Can't Buy Me Love?
Subsidizing the Care of Related Children

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Abstract:

This paper uses a reform in Illinois that reduced the monthly subsidy offered to relatives asked to provide foster care as a plausibly exogenous change in the cost of caring for related children. Families offered a 30% lower wage were 15% less likely to provide care, with especially large declines for children who require mental health services, infants and teenagers. One innovation is a sample selection model that uses the foster care placement tendency of child protection investigators to predict entry into the sample—an instrument that should be unrelated to family characteristics due to a rotational assignment process that effectively randomizes investigators to families. Meanwhile, child health, education, and placement outcomes do not appear to suffer following the decline in the subsidy offer, consistent with similar quality levels among marginal kin and non-kin caregivers.

JEL: H31, I38; Keywords: Child Welfare, Foster Care, Within-family Altruism, Household bargaining

1. Introduction

With the well-documented rise in the number of single-parent families in the U.S., the safety net of the extended family has grown more important. In 1997, 5.5% of all children in the U.S., or 3.9 million, lived in households headed by grandparents, up from 2.3 million in 1980 (Casper and Bryson, 1998). The extended family is also seen as a natural source of day care with subsidies available to kin following welfare reform (Collins and Carlson, 1998; Fuller et al., 2002).

A similar call for support comes from children in foster care—children who are temporarily removed from their parents due to abuse or neglect. Over two million children are investigated for such abuse in the U.S. each year, and over a half million children currently reside in foster care (US DHHS, 2004a). States spend over $20 billion annually on child protection (Bess et al., 2002), and these programs target children who are at high risk for poor life outcomes. For example, former foster children appear far more likely to commit crimes, dropout of school, enter the homeless population, join welfare, and experience substance abuse and mental health problems (Courtney and Piliavin, 1998; Dworsky and Courtney, 2000; US DHHS, 1999; Clausen et al., 1998).

In the late 1980s there was a substantial shift toward using relatives to care for foster children, in a move considered one of the most important phenomena in child welfare over the past twenty years (Hegar et al., 1997; Wulczyn et al., 1999). In many states, kin are now the first option when a child is removed from home (Geen, et al., 2002). As a result of this change in philosophy, and the relatively large monthly subsidies made available to kin, the number of children placed with relatives increased dramatically. For example, in New York City there were 151 official kinship care cases in 1985; five years later there were over 23,000.

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The aim of this paper is to test the effect of a change in the cost of caring for related children on both the willingness to provide care and the quality of care provided. Changes in such costs are usually difficult to quantify and suffer from endogeneity problems. For example, comparing fertility or child outcomes for parents with different wage rates would not only capture differences in costs, but differences in parents as well.

To consider a plausibly exogenous change in the cost of caring for relatives, the empirical strategy compares the willingness of families to provide kinship foster care before and after a reform in Illinois that lowered the wage offer by approximately 30%. Rich administrative data is used to control for child characteristics. To consider unobserved differences in families over time, one innovation in the paper is the use of a sample selection model that considers variation in foster care placement—entry into the sample—due to investigator placement tendencies. These tendencies are found to predict entry into foster care, but should be unrelated to family characteristics due to a rotational assignment process that effectively randomizes child protection investigators to families.

The estimates show that relatives offered the 27% lower wage were 10-15% less likely to provide care. Children on the margin of relative placement tend to be those who require mental health services, infants and teenagers. Child and case characteristics are similar before and after the reform, suggesting that the change in the wage offer was not accompanied by a change in child types that could affect relative placement. The results are also robust to the sample selection correction. Meanwhile, child health, education, and placement outcomes do not appear to suffer following the reform. These results are consistent with a similar level of quality among marginal kin and non-kin caregivers.

The paper is organized as follows. Section two presents background on the foster care system and reforms in Illinois. Section three discusses the previous evidence to provide a context for the results. Section four describes the rich administrative data available in Illinois. Section five presents the empirical models and results, including the description of the sample selection correction. Section six examines whether the reform had an effect on child outcomes, and section seven concludes.

2. Background

To provide some context for the results, the next two subsections briefly describe child welfare practice in Illinois and the wage reform that will be considered in the empirical analysis.

2.1 Child Welfare Policy in Illinois

Children enter foster care when their parents are suspected of abuse or neglect following reports from physicians, school officials, police, and family members. This is meant to be a temporary placement while the family is rehabilitated or an adoptive home is sought. Average lengths of stay in foster care are two years in the U.S. (US DHHS, 2005), and foster families are paid a monthly subsidy that averages approximately $400 per child per month (CWLA, 1999). This is three times the average AFDC/TANF monthly subsidy (US DHHS, 2004b).

In Illinois, the Department of Children and Family Services (DCFS) conducts child protection investigations and reports evidence of abuse to child protection judges who decide whether children should be placed in foster care.[1] Once this decision has been made, DCFS policy is to seek a relative first to care for the child. Kinship placements are thought to be less traumatic and encourage contacts with the biological parents. In addition, kinship foster care is

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[1] Most children enter into a family setting. Children who are homicidal, suicidal, or have other special needs may be determined ineligible for family foster care. The analysis below focuses on those eligible for family foster care.

seen as a way of decreasing the effective caseload of social workers due to fewer services requested, though DCFS policy regarding supervision is common across placement types. These benefits support the policy of seeking a relative first, which makes it possible to identify children who cannot find a relative willing to provide care—those children who are placed with non-relatives.

In terms of timing, there is an urgency to establish a stable foster family placement as soon as possible for the sake of child wellbeing, and stays in emergency shelter foster homes are typically less than twenty-four hours. Available relatives will usually be known as part of the family investigation, and the wage offer occurs when the decision is made to place the child in foster care. In the sample considered below, the median length of time between an abuse report and placement in a non-emergency foster home is only 6 days. The urgent nature of these actions suggests that comparisons of first-investigated families just before and after the reform will capture the change in the wage offered to relatives.

2.2 The Wage Reform

Calls for reform of the Illinois foster care system grew in the early 1990s at a time of unprecedented growth. In June 1986 there were 13,734 children in care, increasing to nearly 50,000 by June 1995. Spending had increased to over $1 billion. Meanwhile, DCFS was placed under judicial oversight due to the long stays in foster care, averaging over four years. This growth was accompanied by a large increase in the number of kinship foster care placements, and the relatively large payments made available to kin were suggested as a contributor to the growth (Testa, 1997). The subsidies were thought to both encourage families to enter the formal foster care system and discourage family reunification.

A wage reform was decided upon in March 1995 to be implemented at the beginning of the next fiscal year: July 1st. Before the reform, relative caregivers received the same foster care subsidies that non-relatives received. Afterwards, a two-tiered system was created where kin receive the higher subsidies only if they meet the licensing criteria required for non-relative homes. Unlicensed providers receive a 'standard of need' payment that varies by region and number of children. In 2000, a licensed caretaker of a nine-year-old child in Chicago received $410 per month, while an unlicensed relative caretaker received $285. At the time of the reform, the average payment per child was $348 for licensed providers and $255 for unlicensed kinship providers, representing a 27% reduction in the wage offer.

While most relatives providing care at the time of the reform continued to receive the higher subsidy following a set of appeals, few new entrants became licensed to receive the higher subsidy. In each year following the reform, approximately 5% of kinship providers became licensed at the beginning of the child's spell and 12% became licensed in the first year. For providers who began providing care in the first year following the reform and continued to provide care for five years, 25% eventually became licensed. This low rate of licensure is surprising given the financial incentives to both the relatives and the state of Illinois as federal matching funds could be accessed for licensed caregivers. Caseworkers suggest that an unwillingness to enter into a formal arrangement with the state, and stringent licensing standards such as space requirements, are the reasons behind the low take-up rate. In any event, the low rate of licensure suggests that most kin could expect that they would not receive the higher subsidies associated with becoming licensed at the time of the wage offer.

In addition to the wage reform, there were two other reforms to reduce the number of children in foster care and state spending (McDonald, 1999). One reform ended the practice of allowing relatives already providing informal kinship care to enter the formal system under the allegation of "lack of [parental] supervision". The main analysis below focuses on cases of suspected abuse, as opposed to neglect, so that this reform is not applicable.[2]

The second initiative placed new emphasis on family preservation—services that allowed more children to stay at home. In the years following the 1995 reforms, this program was expanded in 1997 and 1999. The new entry criteria suggest that new foster care entrants may differ before and after the wage reform even among the subset suspected of abuse. This is a focus of the empirical work below.

3. Previous Evidence

The wage reform increased the cost to relatives of caring for foster children. Models of labor supply, and the demand for children, predict that lower subsidies would reduce the propensity to provide kinship care (Becker, 1981; Hotz et al., 1997). Past empirical investigations support this conclusion and suggest that higher subsidies increase the quantity of labor supplied to the foster care market (Simon, 1975; Campbell and Downs, 1987; Chamberlain et al., 1992; Testa and Rolock, 1999; Doyle and Peters, 2003). Those papers generally used cross-state variation in subsidies and foster care populations to estimate a supply curve for foster care. This paper goes beyond aggregate relationships and uses longitudinal administrative datasets to study the decisions of relatives to provide care.

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[2] It is possible that families substituted toward abuse allegations in response to the restriction against neglect cases. This would seem unlikely, however, as abuse allegations are more likely to result in criminal proceedings and a termination of parental rights. In any event, such a substitution would make it more difficult to find a change in the relative placement rate among abuse cases following the reform.

A related result is that kinship foster care is associated with more stable, longer stays in care (Wulzcyn and Goerge, 1992; Berrick, 1994). Despite hopes that placement closer to home would facilitate family reunification, if a child were officially returned home, subsidies to the family would decline (Berrick, 1998). Berrick and Needell (1999) show that children in California who were placed with relatives receiving higher subsidies, rather than an AFDC rate, had longer lengths of stay. Testa and Slack (2002) consider the wage reform studied here and find for the small group of families who did not appeal the subsidy decline (and had their subsidies reduced) had a higher exit rate from care. Meanwhile, Chamberlain et al. (1992) and Testa and Rolock (1999) find that placement stability improves with increased wages. While sample sizes were small, (n=45 and 33, respectively), they suggest that larger subsidies lead to higher quality along this dimension. It is not clear, however, whether these results are due to families foreseeing short stays resulting in a lower propensity to seek the higher payment levels. This paper focuses on a plausibly exogenous change in the cost of providing kinship care.

In terms of a broader set of child outcomes, little is known about the effects of foster care placement, and on the effects of kinship placement in particular.[3] Outcomes are rarely observed, and when comparisons are made they usually involve small sample sizes and no comparison group. Cuddeback (2004) provides an extensive review of the kinship foster care literature and notes that selection bias plagues most comparisons. Many studies also consider small groups of children and offer no comparison group. In contrast, this paper explicitly considers the problem

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[3] Detailed reviews of foster care outcome research are provided by Courtney (2000), Gelles (2000), Goerge et al. (1994a), National Research Council (1998), and McDonald et al. (1996). See Runyan and Gould (1985), Elmer (1986), Davidson-Arad, et al. (2003), and Wald et al. (1998) for four small scale studies of foster care outcomes in general. Jonson-Reid and Barth (2000) and Doyle (2006) provide evidence for larger samples and found that adolescent outcomes such as juvenile delinquency tend to be lower when children are not placed in foster care. Altshuler (1998), Benedict et al. (1996), and Iglehart (1994) provide small scale comparisons of kin vs. non-kin placements.

of selection bias and analyzes much larger data sets measuring child health, education, and placement outcomes to test the effects of the reform on the quality of care provided, as marginal kin placements are substituted by marginal non-kin placements.

The cost of children literature can also guide the analysis (Espenshade 1984; Lewin/ICF 1990). In particular, more costly children may be closer to the margin of relative placement. To summarize, the findings generally suggest that parental expenditures increase with age, decline (on average) with the number of children, vary by race and ethnicity, and increase with income. If kin consider traditional placements to be roughly similar across child types, then different responses to the wage reform across child types would provide evidence about differences in the cost of children.

4. Data Description

A unique data set that matches individuals across a wide array of administrative agencies in Illinois is used to carry out the analysis. These data sets have been matched by The Chapin Hall Center for Children, a child welfare research center at the University of Chicago, to construct the Illinois Integrated Database (Goerge et. al, 1994b). Data are collected by Chapin Hall on a monthly or quarterly basis, and the linkages use a probabilistic matching algorithm that includes family identifiers such as names, birthdates, social security numbers, and addresses.

First, the Child Abuse and Neglect Tracking System (CANTS) is used by Illinois DCFS to aid investigations. It records demographic information, the initial reporter of abuse, the allegations, investigation dates, the field team, and the investigator assigned to the case. The

analysis below focuses on all families first investigated between January 1, 1990 and June 30, 2000 where a finding of abuse has been substantiated.[4] This results in over 80,000 children.

Illinois DCFS also uses the Child and Youth Centered Information System (CYCIS) to track children in foster care. These data include demographic information and an indicator for whether the child was referred for mental health services. The system records placement types and movements within the foster care system. This allows outcomes to be considered, such as placement disruption, family reunification, and re-entry into foster care—a measure of re-abuse. To focus on the timing of the wage offer, the 8,511 children who entered family foster care within 90 days of the initial abuse report are considered, and the median time between the report and placement is 6 days.[5]

All foster children are eligible for Medicaid, providing a means to measure health outcomes through the Medicaid Paid Claims database. These data contain demographic variables, primary diagnosis and procedure codes, provider, provider type, category of service, total paid amount, and service dates. Outcomes that will be considered are wellness visits and injuries identified by broken bones. One advantage of studying broken bones as a proxy for caretaker supervision and child safety is that the timing is known due to the urgent care required. Wellness visits have the advantage that foster parents are assigned the task of ensuring that all new foster care entrants receive a medical check-up. A change in these visits will inform the effect of the policy on health care utilization as well as foster parent quality.

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[4] Results are robust to including new entrants to foster care where the family had prior investigations, and when unsubstantiated cases are included. The focus on substantiated cases considers children who are at risk of placement.
[5] To allow some time between the report and the placement, the placements from the month of January 1990 are excluded in the analysis below.

For educational outcomes, the database includes the Chicago Public Schools' Student Information System. Math and reading test scores in the year after the abuse report will be considered. The effect of the reform on test scores is useful in describing the effects on learning and child well-being more generally. If a child's home environment becomes more (or less) stable, this may be found in test scores.

Table 1 provides an idea of the types of children who enter foster care and describes the control variables used in the analysis. The last two columns report the one-year difference in means at the time of the reform. The first row shows that the fraction of children placed with relatives is 63%, and provides an initial estimate of the change in relative placement at the time of the reform: a 10 percentage point drop between FY1995 and FY1996.

The initial reporter of abuse is often a physician (31%), the police (25%), or a family member (12%). Physical abuse was found in 9% of the cases, while a report of "substantial risk of harm" was cited in 71% of the cases. Nearly half of the foster care entrants were under the age of five, were boys, and lived in Cook County (which includes the city of Chicago). African Americans are overrepresented in the foster care system (55% compared to 26% in Cook County), while Hispanics are underrepresented (8% compared to 20% in Cook County). Last, an indicator that the child was referred for mental health services upon entry into foster care will be used to compare the response to the reform across child types. This variable reflects care provided and will not used as a control variable, though the results do not change if this variable is included as a control.

The observable characteristics did not change very much at the time of the reform, with little change between FY1995 and FY1996 in terms of age, race, sex, sibling group size, and

abuse type. The only characteristics associated with a statistically significant difference in FY1996 are an increase in the likelihood that the allegation came from the police, a slight increase in anonymous reports, and a decline in physician reports. This composition change does not appear to be responsible for the decline in relative foster care placements, however, as reports from physicians, police, and anonymous reporters are all positively correlated with finding a willing relative. Taken together, children appear to be similar before and after the reform.

5. Empirical Models & Results

By focusing on each family's first investigation, a family is offered a different subsidy depending on the timing of the abuse report. Figure 1 offers a look at the raw data by reporting the fraction of children who are placed with relatives in each month. In particular, the following model was estimated for child i who entered foster care at time t:

$$(1) \ R_i = \beta_0 + \beta_1 M_t + \beta_2 X_i + \varepsilon_i$$

where R equals one if the child is placed with a relative and zero if the child is placed in a non-relative foster home[6]; M is a vector of month x year dummy variables; and X is the vector of controls listed in Table 1, with indicators for each age level included to permit a more flexible effect of age on relative placement. Figure 1A plots the coefficients of the month indicators from a model with no controls, scaled up by the constant term. The data reveal that the monthly placement rate can be noisy, but generally increases in the early 1990s as kinship foster care was becoming more acceptable on both the labor demand and supply sides. The rate oscillates around 70% beginning in 1994 up until the time of the reform when it falls by approximately 10

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[6] Results here and for models reported below are similar when probit or logit models are used.

percentage points.[7] Results are similar in Figure 1B when the coefficients from a model with controls are plotted.

The tests in the remainder of the paper suggest that the decline continues to be found after smoothing the data and controlling for child and case characteristics, including a sample selection correction.

5.1 Regression Results

First, a model using quarterly dummies is estimated to trace out the change in relative placement over time. For child i entering foster care in quarter t who lives in ZIP code z, the following model is estimated:

$$(2)\ R_i = \beta_0 + \beta_1 Q_t + \beta_2 X_i + \delta_z + \varepsilon_i$$

where Q is a vector of indicators for the calendar quarter when the child entered foster care.[8] ZIP code fixed effects are included to control for neighborhood and family characteristics such as income level, with robust standard errors are clustered at the family level to consider the dependence within sibling groups.

Table 2 reports the results. Three columns correspond to models with no controls, with full controls, and with ZIP code fixed effects. The omitted quarter is the one immediately preceding the wage reform, so that the coefficient estimate on the July-September 1995 quarter provides a test of the change in relative placement just before and after the reform. The first column shows a 16 percentage point drop in the quarter following the reform. This compares

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[7] To describe the time series, a first-differenced, first-order moving average model was found to provide the best fit of the data, though the results were similar with alternative ARIMA specifications. The estimates revealed a 12 percentage point decline at the time of the reform in a model with no controls, and an 8.3 percentage point decline once controls are introduced.

with a 3.3 percentage point drop in the 3rd quarter of 1994, and a 0.7 percentage point increase in the 3rd quarter of 1996. The introduction of controls reduces the magnitude to 10 percentage points, compared to -5.2 and 1.5 in the comparison 3rd quarters.[9]

One limitation to the empirical strategy is that quarterly changes in relative placement are fairly noisy, and a change after the reform may reflect random variation. For example, other quarter-to-quarter changes include large changes such as the 9.6 percentage point drop between the 4th quarter of 1994 and the 1st quarter of 1995 shown in Table 2. What distinguishes the post-reform quarters is that the change in placement rate appears to be a permanent shift compared to the transitory changes in other time periods. The pre-reform relative placement rate oscillated around 70%, while after the reform it drops and remains stable at 10-11 percentage points lower. At no other time in the 1990-2000 period was a 10 percentage point decrease sustained over more than one quarter, and only once was a 10 percentage point increase sustained for two quarters. While a one-quarter drop of 10 percentage points could be spurious, it is the relatively stable change in the placement rate that is consistent with attributing the decline to the subsidy reform.

The remaining results smooth the estimates further by comparing yearly changes in relative placement rates. This is an attempt to extract the signal from the fairly noisy data at the monthly or quarterly level. In particular, the placement rate grows in the early 1990s, but

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[8] Results are similar when the quarter of the abuse report is used as opposed to the quarter of the foster care entry, with an estimated drop in relative placement at the time of the reform of 13.4 percentage points in a model with no controls and 12 percentage points in a model with full controls and ZIP code fixed effects.

[9] On average, relative placement rates are found to be a 2.6 percentage points lower in the 3rd quarter compared to the 2nd quarter in the years prior to the reform, and 4.7 percentage points higher in the 3rd quarter in the years after the reform. Neither figure is statistically significant.

stabilizes in 1994 and appears to drop at the time of the reform. This is summarized by the

following model for child i in fiscal year t living in ZIP code z:

$$(3) \ R_i = \beta_0 + \beta_1 Y_t + \beta_2 X_i + \delta_z + \varepsilon_i$$

where the variables are the same as equation (2), with the exception of the fiscal year dummies

rather than the quarter dummies. The reference year will be Fiscal Year 1995 throughout the

discussion, and an indicator that a family is offered a lower wage is simply an indicator for Fiscal

Year 1996. Robust standard errors will again be clustered at the family level.

Table 3 reports the results of the main variable of interest, FY1996, as well as selected

covariates.[10] Two sets of results are reported: the change in the relative placement rate when the

time of the *foster care placement* began before or after the reform, and the change when the time

of the *abuse report* occurred before or after the reform. Column (1) replicates the result from

Table 1: in a model with no controls, a 10 percentage point decline in relative placement is

found in the year following the reform. Column (2) introduces controls for the case

characteristics, and the magnitude of the estimate decreases slightly to 9.2 percentage points.

When ZIP code fixed effects are introduced as well, Column (3) shows an estimated decline in

relative placement of 10.2%. The results are similar when the time of the abuse report is

considered, with estimated declines of 9.0, 8.2, and 9.3 percentage points for the three models,

respectively. Compared to the 70% of children who were placed with relatives in FY1995, these

results suggest a 13-15% drop in the willingness to provide care after the reform. This implies

an elasticity in the willingness to provide care of approximately 0.5.

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[10] All coefficients are reported in the working paper version of this paper.

The covariates also predict relative placement to varying degrees. If the report of abuse comes from a family member or a physician the child is more likely to find a relative caretaker compared to the other reporter category, with coefficients of 0.16 and 0.08, respectively. As children age they are more likely to be placed with a relative, with children over the age of ten generally 10 percentage points more likely than infants to be placed with relatives. Children with an allegation of physical abuse or sexual abuse are less likely to have a relative placement compared to those at "substantial risk of harm".[11]

Last, the fiscal year indicators continue to show an increase in the early 1990s, and a leveling off in 1994, reflecting the stabilization in relative foster care in the two years prior to the reform. Again, the change in 1996 is not unlike those seen during the policy changes in the early 1990s that increased the emphasis on relative foster care. Rather, the continued lower relative placement rate suggests a permanent effect of the wage reform.

The results were robust to different restrictions of the time period: when observations from March 1995 through the 3rd quarter of calendar year 1995 were excluded to abstract from families timing their entry decisions, and when the sample was restricted to children who were placed in care within a week of the abuse report.

5.2 Controlling for Sample Selection

A main concern of the before-after estimator is that unobserved characteristics of children may have changed at the time of the reform.[12] Fortunately, the rich investigation data include the

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[11] When cases with a secondary allegation of "lack of supervision"—the neglect category directly targeted by the entry reforms—were excluded, the resulting sample size was 6,530 and the estimates were a 9.3 percentage point drop when the foster care start date was used and a 7.6 percentage point drop when the report date was used.
[12] To consider child characteristics at this time in other states, the most comprehensive data on kinship foster care in the mid 1990s was the Multi-State Data Archive—a collaboration among seven large states in the mid-1990s maintained by the Chapin Hall Center for Children. First foster care spells showed no break in trend was found in

investigator of the abuse/neglect report and can be used to estimate a sample selection model. The model considers the tendency for investigators to have children placed in foster care to predict foster care entry, despite a rotational assignment process that effectively randomizes investigators to families.[13]

There are exceptions to the rotational assignment. First, sexual abuse and drug exposure are less likely to enter the rotation given investigators who specialize in these reports, and these cases will be excluded from the analysis below. Second, an attempt is made to assign Spanish-speaking families to Spanish-speaking investigators. Third, some teams further break their county into subregions. To take into account these departures from randomization, the instrument that is used to predict entry into the sample will be calculated within sub-teams defined by investigation team x ZIP code x Hispanic x report year cells.

The instrument is simply the fraction of the investigator's cases that result in a foster care placement, compared to the types of cases that her teams typically consider. For child-protection investigator c assigned to family f in sub-team j the calculation is:

$$(4)\, Z_{c,-j} = \frac{1}{n_{c,-j}} \sum_{k=-j} n_{c,k} (\overline{P}^{c,k} - \overline{P}^{k})$$

where $n_{c,-j}$ is the total number of children assigned to investigator c outside of the family's sub-team, $n_{c,k}$ is the number of children assigned to investigator c in sub-team k, $\overline{P}^{ck}$ is the fraction of children assigned to investigator c in sub-team k that is placed in foster care, and $\overline{P}^{k}$ is the fraction of investigated children in sub-team k that is placed in foster care. Both placement

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any of the other states, with a 1.3 percentage point decline in FY 1996, compared to a mean of 31%. The pre-reform placement rates are not matched in level or in trend compared to Illinois in any of the other states, however.
[13] See Doyle (2006) for a more detailed description.

measures are placements within 90 days of the abuse/neglect report. This investigator placement differential is analogous to an investigator fixed effect in a model predicting placement with sub-team fixed effects. It is calculated for all sub-teams not including the family's sub-team, so that each family's removal decisions do not enter into their calculation.

The measure is constructed on sub-team cells where there is more than one investigator. Even children investigated in a sub-team cell with only one investigator will still have a non-missing instrument, however, as it is calculated for all other sub-team cells. In addition, the calculation is restricted to investigators with at least 10 investigations. This results in 1064 investigators considered, with an average of 53 investigations per investigator used in constructing the measure.[14] The resulting instrument reveals some variation in placement rates across investigators: the de-meaned instrument has a mean of zero and a standard deviation of 5% compared to a 10% placement rate.

If the rotational assignment randomizes families to investigators, then the investigator placement differential should not be associated with child or family characteristics. To test this hypothesis, for child i assigned to an investigator c in sub-team j during year t, the following model is estimated by OLS:

$$(5) \ Z_{c,-j} = \beta_0 + \beta_1 Y_t + \beta_2 X_i + \varepsilon_i$$

The standard errors reported are clustered at the investigator level to reflect the level of variation in the dependent variable.

Table 4 reports the results, and the investigator placement differential is not related to the observable case characteristics. For example, children with a report from a physician are found

to have only a 0.1 percentage point increase in the investigator placement differential, despite the fact that physician reports are highly associated with placement.[15] All of the coefficients are quite small and only 1 predictor out of 43 is found to be statistically significant at the 5% level—the indicator for three or more siblings is associated with a 0.28 percentage point increase, or 5% of a standard deviation, in the investigator placement differential. The differential is also unrelated to the length of stay in foster care, to type of placement, and to case characteristics when the sample is restricted to children who enter foster care. These results are consistent with the idea that the rotational assignment of investigators within investigative teams effectively randomizes families to investigators.

This investigator placement differential is used to estimate a selection model. Under general assumptions, selection bias in a model of relative placement can be written as a nonlinear function in the propensity for a child to enter the foster care sample (Heckman and Robb, 1986; Das et al., 2003). This suggests a simultaneous-equations approach for child i investigated in subteam j by child protection investigator c during year t:

$$(6)\ P_i = \alpha_0 + \alpha_1 Y_t + \alpha_2 X_i + \alpha_3 Z_{c,-j} + \eta_i$$
$$(7)\ R_i = \pi_0 + \pi_1 Y_t + \pi_2 X_i + \phi(\hat{P}_i) + \zeta_i$$

where P is an indicator for family foster care placement within 90 days of the abuse/neglect report; R is an indicator for relative placement; Y is the fiscal year of the abuse/neglect report, as the start of the initial placement is not applicable to children who do not enter care; X is the vector of control variables; Z represents the investigator placement differential, which is

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[14] Heckman (1981) discusses the ability of small sample sizes per group to allow for meaningful estimates of fixed effects with a rule of thumb of eight observations per group.
[15] An F-test of joint significance for all of the observable characteristics results in an F-statistic of 1.27 and a p-value of 0.12. Full results are in the working paper version.

excluded from the relative placement equation. The two assumptions for a valid exclusion restriction are that (1) it predicts entry into the sample, which will be tested below and (2) the instrument does not belong in the relative placement equation, which is suggested by the effective randomization of investigators to families through the rotational assignment process.[16]

To carry out the estimation, a two-step procedure was used with two linear probability models to preserve the interpretation of the FY1996 indicator, though similar results were found imposing a bivariate normal error structure. First, the foster care placement equation is estimated. Then a polynomial in the child's propensity to be placed in foster care is incorporated into the relative placement equation as a control function. The standard errors are clustered at the investigator level to reflect variation in the predicted placement rates at that level.[17]

Table 5 reports the results of the selection models. Column (1) replicates the results in Table 3 but with the slightly different data set of 7,565 observations after excluding sexual abuse allegations that do not enter the investigator rotation, and cases that are assigned to investigators with fewer than ten cases. The next model introduces the investigator placement differential. This variable does not help explain relative placement, with a coefficient of 0.25 and a standard error of 0.16. This reduced form relationship suggests that a two standard deviation increase in the investigator placement differential is associated with a 2.5 percentage point increase in the relative placement rate—a rate that averages 64%. This lack of a relationship with placement

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[16] These models do not include the geographic indicators for Cook County or ZIP code fixed effects, as the instrument was calculated within ZIP code x year cells and differencing out geographic area means would incorporate information from the child's case. Nevertheless, the results are somewhat larger (in absolute value) when geographic controls are included.

[17] For comparison purposes, standard errors were also calculated with a bootstrap of 500 replications to take into account of the variation introduced by the predicted right hand side variable. The standard errors were slightly larger than those presented here and the inference is unchanged. Similar results were found when the standard errors were clustered at the family level as well.

type is consistent with the investigator placement tendency being unrelated to family characteristics.

Estimates from the first stage are shown in Column (3). Assignment to an investigator with a high placement differential has a significant effect on the likelihood that a child is removed from home, with a coefficient of 0.37 (s.e.=0.048). This implies that a two standard deviation increase in the investigator placement differential would increase the foster care placement rate by nearly 4 percentage points, compared to a mean placement rate of 9.3%. In addition, the overall placement rate has a 1.4 percentage point decline in FY1996 (with a standard error of 0.6%), compared to the 10.1% placement rate in FY1995. Due to this drop in the likelihood that a child is placed in foster care, the remainder of the paper considers models that incorporate the sample selection correction.

The second stage incorporates a sixth-order polynomial in the child's predicted placement rate, or propensity score for entry into the sample, which was estimated using the model shown in Column (3). The estimates continue to show a decline in FY1996 of 7.6 percentage points. The linear and cubic terms of the predicted placement rate polynomial are somewhat significant, though the coefficient on FY1996 is robust to its inclusion and to the order of the polynomial used.[18]

To further explore the results when the selection model is used, Table 6 reports the results for subgroups of cases. Given the smaller sample sizes, the differences across groups are often not statistically significantly different from one another. Rather, these estimates should be regarded as more exploratory and provide some evidence of the types of children who are driving the main results.

5.3 Family Reporters & Allegation Type

The main analysis is restricted to abuse cases, as neglect cases were the target of other entry reforms, especially for reports from family members. Further, family members who report the abuse may be inelastic caregivers, as they have already begun the process of protecting the child. To test the effect of the reform for neglect cases, and for family reports versus non-family reports, the first four rows of Table 6 report the results for these subsets of the data.

In abuse cases, the point estimate suggests a smaller response when family members report the abuse: a 6 percentage point drop compared to a mean relative placement rate of over 80%. There are fewer family reports of abuse, however, resulting in a less precise estimate. When non-family reports of abuse are considered—potentially the most exogenous of the wage offers—the drop is 9.6 percentage points, or 14% of the relative placement rate in the year prior to the reform.

The neglect cases, which to this point had been excluded, reflect the concurrent changes in the system and the incentives provided by the higher subsidy for informal kinship caregivers to join the system: a 20 percentage point drop at the time of the reform, or 22% less than the mean relative placement rate. When a non-family member reports the neglect, the results are similar to the abuse reports: an 8 percentage point drop from a mean relative placement rate of 52%. These non-family abuse and neglect cases reaffirm the preliminary estimates of a roughly 15% drop in the ability to find a relative, or an elasticity of 0.5.

5.4 Identifying the Marginal Child Types

Heterogeneity in response to the reform across different types of children can also identify those who are on the margin of finding willing relatives to provide foster care. Marginal

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[18] With polynomial orders of 1 through 6, the estimated coefficient on FY1996 ranges from -0.074 to -0.076.

children can suggest higher costs in caring for particular types of children, as well as the difference in the type of care that can be provided by a relative compared to a non-relative.

The remaining rows of Table 6 report the results for different child types, beginning with age groups. While relative placement rates tend to increase in age, there appears to be a U-shaped response to the reform by the age of the child—the youngest and oldest tend to have the largest responses to the reform with drops of 9 and 10 percentage points compared to 5 percentage points for five to ten year olds—a time when children may be less costly due to school attendance.

There is also a larger response to the reform for children who were referred for mental health services—a 23 percentage point drop in FY1996 for a group that was less likely to find a willing relative before the reform. Meanwhile, the response is larger for girls than for boys (-9.1 vs. -6.1); African American and Hispanic families compared to whites (10 vs. 3.5); and for smaller sibling groups. The only group for which no effect is found is for sibling groups larger than three. One explanation is that larger sibling groups are more likely to be split up if they enter traditional foster care, leading to a less elastic response from relatives.

The results for these subgroups show some heterogeneity in the response to the reform across child types. Still, a drop at the time of the reform is found for nearly all of the groups, demonstrating the robustness of the result.

6. Effect of the Reform on Child Outcomes

The lower subsidy may reduce the quality of care for a number of reasons: fewer resources are provided to the relatives who provide care; a smaller pool of applicants may cause foster care administrators to relax standards; higher subsidies may represent a type of efficiency

22

wage, where foster parents would have more to lose if they performed poorly and were dismissed; and if relative foster care were associated with better outcomes, then the reduction in relative placement would reduce quality.

Alternatively, the quality of care may rise following the reform. There are concerns with placing children in the care of relatives of abusive parents (Dubowitz et al., 1993). In addition, lower subsidies may screen out less altruistic relatives who are most interested in the cash payments. [19] This section compares child health, education, and placement outcomes before and after the reform to test whether quality of care changes as marginal non-kin caregivers substitute for marginal kin caregivers—those who are less likely to provide care at the lower subsidy.

A reduced-form equation is estimated to test whether there was a change in child outcomes at the same time that there was a change in the relative placement rate. For child i investigated at time t:

$$(8)\ O_i = \beta_0 + \beta_1 Y_t + \beta_2 X_i + \varepsilon_i$$

where O represents an outcome such as a medical checkup within a certain time period, while the right hand side mirrors that of equation (7), including the sample selection correction.

Tables 7-9 report the results for placement outcomes, health outcomes, and test scores. Two explanatory variables are reported in each table: the FY1996 indicator as above, and the FY1994 indicator. The omitted category in each regression is FY1995, so these two coefficients represent the change just after the wage reform, along with a comparison with the year prior to

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[19] Frey (1993) discusses the possibility of professionals "crowding out" higher quality volunteers. Titmuss (1971) argued that voluntary blood donation would result in superior quality blood compared to paid donors who were less altruistic. Haltiwanger and Waldman (1993) and Stewart (1992) present models where social capital is important and suggest that the quality of traditionally voluntary services may decrease as pay increases.

the reference year as a placebo test. The 1994 vs. 1995 comparison was chosen given that the relative placement rate stabilized somewhat during this time.

The results suggest that there was little change in outcomes following the reform. This is found when measured at one, two, three, or four years from the abuse report, and the tables report the one- and four-year estimates for three sets of data: all children; those placed with relatives; and those placed with traditional caregivers. The relative placement comparison directly compares households that receive different subsidy levels. The number of observations is slightly different in the one- and four-year comparisons, as children who were investigated in the final year of the sample are excluded from the one-year outcome comparisons, whereas the last four years of the sample are excluded from the four-year outcome comparisons.

Perhaps the most direct measure of quality is the likelihood that a foster family quits— stops providing care before the child is ready to return home. Much has been written about the harm caused to children who must move from one foster home to the next.[20] Relative placements are found to be more stable than non-relative placements: 60% of non-relative placements in these data end in a quit within one year of the abuse report; 31% is the one-year quit rate for relative placements. A 10 percentage point drop in relative placement would imply a 3 percentage point increase in the one-year quit rate, though the quality of relative placements may change following the reform as well.

Panel A of Table 7 reveals a 3 percentage point increase in the year following the reform, though this is found for the placebo test as well. Further, any increase appears to be confined to the traditional placements. Similar results are found when the two-, three-, and four-year quit

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[20] See Zinn, et al. (2006), James et al. (2004), Newton et al. (2000), and Smith et al. (2001).

rates are considered, as well. It appears that there is little change in the quit rate for children who were investigated around the time of the reform.

Another placement outcome of interest in the child welfare literature is whether the initial placement spell results in the child returning home—the usual permanency goal for foster children. Panel B of Table 7 shows that there was a slight reduction in the reunification rate, though the differences are not statistically significant. Still, the decline in the one-year reunification rate is most evident in the relative placements. There is virtually no change in the four-year reunification rate, especially compared to the placebo year.

The final placement outcome is whether the child returned home only to re-enter care. If the goal to increase the rate of family reunification had been achieved, a natural question would be whether this were at the expense of an increase in the re-abuse and re-entry of these children back into foster care. Given the lack of a change in the reunification rate, it is not surprising that the re-entry into foster care does not change very much, as shown in Panel C.

Two child health outcomes are considered: wellness visits and injuries.[21] Table 8 shows that wellness visits did not change substantially following the reform, with an increase of 3 percentage points compared to a mean of 77%. Much of this apparent increase is concentrated among traditional foster placements. In addition, panel B shows that broken bones were unchanged, though the point estimates suggest an increase for relative placements and a decrease in traditional placements. The relative placement increase is not statistically significant, however.

The last set of outcome measures uses math and reading scores of children after they entered care. The control variables now include the grade level and the prior-year's test score, as

well as the controls in Table 1 and the sample selection correction. The data are from the Chicago Public Schools, so only school-aged children in Chicago who have two years of test scores are considered. This sharply restricts the sample, and the relative vs. traditional placement comparison is not possible, as 87% of foster children in this group are placed with relatives.

The math and reading scores are reported in terms of the fraction of the student's grade level. These children are approximately 20% below grade level in both reading and mathematics. The tests are given in April each year. If a child entered foster care between January and March, the April score is used as the dependant variable. If the child entered foster care after March, the following year's score is used as the dependant variable.

Table 9 shows that test scores do not change very much following the reform. Math scores are essentially unchanged, while the reading scores are found to decrease slightly in the first year, and increase slightly three years after the report. With the small sample sizes, the results are not precisely estimated, however.

Overall, Tables 7-9 show that there were no changes in short-term outcomes for children who were investigated just before or after the wage reform. The results for outcome measures that most directly measure foster parent quality—quits and wellness visits—suggest very small changes even at the upper (or lower) limits of the confidence intervals. It appears that the quality of care provided by the marginal traditional foster families is similar to that of the marginal kinship foster caregiver—those who are less likely to provide care when the subsidy is lower.

7. Conclusion

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[21] See Case and Paxson (2001) for a discussion of the role of parents in child health investments.

The reform in Illinois offers a rare glimpse at the response of within-family altruism to a plausibly exogenous change in economic incentives. By concentrating on the first time a family was approached by child protection, it is possible compare families who receive different wage offers due to the timing of the abuse report. While the group may not be representative of families at large (these are mostly parents of parents who abuse or neglect their children), they are often the focus of policy interventions.

With subsidies offered to relative caregivers declining by an average of $120 per child per month, or 27%, the estimates here suggest that the propensity to provide care decreases by 10-15%. The estimate is closer to 15% for cases where the family is not involved in the abuse report, perhaps the most exogenous cases to consider. This result is robust to the treatment of the time variable, controls for case and neighborhood characteristics, and a sample selection correction. The largest responses were found for children who were referred for mental health services, for infants, and for teenagers. These children may represent the most costly child types or a consideration by potential relative caregivers that non-kin placements may provide quality care.

Finally, fewer resources are provided to kinship caregivers, which may result in worse outcomes for children, though the quality of caregivers may improve at the same time. The results suggest that child outcomes such as placement stability, medical care utilization, and test scores do not appear to suffer following the drop in wage offers. When analyzed separately, this is found for both relative and non-relative placements. It appears that the quality of care provided by the marginal traditional foster families may be similar to kin who are less likely to provide care at the lower subsidy offer.

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Figure 1A: Foster Children Going to Relatives
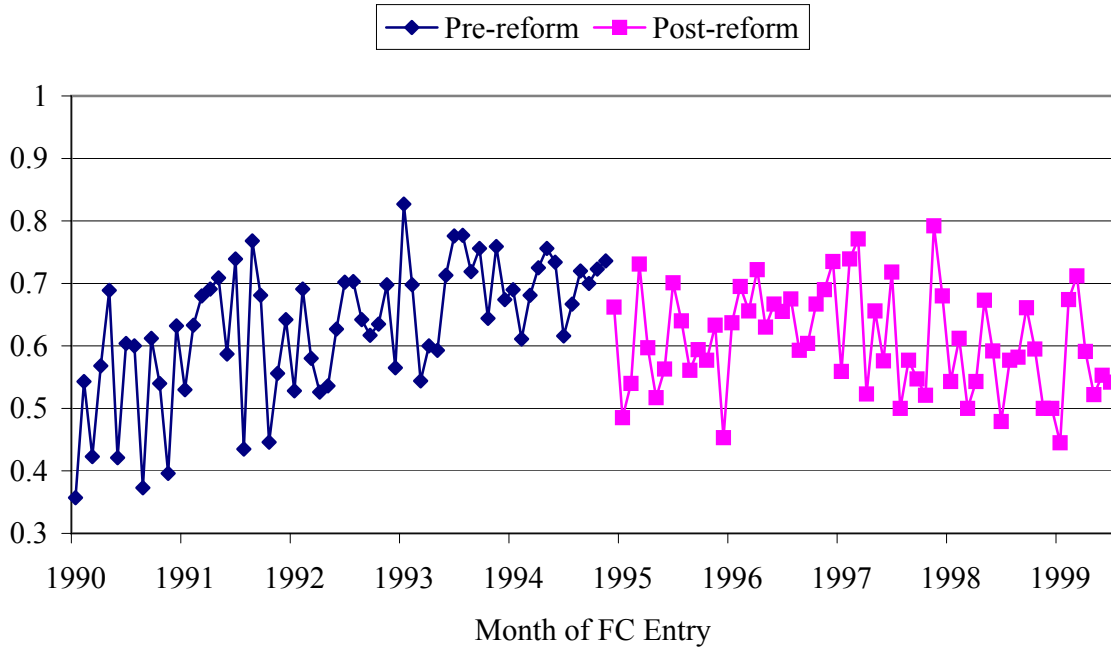
Figure 1B: FC Going to Relatives Estimated w/ Controls
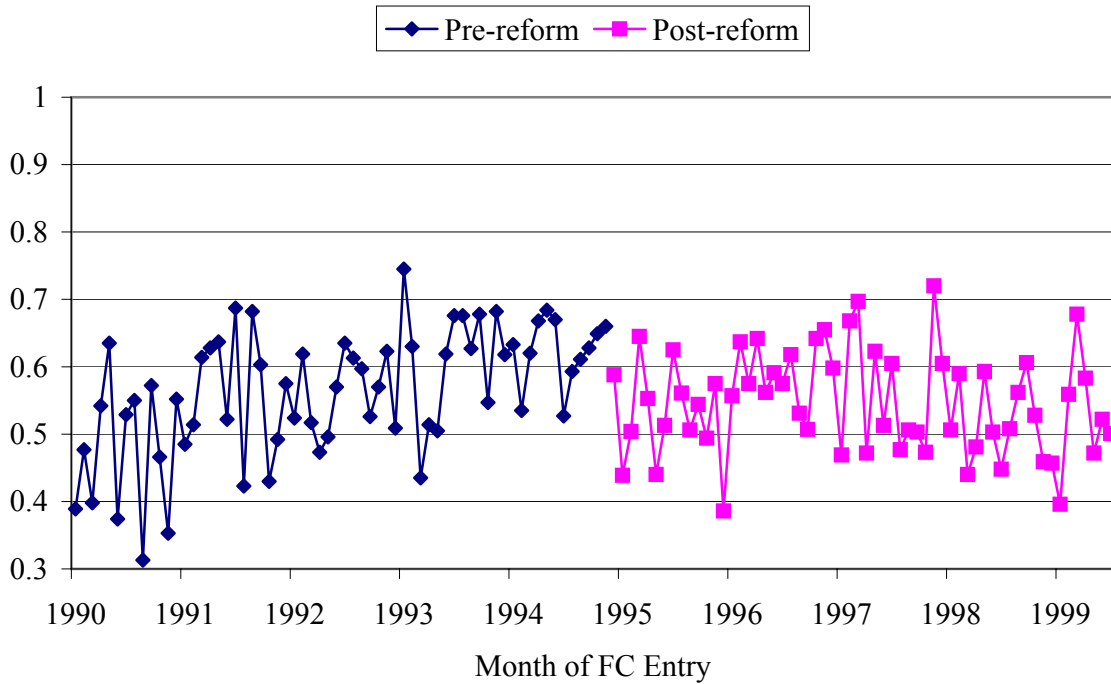
Points represent coefficients on monthly indicators in a model excluding the first month, scaled up by the relative placement rate in the first month. The model used for Figure 1B includes all variables listed in Table 1, including an indicator for each year of age, but not mental health services.

Table 1: Descriptive Statistics

Category		Mean	Std. Dev.	1-year difference: FY1996 - FY1995	t
Placement Type	Relative	0.63	0.48	-0.099	3.27**
Initial Reporter	Physician	0.31	0.46	-0.048	1.6
	School	0.09	0.29	-0.023	1.25
	Police	0.25	0.43	0.082	2.72**
	Family	0.12	0.32	-0.013	0.63
	Neighbor	0.03	0.17	-0.014	1.16
	Other Government	0.08	0.27	-0.019	1.09
	Anonymous	0.04	0.20	0.029	2.07*
	Other Reporter	0.08	0.26	0.006	0.36
Age group	Age <1	0.22	0.41	0.017	0.92
	Age >=1 & <5	0.34	0.48	0.007	0.32
	Age >=5 & <10	0.23	0.42	-0.004	0.18
	Age >=10 & <15	0.16	0.37	-0.014	0.81
	Age >15	0.05	0.22	-0.007	0.73
Sex	Boy	0.47	0.50	-0.021	0.88
Race/Ethnicity	White	0.37	0.48	0.027	0.9
	African American	0.53	0.50	-0.030	0.93
	Hispanic	0.08	0.27	0.003	0.17
Allegation	Physical Abuse	0.09	0.29	0.004	0.28
	Substantial Risk of Harm	0.71	0.45	-0.004	0.28
	Sexual Abuse	0.10	0.30	-0.018	0.81
	Other Abuse	0.10	0.30	0.019	1.28
Sibling Group	No siblings	0.31	0.46	0.017	0.65
	1 sibling	0.28	0.45	-0.003	0.1
	2 siblings	0.21	0.40	-0.033	1.18
	>2 Siblings	0.20	0.40	0.019	0.54
Location	Cook County	0.47	0.50	-0.005	0.02
Special Need	Mental Health Services	0.13	0.34	0.010	0.53
	Observations	8511			

1-year difference relates to the fiscal year of the foster care start date.

t-statistics are calculated using standard errors clustered by family. * significant at 5%; ** significant at 1%

Table 2: Quarterly Changes in Relative Placement

Dependent Variable: Relative Placement

	(1)	(2)	(3)
April-June 1994	-0.025	0.017	0.018
	[0.053]	[0.050]	[0.053]
July-September 1994	-0.058	-0.020	-0.034
	[0.053]	[0.049]	[0.048]
October-December 1994	0.019	0.059	0.046
	[0.050]	[0.046]	[0.047]
January-March 1995	-0.049	-0.048	-0.050
	[0.054]	[0.048]	[0.050]
July-September 1995	-0.155	-0.106	-0.104
	[0.061]	[0.058]	[0.060]
October-December 1995	-0.107	-0.092	-0.116
	[0.058]	[0.054]	[0.059]
January-March 1996	-0.085	-0.077	-0.111
	[0.057]	[0.051]	[0.051]
April-June 1996	-0.144	-0.106	-0.116
	[0.056]	[0.052]	[0.053]
July-September 1996	-0.137	-0.109	-0.101
	[0.059]	[0.054]	[0.056]
Full Controls	No	Yes	Yes
ZIP Fixed Effects	No	No	Yes
R-squared	0.020	0.146	0.223
Mean of Dependent Variable	0.631		
Observations	8511		

Full controls include all variables listed in Table 1, including indicators for each
age level, but not including mental health services.

Standard errors are reported in brackets, clustered by family.

Omitted category is the quarter prior to the reform.

Table 3: Annual Change in Relative Placement (Selected Covariates)

Dependent Variable: Relative Placement

Time Variable:	Fiscal Year of Foster Care Start Date			Fiscal Year of Investigation Date		
	(1)	(2)	(3)	(4)	(5)	(6)
Fiscal Year 1996	-0.099	-0.092	-0.102	-0.090	-0.082	-0.093
	[0.030]	[0.028]	[0.028]	[0.030]	[0.027]	[0.028]
Physician		0.075	0.059		0.075	0.058
		[0.028]	[0.029]		[0.028]	[0.029]
Police		0.020	0.005		0.020	0.004
		[0.029]	[0.030]		[0.029]	[0.030]
Family		0.159	0.142		0.159	0.141
		[0.031]	[0.031]		[0.031]	[0.032]
Other Government		-0.111	-0.127		-0.111	-0.127
		[0.035]	[0.036]		[0.035]	[0.036]
Anonymous		0.038	0.029		0.037	0.028
		[0.042]	[0.043]		[0.042]	[0.043]
Age 0		-0.168	-0.156		-0.168	-0.156
		[0.065]	[0.073]		[0.065]	[0.073]
Age 5		-0.087	-0.083		-0.086	-0.082
		[0.067]	[0.075]		[0.067]	[0.075]
Age 10		-0.094	-0.085		-0.094	-0.085
		[0.068]	[0.076]		[0.068]	[0.076]
Age 15		-0.009	-0.027		-0.009	-0.027
		[0.069]	[0.077]		[0.069]	[0.077]
1 sibling		0.047	0.047		0.047	0.047
		[0.015]	[0.016]		[0.015]	[0.016]
2 siblings		0.058	0.059		0.058	0.058
		[0.019]	[0.020]		[0.019]	[0.020]
>2 Siblings		0.021	0.016		0.020	0.015
		[0.023]	[0.023]		[0.023]	[0.023]
Cook County		0.260			0.260	
		[0.016]			[0.016]	
Full Controls	No	Yes	Yes	No	Yes	Yes
ZIP fixed effects	No	No	Yes	No	No	Yes
R-squared	0.011	0.138	0.216	0.011	0.137	0.216
Mean of Dep. Var.	0.631					
Observations	8511					

Full controls include all variables listed in Table 1, including indicators for each

age level, but not including mental health services. Omitted categories: other reporter; age 17; and no siblings.

Standard errors are reported in brackets, clustered by family. The omitted category is FY1995.

Table 4: Child Characteristics and Investigator Assignment (Selected Covariates)

Dependent Variable: Investigator Placement Differential

Variable		Coeff.	t	p-value
Year (1995 Excluded)	Fiscal Year: 1996	0.0005	0.31	0.759
Initial Reporter	Physician	0.0010	0.64	0.52
(Other Reporter	School	-0.0003	-0.25	0.805
Excluded)	Police	0.0008	0.64	0.519
	Family	0.0002	0.13	0.9
	Neighbor	0.0006	0.32	0.751
	Other Government	-0.0001	-0.04	0.966
	Anonymous	-0.0020	-0.98	0.329
Age at Report	Age = 0	0.0001	0.06	0.956
(Age 17 excluded)	Age = 5	-0.0003	-0.14	0.889
	Age = 10	-0.0006	-0.29	0.776
	Age = 15	-0.0006	-0.29	0.775
Sex	Boy	0.0005	1.22	0.223
Race/Ethnicity	White	-0.0001	-0.07	0.944
(Other race	African American	0.0000	-0.01	0.994
Excluded)	Hispanic	-0.0058	-1.72	0.085
Allegation	Physical Abuse	-0.0018	-1.72	0.085
(Other abuse	Substantial Risk of Harm	-0.0007	-0.57	0.571
Excluded)				
	1 Sibling	0.0011	1.68	0.093
Sibling Group Size	2 Siblings	0.0012	1.53	0.126
	>2 Siblings	0.0028	2.47	0.014
	Mean of Dependent Variable	-0.0006		
	Standard Deviation	0.0518		
	Number of investigators	1064		
	Observations	81622		

t-statistics and p-values are calculated using standard errors clustered by investigator.
FY1996 refers to the year of the abuse report; Omitted category is FY1995.

Table 5: Sample Selection Correction

Dependent Variable:	Relative Placement		Foster Care Placement	Relative Placement
	(1)	(2)	(3)	(4)
Fiscal Year 1996	-0.083	-0.084	-0.014	-0.076
	[0.030]	[0.030]	[0.006]	[0.031]
Investigator Placement Differential		0.252	0.367	
		[0.157]	[0.048]	
Predicted Placement				1.43
				[0.894]
Predicted Placement^2				-2.58
				[7.65]
Predicted Placement^3				-86.5
				[34.9]
Predicted Placement^4				656
				[387]
Predicted Placement^5				-1544
				[1356]
Predicted Placement^6				1187
				[1,454]
Observations	7565	7565	81622	7565
R-squared	0.085	0.086	0.05	0.087
Mean of Dependent Variable	0.641	0.641	0.093	0.641

Data restricted to first indicated abuse cases, excluding sexual abuse and cases assigned to investigators

with less than 10 cases. All models include full controls including all variables listed in Table 1, including indicators for each

age level, but not mental health services nor geographic indicators..

Standard errors are reported in brackets, clustered by investigator. The omitted category is FY1995.

Table 6: Estimates across Subgroups

Dependent Variable: Relative Placement

Separate Regression by:	Coefficient on FY 1996	S.E.	Mean of Dep. Var.	Observations
Allegation & Reporter				
Neglect, Report from Family	-0.196	[0.076]	0.90	1640
Neglect, Report Not from Family	-0.080	[0.056]	0.52	3038
Abuse, Report from Family	-0.062	[0.082]	0.81	884
Abuse, Report Not from Family	-0.096	[0.033]	0.62	6743
Age group				
Age <1	-0.090	[0.045]	0.55	1826
Age >=1 & <5	-0.074	[0.041]	0.67	2745
Age >=5 & <10	-0.049	[0.063]	0.67	1656
Age >=10 & <15	-0.085	[0.074]	0.65	1056
Age >15	-0.097	[0.148]	0.70	282
Sex				
Boy	-0.061	[0.037]	0.64	3826
Girl	-0.091	[0.036]	0.64	3739
Race/Ethnicity				
White	-0.098	[0.040]	0.70	4197
African American	-0.035	[0.053]	0.52	2673
Hispanic	-0.100	[0.086]	0.74	575
Sibling Group				
No siblings	-0.063	[0.040]	0.56	2306
1 sibling	-0.123	[0.053]	0.66	2107
2 siblings	-0.099	[0.073]	0.70	1582
>2 Siblings	-0.008	[0.083]	0.68	1570
Location				
Cook County	-0.089	[0.035]	0.79	3627
Outside Cook County	-0.089	[0.043]	0.50	3938
Special Need				
Mental Health Services	-0.232	[0.078]	0.47	954

Each row represents a separate regression with controls used in Table 5, including the sample selection correction
One exception is the model for children who received mental health services, which has no sample selection correction, as this indicator is known only for those who enter foster care.
Standard errors are reported in brackets, clustered by investigator. The omitted category is FY1995.

Table 7: Placement Outcomes

A. Dependent Variable: Foster Parent Quits

Sample:	All		Relative Placements		Traditional Placements	
	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years
Fiscal Year 1994	0.033	0.059	0.022	0.062	0.029	0.029
	[0.033]	[0.034]	[0.039]	[0.042]	[0.055]	[0.053]
Fiscal Year 1996	0.026	0.037	-0.013	0.005	0.025	0.032
	[0.032]	[0.032]	[0.037]	[0.040]	[0.047]	[0.049]
Observations	7086	5025	4572	3275	2514	1750
R-squared	0.019	0.033	0.031	0.053	0.068	0.072
Mean of Dep. Var.	0.413	0.546	0.309	0.489	0.601	0.654

B. Dependent Variable: Family Reunification

Sample:	All		Relative Placements		Traditional Placements	
	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years
Fiscal Year 1994	-0.015	-0.017	-0.019	-0.020	0.000	-0.001
	[0.027]	[0.030]	[0.032]	[0.037]	[0.045]	[0.047]
Fiscal Year 1996	-0.030	-0.022	-0.054	-0.015	0.010	-0.006
	[0.024]	[0.030]	[0.030]	[0.037]	[0.042]	[0.046]
Observations	7086	5025	4572	3275	2514	1750
R-squared	0.049	0.046	0.06	0.059	0.055	0.057
Mean of Dep. Var.	0.198	0.281	0.188	0.282	0.215	0.28

C. Dependent Variable: Re-enter Foster Care

Sample:	All		Relative Placements		Traditional Placements	
	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years
Fiscal Year 1994	0.017	0.036	0.002	0.020	0.050	0.072
	[0.013]	[0.016]	[0.015]	[0.020]	[0.021]	[0.024]
Fiscal Year 1996	0.018	0.018	-0.005	-0.006	0.058	0.058
	[0.012]	[0.015]	[0.015]	[0.019]	[0.020]	[0.025]
Observations	7086	5025	4572	3275	2514	1750
R-squared	0.016	0.023	0.019	0.027	0.036	0.045
Mean of Dep. Var.	0.054	0.093	0.044	0.077	0.072	0.124

All models include full controls, including the sample selection correction.
The samples are restricted to investigations from 1990-1999 for the 1-year outcomes, and 1990-1996 for the four-year outcomes.
Standard errors are reported, clustered by investigator. The omitted category is FY1995.

Table 8: Health Outcomes

A. Dependent Variable: Wellness Visit

Sample:	All		Relative Placements		Traditional Placements	
	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years
Fiscal Year 1994	0.001	0.008	-0.008	-0.002	0.020	0.031
	[0.024]	[0.022]	[0.028]	[0.025]	[0.047]	[0.043]
Fiscal Year 1996	0.028	0.032	0.018	0.032	0.060	0.050
	[0.025]	[0.021]	[0.029]	[0.023]	[0.042]	[0.040]
Observations	7565	7565	4846	4846	2719	2719
R-squared	0.046	0.018	0.049	0.025	0.059	0.024
Mean of Dep. Var.	0.774	0.847	0.783	0.862	0.758	0.819

B. Dependent Variable: Injury (Broken Bone)

Sample:	All		Relative Placements		Traditional Placements	
	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years	w/in 1 year	w/in 4 years
Fiscal Year 1994	0.002	0.015	0.003	0.020	0.001	0.003
	[0.020]	[0.025]	[0.024]	[0.029]	[0.038]	[0.046]
Fiscal Year 1996	-0.008	0.006	0.032	0.056	-0.072	-0.074
	[0.020]	[0.026]	[0.026]	[0.032]	[0.031]	[0.041]
Observations	7565	7565	4846	4846	2719	2719
R-squared	0.022	0.026	0.028	0.035	0.038	0.042
Mean of Dep. Var.	0.123	0.248	0.128	0.252	0.114	0.242

All models include full controls, including the sample selection correction.

The samples are restricted to investigations from 1990-1999 for the 1-year outcomes, and 1990-1996

for the four-year outcomes.

Standard errors are reported, clustered by investigator. The omitted category is FY1995.

Table 9: Change in Test Scores

A. Dependent Variable: Mathematics (Fraction of Grade Level)

	at 1 year	at 2 years	at 3 years	at 4 years
Fiscal Year 1994	-0.031	-0.007	-0.022	0.023
	[0.035]	[0.036]	[0.042]	[0.051]
Fiscal Year 1996	-0.009	-0.006	0.015	-0.024
	[0.033]	[0.039]	[0.044]	[0.045]
Observations	327	278	211	146
R-squared	0.588	0.525	0.48	0.546
Mean of Dep. Var.	0.824	0.828	0.827	0.833

B. Dependent Variable: Reading (Fraction of Grade Level)

	at 1 year	at 2 years	at 3 years	at 4 years
Fiscal Year 1994	-0.075	0.000	-0.033	0.007
	[0.039]	[0.053]	[0.054]	[0.064]
Fiscal Year 1996	-0.037	0.011	0.052	-0.018
	[0.039]	[0.045]	[0.044]	[0.066]
Observations	331	279	214	148
R-squared	0.469	0.415	0.401	0.416
Mean of Dep. Var.	0.78	0.786	0.806	0.838

All models include full controls, including the grade level, the prior-year
 test score, and sample selection correction.
The samples are restricted to investigations prior to 1, 2, 3, or 4 years from
the end of the sample period for the four columns of results, respectively.
Standard errors are reported in brackets, clustered by investigator.
The omitted category is FY1995.