

**Can't Buy Me Love?
Subsidizing the Care of Grandchildren**

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Abstract:

Altruism is central to economic models of the family, yet little is known about the response of within-family altruism to changes in its cost. On July 1st 1995, Illinois cut the monthly subsidy offered to grandparents and other relatives to care for children entering foster care—those children removed from home due to abuse or neglect. This provides a rare opportunity to test the effect of a plausibly exogenous change in the cost of caring for related children on the propensity to provide care. Following the thirty- percent decline in subsidies, kin are found to have a ten to twenty percent lower propensity to provide foster care. The response is especially large for younger children, and for those who require mental health services. Finally, child outcomes do not appear to suffer following the reform, with child health outcomes tending to improve. Lower subsidies may ‘weed out’ those more interested in the monetary reward, who may be of lower quality.

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1. Introduction

With the well-documented rise in the number of single-parent families in the U.S., the extended family has grown more important. For example, more children are living with their grandparents. 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). In addition, as more mothers make the transition from welfare to work, the extended family is seen as a natural source of day care.

A similar call for support comes from children in foster care—children who are temporarily removed from their parents due to abuse or neglect and placed with foster parents who are paid to provide care. Over five hundred thousand children currently reside in foster care in the U.S., and in the late 1980s there was a substantial shift toward using relatives to care for foster children (Wulczyn, et. al., 1999). In most states, kin are looked to first when a child is removed from home (Jantz, 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 grandparents, or other relatives, increased dramatically, especially in large cities. For example, in New York City there were 151 official kinship care cases in 1985; five years later there were over 23,000. The rise in kinship foster care has been one of the most important phenomena in child welfare over the past twenty years (Scannapieco, Hegar, and McAlpine, 1997).

With the increasing reliance on kin to provide support, a better understanding of their response to economic incentives has the potential to inform policy and shed light on the economics of altruism, fertility, and the cost of children. The economics of the family has received considerable attention, with an emphasis on within-family altruism¹. Nonetheless, the

¹ The role of economic incentives in family decisions, such as fertility and the allocation of resources, has a wide literature. Becker (1981) describes the major issues. Hotz, Kerman, and Willis (1997) offer a review of the fertility literature.

response of the willingness to care for relatives to changes in the cost of caring is not well understood. Changes in such costs are usually difficult to quantify, or suffer from endogeneity problems. For example, consider the demand for children. Children are time intensive, and the wage rate is sometimes used as a measure of the cost associated with caring for them. However, if the wage rate were a function of human capital decisions that were made jointly with fertility decisions, or if market and home productivity were correlated, then comparing fertility or child outcomes for parents with different wage rates would not only capture differences costs, but differences in parents as well.

The aim of this paper is to test the effect of a plausibly exogenous change in the cost of caring for related children on both the willingness to provide care and the quality of care provided. On July 1st, 1995, Illinois began offering relatives approximately 30% lower monthly subsidies to care for children entering foster care. Using longitudinal administrative data, the effect of the subsidy reform on the willingness of kin to provide care will be tested. The usual endogeneity problem is avoided if the subsidy offer is unrelated to the characteristics of relatives that may influence the decision to provide care.

A second goal of the paper is to test whether lower subsidies affected the quality of care, as measured by child outcomes. Lower subsidies imply an income effect that will likely result in worse outcomes for children. Meanwhile, there may be a selection effect. If those with greater outside opportunities respond to the change in subsidy offer, and provide higher quality care, then child outcomes will worsen. On the other hand, lower subsidies may “weed out” those relatives who are more interested in the monetary reward. If those who participate as a ‘labor of love’ are the ones who continue to accept the offer, and if these more altruistic foster parents provide higher quality care, then the selection effect may mitigate the negative income effect.

This idea that the average quality of care may rise with lower wages is a standard result in occupational choice, as those who stay may be higher quality than those who decline the offer. This paper offers a way to test this implication in an area where such a selection effect is plausible and where the answer is important for setting subsidy rate policy.

Foster care provides a unique environment to study within-family altruism, particularly in those families targeted by child welfare policy. Usually within-family altruism is difficult to measure, but, here, the choice to provide foster care and the change in cost are explicit. Further, much is known about the foster care investigation, including the strength of the case and the intensity of the investigation. This allows a novel way to control for sample selection. In particular, the strength of the case strongly predicts whether a child will be placed in foster care, but the willingness of kin to provide care is not related to the strength of the case. By exploiting variation in the sample due to the strength of the cases, as opposed to changes in the types of children entering following the reform, it is possible to obtain cleaner estimates.

Finally, this is an important policy area, with over twenty billion dollars spent annually on child protection in the U.S. (Bess et. al. 2002). In addition, policies are often aimed to serve “at-risk” children. While long-term consequences foster care experiences are less well understood, foster children are found to be more likely to commit crimes, dropout of school, enter the homeless population, join welfare, and get pregnant as a teenager (Courtney and Piliavin, 1998, Dworsky and Courtney, 2000, US DHHS, 1999). Abused and neglected children appear to be the most ‘at risk’.

As a preview, following the roughly 30% drop in subsidies for new foster care entrants in Illinois, the propensity of relatives to provide care declines by 10-20% at the time of the reform, with a larger long-term response. Meanwhile, other states do not show a change in the use of

kinship foster care at this time, suggesting that national trends, such as a strong economy or welfare reform, are not driving the results in Illinois. In addition, larger responses are found for children who require mental health services, and for younger children. This suggests that these children may be more costly. Finally, child outcomes do not appear to suffer following the reform. In fact, child health tends to improve following the reform, which is consistent with higher quality foster parents willing to accept the lower subsidy.

The paper is organized as follows. Section two presents background on the foster care entry process and the reform in Illinois. Section three discusses the discrete choice problem facing relatives who are asked to provide care. It suggests that the quality of foster parents, as measured by their level of altruism toward the child, may rise or fall following the reform as kin choose to participate. Section four describes the rich administrative data available in Illinois. Section five presents the empirical models and results, and section six concludes.

2. Background on Child Welfare Policy in Illinois

Children enter foster care when their parents are suspected of abuse or neglect. In Illinois, the Department of Child Protection determines if the child should be removed from home. Then it determines if a child is suitable for a family foster home as opposed to a more restrictive setting, such as a group home.² For children eligible for family foster care, a relative is sought out first to take the child. This is seen as less traumatic for the child. In addition, caseworkers are not required to monitor kinship cases as strictly as non-relatives, so kinship foster care is also seen as a way of decreasing the effective caseload of social workers. Important for this study, if no willing relative were found, then the child is placed with a non-

² Children who are homicidal, suicidal, or have other special needs may be determined ineligible for family foster care.

relative foster family. That is, if a child is placed with a non-relative, then a willing relative could not be found. This will serve as a measure of a family's willingness to provide care.³

When a relative is approached to provide foster care, monthly subsidies are offered for each child. In Illinois, licensed foster parents receive a monthly subsidy, currently ranging from \$361 to \$445 per child per month depending on the age of the child. This is up to four times higher than the monthly subsidy paid through Temporary Assistance for Needy Families (TANF).

Motivation for Reform

In Illinois, the total number of children in foster care—in both relative and non-relative placements—showed unprecedented growth from 1985-1995. 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, the Illinois Department of Children and Family Services (DCFS) was placed under judicial oversight due to the long stays in foster care, averaging over four years.

The reform was a response to the large growth in the foster care system. One reason suggested for the rise in the foster care population was the relatively large payments made available to kinship foster parents compared to other welfare programs (Testa, 1997). Despite hopes that placement closer to home would facilitate family reunification, children have longer lengths of stay in kinship foster care and have greater placement stability (Berrick, 1998). This is not surprising given the large 'tax on reunification'. That is, if a relative returned a child home, the income from the state to that child would fall significantly. With the increase in the

³ It could be argued that with lower subsidies following the reform, caseworkers may decide that relatives can not afford the child—resulting in lower participation rates among relatives. However, the explicit policy to seek relatives first was continued at this time. Further, placing children with relatives is seen as easier for caseworkers (fewer home visits), and is regarded in Illinois as less traumatic for the child. This suggests that relatives were actively sought throughout this period.

flow of new entrants, and the longer stays, the population of children in kinship foster care grew enormously.

Using the reform in Illinois that will be used here, Testa and Slack (2002) find evidence that subsidy rates to kinship providers do matter in terms of placement duration. Homes that saw their subsidies cut following the reform had a two to three times higher rate of exit compared to those who kept the higher payment. Similarly, Berrick and Needell (1999) show suggestive evidence that the implicit is important. In their work, they exploit the fact that some children in California's kinship foster care program had relatives who received the regular foster care rate, while others received the AFDC rate. Those who received the higher foster care rate were half as likely to be reunified within four years as children who received the lower subsidy. However, it is not clear whether these results are due to the difference in payments or the difference in qualifications to receive the higher payment. In Illinois, relatives already providing care at the time of the reform were able to appeal to the state to retain the higher payments. Approximately half of the relatives did not appeal, and another quarter lost on appeal. However, families who chose not appeal may have foreseen shorter lengths of stay—creating a relationship between subsidy declines and length of stay. My project attempts to overcome selection issues by focusing on new entrants to the foster care system.

The Subsidy Reform

Before July 1st, 1995, relative caregivers received the same foster care subsidies that non-relatives received. Afterwards, a two-tiered system was created where families continue to 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—a 30% decline. 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.

While many of those providing care at the time of the reform became licensed following a set of appeals, few new entrants became licensed. Each year following the reform, approximately five percent of kinship providers became licensed at the beginning of the child's spell. In the first year of care, the fraction that became licensed increased to twelve percent. For providers who began providing care in the first year following the reform, twenty-five percent eventually became licensed over the following five years. This low rate of licensure suggests that, at the time of entry, most kin could expect that they would not receive the higher subsidies associated with becoming licensed.

However, there is a puzzle why more kin were not licensed. The kin would receive a higher subsidy, and the state of Illinois would receive matching funds from the federal government. However, the licensing standards can be stringent, including space requirements—such as a separate bedroom in some cases—that may limit the ability of kin to become licensed. When asked, kin also report that they see no reason to enter an even more formal relationship with a relative. It appears that kin are not willing to submit to the more intrusive home examination that licensing entails, especially as foster care stays are thought to be temporary arrangements.

Following the reform, relatives appear less willing to take children. Figure 1 compares new entrants into foster care in Illinois with new entrants outside of Illinois using the Multi-State Data Archive—the largest data source for kinship foster care over this time period (Wulczyn, Brunner, and Goerge, 1999). In Illinois, the fraction of children finding willing relatives grows

from 1990-1995. After the reform, that trend breaks and begins to decline. This dramatic change in the use of kinship foster care following the reform suggests a significant effect of the subsidy reform on the willingness of kin to provide care.

Outside of Illinois there is no such break in trend. The flat line is the result of declines in New York and increases in most other states, but no other state demonstrates a change in trend at the time of the Illinois reform. Of course, these other states are not an ideal matched comparison—Illinois relied on kinship foster care more than other states, and the popularity grew from 1990 to 1995. However, the comparison demonstrates that national trends, such as the booming economy, do not appear to drive the decline in the number of relatives found willing to provide care.

Concurrent Foster Care Reforms

Due to this growth in children and spending, the reform of the payments to kin was accompanied by other efforts to slow the growth of foster care (McDonald, 1999). In particular, Illinois sought to stop formalizing previously existing, informal kinship care, and the number of entrants who were referred by family members drops considerably following this change. In addition, new emphasis was placed on keeping children at home. As a result of these ‘entry reforms’, the total number of children entering foster care also rises and falls during this time period.⁴ These changes may mean that new foster care entrants may differ before and after the reform—an issue that is dealt with below.

3. A Discrete Choice Model of Caretaking

⁴ The child population in Illinois also rises and falls during this time period, partly explaining the rise and fall in entry as well.

The reform increased the cost of caring for children. Theory suggests that this will lower the propensity to provide foster care. For example, models of labor supply, or the demand for children, predict that lower subsidies would be associated with a smaller propensity to provide foster care. In addition, the quality of foster parents may rise or fall following the reform. Lower subsidies would imply a negative income effect on child outcomes. A selection effect may mitigate this income effect, however, if those who accept the lower subsidy are more altruistic, and if more altruistic caretakers provide higher quality. Overall, the effect of the subsidy reform on this selection effect is ambiguous.

Discrete Choice Model

The decision to accept a child who was removed from home is modeled as a discrete choice⁵. A potential relative foster parent will accept the child if utility from providing care is greater than allowing the child to be placed with a non-relative:

$$(1) \quad \max_{c_p, c_k} \begin{cases} U_p(c_p) + aU_k(c_k) \\ c_p + c_k < Y + S \end{cases} > \max_{c_p} \begin{cases} U_p(c_p) + aV_k(\tilde{c}_k) \\ c_p < Y \end{cases}$$

where c_p, c_k are consumption for the potential relative foster parent and child, Y is income, S is the subsidy, \tilde{c}_k is the consumption of the child in a non-relative's home, and a is an altruism parameter. V_k is the child's utility when placed with a non-related family, which will depend on the income and altruism of that family, and is written as V to suggest that utility may be state dependent. Notice that the right hand side of this inequality does not depend on the subsidy

⁵ In reality, there may be more than one relative approached to care for the child. This model can be reinterpreted as the decision of the caretaker most likely to take the child. This distinction should not affect the results if the number of potential caretakers does not differ before and after the reform.

level. This is especially appropriate here, as the reform did not alter the subsidies offered to non-relatives.

Substituting in the budget constraint, this problem can be described by a sharing rule (Chiappori, 1992). The choice simplifies to taking the child if:

$$(2) \quad U_p[(Y + S)(1 - \omega(a, Y, S))] + aU_k[(Y + S)\omega(a, Y, S)] > U_p(Y) + aV_k(\tilde{c}_k)$$

where ω is the fraction of resources that are consumed by the foster child.

As the subsidy offered to kin is lowered, consumption will decrease for both the foster parent and the child if consumption is a normal good. In other words, both the potential parent's and the child's utility are lowered following the reform—the two terms on the left hand side of the inequality—and the propensity to provide care is lowered.

Past empirical investigations 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, 2001). 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 studies the decisions at the child level for a more direct comparison. Further, the response to the change in subsidies is compared across different types of children. This can inform the cost of children literature, as we may expect a larger decline in the willingness to care for the most 'costly' children.⁶

⁶ Differences in the willingness to care for these children may be due to variation in the altruism parameter, or the utility functions of children, by these child characteristics, in addition to potential costs. This caveat is discussed more fully below.

Quality of Foster Care

Quality of care may rise or fall following the reform. The lower subsidy will provide fewer resources for children, and this may lower the quality of care directly. In addition, having fewer potential foster parents may lower quality. If foster care administrators were to lower standards for foster parents given the smaller pool of applicants, then quality could fall. Another argument stems from efficiency wage theory. Higher subsidies imply that foster parents would have more to lose if they performed poorly and were dismissed. Thus, lowering subsidies may translate into lower quality.

Alternatively, fears that large subsidies may attract low quality caretakers have a long history. The director of the Child Welfare League of America in 1922 stated, “no home should be used that does not give the boarded child much more than is being paid for by the board money. This provides against the commercialization of the work” (Waldinger, 1985). These fears suggest that higher subsidies could lower quality in two ways. First, applicants interested in supplementing their income and less interested in helping children may choose to become foster parents if subsidies are set high enough. Second, if fostering is seen as an altruistic occupation it may attract high quality foster parents. If that perception is changed to one of professionals, these altruists may no longer see a need to donate their time. Frey (1993) discusses this possibility of professionals “crowding out” higher quality volunteers.

Economic models of altruism, which stress the social nature of the activity (my supply decision depends upon how much others supply), may offer insights into these questions.⁷ An analogy can be made to blood donation. Titmuss (1971) raised a controversial argument that the British voluntary system provided a safer blood supply than the American system that used

⁷ Haltiwanger and Waldman (1993) and Stewart (1992) present models where social capital is important and suggest that traditionally voluntary services can decrease as pay increases.

voluntary and paid donors. If those who donate for charitable purposes are healthier than those who “do it for the money”, then the quality of the blood supply may increase when subsidies are reduced to zero. In the same way, paying higher foster care subsidies may attract lower quality foster parents.

The potential for higher average quality with lower subsidies is the result of a change in the composition of the labor force. In a Roy model framework, it is well established that as the price paid for a skill increases, the average quality can fall as the new entrants to the field may be of lower quality than the incumbents (Heckman and Honore, 1990).

In the foster care labor market, skill levels are not distinguished in the wage function. Instead, we can consider heterogeneity in the disutility of work instead of differences in skill levels. As the wage falls, those most likely to decline the opportunity to support their related children are likely those with the greatest disutility. If lower disutility is associated with higher quality care—you perform better at tasks that you enjoy—then the lower wage would be associated with higher average quality. Of course, if those with the best outside options are also those who would make the best foster parents, then lower wages may translate into lower quality.

To fix ideas, consider the discrete model discussed above. Figure 2 shows that this decision can be characterized by a reservation subsidy. For subsidies greater than S^{res} , the relative would agree to take the child. S^{res} can be solved from (2) by setting the two sides equal:

$$(3) \quad U_p[(Y + S^{res})(1 - \omega(a, Y, S^{res}))] + aU_k[(Y + S^{res})\omega(a, Y, S^{res})] - U_p(Y) - aV_k = 0$$

Exploiting the reservation subsidy nature of this problem, relatives with a large altruism parameter may have higher or lower reservation subsidy rates. Using the first order conditions:

$$(4) U'_p = aU'_k$$

We can solve for:

$$(5) \frac{dS^{res}}{da} = \frac{(Y+S)\omega_a(a,Y,S)[U'_p - aU'_k] + V_k - U_k}{((Y+S)\omega_s + \omega)[aU'_k - U'_p] + U'_p}$$

$$= \frac{V_k - U_k}{U'_p}$$

With foster parents' utility increasing in consumption ($U'_p > 0$), the sign of the numerator determines the sign of the derivative. This states that for foster parents at the margin, if the child were better off in the care of non-relatives ($V_k > U_k$), then those with a high altruism parameter would have a higher reservation subsidy compared to relatives who care less about the children. However, if the child were better off in the care of the relative ($V_k < U_k$), then those relatives who care more about the children will have a lower reservation subsidy. As the reform lowers the subsidy, those at the margin of providing care at the higher subsidy will no longer do so, and this will induce a selection of caregivers who may have higher or lower levels of altruism. If greater altruism is associated with higher quality care, then child outcomes may improve or worsen following the reform.

While there is a large literature on the cost of children, and some evidence that the supply of foster care is positively related to subsidies, the effect of subsidies on the quality of care is much less studied. The quality of foster parents may rise or fall following the reform as a result of both an income effect and a selection effect. The lower subsidy will provide fewer resources to the household, and this can be expected to worsen child outcomes. However, the lower

subsidies may attract only high quality foster parents—‘weeding out’ those more interested in the subsidy.

Chamberlain et. al. (1992) and Testa and Rolock (1999) find mild evidence of increased placement stability with increased subsidies. That is, children were more likely to remain in a single placement instead of having to suffer through multiple disruptions. While sample sizes were small, (n=45 and 33, respectively), they do suggest that greater subsidies lead to greater quality along this dimension.

This paper considers child outcomes, such as child health and placement stability, to test the effect of the change in subsidies on quality⁸. If child outcomes improve following the reform, this would suggest that higher quality foster parents are the ones willing to accept the lower subsidy. If outcomes worsen afterwards, it may be the result of both fewer resources due to the reform and the types of foster parents agreeing to participate.

4. Data Description

This paper uses longitudinal, administrative data used by the Illinois Department of Children and Family Services from July 1st, 1990 to July 1st, 2001. The Department’s Child and Youth Centered Information System (CYCIS) tracks children in foster care, including demographic data such as age, race, sex, reason for entry, mental health needs, and placement start and end dates. The Child Abuse and Neglect Tracking System (CANTS) provides details of the child abuse investigation. Data from these systems, and other social programs, are collected quarterly by the Chapin Hall Center for Children (Goerge, Van Voorhis, and Lee, 1994). In

⁸ Differences in educational test scores in Chicago Public Schools were also considered. However, given that over half the children are under five years old, and that those over five are often found to be neglected when they first enter care (and, thus, there is little pre-entry information), there number of observations was small. In addition, the vast majority of these Chicago students were in relative placements, and the first stage effect of the subsidy change on relative placement was smaller. The results were noisy, but no differences in test score changes were found.

particular, administrative data from Medicaid are available to investigate health care use by foster children, as all foster children are enrolled in Medicaid.⁹ To gain insights into the foster care providers, the child welfare data can also be matched to Department of Public Aid data, which contains demographic data for those who have had contact with Public Aid, such as TANF recipients. Illinois Department of Labor administrative data—collected from firms as part of the workers compensation insurance program—provide information on foster parents’ employment and earnings. Finally, Chapin Hall maintains the Multi-state Data Archive—a collaboration among eleven states to track trends in foster care (Wulczyn, Brunner, and Goerge, 1999). The archive represents over half of the foster care population in the United States. For the time period discussed here, seven states have data available for kinship foster care: California, Maryland, Michigan, Missouri, New York, Wisconsin, and Illinois. In sum, by matching individuals across a wide array of administrative agencies in Illinois, a uniquely rich data source is available to carry out the analysis.

The data are first restricted to cases eligible to enter a relative’s foster home—those without major behavioral or health problems. In addition, only first spells in foster care are considered to abstract from the effect of previously established kinship foster care relationships.

To consider the data available in Illinois, Table 1 shows summary statistics for new entrants into foster care. After the reform, they are slightly younger, more likely to be white, and are less likely to require mental health services. The biggest difference is that they are more likely to have been abused as opposed to neglected (39% before, 58% after). This reflects the attempts to lower the intake of children into foster care, as cases of neglect were more likely to be treated within the biological family following the entry reforms. For example, grandchildren

⁹ Medicaid data are available up to September 1, 2000.

already living with grandparents were eligible to enter the formal foster care system as a result of parental neglect. After the reforms, these cases were no longer eligible. Fortunately, the child abuse investigation data make it possible to focus on cases where the family was not the reporter of the abuse or neglect, and, therefore, less likely to be mechanically impacted by these entry reforms. The lower panel shows means for this sub-population. The new entrants following the reform are more similar than in the full population, though some differences remain. For example, there are still more abuse cases following the entry reforms (61% compared with 73%).

Table 1 also shows that the fraction of placements to relatives declined slightly in the post period. However, Figure 1 shows that comparing the average fraction of children who go to relatives before and after the reform may be misleading, as there appears to be a break in trend. The estimation of the change in trend, while controlling for child characteristics and sample selection, is developed below.

5. Empirical Model and Results

To test the effect of the subsidy change on the willingness of relatives to provide care, the break in trend found in Figure 1 will be estimated. A nonparametric regression of relative placement on calendar time yields a similar picture to the more aggregated Figure 1. These results suggest that a model with a discontinuous break in the trend at the time of the reform and a change in the slope of the trend following the reform should be estimated: for child i from ZIP code z at time t .¹⁰:

$$(6) \text{Relative}_{izt} = \beta_0 + \beta_1 \text{post95}_{izt} + \beta_2 t + \beta_3 \text{post95}_{izt} * t + \beta_4 X_{izt} + \delta_z + \varepsilon_{izt}$$

¹⁰ Results are similar when probit and logit models are used.

Relative_{izt} equals one if the child is placed with a relative and zero otherwise. Recall that relatives are sought first following the placement of a child into foster care, so when a child is placed with a non-relative it can be inferred that no relative was willing or available to provide care. Post95 is a dummy variable equal to one if the child entered care on or after July 1st, 1995. The interacted trend term allows the slope to change following the reform. X_{izt} is a vector of control variables listed in Table 1, with age categories broken into one-year intervals. ZIP code fixed effects are used in an attempt to control for the child's neighborhood characteristics, such as income level. To explore how relatives respond to the reform for different types of children, this model will be estimated for subsets of the data as well. Huber-White standard errors are calculated, clustered by family to account for the lack of independence among observations for sibling groups.

The time variable is measured in years, and it is centered at the time of the reform (t=0 at July 1st, 1995). This allows β_1 to be interpreted as the discontinuous change at the time of the reform:

$$\begin{aligned}
 (7) \quad & E[\text{Relative} | X, \text{post95} = 1, t = \text{July 1, 1995}] - E[\text{Relative} | X, \text{post95} = 0, t = \text{July 1, 1995}] \\
 &= (\beta_0 + \beta_1 + \beta_4 X) - (\beta_0 + \beta_4 X) \\
 &= \beta_1
 \end{aligned}$$

Table 2 shows the results. Columns (1)-(5) progressively add control variables. Column (1) considers only a mean shift following the reform, with ZIP code fixed effects. β_1 is found to be -0.032—a 3.2 percentage point drop in the propensity of relatives to provide foster care from a mean level of 71%. However, as discussed above, the data suggest that there may have been an immediate effect followed by a greater response over time, as kin became less willing to

provide care. Columns (3)-(4) show that the models are not very sensitive to the introduction of control variables, and the immediate response is estimated to be a 5.9 percentage point drop in the propensity to provide foster care. Each year following the reform is associated with a further 4.6 percentage point decline, compared to the trend before the reform. Given the mean of 71% of children going to relatives, this suggests that the immediate impact is an 8% drop in the willingness to foster. Five years later, the effect would grow to 20%.¹¹

Recall that there were other entry reforms at the time that slowed the growth of foster care. Columns (5) and (7) restrict the analysis to sub-populations that are less likely to be affected by these policy changes: those removed following a report by police and following an allegation of abuse (as opposed to neglect). The results are similar with a slightly greater response for the population of children whose abuse was reported by the police. With an average relative placement rate of 65% and 68%, respectively, these estimates suggest a 10% and 8% initial drop in the placement rate. In comparison, Column (7) shows the results for children who enter as a result of a family report. The result is similar here as well with a mean relative placement rate of 77%, suggesting a 7% initial decline in relative placements. While this demonstrates the overall robustness of the result, this sub-population is more vulnerable to a change in the composition of children following the entry reforms.

Cross-state Comparison

If the cost of fostering or the types of children entering care change due to the booming economy, welfare reform, or other national trends, then this may explain the decline in the willingness to provide foster care. For example, if welfare reform is more likely to affect relative

¹¹ Note that this assumes the linear trend before the reform would have continued for five more years—a 14.4 percentage point decline from 1995-2001. Further, while the reform represents a discrete jump in subsidies and does not trace out a wage response over a range of wages, the 27% drop in subsidies implies a short-term supply elasticity of 0.31 and a longer-term supply elasticity of 0.74.

foster parents, who tend to be poorer, then the increase in employment (and the opportunity cost of time) may result in a decline in the willingness of relatives to provide care. However, we would expect these types of changes to affect other states as well.

For a comparison to other states, the Multi-state Data Archive is used to estimate a model for for state s at month t :

$$(8) \text{Relative}_{st} = \beta_0 + \beta_1 \text{post95}_{st} + \beta_2 t + \beta_3 \text{post95}_{st} * t + v_{st}$$

Here, Relative_{st} is the fraction of new entrants each month in each state that find a willing relative. The model is estimated using weighted least squares, with total entrants into foster care each month in each state serving as the weights.

Columns (8) and (9) report the estimates of this model. For states other than Illinois, the immediate effect of entering on or after July 1st, 1995 is minus one percentage point. From a mean of thirty-one percent, this is a minor change as suggested by Figure 1. In Illinois, the response is estimated to be an 8-percentage point drop in the propensity to find a willing relative immediately following the reform, followed by 7 percentage point declines each year afterwards compared to the overall upward trend. While these models do not have controls for child or ZIP code characteristics, it demonstrates that there was a change in Illinois while there was no break in trend for the other large states at the time of the reform. It appears, then, that national trends do not explain the change in the willingness of kin to provide care in Illinois.

Selection of Children Who Enter Foster Care

As discussed in section 2, the total number of children entering foster care also displays a rise and fall over this time period. One issue, then, is that children who enter prior to the reform

may differ from those who enter after the reform. If more costly children enter following the reform, then the comparison would pick up not only the response of relatives to the change in subsidies, but the response to different types of children as well.

However, observable characteristics do not suggest that children afterwards are particularly more costly. For example, Table 1 shows that fewer require mental health services for instance. As the main results concentrate on changes in trends, Table 3 shows how trends in the child characteristics change. Each set of estimates represents a separate regression of the child characteristic on the Post95 indicator, the time trend, and their interaction. The results suggest that trends in the age and sex of the children do not change much over this time. Children are more likely to be white in the years following the reform, but the difference is smaller when family reports of abuse or neglect are excluded. Still, these differences demonstrate the importance of both controlling for child characteristics and dealing with sample selection.

The concurrent reforms that restricted the growth of the overall foster care population call for a careful consideration of sample selection. The total number of children in care rises and falls similar to the pattern found in the willingness of kin to accept foster children. One entry reforms prohibited cases where children were already living with relatives. Of course all of those cases were relative placements, so eliminating them would mechanically cause a drop in fraction of cases going to relatives. To deal with these selection issues, the results have been shown for abuse cases and cases where family members did not report the abuse or neglect. The similar results suggest that these reforms are not driving the main results.

Further, a sample selection model can be estimated by exploiting the child abuse investigation data. To abstract from the effect of prior offenses, each family's first case where

abuse or neglect was indicated is considered. This includes 152,028 children over these ten years.¹² 10,600 of these children are removed from home and are eligible for family foster care. This differs from the earlier sample because many children are removed in subsequent investigations, and observations considered here are only for the first child protection investigation.

A simultaneous-equations approach is taken for child i in county c at time t , where:

$$(9) \text{Placed}_{ict} = \alpha_0 + \alpha_1 \text{post95}_{ict} + \alpha_2 t + \alpha_3 \text{post95}_{ict} * t + \alpha_4 W_{cit} + \alpha_5 Z_{ict} + \delta_c + \eta_{ict}$$

$$(10) \text{Relative}_{ict} = \beta_0 + \beta_1 \text{post95}_{ict} + \beta_2 t + \beta_3 \text{post95}_{ict} * t + \beta_4 W_{ict} + \gamma_c + \zeta_{ict}$$

W_{ict} are the usual controls, in addition to county indicators, variables for the initial reporter of the abuse or neglect, such as police, school, or family member.

The key to identification in this model is finding exclusion restrictions—variables that predict entry into foster care but do not predict whether the child can find a willing relative, written as Z_{ict} in equation (9). The child abuse investigation data include the number of caseworker contacts about the child. For example, if the initial report came from the school, and the caseworker has spoken with a doctor and a daycare provider about the case, then the number of additional contacts would be two. This presents a measure of the intensity of the investigative team and the strength of the evidence that the abuse or neglect occurred—the more contacts, the more likely the child is removed from home. However, if the strength of the case does not enter the kin’s decision, either because she does not know the strength of the case or it is unrelated to her discrete choice, then these variables can be used to control for sample selection bias.

¹² Results are similar each family’s first contact with child protection, even when abuse or neglect was not substantiated, is considered. Unsubstantiated allegations do not lead to placement, but often there will be another contact shortly after the first where abuse or neglect is substantiated. Using the first contact where the case was substantiated focuses on cases where children are at risk of placement.

Valid exclusion restrictions allow variation in the sample selection that is unrelated to the willingness of kin to provide care.¹³ To estimate the model, η_{ict} and ζ_{ict} are assumed to be bivariate normal, with a correlation coefficient ρ , though the identification stems from the exclusion restrictions, not this particular functional form assumption.¹⁴ The variance of ζ_{ict} is normalized to one, and the model is estimated using maximum likelihood. Standard errors are again estimated using a robust sandwich estimator of variance, clustered by family.

Table 4 shows the results of the selection correction model. Column (1) replicates the results in Table 2, but for the ‘first-contact with child protection’ population and with county indicators instead of zip code fixed effects. Estimates are similar regardless of which geographic control is used. Here, the effect at the time of the reform is much larger -0.16 , representing a 26% drop in the placement rate at the time of the reform from a mean level of 62%.

Column (2) includes a categorical variable equal to one if the caseworker made additional contacts as part of the investigation. Forty-seven percent of the investigated children had additional contacts.¹⁵ Table 4 shows that additional contacts are not associated with finding a willing relative. One way additional contacts may enter the choice of relatives is that these contacts suggest more severe abuse. In this way, the selection model directly addresses the idea that the entry reforms affect the severity of the cases. If these children are more costly, then relatives may be less likely to offer care to these children. While the effect is not meaningfully

¹³ It may be that more severe cases of abuse are more likely to be reported by more than one person, and that children who enter with more contacts may therefore be more difficult to foster. However, these contact variables do not explain relative placement as shown below, and with child outcomes tending to improve following the reform it appears that children are not more difficult to foster.

¹⁴ The exclusion restrictions make this functional form assumption less restrictive. Accordingly, including a polynomial in the propensity to be removed from home on the right-hand-side of (10) to form a semiparametric version of the selection correction yielded nearly identical results.

¹⁵ The initial report of abuse or neglect does not count as a caseworker contact, resulting in cases with zero contacts.

different from zero, the sign of the effect of contacts on relative placement is actually positive. Of course, it may be that more severely abused children receive more sympathy from relatives. While it is possible to test for valid exclusion restrictions, the estimates suggest that these contacts do not enter the relatives' decision to provide care. In contrast, Column (3) shows that contacts are strongly related to increased likelihood of removal from home. In addition, Column (3) shows that placement rates do fall after July 1995, reflecting the entry reforms.

Using the simultaneous-equations approach, the estimates are similar. Note that in this subgroup, the trend in relative placements is 4.3 percentage points lower than the overall trend. That is, the trend becomes roughly flat: 4.3 percentage points lower than the positive trend before the reform of 4.1 percentage points per year. These results suggest that the selection of children into the foster care system is not driving the decline in the propensity for relatives to provide care. The immediate drop in the willingness of relatives to provide care, followed by stable rates of relative placement suggests that this subgroup is picking up the effect of the one-time reform as opposed to changes in information that disseminates over time. In particular, if individuals are were claiming neglect in order to collect higher subsidies, then the effect might be spread out over time as families learn of the subsidy change. However, if the family has an unexpected shock and relatives are asked to provide care, as is more likely in this 'first-contact' population, we would expect this unplanned event to have more stable effects over time.

Effect of the Reform, by Child Characteristics

There is a large literature on the cost of children (Calhoun and Espenshade, 1988; Espenshade 1973, 1984; Lewin/ICF 1990; and Turchi, 1973). Such analysis guides policy from setting payments for welfare to child support. Some conclusions of this literature are that: (1) expenditures increase with the age of the child; (2) economies of scale exist in raising children;

(3) race and ethnicity are correlated with parental expenditure levels; and (4) expenditures increase with family income.

Table 4 shows the results of the response to the reform by different child characteristics. If we supplement the model in section three to include an additional cost that varies by the type of child, including the time cost of raising children, then the reservation subsidy would be increasing in that cost. By comparing the response to the reform across child characteristics, larger declines in the willingness to participate would be consistent with higher costs for these types of children.

One caveat is that the treatment of children in non-relative homes may also differ across child characteristics. A larger response following the reform for a particular type of child reflects the willingness of kin to care for children of that type, which may be due to the child's cost and the treatment the child would receive in a non-relative home. For example, if kin thought that older children are much better off in a relative home, then we may see a smaller response for older children. If kin rely on licensing of non-relative homes to provide a minimum quality standard and consider non-relative homes roughly similar across types of children, then different responses to the reform will likely isolate the differences in the costs of children.

First, there is a larger response to the reform for children who require mental health services. Recall that the population studied here is eligible for family foster care, so these children do not likely suffer from severe mental health problems. Nonetheless, the larger response suggests that these children are costlier compared to children who do not require such services. The initial response is an 11-percentage point drop in the propensity to take the child immediately following the reform. Given that the mean acceptance rate for this sub-population is only 57%, this represents a 19% decline in the willingness to provide care initially.

Second, while the cost of children literature suggests that expenditures increase with the age of the child, the analysis here suggests that the overall cost is larger for young children. Of course the time cost of young children is likely to be greater than the cost of children who attend school. Perhaps surprisingly, the effect is smaller for teenagers suggesting that they are less costly. Boys, however, appear to be costlier than girls with a 7.0 percentage point drop in the propensity to care for a boy following the reform, compared to a 5.9 percentage-point drop for the full population.

Table 4 also shows that the response varies by race. Hispanics show a smaller response immediately, though over time they do become less likely to provide care. The opposite is true for white families. There appears to be a large effect initially with less of a response as time goes on. The effect is especially large considering that kinship foster care is more common for nonwhite families. Black families exhibit both a 4.8 percentage point drop at the time of the reform and a yearly drop of 5.4 percentage points each year, compared to the trend prior to the reform.

Last, the response by the number of siblings in need of care is tested. Those with no or one sibling show large responses initially compared to larger sibling groups. This is especially true considering that those with no siblings were less likely to find a willing relative before the reform (possibly due to fewer relatives to choose from if family size is correlated across generations). This smaller response initially is also consistent with economies of scale in raising children. While the 'standard of need' payments do decrease with the number of children, if this does not match the true economies, then foster parents may be more willing to take larger sibling groups. However, the negative response does grow as time goes on for the larger sibling groups.

Effect of the Reform on Child Outcomes

As discussed above, the subsidy change will have an income effect and a selection effect. The lower subsidy will provide fewer resources to the household, and this can be expected to worsen child outcomes. Second, the quality of foster parents may rise or fall following the reform. Examining child outcomes will shed some light on the effect of subsidies on the quality of foster parents.

Two child health outcomes are considered: injuries and wellness visits within the first year of care.¹⁶ All foster care entrants are enrolled in Medicaid, which provides a way to monitor their use of health care. The Medicaid data are available up to September 30, 2000, so these analyses are conducted using children who entered care prior to September 30, 1999.

Considering injuries, such as broken bones, has the advantage that immediate care is usually required. Care is less likely to be the result of poor treatment by parents before entry into foster care, as opposed to more chronic conditions.¹⁷ Wellness visits, such as vaccinations, have the advantage that they are the result of actions by the parent and not the child. All children who enter foster care are supposed to receive a medical check-up, and it is largely up to the child's caseworker and foster family to enforce this requirement.

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 willingness to accept children. For child i from ZIP code z at time t :

$$(11) \text{Outcome}_{izt} = \beta_0 + \beta_1 \text{post95}_{izt} + \beta_2 t + \beta_3 \text{post95}_{izt} * t + \beta_4 X_{izt} + \delta_z + \varepsilon_{izt}$$

¹⁶ See Case and Paxson (2001) for a discussion of the role of parents in child health investments.

¹⁷ Injuries in the first week of care are excluded in case they are the result of the original abuse.

Here, the same controls are used as in Table 2, in addition to the duration in care. Again, ZIP code fixed effects are used in an attempt to control for neighborhood characteristics such as income level.

Table 6 shows that there was a large decrease in the fraction of children who get injured following the reform. The estimates suggest that injuries fell by 1.0 percentage point and continued to fall by 1.5-percentage points per year. With a mean of 6.0% of children suffering an injury during the first year in care, these are large effects. It suggests that the change in the composition of foster parents may have dramatically lowered injuries. Further, the drop in injuries is concentrated among children in kinship foster care, as shown in Figure 3. Using the non-relative placements as a check on overall trends in injuries for this at-risk population, the decline for children in relative placements is dramatic. While the decline begins one year prior to the reform, a decline is seen for both relatives and non-relatives in 1994. The longer-term decline is only present for the relative placements.

Table 6 also shows that wellness visits fell in the years following the reform in comparison with the overall upward trend. However, the declines in wellness visits late in the period are true for both kinship and traditional foster parents (see Figure 4). Recall that it is the responsibility of the foster parent and the caseworker to ensure that the child receives a check-up. Changes in program outreach over this time period may contribute to the estimated decrease each year following the reform. In any event, this measure suggests that foster parent quality may have declined along this dimension.

Another quality measure is the likelihood of a foster family to stop fostering 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 (see Jonson-Reid and Barth, 2000). Figure 5 shows

that relative placements are more stable than traditional foster care. It also shows that the fraction of children who experienced a disruption in the first year of care increased somewhat following the reform. In an effort to control for child characteristics, Table 5 shows regression results where the dependent variable is a dichotomous zero-one variable for whether the foster parent stopped fostering and the child moved to another foster or group home within the first year.¹⁸ Since the outcome is a length of stay measure, the duration in care is no longer used as a control variable. The estimates suggest that foster parents were more likely to stop fostering before the child is ready to return home following the reform. This is consistent with the drop in the willingness to provide care, in general. However, unlike the injuries, children appear to be worse off along this dimension of foster parent quality.

Controlling for Selection

Using a selection model similar to equations (9) and (10) requires restricting the data to first indicated reports of abuse or neglect. In this sub-population, the estimates of effects on placement stability continue to show the same sign, but the results are much smaller and noisier. The wellness visits show very similar results.

Perhaps most interesting is the injury result. In this sub-population of first contacts with child protection, child injuries are found to decline by a smaller amount initially with a coefficient on Post95 of -0.006 , though it is not measured very precisely. However, the decline in the trend following the reform continues and the interacted trend term has a coefficient of -0.017 and a standard error of 0.005 . The exclusion restrictions seem reasonable, and ‘caseworker contacts’ are not significantly different from zero in predicting injury rates. When the selection correction is applied, the results do not change—that is, most of the difference in

¹⁸ For comparability, the data are restricted to cases that entered foster care before July 1, 2000—one year prior to the end of the database.

the estimates stem from smaller sample. Recall that the decline in relative placements was more concentrated at the time of the reform for this sub-population, while the change in injuries is evident in the trend following the reform. This is somewhat inconsistent with the reform leading to declines in injuries, yet the decline is so strong for the relative placements that quality does appear to have taken time to improve. These results suggest that the drop in subsidies was not associated with deterioration in health outcomes, and there is evidence that child health actually improved.

Foster Parent Characteristics

One explanation for the improvement in child injuries is that children are different after the reform. However, the reforms implemented would suggest that children would be more difficult to foster as cases on the margin remain in the care of the biological family. This would suggest worse outcomes, not improved outcomes as found here.

Another question is whether the foster parents are different. It has been suggested that they may have a higher level of altruism leading to higher quality. However, they may just be wealthier following the reform with a wealth effect explaining the result. The main results controlled for neighborhood characteristics in an attempt to control for income.¹⁹ However, for foster parents who have had contact with the Department of Public Aid (DPA), such as AFDC/TANF recipients, then demographic data can be matched with the DPA administrative data. To maximize the match, demographic data from the DPA data from 1990-2001 were used, regardless of when the relative became a foster parent. Table 7 shows that relative foster parents are more likely to be white following the reform, matching the earlier estimates as a check on the quality of the DPA data match. Other characteristics, including exposure to Public Aid; marital

¹⁹ The above results include ZIP code fixed effects, for the ZIP code of the child's permanent residence. Using ZIP code fixed effects where for the ZIP code of the foster parent yielded nearly identical results.

status, health, work experience, and occupations at the time of entry into DPA data; and age at the time of entry into foster care service are quite similar across the time periods.

Further, earnings data are available from the Illinois Department of Labor after January 1995. These data were matched with the child welfare data foster parent listed, generally the head of the household. For a matched comparison, wages and employment were compared at July 1995 for relative foster parents who entered between July 1993 and July 1996. Table 7 shows that wages are slightly higher for those who agreed to accept the lower subsidy.²⁰ However, the relative foster parents who accepted the lower wage were slightly less likely to have observed wages in July 1995. This suggests that employed relatives, who may have a higher opportunity cost of time, were slightly less likely to provide care following the reform.

6. Conclusion

The reform in Illinois offers a rare glimpse into the response of within-family altruism to a plausibly exogenous change in economic incentives. 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 roughly 20%. While changes in the types of children entering care may explain part of the result, examining sub-populations that were less likely to vary over time, using a selection-correction model, and comparing the results with other large states suggest that selection of children into foster care is not driving this result. Further, if children entering care following the reform had more problems, then this may explain

²⁰ If parents who entered between 1990-2001 are considered, the fraction of foster parents with no reported wages is much lower following the reform (37% vs. 50%). While this is consistent with wealthier foster parents agreeing to accept the lower subsidy, it is likely an artifact of the timing—those who enter later are more likely to have worked in 1995 than those who entered much earlier.

the drop in participation by relatives. However, some child outcomes tend to improve following the reform, not worsen as would be expected if this were the case.

By examining the response to the reform across different types of child characteristics, it is also possible to examine which children may be more costly. Larger responses were seen for children who require mental health services, as well as younger children, white children, and boys. Larger responses to the lower subsidies reflect more relatives on the margin for these types of children. This may be due to the higher cost of caring for children with these characteristics.

Finally, child health and educational outcomes appear to improve following the reform, suggesting that higher quality foster parents are willing to accept the lower subsidy. At the same time, foster parent quality may be lower in terms of placement stability and preventative medical care visits, though these results are less robust. The decline in injuries, especially within relative placements, suggests that foster parent quality may have improved following the reform.

The decline in subsidies appears to have had a large effect on participation, while child outcomes did not suffer. In fact, there is some evidence that health and educational outcomes improved following the decline in subsidies. This suggests that the selection of more altruistic, or potentially higher quality, foster parents tends to outweigh the income effect expected to worsen child outcomes. Considering the effect of lower wages in other altruistic occupations seems ripe for future research.

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Table 1: Descriptive Statistics

All	Pre		Post		t
	Mean	Std. Dev	Mean	Std. Dev	
Relative Placement	0.73	0.45	0.67	0.47	-13.24
under 5	0.49	0.50	0.52	0.50	5.23
age 5-10	0.27	0.44	0.27	0.44	-0.46
age 10-17	0.23	0.43	0.21	0.41	-5.76
black	0.69	0.46	0.60	0.49	-18.99
hispanic	0.05	0.21	0.07	0.25	9.16
white	0.25	0.43	0.30	0.46	13.07
boy	0.48	0.50	0.49	0.50	2.31
mental services	0.10	0.31	0.09	0.29	-4.97
abuse	0.39	0.49	0.58	0.49	42.00
family report	0.49	0.50	0.31	0.46	-37.44
police report	0.14	0.35	0.18	0.38	8.94
Observations	29342		17078		
<u>Allegation Not Reported by Family Members</u>					
Relative Placement	0.66	0.47	0.64	0.48	-3.83
under 5	0.51	0.50	0.54	0.50	4.75
age 5-10	0.26	0.44	0.26	0.44	0.60
age 10-17	0.23	0.42	0.20	0.40	-6.40
black	0.61	0.49	0.57	0.49	-5.52
hispanic	0.05	0.23	0.07	0.25	4.50
white	0.32	0.47	0.33	0.47	1.54
boy	0.48	0.50	0.49	0.50	2.12
mental services	0.12	0.33	0.09	0.29	-7.31
abuse	0.61	0.49	0.73	0.44	21.40
family report
police report	0.28	0.45	0.26	0.44	-4.86
Observations	15036		11747		

Table 2: Change in the Probability of Relative Care

Dependent Variable: Relative Placement										
Sample	All (1)	All (2)	All (3)	All (4)	All (5)	Reporter of Abuse/Neg.			Illinois (9)	Outside IL (10)
						Not Family (6)	Family (7)	Abuse (8)		
post95	-0.032 (0.006)	-0.098 (0.010)	-0.065 (0.010)	-0.059 (0.010)	-0.059 (0.010)	-0.067 (0.014)	-0.053 (0.014)	-0.052 (0.015)	-0.080 (.013)	-0.013 (.007)
t		0.014 (0.002)	0.030 (0.002)	0.029 (0.002)	0.029 (0.002)	0.035 (0.003)	0.023 (0.003)	0.036 (0.004)	0.033 (.004)	0.001 (.011)
post95*t			-0.049 (0.004)	-0.047 (0.004)	-0.046 (0.004)	-0.053 (0.005)	-0.044 (0.006)	-0.053 (0.006)	-0.071 (.005)	0.002 (.018)
black				0.109 (0.010)	0.110 (0.010)	0.091 (0.012)	0.144 (0.016)	0.091 (0.013)		
hispanic				0.027 (0.015)	0.025 (0.015)	0.016 (0.018)	0.050 (0.023)	0.026 (0.019)		
other race				-0.027 (0.022)	-0.028 (0.022)	-0.043 (0.028)	0.002 (0.034)	-0.026 (0.030)		
boy					-0.002 (0.004)	-0.003 (0.005)	0.001 (0.005)	0.002 (0.006)		
abuse					0.049 (0.006)	0.043 (0.008)	0.058 (0.010)	0.000 (0.000)		
family report					0.076 (0.006)	0.000 (0.000)	0.000 (0.000)	0.091 (0.010)		
police report					0.013 (0.009)	0.015 (0.009)	0.000 (0.000)	0.036 (0.011)		
Constant	0.716 (0.003)	0.758 (0.006)	0.808 (0.007)	0.649 (0.011)	0.599 (0.012)	0.629 (0.015)	0.631 (0.018)	0.646 (0.016)	0.739 (.010)	0.320 (.109)
Observations	46420	46420	46420	46420	46420	26783	19637	21269	128	720
R-squared	0.23	0.23	0.24	0.25	0.26	0.25	0.30	0.27	0.66	0.00

Robust standard errors in parentheses,

clustered by family in models (1)-(7) and by state in model (9)

Models (4)-(7) include indicators for each age

Models (1)-(7) include ZIP code fixed effects

Outside IL model estimated using foster care population-weights. Sum of weight: 489,530

Table 3: Trends in Child Characteristics

Dependant Variable:	Age			Race		
	under 5 (1)	age 5-10 (2)	age 11-17 (3)	black (4)	hispanic (5)	white (6)
post95	0.006 (0.009)	-0.009 (0.008)	0.003 (0.008)	-0.043 (0.009)**	0.011 (0.004)**	0.029 (0.008)**
t	0.007 (0.002)**	0.001 (0.002)	-0.008 (0.002)**	0.015 (0.002)**	0.002 (0.001)	-0.016 (0.002)**
post95*t	-0.007 (0.003)*	0.001 (0.003)	0.006 (0.003)*	-0.051 (0.003)**	0.001 (0.001)	0.046 (0.003)**
Constant	0.517 (0.006)**	0.273 (0.006)**	0.210 (0.005)**	0.736 (0.006)**	0.052 (0.003)**	0.199 (0.006)**
Observations	46420	46420	46420	46420	46420	46420

	boy (7)	# siblings (8)	abuse (9)	family report (10)	mental svcs. (11)
	post95	0.000 (0.009)	-0.075 (0.030)*	0.043 (0.009)**	-0.037 (0.009)**
t	0.002 (0.002)	0.077 (0.006)**	0.031 (0.002)**	-0.023 (0.002)**	0.003 (0.001)**
post95*t	0.000 (0.003)	-0.191 (0.011)**	0.002 (0.003)	-0.011 (0.003)**	-0.016 (0.002)**
Constant	0.488 (0.006)**	3.171 (0.021)**	0.481 (0.006)**	0.416 (0.006)**	0.113 (0.004)**
Observations	46420	46420	46420	46420	46420

Standard errors in parentheses

* significant at 5%; ** significant at 1%

Table 4: Sample Selection

Dep. Var:	<u>All First Contacts</u>			
	relative (1)	relative (2)	removal (3)	relative (4)
post95	-0.160 (0.020)	-0.160 (0.020)	-0.021 (0.002)	-0.128 (0.034)
t	0.041 (0.004)	0.041 (0.004)	0.002 (0.003)	0.035 (0.003)
post95*t	-0.043 (0.006)	-0.044 (0.006)	-0.003 (0.0007)	-0.037 (0.005)
Additional Contacts		0.019 (0.011)	0.049 (0.001)	
Mean of Dep. Var.	0.620	0.620	0.070	0.620
Observations	10596		152028	

Data restricted to first indicated abuse/neglect cases

Models include full controls

Column (3) presents marginal effects from a probit estimation

Robust standard errors in parentheses, clustered by family

Table 5: Response by Child Characteristics

Dependant Variable: Relative Placement								
	All (1)	mental svcs. (2)	Age			Race		
			under 5 (3)	age 5-10 (4)	teen (5)	black (6)	hispanic (7)	white (8)
post95	-0.059 (0.010)	-0.106 (0.033)	-0.077 (0.013)	-0.059 (0.017)	-0.031 (0.018)	-0.048 (0.012)	-0.035 (0.043)	-0.067 (0.023)
t	0.029 (0.002)	0.026 (0.007)	0.031 (0.003)	0.029 (0.004)	0.026 (0.004)	0.024 (0.002)	0.043 (0.011)	0.036 (0.005)
post95*t	-0.046 (0.004)	-0.031 (0.013)	-0.054 (0.005)	-0.041 (0.007)	-0.032 (0.007)	-0.054 (0.005)	-0.064 (0.016)	-0.032 (0.008)
Constant	0.599 (0.012)	0.409 (0.037)	0.646 (0.015)	0.702 (0.022)	0.570 (0.024)	0.777 (0.011)	0.690 (0.043)	0.424 (0.023)
Observations	46420	4614	23366	12545	10509	30565	2526	12484
R-squared	0.26	0.35	0.27	0.31	0.34	0.21	0.42	0.22

	boy (9)	Siblings			
		no siblings (10)	1 sibling (11)	2 siblings (12)	>2 siblings (13)
post95	-0.070 (0.013)	-0.089 (0.018)	-0.069 (0.021)	-0.044 (0.024)	-0.038 (0.015)
t	0.031 (0.003)	0.030 (0.004)	0.028 (0.004)	0.028 (0.005)	0.027 (0.003)
post95*t	-0.045 (0.005)	-0.033 (0.006)	-0.038 (0.008)	-0.050 (0.010)	-0.055 (0.007)
Constant	0.603 (0.015)	0.486 (0.020)	0.606 (0.024)	0.697 (0.029)	0.696 (0.022)
Observations	22539	10592	11434	10440	24394
R-squared	0.27	0.24	0.31	0.33	0.29

Robust standard errors in parentheses, clustered by family
 Models include all controls and ZIP code fixed effects

Table 6: Child Outcomes

Dependant Variable	Child Health		Placement Stability
	injury	wellness	Quits
post95	-0.010 (0.006)	0.012 (0.012)	0.021 (0.008)
t	0.007 (0.001)	0.064 (0.002)	0.0005 (0.002)
post95*t	-0.015 (0.002)	-0.075 (0.005)	0.006 (0.003)
Obs	44950	44950	44950
Mean of Dep. Var	0.06	0.50	0.38
Std. Dev. of Dep Var	0.24	0.50	0.48

Outcomes in the first year of care.

Models include full controls and ZIP code fixed effects

Health and Education models also control for duration of the foster care spell.

Health and Stability data restricted to entrants prior to Sept. 1, 1999.

Robust standard errors in parentheses, clustered by family

Table 7: Characteristics of Relative Foster Parents

	Pre			Post			t
	Mean	Std. Dev	Obs.	Mean	Std. Dev	Obs.	
Contact with DPA	64	48	14134	65	48	8982	1.53
Age	44	12	8958	43	13	5775	-1.71
White	18	39	8789	26	44	5660	10.7
Black	77	42	8789	68	47	5660	-12.89
Hispanic	3.7	19	8789	5.9	24	5660	6.14
Married	7.5	26	4072	7.4	26	6699	-0.08
Healthy	95	22	4072	95	22	7002	0.55
No Work Exp.	28	45	4072	28	45	2713	0.37
Professional	3.1	17	4072	3.6	19	2713	1.03
Clerical	12	32	4072	12	32	2713	-0.61
Service	30	46	4072	30	46	2713	0.07
No Wage Observed 7/95	46	50	4518	43	49	3273	-2.69
Quarterly Wage 7/95	4864	4425	2451	4954	5199	1876	0.62

Data match providers with the Illinois Department of Public Aid
Wage data from match with Illinois Department of Labor

Appendix A: Figures

Figure 1: Fraction of Foster Children Going to Relatives

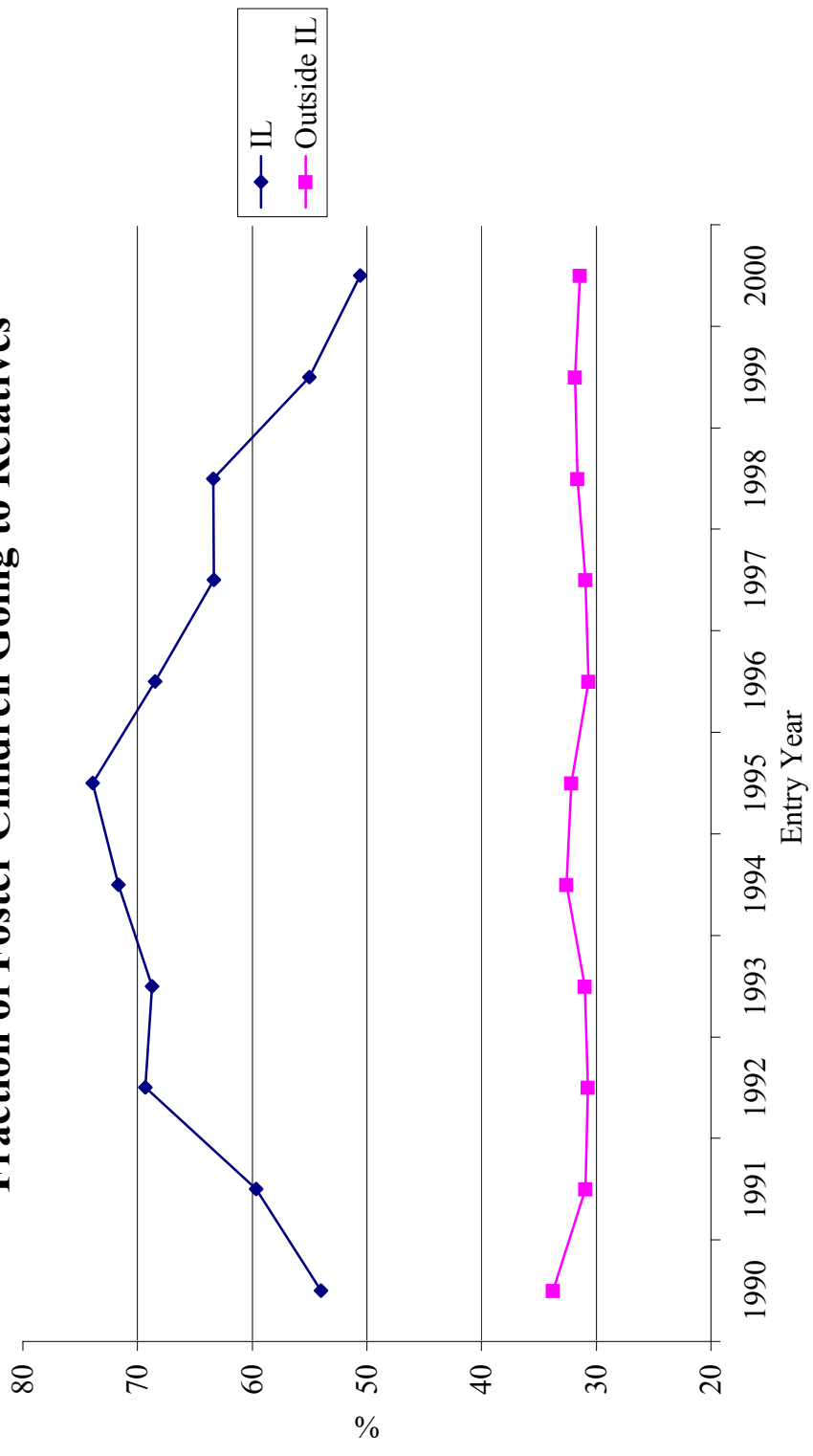


Figure 2: Reservation Subsidy Representation

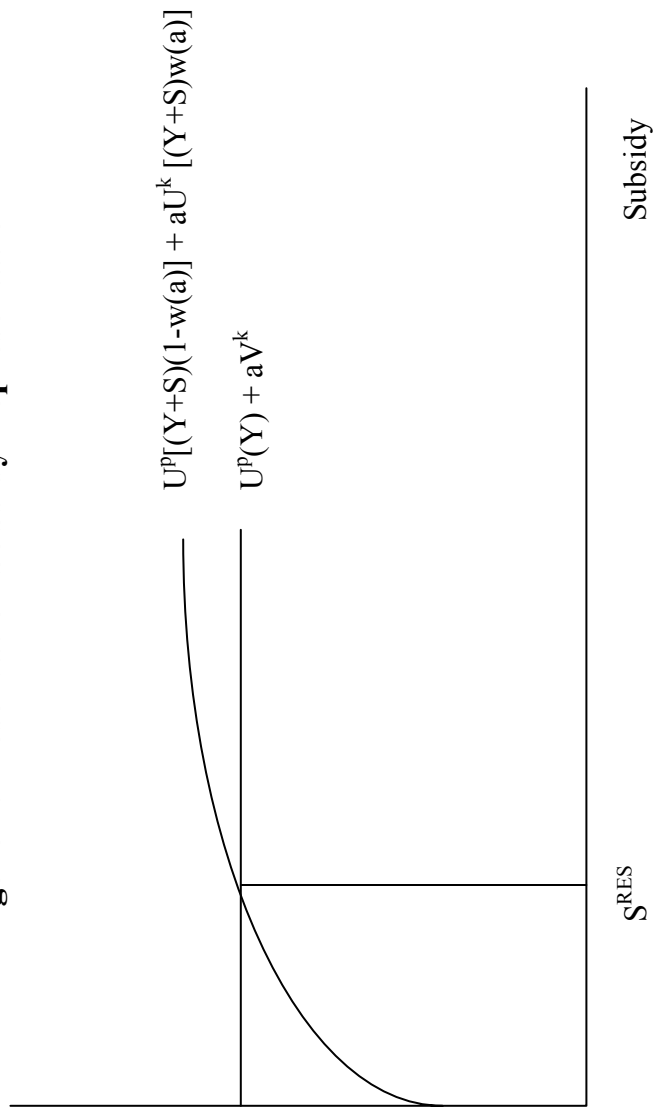


Figure 3: Injuries in the 1st Year of Care

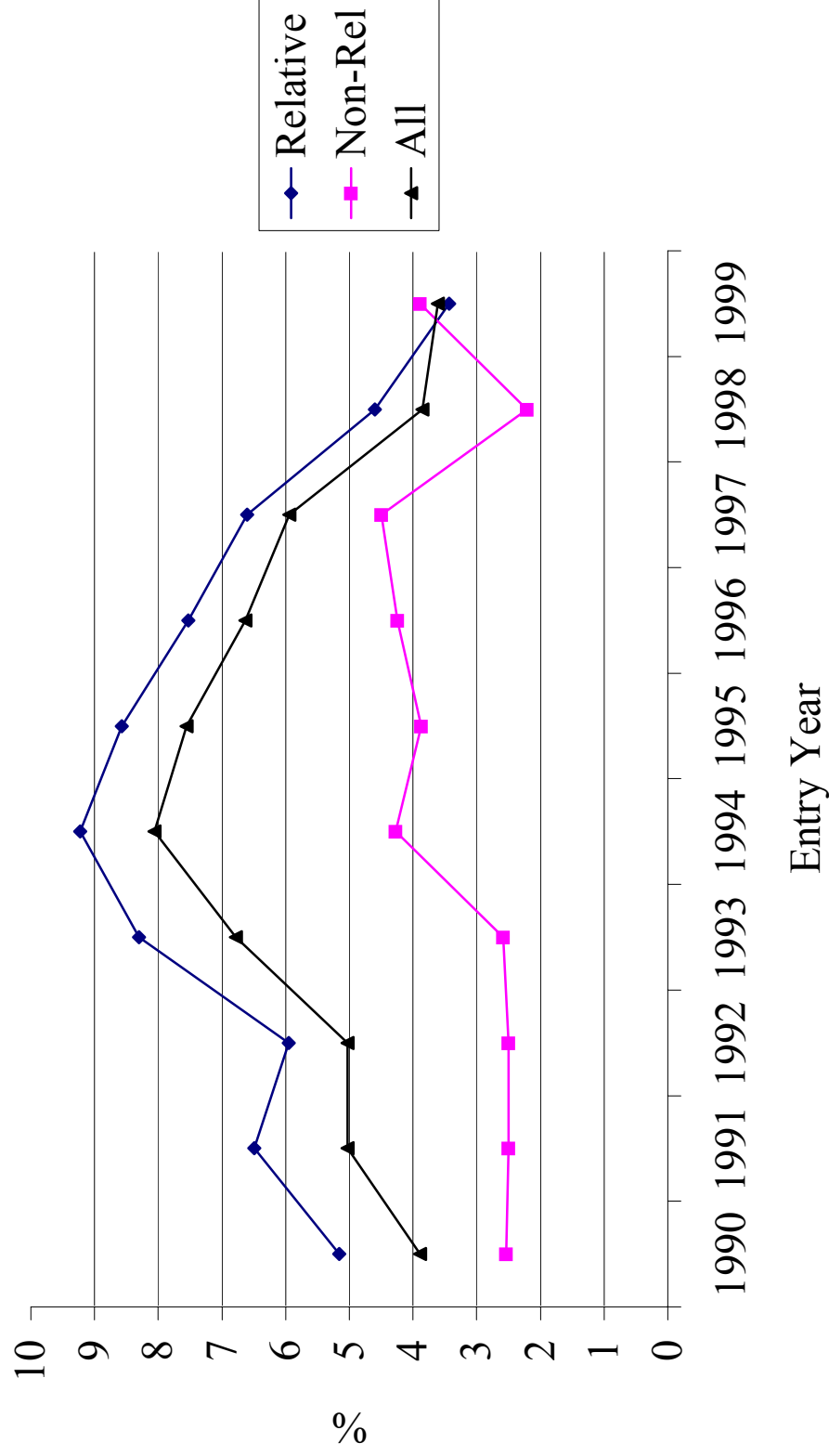


Figure 4: Wellness Visits in the 1st Year of Care

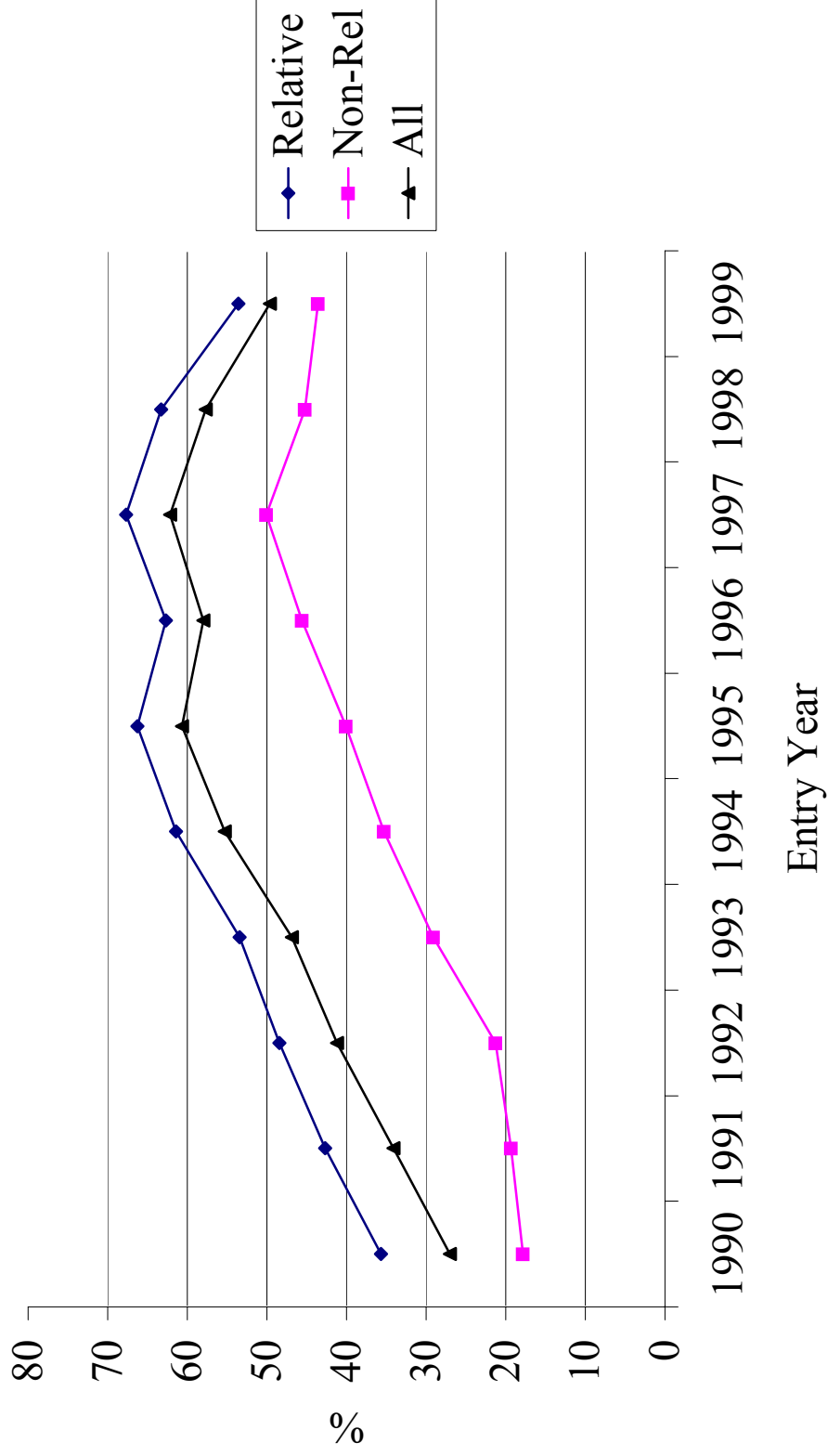


Figure 5: Foster Parent Quits in the 1st Year of Care

