2	40.7
Weighted	N	Y
County, month x year fes	Y	Y
Mean dep var	0.500	0.514
Number observations	4,129	4,129

Notes: This table shows OLS regressions of the endogenous variable of percent of children placed in non-kin families on the raw instrument, number of exits, across county-month-year cells. Column (2) further weights these regression results by the number of total children in the corresponding county. Standard errors are clustered at the county level. Table A1 has more comprehensive results at the child-level for the outcome, eligible and old children sample, and with different instrument specifications.

Table 3: Instrument and Endogenous Variable Correlation with Observables

	Instrument: Non-Kin Exits Month / log(Population)			Endogenous Variable: Initial Placement with Non-Kin Family		
	Outcome Sample	Eligible Sample	Old Children Sample	Outcome Sample	Eligible Sample	Old Children Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Sex: male	−0.022	−0.014	−0.011	−0.114***	−0.129***	−0.114***
	(0.018)	(0.011)	(0.007)	(0.012)	(0.007)	(0.004)
Race: white	−0.053	0.027	−0.151	−0.038	−0.042**	−0.013*
	(0.032)	(0.023)	(0.161)	(0.034)	(0.018)	(0.007)
Race: black	−0.039	0.032	−0.162	−0.071**	−0.053***	−0.020***
	(0.035)	(0.024)	(0.184)	(0.035)	(0.018)	(0.007)
Race: hispanic	−0.053	0.020	−0.145	−0.021	−0.020	0.013*
	(0.034)	(0.023)	(0.143)	(0.034)	(0.018)	(0.007)
Age: 15	−0.049	−0.027*	−0.002	−0.014	−0.037**	−0.028***
	(0.051)	(0.016)	(0.003)	(0.030)	(0.015)	(0.003)
Age: 16	−0.062	−0.008	−0.004	−0.018	−0.045***	−0.039***
	(0.050)	(0.019)	(0.003)	(0.030)	(0.016)	(0.004)
Age: 17	−0.004	−0.030	−0.009*	−0.039	−0.064***	−0.049***
	(0.040)	(0.021)	(0.005)	(0.038)	(0.016)	(0.006)
Physical abuse	0.002	0.026	0.015	0.107***	0.076***	0.093***
	(0.029)	(0.038)	(0.013)	(0.020)	(0.015)	(0.009)
Sexual abuse	0.068	0.001	−0.004	0.038	0.034**	0.064***
	(0.046)	(0.030)	(0.015)	(0.027)	(0.016)	(0.008)
Neglect	0.063	0.034	0.031	0.132***	0.109***	0.112***
	(0.045)	(0.038)	(0.026)	(0.020)	(0.013)	(0.011)
Parent alcohol abuse	0.041	0.0003	−0.029*	0.022	0.086***	0.072***
	(0.044)	(0.024)	(0.016)	(0.036)	(0.020)	(0.008)
Parent drug abuse	−0.028	−0.037**	−0.030*	0.083***	0.076***	0.081***
	(0.026)	(0.015)	(0.017)	(0.029)	(0.013)	(0.010)
Child alcohol abuse	−0.044	0.003	−0.037*	−0.092*	−0.043*	−0.036***
	(0.045)	(0.026)	(0.021)	(0.047)	(0.024)	(0.009)
Child drug abuse	0.043	0.009	−0.008	−0.061*	−0.085***	−0.095***
	(0.039)	(0.028)	(0.022)	(0.036)	(0.015)	(0.009)
Child disability	−0.020	−0.013	−0.035	−0.048	−0.068***	−0.069***
	(0.048)	(0.026)	(0.024)	(0.037)	(0.025)	(0.015)
Child behavior problem	−0.054	−0.053*	−0.047*	−0.265***	−0.242***	−0.253***
	(0.046)	(0.028)	(0.026)	(0.025)	(0.016)	(0.014)
Parent(s) died	−0.105	−0.081**	−0.017	0.063	0.115***	0.159***
	(0.083)	(0.040)	(0.012)	(0.061)	(0.037)	(0.013)
Parent(s) jail	−0.066	−0.011	0.0005	0.025	0.053**	0.063***
	(0.051)	(0.018)	(0.010)	(0.039)	(0.021)	(0.007)
Inability to cope	0.014	−0.016	0.004	0.049**	0.067***	0.074***
	(0.024)	(0.017)	(0.011)	(0.020)	(0.011)	(0.007)
Abandonment	0.001	0.009	−0.004	0.037	0.031*	0.037***
	(0.024)	(0.019)	(0.010)	(0.029)	(0.016)	(0.008)
Relinquished	0.042	0.064	0.015	0.127***	0.079***	0.081***
	(0.049)	(0.040)	(0.018)	(0.040)	(0.021)	(0.017)
Housing problem	−0.019	−0.001	−0.015	0.084***	0.059***	0.063***
	(0.049)	(0.022)	(0.028)	(0.030)	(0.015)	(0.008)
Number observations (children)	5,113	18,461	208,808	5,113	18,461	209,075
Mean outcome variable	1.98	1.83	1.74	0.5	0.432	0.379
R^2	0.976	0.968	0.954	0.440	0.331	0.296
F-statistic (p-value)	0.741	1.14	1.409	21.86	46.97	106
	(0.799)	(0.2955)	(0.0989)	(<0.001)	(<0.001)	(<0.001)
County, month x year fes	Y	Y	Y	Y	Y	Y

Notes: Columns (1)-(3) report OLS regression results from regressing the instrument, normalized by log population, on all child demographics and entry reasons. Columns (4)-(6) report OLS regression results from regressing the endogenous variable, initial placement in a non-kin family, on all child demographics and entry reasons. F-statistics are for statistical tests where the null hypothesis is that all coefficients on observables are 0. See Table 1 and the text of the paper for descriptions of the different samples. The instrument is not defined for some very small counties in the old children sample, explaining the discrepancy between the number of observations in columns (3) and (6). Standard errors are clustered at the county level. *p<0.1; **p<0.05; ***p<0.01

Table 4: Instrument Correlation with Earlier and Later Child Outcomes

	Outcome Index Age 17		Outcome Index Age 21	
	(1)	(2)	(3)	(4)
Non-kin exits month / log(pop)	0.0240	-0.0274	0.0947	0.0688
	(0.0277)	(0.0283)	(0.0328)	(0.0398)
County, month x year fes	Y	Y	Y	Y
Child demographic, entry reason controls	N	Y	N	Y
Number observations (children)	2,996	2,996	2,996	2,996

Notes: This table implements OLS regressions of outcomes at age 17 and before, and outcomes at age 21 on the instrument. To minimize the issue that outcomes at age 17 could be caused by placements at earlier ages, we focus on children removed at age 16 or 17. This is what causes the smaller sample than the outcome sample. We include specifications with and without demographic and entry reason controls. All specifications have county and month-by-year fixed effects. Standard errors are clustered at the county level.

Table 5: Instrument Correlation with Non-Kin and Kin Placement

	Placement with Non-Kin Family			Placement with Kin Family		
	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome Sample	Eligible Sample	Old Children Sample	Outcome Sample	Eligible Sample	Old Children Sample
Instrument: non-kin exits / log(pop)	0.022	0.014	0.012	0.005	0.002	−0.002
	(0.005)	(0.003)	(0.002)	(0.006)	(0.002)	(0.002)
Mean outcome variable	0.420	0.368	0.313	0.160	0.147	0.175
Number observations (children)	6,088	21,638	252,960	6,088	21,638	252,960
County, month x year fes	Y	Y	Y	Y	Y	Y
Child demographic, entry controls	Y	Y	Y	Y	Y	Y

Notes: Columns (1)-(3) give coefficient estimates on the instrument for a regression of placement with non-kin family on the instrument, demographic and entry reason controls and county and month-year fixed effects. Columns (4)-(6) do the same with a regression of placement with kin family. The samples in all columns are the same as in Table 3 but also include foster children whose initial placement is with a kin family. Standard errors are clustered at the county level.

Table 6: Impact of Non-kin Family Placement on Outcomes of Foster Children

	Economic and Social Outcome Index				Employment or Enrollment		Incarceration		Homelessness		Substance Abuse	
	OLS		IV		OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Initial non-kin family placement	0.886	0.646	2.016	2.056	0.0941	0.107	-0.115	-0.249	-0.078	-0.395	-0.048	-0.246
	(0.067)	(0.067)	(0.513)	(0.727)	(0.016)	(0.193)	(0.014)	(0.148)	(0.016)	(0.182)	(0.011)	(0.110)
County, month x year fes	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Child demographic, entry controls	N	Y	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number observations (children)	5,113				5,113		5,039		5,036		5,011	
Mean of outcome	1.01				0.687		0.227		0.321		0.128	
Sd of outcome	2.08				0.464		0.419		0.467		0.334	
First stage F-stat	43.0				43.0		34.3		34.8		37.2	
Instrument for IV specifications	Non-kin exits / log(population)											

Notes: This table presents OLS and IV results for β, the coefficient on initial non-kin family placement, in equation (1) for different outcome variables and with different specifications. Columns (1)-(4) present results with the economic and social outcome index, described in Section 2.3 which includes variables on employment, enrollment, incarceration, homelessness and substance abuse referrals. They include OLS results with and without the set of demographic and entry reason controls, and IV results with and without the set of demographic and entry reason controls. Columns (5)-(6) present OLS and IV results for an indicator variable for whether a child is employed or enrolled at age 21 (at the time of the survey). These only include specifications with full controls. Columns (7)-(8) present OLS and IV results for an indicator variable for whether a child has experienced incarceration in the past two years since the survey, surveyed at age 21. These only include specifications with full controls. Column (9)-(10) present OLS and IV results for an indicator variable for whether a child has experienced homelessness in the past two years since the survey, surveyed at age 21. These only include specifications with full controls. Columns (11)-(12) present OLS and IV results for an indicator variable for whether a child has had a substance abuse referral in the past two years, surveyed at age 21. The set of controls include demographics with age of entry categories, sex (male or female), and race (white, black, hispanic, other). The set of controls also includes a set of 15 indicator variables indicating the reasons a child was removed from their family.

Table 7: Placement Equation Parameter Estimates for Preferences and Instrument

	Placement with Non-Kin Family
	(1)
λ	
Non-kin exits / log(pop)	-0.081
	(0.012)
α	
Sex: Male	-0.627
	(0.060)
Age: 15 (ref 14)	-0.167
	(0.036)
Age: 16	-0.251
	(0.054)
Age: 17	-0.352
	(0.086)
Race: white (ref: other)	-0.129
	(0.053)
Race: black	-0.220
	(0.059)
Race: hispanic	-0.077
	(0.054)
County fes	Y
Month-Year fes	Y
Observations	38,543
Pseudo R^2	0.1364

Notes: This table shows results from the probit regression estimated in equation (7). The model includes fixed effects for counties and month-by-year with standard errors clustered at the county level.

Table 8: Selection Corrected Model Outcome Estimates

	Selection Corrected Model Estimates	
	Constant Effect	Interaction with Treatment
	(1)	(2)
ATE	1.423 (0.912)	-
Unobservable selection (γ_0, γ_1)	-0.199 (0.561)	-0.338 (0.231)
Male	-0.477 (0.206)	0.352 (0.180)
Age 15 (ref: age 14)	-0.299 (0.202)	0.143 (0.197)
Age 16	-0.324 (0.214)	0.228 (0.199)
Age 17	-0.295 (0.283)	0.118 (0.324)
Race white (ref: race other)	0.003 (0.241)	0.060 (0.273)
Race black	0.105 (0.241)	-0.132 (0.289)
Race hispanic	0.146 (0.246)	0.109 (0.281)
Number children	4,499	

Notes: This table presents estimates of the parameters in (9) using the outcome index defined in the text. Column (1) provides estimates of the estimated ATE, and β_0 and γ_0. Column (2) provides estimates of β_1 and γ_1. The sample of estimation is all children in the outcome sample for whom a valid control function estimate $\hat{\xi}_{it}$ can be formed due to sufficient variation in placement in their market. Standard errors for all parameters are computed using a block bootstrap where the blocks are counties with 250 bootstrap replications.

Table 9: Model and IV Treatment Effects

	Treatment Effect			
	LATE	ATE	ATT	ATNT
	(1)	(2)	(3)	(4)
Model	1.380 (0.914)	1.423 (0.912)	1.135 (0.925)	1.656 (0.930)
IV	1.879 (0.505)	-	-	-
90% confidence intervals $\beta_{IV} - \beta_{model}$	[-0.572, 4.26]	-	-	-
$ATNT - ATT$	-	-	[-1.068, 0.030]	
Number observations (children)	4,499			

Notes: This table computes model and IV derived treatment effects and confidence intervals for differences for those treatment effects. The IV LATE is the standard LATE computed from 2SLS while the model LATE, ATT and ATNT are derived in Appendix A.7. Standard errors are shown in parenthesis and are computed using a block bootstrap where the blocks are counties with 250 bootstrap replications.

Table 10: Counterfactuals on Scarcity and Allocation

Counterfactual	Mean Outcome (Index)	Mean Outcome - Baseline Mean Outcome 90% Confidence Interval	Proportion less than baseline
	(1)	(2)	(3)
Baseline	0.953	-	-
Add 50% families	1.193	[-0.003 0.452]	0.056
Random matching	1.065	[0.009, 0.237]	0.036
Place twice as many boys as girls	1.064	[0.023, 0.202]	0.020
Optimal matching on observables	1.126	[0.059, 0.333]	0.008
Optimal matching on observables and unobservables	1.156	[0.062, 0.399]	0

Notes: This table computes counterfactual outcomes for children in county-month-years that have a child in the survey data and have non-trivial variation in placement. Column (1) gives the mean outcome on the outcome index defined in the text. Column (2) gives 90% confidence intervals for the difference between the counterfactual mean and the baseline mean using block bootstrap where counties are blocks and we use 250 bootstrap replications. Column (3) gives the proportion of simulations of these 250 bootstrap replications where the counterfactual mean is less than the baseline using the same bootstrap technique. The details of each counterfactual are provided in the text.

A Online Appendix

A.1 Institutional Details Appendix

This paper uses exits of children from non-kin foster families as an instrumental variable for other children's placement with a non-kin family. Cherry and Orme (2013) document that in foster care there are two types of foster parents. There is a set of "vital few" foster mothers: foster mothers that account for a small proportion of foster parents in the system, and provide a disproportionate amount of care for children. Their analysis finds that 21% of foster mothers cared for 73% of foster children. In their sample, these foster parents fostered on average 104 children over almost 16 years of care. They adopt only 1.6 children on average. Other foster parents foster less but are more likely to adopt, caring for 11 children on average and adopting 0.8 children. It is thus conceivable that the availability of these foster parents that foster over many years could drastically impact a foster child's chances of being placed with a foster family, and that foster children's exits could affect availability of these foster parents. Foster parents that serially foster may differ in important ways from other families, and these differences may be correlated with differences in treatment effects at the family level. Cherry and Orme (2013) show that these serial fosterers are less likely to work outside the home and have more time to foster, along with more professional support for fostering.

A.2 Data Appendix

Important outcome variables in the NYTD survey:

- Incarceration: A youth is considered to have been incarcerated if the youth was confined in a jail, prison, correctional facility, or juvenile or community detention facility in connection with allegedly committing a crime (misdemeanor or felony).
  - For a 17-year-old youth in the baseline population, the data element relates to a youth's lifetime experience.
  - For a 19- or 21-year-old youth in the followup population, the data element relates to the youth?='s experience in the past two years.
- Homeless: A youth is considered to have experienced homelessness if the youth had no regular or adequate place to live. This definition includes situations where the youth is living in a car or on the street, or staying in a homeless or other temporary shelter.
  - For a 17-year-old youth in the baseline population, the data element relates to a youth's lifetime experience.
  - For a 19- or 21-year-old youth in the followup population, the data element relates to the youth's experience in the past two years.

- Substance abuse: A youth has received a substance abuse referral if the youth was referred for an alcohol or drug abuse assessment or counseling. This definition includes either a self-referral or referral by a social worker, school staff, physician, mental health worker, foster parent, or other adult. Alcohol or drug abuse assessment is a process designed to determine if someone has a problem with alcohol or drug use.
  - For a 17-year-old youth in the baseline population, the data element relates to a youth's lifetime experience.
  - For a 19- or 21-year-old youth in the followup population, the data element relates to the youth's experience in the past two years.
- Current enrollment and attendance: "Yes" means the youth is enrolled in and attending high school, GED classes, or postsecondary vocational training or college, as of the date of the outcome data collection. A youth is still considered enrolled in and attending school if the youth would otherwise be enrolled in and attending a school that is currently out of session.
- Current full time employment: A youth is employed full-time if employed at least 35 hours per week, in one or multiple jobs, as of the date of the outcome data collection.
- Current part time employment: A youth is employed part-time if employed between one and 34 hours per week, in one or multiple jobs, as of the date of the outcome data collection.
- Employment or enrollment (created variable): An indicator variable if current enrollment and attendance is 1 or current full time employment is 1 or current part time employment is 1.

Children with outcomes in the NYTD data at age 21 may have multiple entries and exits into and out of foster care before age 21. If a child has multiple entries, I take only their latest entry. In my main sample I only consider children whose latest entry occurred at age 14 or older. This makes the sample more representative of "older" foster children and removes children that enter very young but linger in foster care for a long time. Those children may be substantially different on unobservables than other older children in the sample. Robustness of the main results to different age cutoffs (ages 12, 13, and 15) are included in the Appendix and show that the choice of the cutoff is immaterial to the main results. Finally, because the instrumental variable strategy used in the analysis in this paper requires knowing a child's county of removal, children without an identified county of removal are dropped. Some small counties are not included in AFCARS because of privacy concerns (too few children are removed from their families).

I supplement the main AFCARS and NYTD data with NYTD services data which provides information on the services provided to foster children such as academic support, career preparation services and room and board financial assistance, and also measures their education at different points in time.

A.3 LATE and OLS

Table 6 shows that the estimated LATE is larger than OLS. Angrist and Pischke (2008) show that the OLS estimator is an average treatment effect on the treated and a selection bias term while the LATE is the average treatment effect on compliers.[41] Thus the discrepancy between the OLS and LATE in Table 6 could come from a difference between the ATT and LATE, or a negative selection bias.

I defer a more comprehensive comparison of the ATT and LATE to the model in Section 3 but provide some suggestive evidence on treatment effect heterogeneity on observables here. Table A5 follows methods developed in Abadie (2003) to examine OLS treatment effect estimates for compliers. Children with pre-existing conditions of homelessness and drug abuse are more likely to be compliers, and some of these children have OLS treatment effects close to the IV estimate, giving some suggestive evidence that hetereogeneity in treatment effects could play an important role in explaining the IV and OLS difference.[42]

An alternative but not mutually exclusive reason for the LATE-OLS discrepancy is measurement error in placements causing attenuation in OLS. Placements are reported every 6 months and children may change placements between the time of entry and the report time. To test for this possibility I look at OLS estimates in the subsample of children whose entries occur in the same month as the reporting period. Table A16 shows that the OLS estimate increases by almost 50% and can explain about 29% of the difference between OLS and IV difference.

A.4 Other Reduced Form Results: Mechanisms

Why do families make children better off relative to group homes? One potential pathway suggested in the literature is a meaningful sense of connection to an adult or family. This has been hypothesized to be an important component of a foster child's successful transition to adulthood (Freundlich and Avery, 2006).[43] However, achieving these connections can be challenging in practice, and little causal evidence has been found to suggest that

[41] Consider using the potential outcome framework for outcomes $Y_i(1)$ and $Y_i(0)$. Letting $P_i(E_i)$ be the placement treatment variable and E_i be a binary version of the instrument, following Angrist and Pischke (2008) we can write OLS and LATE as

$$OLS = \mathbb{E}[Y_i|P_i = 1] - \mathbb{E}[Y_i|P_i = 0] = \mathbb{E}[Y_i(1) - Y_i(0)|P_i = 1] + \mathbb{E}[Y_i(0)|P_i = 1] - \mathbb{E}[Y_i(0)|P_i = 0]$$
$$LATE = \mathbb{E}[Y_i(1) - Y_i(0)|P_i(1) > P_i(0)]$$

The OLS estimate measures an average treatment effect on the treated (ATT) $\mathbb{E}[Y_i(1) - Y_i(0)|P_i = 1]$ and a selection bias $\mathbb{E}[Y_i(0)|P_i = 1] - \mathbb{E}[Y_i(0)|P_i = 0]$ whereas the LATE measures an average treatment effect on compliers $P_i(1) > P_i(0)$.

[42] I also investigate the possibility that reweighting on observables can explain the discrepancy between the LATE and the OLS following Dahl, Kostol and Mogstad (2014) and Bhuller, Dahl, Løken and Mogstad (2020). Table A15 Column (2) performs this exercise. The change in the coefficient shows a very small increase in the estimated treatment effect.

[43] Biehal (2014) also studies what belonging means in substitute foster families.

foster children more easily develop these support systems and connections through family placements.

Table A17 Panel A columns (1) and (2) includes IV and OLS estimates of placement with a family on connections with an adult at age 21.[44] The IV estimate suggests a statistically significant 49 percentage point increase in the probability of developing a connection, or 57 percent on the mean outcome of 0.896. While methods to more formally test whether connection to an adult is an important mediator of the economic and social outcomes considered above are not appropriate in this setting (Dippel, Gold, Heblich and Pinto, 2020), the evidence is consistent with this connection to adult being correlated with these outcomes and potentially being an important mediator.[45]

The other results in Panel A of Table A17 show that the IV estimates do not estimate precise strong effects for other outcomes such as having children or receiving payments. The IV estimates do suggest that placement with a family leads to a large decrease in the probability of participating in an apprenticeship or on-the-job training during age 20. This could be consistent with families shifting children into more enrollment as opposed to employment to invest in human capital to increase lifetime earnings, but I lack the power to precisely test this hypothesis.

One important question about how children achieve better outcomes through placement with families is whether they rely on social services to achieve these gains. If so, this might dampen the overall monetary benefit of family placement, as this benefit comes with a social cost of welfare take-up. Panel B of Table A17 hows OLS and IV estimates of the effect of family placement on take-up of social services. It includes a measure of total public aid, which sums the social security, food stamps, housing vouchers and other cash welfare measures. The IV estimate suggests that placement in families leads children to take-up less public aid, with the results seeming especially strong (and marginally statistically significant) for food stamps and housing vouchers. The point estimate for educational aid take-up is negative though with wide confidence intervals.

The final set of results in this subsection look at potential mechanisms and mediators in intermediate outcomes in foster care including placement stability and permanency. These are closely studied in the literature (Becker, Jordan and Larsen, 2007; Koh and Testa, 2008; Andersen and Fallesen, 2015) but focus more on the differences in achieving stability and permanency in kin and non-kin placements. These outcomes are of first order importance to foster care policy makers as short-term markers of how well the foster care system is working. I contribute to this literature by looking at differences contributed by group homes and foster family placements. These could also be important mediators for the effects on

[44] The wording of the question involves that the adult is someone “who he or she can go to for advice or guidance when there is a decision to make or a problem solve, or for companionship when celebrating personal achievements. The adult must be easily accessible to the youth, either by telephone or in person. This can include, but is not limited to adult relatives, parents or foster parents.” (NYTD Outcomes Codebook p. 37).

[45] Interestingly the OLS coefficient estimates a precise 0 on connection to an adult for children. This is quite drastic and different, but consistent with the treatment effect heterogeneity found elsewhere, where family effects are amplified for the complier population.

social and economic outcomes estimated.

Table A18 shows IV and OLS estimates of adoption and guardianship by age 18 and the total number of placements after entry. Because these outcomes are observed in the AFCARS data, I examine the results in all three analysis samples, but the preferred specifications in columns (5) and (6) use the larger older children sample. The IV and OLS estimates in columns (5) and (6) both suggest that adoption and guardianship is shifted by a large and statistically significant percentage. The number of placement estimates are consistent but the IV is less precise and cannot reject 0 effects or even positive effects. These results show that placement with a foster family significantly boosts the probability of adoption or guardianship and they are consistent with placement increasing placement stability, though there is less precision for this result.

A.5 Comparison to Doyle (2008)

This section makes an explicit comparison to the literature looking at the causal effects of entry into foster care on subsequent outcomes. This paper provides one way to think about heterogeneity in the treatment of entry into foster care and shows that there can be substantial heterogeneity in foster care impacts on subsequent outcomes through placement types. Quantitatively, I compare the estimates in this paper to those found in the literature and perform some back-of-the-envelope calculations.

Doyle (2008) estimates the causal effect of foster care placement for children of average age 11 on incarceration at ages 18 or older in Cook County. He finds that placement into foster care causes a 22.5 percentage point increase in the probability of incarceration (Table 4, Panel C, Column 4) on a mean of 0.066 (Table 4, Panel C, Column 1). This paper shows that it is possible that placement into foster care *and* placement in a group home could be an important part of these negative effects, which are also found for other outcomes in Doyle (2007b).[46]

This paper estimates that the effect of placement with a family relative to a group home for children in foster care causes a 24.9 percentage point decrease in the probability of incarceration. Moreover, between 2005 and 2015, the placement rate of children into families (kin and non-kin) in Cook County for children entering between ages 14 and 17 is 0.264. For simplicity I assume that treatment effects are the same for kin families as for non-kin families relative to group homes.

Now suppose that the causal effect of placement into foster care estimated in Doyle (2008) can be written as

$$\beta_{overall} = \beta_0 + \beta_{family} F + e \tag{10}$$

where e is some random noise, so that the treatment effect is now a random coefficient that also depends on family placement. Using this setup and the numbers above, the expected treatment effect as a function of average family placement in Cook County can be written

[46]However, some recent studies have found positive effects on children. These include (Bald et al., 2019; Gross and Baron, Forthcoming).

as

$$\mathbb{E}[\beta_{overall}] = 0.291 - 0.249\mathbb{E}[F]. \tag{11}$$

Equation (11) gives a rough and simple way to understand the implications of family placement for the overall effect of foster care. If all children were placed in families in Cook County, this method would estimate that the probability increase in incarceration would be reduced to 4.2 percentage points, and that if no children were placed with families, the probability increase would jump up to 29.1 percentage points. This suggests a large role for family placements and placement types in understanding the overall effects of foster care. However, this example shows that even with full placement policy, there is an expected increase in incarceration. This result might suggest future research on studying how foster care shapes child outcomes through channels other than family placement or institutionalization, such as the trauma of being separated from a birth family.

A.6 Control Function Method

The condition for a child being placed with a family $Place_{it} = 1$ is:

$$u_{it} \geq \lambda Exits_t + \eta_{c(t)} + \eta_{m(t)} \tag{12}$$

which can be rewritten as

$$\xi_{it} \geq -X_{it}\alpha + (\lambda Exits_t + \eta_{c(t)} + \eta_{m(t)}) \tag{13}$$

Because $\xi_{it} \sim N(0,1)$ we can use properties of the truncated normal distribution which state that if a variable $z \sim N(0,1)$ then

$$\mathbb{E}[z|z > a] = \frac{\phi(a)}{1 - \Phi(a)} \tag{14}$$

where $\phi(\cdot)$ is the standard normal pdf and $\Phi(\cdot)$ is the standard normal cdf.

Applying (14) to this case we get that

$$\mathbb{E}[\xi_{it}|X_{it}, Place_{it} = 1, Exits_t, c(t), m(t)] = \frac{\phi(-X_{it}\alpha + (\lambda Exits_t + \eta_{c(t)} + \eta_{m(t)}))}{1 - \Phi(-X_{it}\alpha + (\lambda Exits_t + \eta_{c(t)} + \eta_{m(t)}))} \tag{15}$$

and I form plug in estimates of this by replacing parameters by those estimated in the first stage.

The computation is similar if $Place_{it} = 0$ using the fact that

$$\mathbb{E}[z|z < a] = \frac{-\phi(a)}{\Phi(a)} \tag{16}$$

A.7 Treatment Effect Method

To compute the LATE we need to characterize the distribution of X_{it} for compliers and the ξ_{it} of compliers. Suppose we make the instrument into a binary version $Z_t = \mathbf{1}\{Exits_t \geq \bar{Exits}_{c(t)}\}$ where $\bar{Exits}_{c(t)}$ is the mean exits in county $c(t)$. Let $\bar{u}_t(Z_t)$ be a function of the binary instrument and let $\bar{v}_t = -\bar{u}_t$. Then a complier satisfies

$$-\bar{v}_t(0) \geq u_{it} \geq -\bar{v}_t(1)$$

or

$$-(\eta_{c(t)} + \eta_{m(t)}) \geq X_{it}\alpha + \xi_{it} \geq -(\lambda + \eta_{c(t)} + \eta_{m(t)})$$

or

$$-(X_{it}\alpha + \eta_{c(t)} + \eta_{m(t)}) \geq \xi_{it} \geq -(X_{it}\alpha + \lambda + \eta_{c(t)} + \eta_{m(t)})$$

Thus the mean outcome for a complier child when placed is predicted to be

$$\begin{aligned}&\mathbb{E}[Y_{it}(1)|X_{it}, -(X_{it}\alpha + \eta_{c(t)} + \eta_{m(t)}) \geq \xi_{it} \geq -(X_{it}\alpha + \lambda + \eta_{c(t)} + \eta_{m(t)})] \\ &\quad = X_{it}\beta_1 + \gamma_1 \mathbb{E}[\xi_{it}| - (X_{it}\alpha + \eta_{c(t)} + \eta_{m(t)}) \geq \xi_{it} \geq -(X_{it}\alpha + \lambda + \eta_{c(t)} + \eta_{m(t)})]\end{aligned}$$

and similar for predicting the mean potential outcome when a complier is not placed.

To get the treatment effect I compute this $\hat{\mu}_i^c(1)(X_{it}) - \hat{\mu}_i^c(0)(X_{it})$ for each individual i. Then I compute the probability of being a complier conditional on observables as

$$p_i^c = Pr\Big(- (X_{it}\alpha + \eta_{c(t)} + \eta_{m(t)}) \geq \xi_{it} \geq -(X_{it}\alpha + \lambda + \eta_{c(t)} + \eta_{m(t)})\Big)$$

using the normal distribution assumption.

Finally I take a weighted average of these treatment effects, weighting by the probability each child i is a complier to get the implied LATE

$$L\hat{ATE} = \sum_i \left(\frac{p_i^c}{\sum_j p_j^c}\right)(\hat{\mu}_i^c(1)(X_{it}) - \hat{\mu}_i^c(0)(X_{it}))$$

To compute the ATT and ATNT similar methods are used. In particular, to compute the ATT I use the fact that a treated child satisfies

$$u_{it} \geq -\bar{v}_t$$

or

$$X_{it}\alpha + \xi_{it} \geq -(\lambda Exits_t + \eta_{c(t)} + \eta_{m(t)})$$

or

$$\xi_{it} \geq -(X_{it}\alpha + \lambda Exits_t + \eta_{c(t)} + \eta_{m(t)})$$

and then get the probability of each child i being treated according to the model.

To compute the ATNT I use the fact that a non-treated child satisfies

$$u_{it} \leq -\bar{v}_t$$

or

$$\xi_{it} \leq -(X_{it}\alpha + \lambda Exits_t + \eta_{c(t)} + \eta_{m(t)})$$

and then get the probability of each child i not being treated according to the model.

A.8 Appendix Figures and Tables

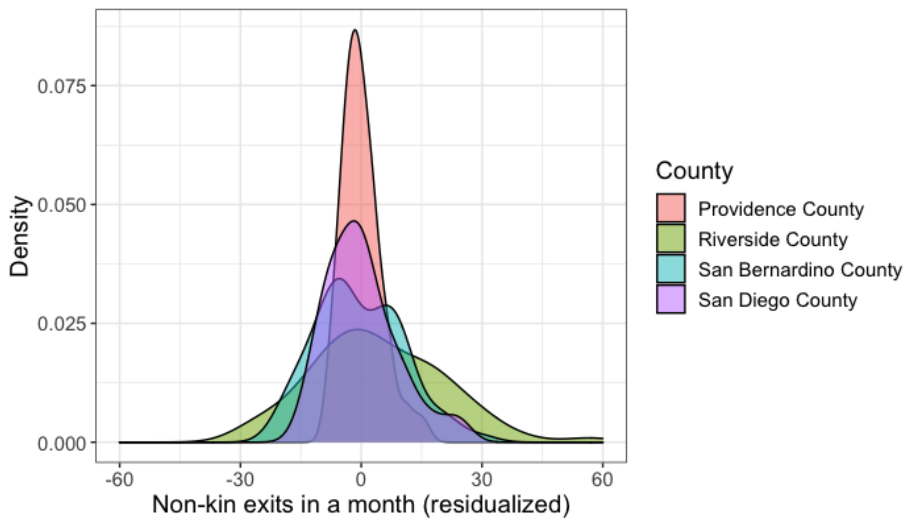

Figure A1: Raw Residualized Instrument Variation

Notes: This figure plots the residual of the exits instrument $\widetilde{\text{Exits}}_m$ on county and month-by-year fixed effects defined in the text across 4 different counties. Each observation contributing to the density plot for each county is a month-year.

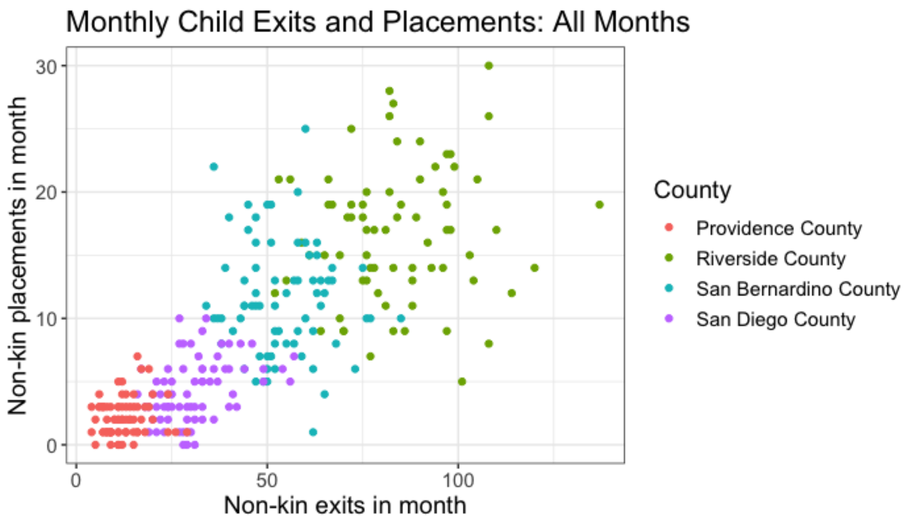

Figure A2: Raw Correlations for IV

Notes: These figures plot the instrument Exits_t at the county-month-year level against total non-kin placements at the same level for four counties in the data.

Table A1: First Stage

	Outcome Sample (1)	Eligible Sample (2)	Eligible Weighted (3)	Old Children Sample (4)	Old Children Weighted (5)
Panel A:					
Instrument: Non-kin exits					
First stage coefficient and s.e.	0.00206	0.00128	0.00091	0.00083	0.00088
	(0.00032)	(0.00019)	(0.00018)	(0.00025)	(0.00015)
Cluster robust F-statistic	41.7	43.5	23.7	10.7	33.2
Panel B:					
Instrument: Non-kin exits / log(county pop)					
First stage coefficient and s.e.	0.0319	0.0195	0.0146	0.0125	0.0137
	(0.0049)	(0.0032)	(0.0029)	(0.0042)	(0.0026)
Cluster robust F-statistic	43.0	36.8	25.1	9.0	27.8
Panel C:					
Instrument: Non-kin exits / log(county pop) w/ total entry control					
First stage coefficient and s.e.	0.0315	0.0209	0.0161	0.0154	0.0153
	(0.0048)	(0.0031)	(0.0031)	(0.0035)	(0.0026)
Cluster robust F-statistic	42.3	45.0	26.7	19.6	33.6
Panel D:					
Instrument: log(1+ non-kin exits)					
First stage coefficient and s.e.	0.0234	0.0064	0.0465	0.0202	0.0314
	(0.0142)	(0.0084)	(0.0171)	(0.0035)	(0.0119)
Cluster robust F-statistic	2.7	0.6	7.4	33.2	7.0
County, month x year fixed effects	Y	Y	Y	Y	Y
Child demographic, entry controls	Y	Y	Y	Y	Y
Weighted by county representation in outcome sample	N	N	Y	N	Y
Number observations (children)	5,113	18,461	18,461	209,075	209,075

Notes: This table reports OLS first stage coefficients and cluster robust F-statistics where standard errors and F-statistics are computed with county-clustered robust standard errors. Column (1) shows results in the outcome sample, column (2) shows results in the eligible sample, column (3) shows results in the eligible sample where observations are weighted by county representation in the outcome sample (observation weight = percent of observations in outcome sample with same county as observation), column (3) shows results in the old children sample and column (4) shows results in the old children sample where observations are weighted by county representation in the outcome sample. Panel A presents specifications with the raw instrument and no county normalization. These coefficients can be interpreted as the probability increase in placement with a family for one more exit of a child from a non-kin family in the same county-month-year in which the child exits through reunification or emancipation. Panel B presents specifications with instrument divided by log county population. Panel C presents specifications where the instrument is divided by log county population with an additional covariate of total entries in that same county-month-year. Panel D presents specifications where the instrument is log(1+exits) where exits is defined as in Panel A.

Table A2: First Stage from Reunification, Emancipation, and Group Home Exits

	Dependent Var: Placement with Non-Kin Family								
	Outcome Sample			Eligible Sample			Old Children Sample Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Non-kin exits from reunification / log(pop)	0.039 (0.006)			0.022 (0.006)			0.014 (0.003)		
Non-kin exits from emancipation / log(pop)		0.025 (0.020)			0.021 (0.011)			0.030 (0.005)	
Exits from group homes / log(pop)			-0.005 (0.013)			-0.002 (0.002)			-0.003 (0.002)
Child demographics and entry reason controls		Y			Y			Y	
County and month-by-year fes		Y			Y			Y	
Weighted by county representation in outcome sample		N			N			Y	
Number observations (children)		5,113			18,461			209,075	

Notes: This table reports OLS regression coefficients from three regression specifications on three samples. The first specification regresses the placement variable on non-kin exits due to reunification normalized by log population. The second specification regresses on non-kin exits due to emancipation from foster care normalized by log population. The third specification regresses on exits from group homes normalized by log population. These are run on the outcome sample, the eligible sample, and the old children sample. The old children sample is further weighted by county representation in the outcome sample. All three specifications include demographic and entry reason controls and and county and month-by-year fixed effects. All standard errors are clustered at the county level.

Table A3: Correlations between Instrument and Services Received at Entry

	Coefficient on instrument (1)	p-value (2)	Outcome mean (3)	Number observations (children) (4)
Special education services	-0.0042 (0.0027)	0.116	0.188	28,589
Independent living needs assessment	-0.0207 (0.0112)	0.0642	0.487	28,589
Academic support services	-0.0195 (0.0124)	0.115	0.501	28,589
Career services	-0.0274 (0.0193)	0.156	0.295	28,589
Employment vocational services	-0.0301 (0.0211)	0.156	0.144	28,589
Financial management services	-0.0278 (0.0162)	0.0876	0.283	28,589
Housing education and management	-0.0265 (0.0210)	0.207	0.329	28,589
Health education	-0.0262 (0.0133)	0.0492	0.364	28,589
Mentor services	-0.0305 (0.0160)	0.0565	0.168	28,589
Educational financial assistance	-0.0457 (0.0268)	0.0887	0.0858	28.589
Other financial assistance	-0.0432 (0.0290)	0.137	0.167	28,589
Instrument	non-kin exits / log(pop)			
County, month x year fes	Y			
Child demographic, entry reason controls	Y			

Notes: Each row of this table is associated with a separate regression of a different service outcome on a child entry. Each of these regressions includes demographic, entry reason controls, and county and month by year fixed effects. The sample for each regression is all children entering between 14 and 17 years old receiving any services as defined in the NYTD services database.

Table A4: Correlations between Instrument and Number of Children in Family Placement

	Number Children in Family Placement (1)	Indicator for More Than 1 Child in Family Placement (2)
Non-kin exits / log(pop)	0.1327 (0.0501)	0.0432 (0.0218)
County, month x year fes	Y	Y
Child demographic, entry reasons	Y	Y
Children placed with families only	Y	Y
Mean outcome	2.25	0.553
Number observations (children)	2,071	2,071

Notes: Column (1) provides the coefficient estimate on the instrument for a regression of number of children estimated in a child's initial placement for children from the outcome sample placed with a family who also have a valid measure of number of children in placement. A family has a valid number of children in their placement if, after accounting for the sequential arrival and exit of foster children in the AFCARS data, they have 8 or less children in their care. A family is identified by a unique sequence of county, family structure, age of primary caretaker, age of secondary caretaker, race of primary caretaker and secondary caretaker. Column (2) provides the coefficient estimate on the instrument for a regression of an indicator of having more than 1 child in a placement. Standard errors are clustered at the county level.

Table A5: Condensed Complier Table

	First Stage Dep Var: Placement (1)	Reduced Form Dep Var: Outcome Index (2)	IV Dep Var: Outcome Index (3)	OLS Dep Var: Outcome Index (4)	First Stage Eligible Sample (5)
Full Sample					
Coefficient and s.e.	0.0454 (0.006)	0.0916 (0.022)	2.016 (0.513)	0.886 (0.067)	0.0193 (0.003)
Cluster robust F-statistic	64.6	-	-	-	36.1
Number of children	5,113				18,461
Subgroup: Drug Abuse Child					
Coefficient and s.e.	0.256 (0.109)	1.093 (0.799)	4.274 (1.834)	0.916 (1.011)	0.0554 (0.018)
Cluster robust F-statistic	5.5	-	-	-	9.8
Number of children	214				1,103
Subgroup: Housing Problems					
Coefficient and s.e.	0.215 (0.038)	0.594 (0.301)	2.768 (1.127)	2.234 (0.923)	-0.0477 (0.016)
Cluster robust F-statistic	31.7	-	-	-	8.6
Number of children	252				979
Subgroup: Age 15					
Coefficient and s.e.	0.061 (0.015)	0.083 (0.055)	1.357 (0.815)	0.959 (0.131)	0.0435 (0.007)
Cluster robust F-statistic	17.0	-	-	-	38.7
Number of children	1,454				4,560
Subgroup: No Neglect					
Coefficient and s.e.	0.0504 (0.0076)	0.147 (0.035)	2.917 (0.754)	0.891 (0.095)	0.0334 (0.00498)
Cluster robust F-statistic	44.4	-	-	-	44.8
Number of children	3,109				11,688

Notes: This table presents first stage, reduced form (ITT), instrumental variable and OLS regression results for different subsamples of the outcome sample. It also includes the first stage regressions in the eligible sample. All models include county and month by year fixed effects. Standard errors are clustered at the county level. The full sample is the entire outcome sample. The drug abuse child subsample is children that enter at least in part due to their use of narcotics. The housing problems subsample is children that enter at least in part due to inadequate housing, including homelessness. The age 15 subgroup is children whose entry is at age 15. The no neglect subsample is children who do not enter because of a failure to provide adequate food, clothing shelter or care.

Table A6: OLS and IV with Demographic Controls Only

	Economic and Social Outcome Index	
	OLS	IV
	(1)	(2)
Initial placement with a non-kin family	0.790	1.907
	(0.065)	(0.549)
County, month x year fes	Y	
Child demographic controls only	Y	
Number children	5,113	

Notes: This table shows OLS and IV results for the outcome index in the outcome sample in which the only controls are child demographic controls. No removal reason controls are used. Standard errors are clustered at the county level.

Table A7: Heterogeneous Effects: Gender and Race

	First Stage (1)	Reduced Form (2)	IV (3)	OLS (4)	First Stage Eligible Sample (5)
Subgroup: Female					
Coefficient and s.e.	0.044	0.0498	1.125	0.682	0.0269
	(0.0091)	(0.0308)	(0.671)	(0.079)	(0.0067)
Cluster robust F-statistic	23.5	-	-	-	15.88
Number of children		2,967			
Instrument		Non-kin exits / log(population)			
Subgroup: Male					
Coefficient and s.e.	0.038	0.153	4.064	0.943	0.0271
	(0.010)	(0.036)	(1.292)	(0.124)	(0.0050)
Cluster robust F-statistic	14.0	-	-	-	28.50
Number of children		2,146			
Instrument		Non-kin exits / log(population)			
Subgroup: Black					
Coefficient and s.e.	0.0238	0.071	2.46	0.731	0.020
	(0.0122)	(0.070)	(3.12)	(0.121)	(0.0072)
Cluster robust F-statistic	3.8	-	-	-	7.8
Number of children		1,532			
Instrument		Non-kin exits / log(population)			
Subgroup: Hispanic					
Coefficient and s.e.	0.0541	0.163	3.092	1.127	0.043
	(0.0129)	(0.043)	(0.872)	(0.151)	(0.0047)
Cluster robust F-statistic	17.6	-	-	-	85.9
Number of children		1,051			
Instrument		Non-kin exits / log(population)			
Subgroup: White					
Coefficient and s.e.	0.0467	0.022	0.473	0.979	0.0236
	(0.015)	(0.079)	(1.68)	(0.12)	(0.011)
Cluster robust F-statistic	10.13	-	-	-	4.25
Number of children		2,265			
Instrument		Non-kin exits / log(population)			

Notes: This table shows OLS results for the first stage (initial placement with family indicator on instrument), reduced form (outcome index on instrument), IV treatment effects and OLS treatment effects in the outcome sample, and the first stage in the eligible sample in different subgroups of the data based on child observables. All models do not include child demographic and entry reason controls. All models include county and month-by-year fixed effects, and standard errors are clustered at the county level. Blank entries in the table indicate that the sample size is too small to provide informative estimates.

Table A8: Heterogeneous Effects: Age

	First Stage	Reduced Form	IV	OLS	First Stage Eligible Sample
	(1)	(2)	(3)	(4)	(5)
Subgroup: Age 14					
Coefficient and s.e.	-0.007	0.0264	-	0.773	-0.023
	(0.0023)	(0.0867)		(0.178)	(0.025)
Cluster robust F-statistic	0.09	-	-	-	0.86
Number of children		615			1,809
Instrument		Non-kin exits / log(population)			
Subgroup: Age 15					
Coefficient and s.e.	0.061	0.083	1.357	0.959	0.0435
	(0.015)	(0.055)	(0.815)	(0.131)	(0.0070)
Cluster robust F-statistic	17.0	-	-	-	38.7
Number of children		1,454			4,560
Instrument		Non-kin exits / log(population)			
Subgroup: Age 16					
Coefficient and s.e.	0.034	0.061	1.792	0.949	0.0276
	(0.0078)	(0.030)	(0.989)	(0.098)	(0.00345)
Cluster robust F-statistic	19.3	-	-	-	64.3
Number of children		2,722			10,272
Instrument		Non-kin exits / log(population)			
Subgroup: Age 17					
Coefficient and s.e.	0.0623	0.875	-	1.140	0.020
	(0.0500)	(0.206)	-	(0.592)	(0.020)
Cluster robust F-statistic	1.6	-	-	-	1.0
Number of children		322			1,820
Instrument		Non-kin exits / log(population)			

Notes: This table shows OLS results for the first stage (initial placement with family indicator on instrument), reduced form (outcome index on instrument), IV treatment effects and OLS treatment effects in the outcome sample, and the first stage in the eligible sample in different subgroups of the data based on child observables. All models do not include child demographic and entry reason controls. All models include county and month-by-year fixed effects, and standard errors are clustered at the county level. Blank entries in the table indicate that the sample size is too small to provide informative estimates.

Table A9: Heterogeneous Effects: Physical and Sexual Abuse

	First Stage (1)	Reduced Form (2)	IV (3)	OLS (4)	First Stage Eligible Sample (5)
Subgroup: No Physical Abuse					
Coefficient and s.e.	0.049	0.0806	1.648	0.930	0.0288
	(0.0052)	(0.0236)	(0.510)	(0.071)	(0.0042)
Cluster robust F-statistic	88.5	-	-	-	46.8
Number of children		4,550			16,666
Instrument		Non-kin exits / log(population)			
Subgroup: Physical Abuse					
Coefficient and s.e.	0.0277	0.143	5.185	0.664	0.0177
	(0.0191)	(0.061)	(3.261)	(0.231)	(0.00863)
Cluster robust F-statistic	2.1	-	-	-	4.2
Number of children		563			1,795
Instrument		Non-kin exits / log(population)			
Subgroup: No Sexual Abuse					
Coefficient and s.e.	0.0459	0.0888	1.935	0.921	0.0290
	(0.0057)	(0.0223)	(0.514)	(0.070)	(0.00379)
Cluster robust F-statistic	65.4	-	-	-	58.7
Number of children		4,670			17,241
Instrument		Non-kin exits / log(population)			
Subgroup: Sexual Abuse					
Coefficient and s.e.	0.0254	-0.151	-	0.156	0.0391
	(0.0421)	(0.098)		(0.251)	(0.0150)
Cluster robust F-statistic	0.4	-	-	-	6.8
Number of children		443			1,220
Instrument		Non-kin exits / log(population)			

Notes: This table shows OLS results for the first stage (initial placement with family indicator on instrument), reduced form (outcome index on instrument), IV treatment effects and OLS treatment effects in the outcome sample, and the first stage in the eligible sample in different subgroups of the data based on child observables. All models do not include child demographic and entry reason controls. All models include county and month-by-year fixed effects, and standard errors are clustered at the county level. Blank entries in the table indicate that the sample size is too small to provide informative estimates.

Table A10: Heterogeneous Effects: Neglect and Inability to Cope

	First Stage (1)	Reduced Form (2)	IV (3)	OLS (4)	First Stage Eligible Sample (5)
Subgroup: No Neglect					
Coefficient and s.e.	0.0504	0.147	2.917	0.891	0.0334
	(0.0076)	(0.035)	(0.754)	(0.095)	(0.00498)
Cluster robust F-statistic	44.4	-	-	-	44.8
Number of children		3,109			11,688
Instrument		Non-kin exits / log(population)			
Subgroup: Neglect					
Coefficient and s.e.	0.0248	-0.0036	-0.145	0.736	0.0058
	(0.0072)	(0.0341)	(1.382)	(0.113)	(0.00495)
Cluster robust F-statistic	11.78	-	-	-	1.36
Number of children		2,004			6,773
Instrument		Non-kin exits / log(population)			
Subgroup: No Inability to Cope					
Coefficient and s.e.	0.0458	0.143	3.116	0.840	0.0309
	(0.0062)	(0.024)	(0.648)	(0.088)	(0.0061)
Cluster robust F-statistic	55.32	-	-	-	25.5
Number of children		3,964			14,681
Instrument		Non-kin exits / log(population)			
Subgroup: Inability to Cope					
Coefficient and s.e.	0.0360	-0.0024	-0.067	0.931	0.0293
	(0.010)	(0.0478)	(1.334)	(0.145)	(0.0068)
Cluster robust F-statistic	12.4	-	-	-	18.7
Number of children		1,149			
Instrument		Non-kin exits / log(population)			

Notes: This table shows OLS results for the first stage (initial placement with family indicator on instrument), reduced form (outcome index on instrument), IV treatment effects and OLS treatment effects in the outcome sample, and the first stage in the eligible sample in different subgroups of the data based on child observables. All models do not include child demographic and entry reason controls. All models include county and month-by-year fixed effects, and standard errors are clustered at the county level. Blank entries in the table indicate that the sample size is too small to provide informative estimates.

Table A11: Heterogeneous Effects: Alcohol Abuse, Drug Abuse Parent

	First Stage (1)	Reduced Form (2)	IV (3)	OLS (4)	First Stage Eligible Sample (5)
Subgroup: No Alcohol Abuse Parent					
Coefficient and s.e.	0.0460 (0.0058)	0.0953 (0.0224)	2.073 (0.529)	0.889 (0.067)	0.0294 (0.0041)
Cluster robust F-statistic	63.2	-	-	-	51.8
Number of children		4,919			17,776
Instrument		Non-kin exits / log(population)			
Subgroup: Alcohol Abuse Parent					
Coefficient and s.e.	-	-	-	-	-
Cluster robust F-statistic	-	-	-	-	-
Number of children		-			-
Instrument		Non-kin exits / log(population)			
Subgroup: No Drug Abuse Parent					
Coefficient and s.e.	0.0459 (0.0056)	0.0896 (0.0241)	1.950 (0.577)	0.937 (0.068)	0.0312 (0.00364)
Cluster robust F-statistic	67.7	-	-	-	73.3
Number of children		4,672			16,607
Instrument		Non-kin exits / log(population)			
Subgroup: Drug Abuse Parent					
Coefficient and s.e.	0.0601 (0.0460)	-0.138 (0.289)	-	0.389 (0.411)	0.0112 (0.0158)
Cluster robust F-statistic	1.7	-	-	-	0.5
Number of children		441			1,854
Instrument		Non-kin exits / log(population)			

Notes: This table shows OLS results for the first stage (initial placement with family indicator on instrument), reduced form (outcome index on instrument), IV treatment effects and OLS treatment effects in the outcome sample, and the first stage in the eligible sample in different subgroups of the data based on child observables. All models do not include child demographic and entry reason controls. All models include county and month-by-year fixed effects, and standard errors are clustered at the county level. Blank entries in the table indicate that the sample size is too small to provide informative estimates.

Table A12: Heterogeneous Effects: Alcohol Abuse, Drug Abuse Child

	First Stage	Reduced Form	IV	OLS	First Stage Eligible Sample
	(1)	(2)	(3)	(4)	(5)
Subgroup: No Alcohol Abuse Child					
Coefficient and s.e.	0.0461	0.0936	2.028	0.875	0.0299
	(0.0056)	(0.0218)	(0.513)	(0.068)	(0.0042)
Cluster robust F-statistic	68.3	-	-	-	50.0
Number of children		5,016			18,045
Instrument		Non-kin exits / log(population)			
Subgroup: Alcohol Abuse Child	-	-	-	-	-
Coefficient and s.e.	-	-	-	-	-
Cluster robust F-statistic	-	-	-	-	-
Number of children		-			
Instrument		Non-kin exits / log(population)			
Subgroup: No Drug Abuse Child					
Coefficient and s.e.	0.0471	0.0917	1.947	0.853	0.0294
	(0.0057)	(0.0205)	(0.469)	(0.071)	(0.0044)
Cluster robust F-statistic	68.3	-	-	-	44.7
Number of children		4,899			17,358
Instrument		Non-kin exits / log(population)			
Subgroup: Drug Abuse Chld					
Coefficient and s.e.	0.256	1.093	4.274	0.916	0.0554
	(0.109)	(0.799)	(1.834)	(1.011)	(0.0177)
Cluster robust F-statistic	5.5	-	-	-	9.8
Number of children		214			1,103
Instrument		Non-kin exits / log(population)			

Notes: This table shows OLS results for the first stage (initial placement with family indicator on instrument), reduced form (outcome index on instrument), IV treatment effects and OLS treatment effects in the outcome sample, and the first stage in the eligible sample in different subgroups of the data based on child observables. All models do not include child demographic and entry reason controls. All models include county and month-by-year fixed effects, and standard errors are clustered at the county level. Blank entries in the table indicate that the sample size is too small to provide informative estimates.

Table A13: Heterogeneous Effects: Child Disability, Behavioral Problem

	First Stage	Reduced Form	IV	OLS	First Stage Eligible Sample
	(1)	(2)	(3)	(4)	(5)
Subgroup: No Child Disability					
Coefficient and s.e.	0.0458	0.103	2.247	0.925	0.0290
	(0.0057)	(0.022)	(0.543)	(0.068)	(0.0040)
Cluster robust F-statistic	63.7	-	-	-	51.3
Number of children		4,905			17,660
Instrument		Non-kin exits / log(population)			
Subgroup: Child Disability					
Coefficient and s.e.	-0.235	0.456	-	0.133	-0.0841
	(0.216)	(1.165)		(0.486)	(0.0365)
Cluster robust F-statistic	1.2	-	-	-	5.3
Number of children		208			801
Instrument		Non-kin exits / log(population)			
Subgroup: No Child Behavior Problem					
Coefficient and s.e.	0.0346	0.0484	1.401	0.795	0.0208
	(0.0057)	(0.0272)	(0.779)	(0.082)	(0.0048)
Cluster robust F-statistic	35.9	-	-	-	18.6
Number of children		3,039			9,886
Instrument		Non-kin exits / log(population)			
Subgroup: Child Behavior Problem					
Coefficient and s.e.	0.0193	0.0755	-	0.628	0.0074
	(0.0124)	(0.0485)		(0.133)	(0.0047)
Cluster robust F-statistic	2.4	-	-	-	2.5
Number of children		2,074			8,575
Instrument		Non-kin exits / log(population)			

Notes: This table shows OLS results for the first stage (initial placement with family indicator on instrument), reduced form (outcome index on instrument), IV treatment effects and OLS treatment effects in the outcome sample, and the first stage in the eligible sample in different subgroups of the data based on child observables. All models do not include child demographic and entry reason controls. All models include county and month-by-year fixed effects, and standard errors are clustered at the county level. Blank entries in the table indicate that the sample size is too small to provide informative estimates.

Table A14: Heterogeneous Effects: Relinquishment, Abandonment, Housing Problems

	First Stage (1)	Reduced Form (2)	IV (3)	OLS (4)	First Stage Eligible Sample (5)
Subgroup: No Abandonment					
Coefficient and s.e.	0.0492 (0.0063)	0.0957 (0.0215)	1.945 (0.483)	0.887 (0.073)	0.0287 (0.0042)
Cluster robust F-statistic	60.4	-	-	-	47.0
Number of children		4,579			16,556
Instrument		Non-kin exits / log(population)			
Subgroup: Abandonment					
Coefficient and s.e.	0.0345 (0.0593)	0.250 (0.212)	-	0.508 (0.243)	0.0656 (0.0263)
Cluster robust F-statistic	0.3	-	-	-	6.2
Number of children		534			1,905
Instrument		Non-kin exits / log(population)			
Subgroup: No Relinquishment					
Coefficient and s.e.	0.0452 (0.0058)	0.0927 (0.0214)	2.052 (0.509)	0.888 (0.069)	0.0286 (0.0041)
Cluster robust F-statistic	61.3	-	-	-	47.9
Number of children		4,987			18,049
Instrument		Non-kin exits / log(population)			
Subgroup: Relinquishment					
Coefficient and s.e.					
Cluster robust F-statistic		-	-	-	
Number of children		126			
Instrument		Non-kin exits / log(population)			
Subgroup: No Housing Problems					
Coefficient and s.e.	0.0434 (0.0060)	0.0880 (0.0206)	2.025 (0.547)	0.889 (0.070)	0.0318 (0.0040)
Cluster robust F-statistic	52.8	-	-	-	64.2
Number of children		4,861			17,482
Instrument		Non-kin exits / log(population)			
Subgroup: Housing Problems					
Coefficient and s.e.	0.215 (0.0381)	0.594 (0.301)	2.768 (1.127)	2.234 (0.923)	-0.0477 (0.0162)
Cluster robust F-statistic	31.7	-	-	-	8.6
Number of children		252			979
Instrument		Non-kin exits / log(population)			

Notes: This table shows OLS results for the first stage (initial placement with family indicator on instrument), reduced form (outcome index on instrument), IV treatment effects and OLS treatment effects in the outcome sample, and the first stage in the eligible sample in different subgroups of the data based on child observables. All models do not include child demographic and entry reason controls. All models include county and month-by-year fixed effects, and standard errors are clustered at the county level. Blank entries in the table indicate that the sample size is too small to provide informative estimates.

Table A15: Complier Adjusted OLS Results

	OLS (1)	OLS Weighted (2)	OLS Housing Problem Subsample (3)	IV (4)
Outcome: Economic and Social Outcome Index				
Non-kin family placement	0.886 (0.067)	0.903 (0.126)	2.234 (0.923)	2.016 (0.513)
Outcome: Incarceration				
Non-kin family placement	-0.189 (0.013)	-0.187 (0.135)	-0.278 (0.137)	-0.351 (0.110)
Outcome: Homeless				
Non-kin family placement	-0.087 (0.016)	-0.095 (0.017)	-0.396 (0.167)	-0.270 (0.119)
Outcome: Substance Abuse				
Non-kin family placement	-0.068 (0.010)	-0.074 (0.011)	-0.528 (0.141)	-0.210 (0.074)
Outcome: Employment or Enrollment				
Non-kin family placement	0.108 (0.016)	0.107 (0.018)	0.015 (0.144)	0.136 (0.135)
County, month-year fixed effects	Y	Y	Y	Y
Child demographic, entry reason controls	N	N	N	N
Number observations (children)	5,113	5,113	252	5,113

Notes: This table presents various OLS specifications and IV results across the outcome index and the outcomes that make up the outcome index. Column (1) presents OLS results. Column (2) presents OLS results where the sample is weighted according to first stage coefficient of the housing subsample following Dahl, Kostol and Mogstad (2014) and Bhuller, Dahl, Løken and Mogstad (2020). Column (3) presents OLS results only looking at the subsample of children that enter at least partly due to inadequate housing or homelessness. Column (4) presents IV results. All specifications include county and month by year fixed effects, but do NOT include demographic or entry reason controls, following closely the procedure in Bhuller, Dahl, Løken and Mogstad (2020). Standard errors are clustered at the county level throughout.

Table A16: Measurement Error: OLS Results on More Precise Subsample

	Economic and Social Outcome Index		
	OLS	OLS Precise Measurement Subsample	IV
	(1)	(2)	(3)
Initial non-kin family placement	0.886 (0.0673)	1.281 (0.234)	2.016 (0.513)
Number observations (children)	5,113	752	5,113
County, month-year fes	Y	Y	Y
Child entry, demographics	N	N	N
% IV - OLS difference explained		29.3%	

Notes: This table presents results from OLS and IV regressions of the outcome index on an indicator for a child's initial placement being with a non-kin family estimated in different subsamples. All regressions include county and month-by-year fixed effects but do not include child-level controls. Column (1) gives OLS results for the full outcome sample. Column (2) gives OLS results for children that enter foster care in the same month as the reporting period for the data, or the precise measurement subsample. Column (3) gives IV results for the full outcome sample. Standard errors are clustered at the county level throughout.

Table A17: Connection to Adult, Public Welfare Outcomes and Other Economic and Social Outcomes

Panel A: Other Economic and Social Outcomes

	Connection to Adult		Had Children		Private Financial Payments: Family, Child Support, Legal		Apprenticeship, Internship, On-the-Job Training	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Initial non-kin family placement	-0.006	0.490	-0.046	0.103	-0.002	-0.247	0.026	-0.553
	(0.012)	(0.158)	(0.015)	(0.180)	(0.011)	(0.166)	(0.016)	(0.247)
Child demographic, entry controls					Y			
County, month x year fes					Y			
Mean outcome	0.896		0.275		0.115		0.315	
Number children	5,097		5,063		5,052		5,099	

Panel B: Social Services

	Total Public Aid		Social Security		Educational Aid		Food Stamps		Housing Vouchers		Other Cash Welfare	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Initial non-kin family placement	-0.168	-1.114	-0.063	-0.013	0.091	-0.028	-0.056	-0.612	-0.017	-0.352	-0.029	-0.296
	(0.034)	(0.490)	(0.012)	(0.158)	(0.014)	(0.238)	(0.020)	(0.366)	(0.010)	(0.186)	(0.011)	(0.194)
Child demographic, entry controls							Y					
County, month x year fes							Y					
Mean outcome	0.592 (sd = 0.831)		0.103		0.203		0.315		0.0752		0.0989	
Number children	4,122		5,064		5,048		4,241		4,228		4,228	

Notes: This table presents OLS and IV results from other economic and social outcomes and public welfare use outcomes. The other economic and social outcomes contained in the NYTD data include whether the child has a connection to an adult they feel comfortable going to for advice, they have mothered or fathered children in the past 2 years, they receive financial payments from a family, child support or other legal source. The public welfare use source includes an index of total public aid which adds together indicators for social security, food stamps, housing vouchers and other cash welfare. These are also broken out separately, with the addition of an outcome on whether the child receives financial aid. All regressions include child demographic and entry reason controls, and county and month by year fixed effects. Standard errors are clustered at the county level.

Table A18: Intermediate Foster Care Outcomes

Panel A: IV	Outcome Sample		Eligible Sample		Old Children Sample (Weighted)	
	Adopt or Guardian by 18	Number Placements after Entry	Adopt or Guardian by 18	Number Placements after Entry	Adopt or Guardian by 18	Number Placements after Entry
	(1)	(2)	(3)	(4)	(5)	(6)
Initial non-kin family placement	0.0918	-3.495	-0.0599	-1.122	0.0795	-0.550
	(0.1470)	(2.611)	(0.1562)	(1.806)	(0.0383)	(0.636)
Instrument	Non-kin exits / log(county population)					
County, month x year fes	Y	Y	Y	Y	Y	Y
Child demographic, entry reason controls	Y	Y	Y	Y	Y	Y
Mean outcome	0.045	4.46	0.043	4.42	0.025	2.46
Number observations (children)	3,619	4,454	13,840	15,731	143,409	151,372

Panel B: OLS						
	(1)	(2)	(3)	(4)	(5)	(6)
Initial non-kin family placement	0.0459	-0.734	0.0413	-0.747	0.0349	-0.439
	(0.011)	(0.169)	(0.0052)	(0.096)	(0.0094)	(0.076)
County, month x year fes	Y	Y	Y	Y	Y	Y
Child demographic, entry reason controls	Y	Y	Y	Y	Y	Y
Mean outcome	0.045	4.46	0.0431	4.42	0.0250	2.46
Number observations (children)	3,619	4,454	13,840	15,731	143,409	151,372

Notes: This table presents OLS and IV regression results of adoption or guardianship indicator variables and number of placement numeric variables on the initial placement with non-kin indicator variable. It does this across the outcome, eligible and old children sample, where observations in the old children sample are weighted according to (obs weight = percent observations with same county in outcome sample) to ensure a stronger first stage. The samples for adoption and guardian by 18 models exclude children who do not exit by age 18. Smaller sample sizes for number placements are smaller because missing value in the number placements variable. Throughout models include child demographic and entry controls, and county and month by year fixed effects. Standard errors are clustered by county.

Table A19: Treatment Effects with Time in Foster Care as Endogenous Variables

	Economic and Social Outcome Index			
	IV		OLS	
	(1)	(2)	(3)	(4)
Months in non-kin family placement	0.0618 (0.0193)		0.0230 (0.002)	
Percent time in non-kin family placement		2.964 (1.107)		0.830 (0.0750)
County, month x year fes	Y	Y	Y	Y
Child demographics, entry reason controls	Y	Y	Y	Y
Sd endogenous variable	15.0	0.437	15.0	0.437
Mean endogenous variable control	4.20	0.131	4.20	0.131
Mean endogenous variable treatment	24.0	0.838	24.0	0.838
Number observations (children)	5,113	5,113	5,113	5,113

Notes: This table reports treatment effects estimated by IV and OLS on two alternative endogenous variables for the economic and social outcome index. The models are identical to those in Table 6 except for the endogenous variables. Months in non-kin family placement is a numeric variable that counts the number of placements recorded at and after entry that are non-kin family placements and multiplies by 6 months (the length between reporting periods). Percent time in non-kin placements looks at the percentage of placements reported for the child at and after entry that are non-kin placements. Standard errors are clustered at the county-level.

Table A20: OLS and Intent-to-Treat Attrition

Panel A: Correcting for Non-Response Bias with Observables	Outcome Index			
	Non Weighted	Weighted		
	(1)	(2)		
Initial non-kin family placement	2.056	2.686		
	(0.726)	(0.972)		
Instrument	Non-kin exits / log(pop)			
Inverse propensity score weighted	N	Y		
County, month x year fes	Y	Y		
Child demographic, entry reason controls	Y	Y		
Number observations (children)	5,113	5,113		
	OLS	ITT	OLS	ITT
Panel B: Lee (2009) Attrition Bounds	Sample A	Sample A	Sample B	Sample B
	(1)	(2)	(3)	(4)
Initial non-kin family placement	0.6459		0.7090	
	(0.0667)		(0.0747)	
Non-kin exits / log(pop)		0.1288		0.1511
		(0.0710)		(0.0825)
Lee (2009) upper bound	1.2410	0.1288	1.2734	0.1511
Lee (2009) lower bound	0.6459	0.0146	0.7089	0.0382
Response rate treatment	0.621	0.556	0.630	0.560
Response rate control	0.521	0.575	0.516	0.575
p-value response rates differ	<0.001	0.308	<0.001	0.616
County, month x year fes	Y	Y	Y	Y
Child demographic, entry reason controls	Y	Y	Y	Y
Number observations (children)	5,113	5,113	3,877	3,877

Notes: This table contains two panels of results. Panel A undertakes the exercise in Sacerdote (2007) suggested by Wooldridge (1999) and corrects for non-response bias on observables by creating a propensity score for response to the survey at age 21 using a logistic regression model, and weighting observations according to $1/$fitted prob response. All demographics and entry reason variables are used to create the weights. Panel B computes Lee (2009) bounds for OLS treatment effects and intent-to-treat effects from the reduced form. The outcome variable is the outcome index used throughout the paper. Columns (1) and (2) use Sample A: children that responded to the survey at age 17 and that were sampled by states that randomly sample eligible children. These are the only children eligible to take the survey at age 21. Column (3) and (4) use Sample B: the subset of the outcome sample in states that do not randomly sample eligible children, and compute response rates in those samples, too. Throughout standard errors are clustered at the county level.

Table A21: Robustness to Age Cutoff for Children Included in Sample

Panel A: IV	Children Last Entry 12 Years or Older	Children Last Entry 13 Years or Older	Children Last Entry 15 Years or Older
	(1)	(2)	(3)
Initial non-kin family placement	1.207	1.388	1.723
	(0.529)	(0.521)	(0.915)
Instrument		non-kin exits month / log(pop)	
First stage F-statistic	57.3	68.5	39.2
County, month x year fes	Y	Y	Y
Child demographic, entry reason controls	Y	Y	Y
Number observations (children)	5,699	5,545	4,498

Panel B: OLS			
	(1)	(2)	(3)
Initial non-kin family placement	0.627	0.629	0.658
	(0.065)	(0.066)	(0.071)
County, month x year fes	Y	Y	Y
Child demographic, entry reason controls	Y	Y	Y
Number observations (children)	5,699	5,545	4,498

Notes: This table includes OLS and IV estimates for regressions of the outcome index at age 21 used in Table 6 on an indicator for a child's initial placement in a non-kin family with various samples of children that vary by the age cutoff. Column (1) provides IV (Panel A) and OLS (Panel B) estimates for the sample of foster children that enter between ages 12 and 17. Column (2) provides IV and OLS estimates for the sample of foster children that enter between ages 13 and 17. Column (3) provides IV and OLS estimates for the sample of foster children that enter between ages 15 and 17. All models include demographic and child entry controls, and county and month-by-year fixed effects. Standard errors are clustered at the county level.

Table A22: IV Specification and Index Robustness

Panel A: Specification Tests	Old Child Exits	Drop Outlier County x Month x Years	Drop Very Small Counties	Dropping Endpoints of Data
	(1)	(2)	(3)	(4)
First stage coefficient on	0.0489	0.0449	0.0312	0.0316
instrument	(0.0108)	(0.0120)	(0.00487)	(0.00489)
IV coefficient on	3.545	2.613	1.755	1.987
economic and social outcome index	(0.927)	(0.757)	(0.745)	(0.744)
Instrument	Non-kin exits month 14 years+ / log(population)		Non-kin exits month / log(population)	
County, month x year fes	Y	Y	Y	Y
Child demographic, entry controls	Y	Y	Y	Y
Number observations (children)	5,113	4,277	3,923	5,037

Panel B: Outcome Indices	Incarceration, Homelessness, Substance Abuse Index	Employment, Enrollment Alternate Index	Incarceration, Homelessness, Substance Abuse, Employment, Enrollment Alternate Index with High School Education	Economic and Social Outcome Index with High School Education
	(1)	(2)	(3)	(4)
IV coefficient on	1.949	1.308	3.683	2.482
specified outcome	(0.646)	(0.677)	(1.100)	(0.878)
Instrument		Non-kin exits month / log(population)		
Mean outcome	0.323	0.217	0.661	1.13
Sd outcome	1.94	1.63	3.09	2.44
County, month x year fes	Y	Y	Y	Y
Child demographic, entry controls	Y	Y	Y	Y
Number observations (children)	5,113	5,113	5,113	5,113

Notes: Panel A provides first stage and IV regressions on different subsamples and with different instruments. Column (1) of panel A uses 14 year old non-kin exits as the instrument; column (2) drops county-month-year level observations where the instrument value falls outside the 5th and 95th percentile of the county-specific instrument distribution; column (3) drops all counties with 4 or less children in the sample; column (4) drops children with observed entries in the same month as the first reporting period. Panel B provides IV regressions on different outcome indices. Column (1) uses an index that adds incarceration, homelessness and substance abuse; column (2) uses an index that adds part-time employment, full-time employment and enrollment status; column (3) uses an index that adds the indices in columns (1) and (2) and also adds in high school education; column (4) uses the original index used in the main results and adds high school education. In all regressions standard errors are clustered at the county-level.

Table A23: Descriptive Evidence on Foster Family Preferences

	Placement with Non-Kin Foster Family	
	(1)	(2)
(Intercept)	0.545	0.527
	(0.004)	(0.006)
Sex: male	−0.195	−0.211
	(0.002)	(0.003)
Race: black	−0.050	−0.071
	(0.004)	(0.006)
Race: white	−0.00001	−0.042
	(0.004)	(0.006)
Race: hispanic	0.004	0.009
	(0.004)	(0.006)
Age: 15	−0.060	−0.065
	(0.003)	(0.004)
Age: 16	−0.084	−0.090
	(0.003)	(0.004)
Age: 17	−0.093	−0.099
	(0.003)	(0.004)
Observations	231,342	93,606
R^2	0.050	0.066

Notes: This table presents OLS regressions of an indicator variable for placement with non-kin foster family on entry (versus placement in a group home). Column (1) includes all child entries for children with non-missing demographics entering between the ages of 14 and 17. Column (2) includes child entries in county-month-years where at least 10 children entered in the same county-month-year. The reference group for race is asian pacific islander and native american, and the reference group for age is entering at 14 years old. Standard errors clustered at the county level are given in parentheses.