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Kinship Guardianship: Understanding the Changing Incentives of TANF-Recipient  
Families under Sanctions

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## Abstract

### Kinship Guardianship: Understanding the Changing Incentives of TANF-Recipient Families under Sanctions

By Olivia Lofton

This paper evaluates the effect of welfare sanctions on non-foster kinship care rates. With the passage of Temporary Assistance for Needy Families (TANF), states were granted permissions to levy full-family welfare sanctions against recipient families failing to meet work requirements. At the same time, TANF granted child-only subsidies to informal relative caregivers. I use a difference-in-differences model as well as an event study design to compare the relationship between timing of full-family sanction legislation and incidence of non-foster relative care in 48 states from 1989 to 2018. I find that full-family sanctions have a negative effect on non-foster kinship care rates when only examining children likely to be placed into non-foster kinship care. I argue that full-family sanctions may have caused parental preferences to shift from placing children in non-foster relative care to instead placing children into foster relative care.

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# 1 Introduction

In the years following the 1996 overhaul of the American welfare system, questions concerning what effect the entailed changes might have on child well-being began to arise. The implementation of entitlement program Temporary Assistance for Needy Families led child advocates to worry that the new system's approach to welfare, one in which benefits are conditioned on work requirements, might inadvertently harm children's outcomes. Indeed, studies emerging after initial reform took place suggested that TANF-recipient families under sanctions may generally face more obstacles in maintaining their children's well-beings (Kalil et al. 2002, Skalicky and Cook 2002, Haider et al. 2003, Kaestner et al. 2005, Reichman et al. 2005, Larson et al. 2011). Concurrently, policy experts observed significant increases in foster care caseloads (Paxson and Waldfogel 2003, Slack et al. 2003, Swann and Sylvester 2006).

While children placed in foster care may be forcibly removed from their family homes by Child Protective Services (CPS), children living in non-foster kinship care are typically voluntarily placed in such out-of-home care by family members. Without distinguishing between foster and non-foster kinship care, research has broadly shown that incidence of relative care has also risen in the wake of welfare reform (Bitler et al. 2004, Bitler et al. 2006). In contrast to research centered around foster care, however, the reasons why children may be more likely placed in non-foster kinship care are less understood. Given that prior research has shown that children placed in non-foster kinship care may face outcomes different from those residing in foster kinship care, it becomes important to know whether welfare reform has also impacted the rate at which non-foster relative care is arranged, and if so, why (Swann and Sylvester 2006, Strozier and Krisman 2007, Harnett et al. 2012, Stein et al. 2014).

If an increase in incidence of non-foster kinship care exists similar to the one that prevails in foster-care caseloads, it could likewise be due to similar reasons. It may be that welfare reform made instances of child abuse, parental drug abuse, and parental incarceration - all situations associated with the child leaving the family home either formally or informally - more common. Still, non-foster kinship caregivers qualify in all states to receive child-only TANF benefits. These child-only TANF funds may then act as monetary incentives, encouraging sanctioned families facing financial strains to informally transfer guardianship of their children to relatives so that their children may continue to receive welfare benefits. While TANF child-only benefits are worth considerably less than payments to caregivers fostering children, they are not conditional on standard TANF work requirements nor are they time-limited. Like foster care payments, children in kinship care are eligible to receive child-only TANF benefits regardless of their caregivers' incomes. It is noteworthy that such child-only units have grown steadily from the point of TANF's conception, at one point making up half of all TANF cases in 2008 (Mauldon et al. 2012).

In theory, then, the own-children of a sanctioned, TANF-recipient family facing a complete loss of cash assistance could again become eligible for TANF benefits if they were informally placed in other relatives' care. No study has yet explored how TANF benefit sanctions and child-only payments may incentivize parents to make guardianship choices when faced with financial constraints. Using individual-level data from the Annual Social and Economic Component of the Current Population Survey and state-level data from the Urban Institute's Welfare Rules Database, I examine the incentives and implications that underlie the relationship between sanctioning and informal guardianship transfers. I hypothesize that harsher sanction policies will lead to increased likelihood that sanctioned families place their children in non-foster kinship care. The state-level variation in sanction policies over time allows me to test for a causality in the relationship between sanction severity and incidence of non-foster kinship care arrangements.

Using both a difference-in-differences model as well as an event study design, I find that full-family sanctions have either zero or negative effects on non-foster kinship care rates. Full-family sanctions have the greatest effect on non-foster relative care rates for children who are predisposed to being placed in kinship care. These findings prove important for policy concerning child welfare as they show that parents may make unexpected decisions regarding their children's living arrangements when met with incentives that could alleviate their financial constraints. In particular, parents may prefer to place their children in foster relative care to gain higher subsidy payments for their children.

If such preferences exist and are acted on, policymakers should consider enlarging the social safety net in place for children of sanctioned families; children who are at greater risk of foster care entry could especially benefit from such a change in policy. Additionally, policymakers should question the ultimate utility derived to society in enacting harsh sanctions against welfare reliant families.

## 1.1 Kinship Care

In the most broad terms, kinship care can be defined as "the care of children by relatives or others to whom a kinship relationship is ascribed" (Lee et al. 2017). Kinship care can be further classified into three distinct sub-categories. "Formal" kinship care refers to court-mandated alternative care in which child welfare services officially recognize the relative caregivers as foster parents, this type of care also being known as foster relative care (Testa et al. 1996). Both "informal" and "private" kinship-care, on the other hand, are voluntarily agreed-upon arrangements, with the difference between the two being that the former is still facilitated by the courts while the latter is not. (MacDonald et al. 2018).

While children placed in kinship care are typically able to obtain emotional support and placement stability to a greater extent than their counterparts in non-relative foster care, the effect of being separated

from one's parents still puts children at risk for adverse outcomes in well-being (Tapsfield 2001, Winkour et al. 2008). Research shows that such children are more likely to suffer from poor mental and physical health, disengage from school and extracurricular activities, and experience lower levels of cognitive stimulation (Bowlby 1973, Halforn et al. 1995, Karen 1998, Billing et al. 2002).

Additionally, it is important to note that outcomes for children in kinship care may differ conditional on the child's type of kinship care arrangement. While it has been reported that the majority of American children living in kinship care - around 75% - do so in private arrangements, these kinship care cases are also the most understudied in research, precisely because such cases are mainly unknown to the court system and welfare agencies (Andrews et al. 2002). What little is known about relatives providing care outside of the child welfare system reveals that such caregivers are more likely to live under the poverty line and suffer from food insecurity (Swann and Sylvester 2006, Harnett et al. 2012). This relative disadvantage in caretaking is further compounded by the fact that informal and private caregivers and the children they care for receive less financial and health support services from outside agencies (Strozier and Krisman 2007, Stein et al. 2014).

Given the additional uncertainty that surrounds the outcomes of children residing in non-foster relative care, it becomes interesting to consider whether such children are removed from their parents' homes for reasons similar to those that lead CPS to place other children in kinship foster care. Many children are put in informal and private kinship care for reasons that - if known by authorities - would be of interest to CPS (Gibbs et al. 2004). Adding to this, initiation of an informal care arrangement is more likely to arise after a family has been investigated for child maltreatment (Walsh 2013). Familial cases of child abuse, parental addiction, and parental incarceration make it more likely that a given child will enter any kind of kinship care.

Still, if a child's biological parents lack adequate financial resources or general parenting experience, there is a greater probability that their child may be placed in relative care arranged outside the scope of welfare services (Gleeson et al. 2009). Parents responding to such challenges may exercise a greater degree of autonomy in making decisions about their children's living arrangements than, say, incarcerated parents or parents battling substance abuse. It would be expected that parents with greater leveraging power would also be the most likely to respond to financial incentives tied to potential non-foster kinship care arrangements; because the current literature does not typically differentiate between kinds of kinship care, however, studies on such incentives are lacking. More research is needed to understand how many such cases of non-foster relative care arise due to changing policy incentives. It is my hope that this research in addition to essential future research can give essential insight into the ways in which welfare reform and benefit sanctions affect arrangements of non-foster kinship care specifically.

## 1.2 “Welfare-to-Work” and TANF Sanctions

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) marked a dramatic shift in welfare ideology for the federal government. Prior to the bill’s passage, the long-standing Aid to Families with Dependent Children (AFDC) served as America’s primary federal assistance program for poor families, providing income subsidies to eligible families with few strings attached. PRWORA subsequently replaced AFDC with the Temporary Assistance for Needy Families (TANF) program. In contrast to AFDC, TANF imposed time limits and work requirements on recipient families, while also allowing states to introduce additional program stipulations as their respective governments saw fit (Page and Lerner 1997).

Politicians championed such program constraints to be reflective of the new “welfare-to-work” attitude that was reshaping welfare politics at the time. Murphy and Sales (2001) explain this attitude to be the result of society’s evolved willingness to cast the poor’s collective work ethic, rather than structural inequality, as the root cause of American poverty. To incentivize welfare recipients to work, TANF required states to enforce “sanctions” against families who failed to comply with work requirements.

Parrott and Sherman (2006) and Falk (2016) show that the implementation of TANF was correlated with an increase in workforce participation rates and a steep decline in welfare caseloads. In part due to other redistributive programs - such as the expansion of the Earned Income Tax Credit that took place during the 1990s - TANF recipients experienced larger increases in net incomes when moving from no work to part- or full-time work. These work incentives led a majority of clients exiting the TANF caseload to seek out and gain employment opportunities (Acs et al. 1998, Danziger et al. 2002, Acs and Loprest 2004). Among late welfare leavers, stringent as opposed to lenient TANF work requirements were shown to be positively correlated with obtaining future employment (Lim et al. 2009).

Despite this perceived success in making welfare recipients more self-sufficient, other research has claimed that welfare caseloads have declined largely in part because welfare requirements enacted under TANF have made less families eligible to receive cash assistance (Falk et al. 2014). Studies have also shown that past welfare recipients who lacked prior job experience, had no high school degree, or faced other barriers to employment were often unable to secure welfare benefits in the post-AFDC era (Zedlewski 1999, Zedlewski and Alderson 2001). More generally, research exploring falling caseloads has attributed the phenomenon to a more ambiguous, mixed effect, in which clients leave welfare both because they are more likely to participate in the workforce under TANF and because they face work-related sanctions and binding time-limits (Blank 2001).

Given the somewhat contradictory results of TANF’s goal to lift welfare recipients out of poverty and into the workforce, many scholars have concluded that TANF has only partially done this, by decreasing

poverty rates overall while simultaneously contributing to an increase in deep poverty (Danziger et al. 2015, Bitler and Hoynes 2016, Moffitt 2016, Ziliak 2016). Among the implications of this latter increase is the one suggesting that the number of children living in extreme poverty has also spiked since TANF was first implemented; in fact, research has documented a substantial increase in the number of single mothers and children who now live under \$2 a day in the U.S. (Shaefer et al. 2015, Sherman and Trisi 2015, Shaefer and Edin 2018).

While family and child outcomes may have improved for those families in which parents were able to acquire employment and realized a subsequent gain in net earnings, families who were “left behind” by TANF faced different challenges. Sanctioned families in particular have been shown to be at greater risk for undergoing economic hardship than non-sanctioned families. For example, positive correlation exists between familial sanction and the likelihood of facing utility shutoffs, inadequate medical care, maternal hunger, and housing instability (Kalil et al. 2002, Reichman et al. 2005).

Such family hardships are likely to affect child well-being indirectly by compromising sanctioned parents’ abilities to care for their children; still, other studies have directly linked sanctions and child outcomes. Starting at birth, children of sanctioned families are more likely to experience low birth-weight and are also less likely to be born to mothers who breastfeed or receive proper prenatal care (Haider et al. 2003, Kaestner et al. 2005). As children age, there is a greater probability that sanctioned children will face food insecurity, be hospitalized, and have poor school attendance rates (Skalicky and Cook 2002, Larson et al. 2011).

In recognizing this causal relationship between TANF sanctions, deep poverty, and adverse child outcomes, it becomes noteworthy to recognize that benefit reduction rates have spiked during the past two decades. From the onset of TANF’s implementation, sanctions varied by state in severity and length of enforcement; however, many of states still initially refrained from taking away a sanctioned family’s entire benefit. States instead enforced “partial” sanctions. These partial sanctions allowed families who had fallen out of compliance to still receive benefits for their children (Bopp and Falk 2012). States gradually began to levy harsher sanctions, though, and by 2018, nearly all states were willing to revoke a noncompliant family’s entire benefit as a “most severe” punishment. Additionally, almost half of states chose to implement these “full family” sanctions against first-time offenders as an “initial” sanction.

One may expect that families facing more stringent sanctions are more likely to undergo such economic hardships, yet, the effects of increasing sanction severity are still largely understudied. A few studies, however, have shown that mothers facing stringent sanctions are more likely to both exit welfare without a job and suffer from mental health-related issues in comparison to mothers facing more lenient sanctions (Chi-Fang Wu 2007, Davis 2019). These outcomes, in addition to those associated with sanctions more generally, have been shown to hold important implications for the living arrangements of TANF-recipient

families.

### 1.3 TANF and Family Living Arrangements

Much of the research conducted thus far on the impact of welfare reform on family living arrangements has focused mainly on marriage and fertility rates, this most likely owing to the fact that TANF made a point to both encourage two-parent families and decrease out-of-wedlock birth rates (Center on Budget Priorities 2018). Although such research does not explicitly examine how TANF may influence the incidence of arranged kinship care, it does indirectly implicate the choices TANF recipients must make in deciding how their children are cared for.

That being said, the limited literature concerning TANF and living arrangements also consists of many ambiguous findings. Prior research has largely been unable to make robust associations between state TANF policies and family outcomes concerning marriage, fertility rates, children's living arrangements, and female headship (Grogger et al. 2002, Fitzgerald and Ribar 2004, Dunifon et al. 2009, Blau and van der Klaauw 2010). Other research has uncovered more conclusive, yet modest effects of welfare reform on marriage rates, finding that such reform leads to a simultaneous decrease in new marriages and new divorces among TANF recipients (Bitler et al. 2004, Teitler et al. 2009). Research focusing specifically on children's living arrangements likewise found perplexing results, mainly that welfare reform was associated with the increased likelihood that children would live with either both parents or no parent at all (Acs and Nelson 2004, Bitler et al. 2004, Bitler et al. 2006).

The research reviewed thus far indicates that welfare reform indirectly impacted children's well-beings by altering their living arrangements. For instance, children of unwed parents or one-parent homes - both familial types made more and less common by TANF - typically suffer poorer physical and mental health, in addition to a lack of financial resources (McLanahan and Gary 1994, Amato 2005, Bzostek and Beck 2008, Liu and Frank 2008). The research has also made important contributions to understanding how welfare reform has increased incidence of kinship care arrangements, yet it did not attribute such an increase to any specific TANF policy component(s), nor did it distinguish between the effect of reform on foster care kinship care versus informal or private kinship care.

Although such research does not exist regarding non-foster kinship care, studies centered on foster care caseloads found that reductions in welfare cash benefits, tougher sanctions, and stricter time-limits may result in caseload increases (Paxson and Waldfogel 2003, Slack et al. 2003, Swann and Sylvester 2006). While this research plays an essential role in explaining why the share of TANF child-only assistance units have risen in recent years, it does not answer whether non-foster kinship care cases may have similarly contributed to

the increase in child-only TANF cases. Given that survey-designed research studies have provided evidence that relative caretakers may seek informal guardianship of a child to ensure the continuation of financial stability and/or receipts of welfare benefits, it is necessary to systematically investigate the possibility that incidence of non-foster kinship care increased in response to the introduction of increasingly severe welfare benefit sanctions (Richardson et al. 2002, Hernández and Berrick 2018).

I thus hypothesize that an increase in sanction stringency causes an increase in the number of children residing in non-foster kinship care. More specifically, I predict that familial units facing full-family sanctions will be incentivized to place their children in relative care. The reasoning that underscores my hypothesis is twofold: First, I believe that families stripped of their children’s welfare benefits may realize that they can regain such benefits by placing their children in kinship care and capitalizing on child-only TANF kinship funds. Secondly, given the ample amount of evidence documenting the adverse effects that sanctions have on children’s wellbeings, I also stipulate that families may be encouraged to place their children in relative care if they believe that doing so may mitigate potential harm to their children’s livelihoods.

As a caveat to my first point, it is worth noting that previous research has shown that many kinship-care families do not take advantage of TANF kinship funds and thus may not realize that a monetary incentive concerning children’s living arrangements exists (Murray et al. 2004). I thus examine to what extent children living in non-foster kinship care take advantage of TANF funds. If TANF funds are truly underused by a majority of the kinship care population, it may be plausible that sanctioned families are instead incentivized to more generally place their children into the care of financially secure relatives. Additionally, even if incentives do exist that encourage sanctioned parents to place their children into kinship care, they must still outweigh parents’ concerns about altering their children’s living arrangements; feelings of parental disempowerment and failure as well as desires to remain in close contact with their children are both reasons why parents may be hesitant to place their children in out-of-home care (Kiraly and Humphreys 2013).

## 2 Data and Methods

### 2.1 Data and Sample

I use the Annual and Social Economic Component (ASEC) of the Current Population Survey (CPS) to estimate the number of children who lived in non-foster kinship care during the period spanning 1989-2018. I include in my sample all person observations who are less than 18 years in age and thus could be residing in relative care as a minor ( $N = 1,263,957$ ). It should be noted that the CPS is a weighted survey, and I therefore use weights in all of my analyses to account for the fact that some sub-populations (such as children

living in kinship care) may be underrepresented in the sample as is.

Additionally, the CPS is a cross-sectional survey, meaning that I am only able to observe respondents in the year that they were surveyed. This effectively bars me from following respondents across years, something I would ideally do in an attempt to explicitly identify situations in which children are removed from their parental homes and placed into relative care. Nonetheless, the CPS is one of the only large-scale, public record surveys that reports on biological relations between respondents in addition to child respondents' involvements with the foster care system. Therefore, I should still be able to identify changes in kinship care across time and state by observing each state's current kinship-care population in a given year.

In collecting data concerning sanction policy, I take advantage of the Welfare Rules Database, a comprehensive dataset maintained by the Urban Institute that annually tracks changes in welfare policy by state. As my study focuses on sanction policy more specifically, I utilize this database to record state and yearly variation in benefit reductions, the primary measure I use to gauge sanction severity.

Although TANF was not passed through legislation at the federal level until 1996, a handful of states received AFDC waivers from the federal government prior to this, allowing them to enact TANF-like work requirements and sanction penalties within their state welfare regimes. The first of these state waivers was received in 1992, meaning that years 1989-1991 in my data serve as a pre-treatment period for all states. With the exception of California, New York, and Vermont, every U.S. state in addition to Washington D.C. is included in the state data. These former three states never introduced the full-family sanction into their welfare policies, and - as I will later explain - my regression models require all state inputs to have variation in their treatment statuses. For this reason, I exclude the three aforementioned states from my analysis.

## 2.2 Treatment Measures

There is no one agreed upon scale to measure sanction severity in the literature, but rather a handful of disparate ones (Grogger and Karoly 2016). For my study in particular, I expect incentives for guardianship transfers to rise considerably when the full-family sanction is enforced. It is at this threshold that sanctioned children lose welfare benefits they could potentially regain by being placed in kinship care. Within the full-family sanction, there exists further variety in sanction severity still. Some states choose to levy the full-family sanction as a last-resort punishment against non-compliant families, while other states take away a family's entire benefit the first time it fails to comply with work requirements. Adding to this variation, different states may also levy the full-family sanctions against families at varying time-lengths.

Studies focused on analyzing shifting choice structures typically measure a population's response to a new incentive beginning when the population is first made aware of the existence of the change. It



is for this reason that I consider any full-family sanction policy to be a “stringent” policy, while all other sanction policies are considered “non-stringent”. Likewise, I consider a state’s sanction policy to be stringent beginning in the year that the full-family sanction was passed through state legislatures as opposed to when such legislation was enacted into law. Using this definition of policy severity, I factor those families who respond to the incentive preemptively into my estimation of the treatment effect.

To create a treatment group for analysis, I first define a policy severity variable,  $D_{ist}$ , which is set to 1 if state  $s$  that child  $i$  lives in during year  $t$  has a stringent policy sanction, and is set to 0 otherwise. In plain terms, then, children are considered to be “treated” if they were living in a state with a severe sanction policy at the time they were surveyed.

It should be noted that this treatment definition does not explicitly control for “movers” or children that were placed in kinship care in states different from the ones in which they originally resided. While the CPS does not allow me to locate the parental homes from which kinship care children were placed out, I assume that the vast majority of informal guardianship transfers occur within, rather than across, states. As mentioned earlier, Kiraly and Humphreys (2013) found that the biological parents of kinship care children maintain strong desires to remain in contact with their children, this evidence making it likely that parents would choose to place their children with relatives placed closer as opposed to farther away from them. Secondly, the majority of Americans live within close proximity to their immediate family; the median American adult lives only 18 miles away from his or her mother, and this statistic trends downward with income, largely due to the fact that poor parents rely on family members in close proximity to provide day-to-day childcare for their children (Molloy et al. 2011, Pew Research Center 2015). It may even be the case that likelihood of kinship care and family proximity reinforce each other, as Compton and Pollack (2014) find that the demographics most likely to make use of kinship care, such as African Americans, are also the most likely to live within close proximity to their immediate family members. Nonetheless, even if short distances between family members transcend borders, research has proven that states within a same geographic region are more likely to have similar welfare policies, and states that are both within the same region and of the same political leanings are likely to evolve their TANF policies in similar manners (Bailey and Rom 2003, Volden and Cohen 2006).

### 2.3 Control Measures

As previously mentioned, the structure of the CPS only allows me to observe children living in kinship care after they have been placed into their relatives’ homes. This implies that I am not able to control in my analysis for every pre-transition factor that may increase the likelihood of a child residing in relative care, as

many of these factors can only be observed within the child’s parental home. If the identifying assumptions of my regression models hold, however, these covariates should be orthogonal to treatment.

That being said, there are a number of relevant characteristics that can be observed at both the individual-level as well as at the level of the relative caregivers’s household. For instance, previous research has shown that a child’s race, gender, and age, in addition to a caregiver’s region of residence, are all predictive factors of a child living in kinship care (Urban Institute 2003). I thus control for all of these observable characteristics in my regression models. Additionally, I use these same factors to differentiate between subgroups of children living in relative care so that I am able to investigate whether the effect of full-family sanctions on kinship care rates varies across demographics.

## 2.4 Outcome Measures

The outcome of treatment is the binary variable  $Kinship_{ist}$  that equals 1 if child survey respondent  $i$  lives in kinship care and 0 otherwise. In determining whether a child’s living arrangement meets the definition of kinship care, I code  $Kinship_{ist} = 1$  if respondent  $i$  both lives in a household without their biological parents and is recorded as a grandchild, sibling, or other secondary relative of the sampling unit’s respective homeowner.

It should be noted that I also code  $Kinship_{ist} = 0$  if respondent  $i$  is reported to be currently living within the foster care system. I am chiefly interested in studying families’ changing incentives in making decisions about their children’s living arrangements; because it is expected that families incentivized to relinquish guardianship of their children do so voluntarily, children living in court-ordered kinship foster-care are not of primary interest in my study and I thus focus solely on children living in non-foster kinship care. As a robustness check, I also exclude foster-care children from my control group in some specifications of my models, this allowing me to compare treatment effects between models that do and do not treat foster-care children as counterfactuals for children in non-foster kinship care. If I am correct in assuming that a causal relationship does not exist between foster kinship care rates and full family sanctions, then there should not be a significant change in my estimated treatment effect when I exclude foster-care children from my sample.

## 2.5 Regression Models

### 2.5.1 Difference-in-Differences Design

To estimate the causal effect of enacting the full-family sanction on incidence of non-foster kinship care, I first use the the following two-way fixed effects difference-in-differences model:

$$(1) \text{Kincare}_{ist} = \alpha + \gamma_s + \phi_t + \beta^{DD} D_{ist} + \tau X_i + e_{ist}$$

where the outcome variable  $\text{Kincare}_{ist}$  is an indicator that child  $i$  lives in kinship care,  $\gamma_s$  and  $\phi_t$  are state- and time-fixed effects determined by child  $i$ 's state of residence  $s$ ,  $D_{ist}$  is a treatment dummy, and  $X_i$  is a vector of control variables for child  $i$ . Here, the difference-in-differences estimate  $\beta^{DD}$  gives the causal effect of full-family sanctions on incidence of non-foster kinship care. To identify this parameter, I assume that the difference in outcomes between the treatment and control groups is constant over time in the absence of treatment. Results of this regression are reported in Table 4.

### 2.5.2 Event Study Design

In order to further break down the estimates I receive from (1), I also estimate the following two-way fixed effects event study model:

$$(2) \text{Kincare}_{ist} = \alpha + \gamma_s + \phi_t + \sum_{n=-5}^{n=-2} \rho_n 1\{t - t_s^* = n\} + \sum_{n=0}^{n=5} \beta_n 1\{t - t_s^* = n\} + \tau X_i + e_{ist}$$

where the variables from (1) maintain the same interpretations, and the sums  $\sum_{n=-5}^{n=-2} \rho_n 1\{t - t_s^* = n\}$  and  $\sum_{n=0}^{n=5} \beta_n 1\{t - t_s^* = n\}$  are sets of dummies measuring respective trends in outcomes  $n$  years after policy enactment took place in year  $t_s^*$ . I use the year before policy is enacted as the base for these trend dummies ( $n = -1$ ); this implies that the  $\rho_n$  coefficients measure pre-treatment outcome trends for four years leading up to the year preceding policy enactment, while the  $\beta_n$  coefficients capture outcome trends in the year of policy enactment and for five years after.

This model is identified by comparing kinship care populations of states that enacted full-family sanctions in different years. This comparison allows for states which enacted the policy at a later point in time to serve as control groups for those which implemented the policy earlier. Variation in treatment timing is graphed below in Figure 2 as well as listed in Table A3 at the end of the paper. My identification assumption is that states treated at different points in time share common trends in untreated potential outcomes.<sup>1</sup> Therefore, nonrandom pre-treatment trends in outcome would violate my assumption and threaten the identification of my model. Statistically significant pre-treatment trends would also violate my assumption of parallel trends underscoring (1), as such a finding implies that factors not accounted for by my model influence kinship care rates. Results of this regression are reported in Table A2.

### 2.5.3 Testing for Heterogeneous Effects

It may be expected that incentives tied to altering children's living arrangements are more effective for families that are more likely to place their children into kinship care regardless of treatment. For this reason,

<sup>1</sup>For further discussion on the identifying assumptions of this model, see Goodman-Bacon (2018).

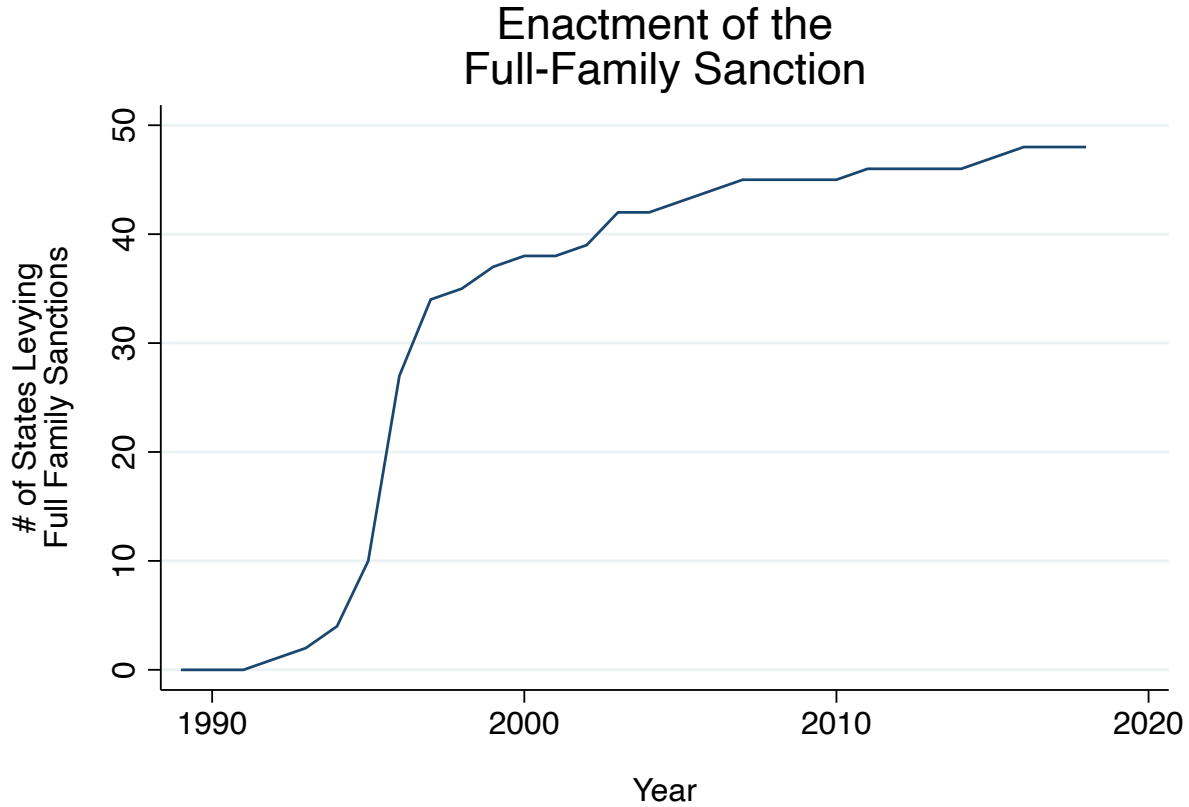


Figure 1: Number of states enacting full-family sanctions over years  
 Source: The Urban Institute's Welfare Rules Database

I differentiate between such children who are more and less likely to be placed into relative care by estimating the median predicted probability of living in kinship care. I do this by fitting the following linear probability model:

$$(3) \text{Kincare}_i = \kappa + \sum_{j=1}^{j=4} \eta_j \text{Race}_i + \theta \text{Age}_i + \lambda \text{Female}_i + \sum_{k=1}^{k=3} \mu_k \text{Region}_i + \pi \text{Met}_i + \xi_i$$

where  $\text{Kincare}_i$  is an individual-level indicator variable,  $\text{Race}_i$  is a set of race dummies,  $\text{Age}_i$  is a categorical age control, and  $\text{Female}_i$  is a gender dummy. The last two variables in the equation control for the relative's area of residence, with  $\text{Region}_i$  being a set of regional dummies as defined by the U.S. Census Bureau and  $\text{Met}_i$  being a binary variable equaling 1 if the relative lives in a defined metropolitan statistical area and 0 otherwise. These covariates are part of the same set of controls I include in my difference-in-differences and event study models. I only include the additional control,  $\text{Region}_i$ , to this regression, as the literature

typically discusses correlation between kinship care rates and residence locations at regional rather than state levels.

After gaining estimates from this preliminary regression, I estimate the median predicted value that a child lives in kinship care. I am able to use the median estimate to create two exclusive subsamples from my original sample: one sample consisting of children who are less likely to live in kinship care (those with a predicted outcome probability below the median) and a second sample consisting of children who are predisposed to living in kinship care (those with a predicted outcome probability above the median). I then test whether the treatment effect is heterogeneous by using (1) to analyze these groups separately. Results of this regression are reported in Table A1.

### 3 Results

#### 3.1 Summary Statistics

Table 1 reports summary statistics relating to the portion of my sample that lives in kinship care. It can be seen that the kinship care population is relatively small when compared to America’s under age eighteen population as a whole. Around 3% of children are reported as living in kinship care. This modest share of the population is roughly the same across the treatment and control groups, with the share of kinship care children in the treatment group being larger by about half a percentage point. Figure 2 charts kinship care rates over time. While the graph displays no clear linear trend in incidence of kinship care, it is shown that the frequency of cases was relatively higher in years after 2001.

Table 1: Sample Share in Kinship Care

	Count	Mean	SD
Share of Total Sample	38238	3.145	17.454
Share of Control Group	8484	2.738	16.319
Share of Treatment Group	29754	3.304	17.876
Observations	1263948		

$N$  = Total sample size

Note: Sample statistics reported as point percentages. Treatment group includes all individuals < 18 living in states with “stringent” policies. Control group includes all other individuals < 18 living in states with non-stringent policies.

Additional summary statistics focusing on the subsample of children living in kinship care are reported in Table 2. As prior research has suggested, I found that only a relatively small percentage (15%) of the kinship care population received TANF funding. While the CPS does not differentiate between different types of

TANF funding, it may be likely that low-income caregivers who qualify for and make use of standard TANF funds are also more likely to take advantage of the child-only TANF funds that are available to relative caregivers. This cannot be known for certain, however, and it may be the case that the share of relative caregivers receiving child-only TANF funds is actually lower than 15%.

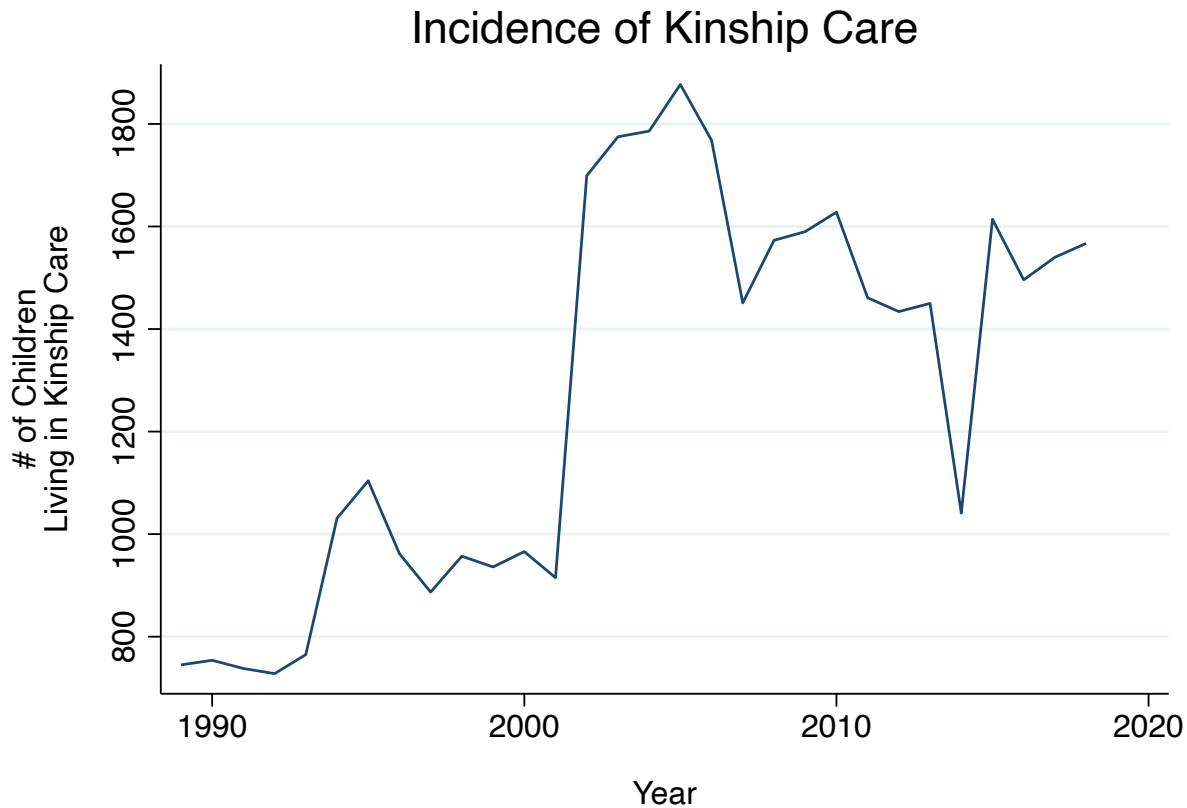


Figure 2: Incidence of kinship care cases over years  
Source: CPS ASEC

Table 2: Kinship Care Population

	Count	Mean	SD
Share Receiving TANF	5668	14.968	35.677
Share Living Under 150% of FPL	18601	49.945	50.001
Observations		38238	

*N* = Size of sample in kinship care

Note: Sample statistics reported as point percentages.

Nearly half of children in kinship care live below 150% of the federal poverty line. When accounting for the fact that many kinship care children originally come from poor households, this may still prove

to be a marginal improvement to the child’s financial security. Still, it is somewhat surprising that the majority of impoverished relative caregivers do not receive child-only TANF funds, as it would be expected that caregivers facing financial strains would take advantage of any government subsidies for which they qualified. More research is needed to determine whether such caregivers are not aware that they qualify for TANF funding or if reasons exist why caregivers may willfully reject such funding.

### 3.2 Regression Results

Results from estimating (3) are reported in Table 3. The results are again generally consistent with the literature. All coefficients are statistically significant at the 5% level, however, some of the factors appear to only have marginal effects on the likelihood of residing in kinship care. Female children, for instance, are only slightly more likely to be in non-foster kinship care than males are.

Black children, on the other hand, make up the demographic most strongly correlated with incidence of kinship care, being more likely to be placed in kinship care than white children by nearly five percentage points. Given the fact that the kinship care sub-population is fairly small in comparison to the country’s population as a whole, an increase of this magnitude is considered to be a sizable effect. For example, given the predicted mean probability of kinship care is 3.015%, an increase in probability by five percentage points would lead to a child being more likely to be placed in kinship care by over 161%. Overall, older, non-white children residing in rural areas have the highest probability of being associated with a non-foster kinship care arrangement. Likewise, children living in the South are the most likely to live in non-foster relative care, while midwestern children are the least likely.

The regression estimates from the difference-in-differences model (1) are displayed in Table 4. The first column of the table is a simple OLS regression that measures correlation between non-foster kinship care rates and full-family sanctions. It is shown that the association between the two are statistically significant at the .1% level, as passage of the full-family sanction corresponds with an almost 19% increase in non-foster kinship care rates. The next four columns of Table 4 includes output from the complete specification of the difference-in-differences model. To check for robustness, I run an unweighted difference-in-differences model, a weighted model, and a weighted model with added controls. The last column of the table is also a weighted controlled model but excludes foster-children from the sample.

None of these latter treatment effects proved to be statistically significant below the 5% level. In light of these results, the positive correlation between non-foster kinship care rates and full-family sanctions suggests that states which implemented the full-family sanction earlier also had larger non-foster kinship care populations. Interestingly, weighting the model causes the difference-in-differences coefficient to switch

Table 3: Observable Characteristics

Black	4.860*** (.075)
Hispanic	1.500*** (.052)
Asian	0.041 (.072)
Other Race	4.160*** (.195)
Age	0.129*** (.004)
Female	0.075* (.037)
Northeast	0.174*** (.053)
South	1.000*** (.046)
West	0.299*** (.05)
Metro	-1.130*** (.049)
Constant	1.290*** (.06)
<i>Kincare</i>	3.015
Observations	1263948

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Note: Regression output from (3) reported as percentage point change. White and Midwest omitted as base groups.



from a positive to negative value, shifting the treatment effect from a 7% increase in kinship care rates to a 5% decrease. This significant change in treatment effects may be expected, however, given that the homes of kinship care children are likely under-sampled by the CPS. Once controls are added to the model, the treatment effect becomes more negative, representing a 5.5% decrease in kinship care rates. While such an effect is not as massive as the effect of race, it is still meaningful and is comparable to the effect that children's ages have on kinship care rates. The upper bound of the 95% confidence interval for this estimate is only a .004% increase in kinship care rates, suggesting that while I cannot definitively rule out a zero treatment effect, the effect is most likely negative.

When removing foster-care children from the sample, the change in treatment effect was not statistically significant at the 5% level. As can be seen in Table A1, there does exist a statistically significant difference at the 1% level when comparing outcomes for children who were less likely to be put in kinship care and vice versa. While the treatment effect for the former group is non-significant, the effect is significant at the 5% level for children predisposed to being placed in kinship care. The treatment effect is also greater for this sub-sample than it is for the sample as a whole, with treatment causing over a 16% decrease in kinship care rates.

Table 4: Difference-in-Differences Results

	1	2	3	4	5
	Weighted	Unweighted	Weighted	Weighted	Weighted
Treated	0.567*** (0.042)	0.216 (0.070)	-0.149 (0.092)	-0.166 (0.091)	-0.165 (0.091)
Constant	2.740*** (0.035)	3.640*** (0.172)	3.880*** (0.195)	1.920*** (0.198)	1.920*** (0.199)
Controls				X	X
$\hat{Kin}care$	3.158	3.025	3.047	3.010	3.020
Observations	1263948	1263957	1263948	1263948	1260024

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Note: Regression output from (1) reported as percentage point change. Model 1 is a basic OLS regression. Model 5 excludes foster-care children from sample.

The estimates from the event study design are reported in Table A2, where I again run (2) first without weights and controls for robustness checks. Effects during the post-treatment years for states that enacted the full-family sanction are mostly negative across all specifications of the model. After weighting the model, some of the post-treatment years are significant at the 5% level, as is one pre-treatment year. These trends along with their estimated 95% confidence intervals are graphed in Figure 3.

## 4 Discussion

The majority of treatment estimates from my model proved to be negative, this result being counterintuitive to my original hypothesis. A negative treatment effect implies that a child is less likely to enter into relative care when living in a state with a full-family sanction policy. It should be noted, however, that the treatment effect estimated from my difference-in-differences model in addition to some of the post-treatment trends from my event study model are statistically insignificant. Moreover, there is reason to believe that my key identification assumption was violated, as I also uncovered effects in pre-treatment years that were statistically significant. Looking at the trends graphed in Figure 3, some - but not all - of the 95% confidence intervals include zero. Given that the negative treatment effect was most strongly identified for the sub-sample of children who were more likely to enter into non-foster kinship care, it seems most likely that full-family sanctions cause non-foster relative care rates to decrease among those who are most vulnerable to being placed in such care. On the other hand, it appears that there is most likely a zero treatment effect for children outside of this subsample.

While it may be the case that child-only TANF funds do not act as a monetary incentive in the way that I projected, it is also plausible that my data did not provide sufficient variation for producing robust estimates. Although variation in timing treatment across states spanned a period of over twenty years, a significant portion of the variation occurred during a period of exponential growth that took place during the course of only four years. Moreover, the size of the sample kinship care population relative to the total sample is small, implying that my model needs a substantial amount of statistical power in order to successfully estimate the true effect of full-family sanctions on non-foster kinship care rates.

I should likewise add that, even though it theoretically might be expected that a sanctioned family will begin to respond to incentives regarding their children's well-beings when funding for their children is lost, this empirically may not hold true. It could be that once a certain percentage of a family's benefit is reduced, additional funds received on behalf of its children are not substantial enough to make up for the absolute financial lost. Alternatively, perhaps sanctioned families are not moved to react when their benefits are reduced for a month, but do react when their benefits are instead taken away for a year. It may be that an alternative measure of sanction severity is more strongly correlated with non-foster kinship care rates than the full-family sanction is.

There may also exist legitimate explanations as to why enactment of the full-family sanction could decrease the likelihood that a child is placed into non-foster relative care. For instance, foster care payments in most states pay considerably more to caregivers than do child-only TANF funds. While it may be expected that many sanctioned parents would be averse to placing their children more generally into the

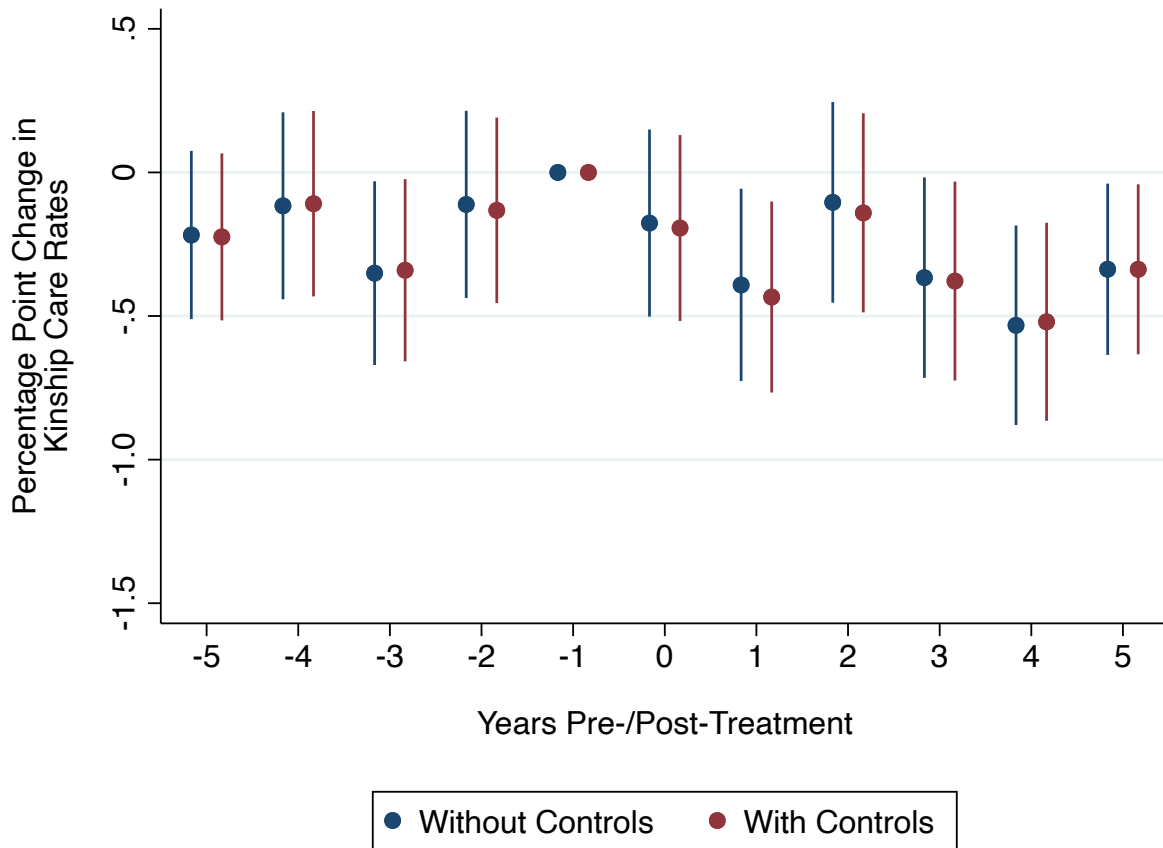


Figure 3: Plot of regression coefficients and 95% confidence intervals showing pre- and post-treatment trends in outcomes

foster care system, some might feel more secure in placing their children in kinship foster care. In recent decades, state governments have made a push to place displaced children with relative rather than stranger caregivers. In doing so, many states also made it easier for former informal kinship caregivers to receive foster licences. It seems feasible, then, that when faced with full reductions in welfare benefits, families responding to potentially multiple financial incentives would act in correspondence with the highest-paying incentive, given that the risk in doing so was not significantly elevated. If such a theory were true, it would explain why rates of non-foster kinship care might decline as households facing full-family sanctions shifted from placing their children in traditional kinship care to placing them instead in a higher-paying foster kinship care system. This would also make clear why those most vulnerable to being placed in kinship care experienced negative treatment effects; families more likely to enter their children into non-foster kinship care may also be more likely to enter their children into foster kinship care when the payoff is sufficiently great.

## 5 Conclusion

Although my difference-in-differences model produced somewhat ambiguous and unanticipated results, my findings also prompted new questions for research. While the shared characteristics between foster and non-foster kinship care children have been researched extensively, research relating to their differences is lacking. More specifically, it would be interesting to further research the relationship between foster and non-foster kinship care rates and to investigate how the relationship varies across different state welfare regimes and child protective services.

Building on this point, I think that the existence of a cohesive, large scale data-set centered on children within non-foster kinship care is needed to facilitate the research I mentioned above. Given that the living arrangements of foster-care children are known to the government, there is a greater supply of data concerning their demographics and outcomes. The same does not exist for children in kinship care, however. To truly be able to differentiate between the two groups, it is necessary to collect data that links a kinship child's parental home to his or her relative caregiver's home. Such information could play a pivotal role in understanding the causes that lead to a child being placed in non-foster kinship care.

It should likewise be investigated whether foster care has become more voluntary in its arrangement as states have expanded access to relative foster care licensing; if this is true, then it may be expected that foster kinship care rates respond to policy incentives in the same way I hypothesized non-foster kinship care rates responding. Further research should help policymakers in deciding whether to enlarge the social safety net in place for children of sanctioned families as well as to more accurately value the utility derived to society in enacting harsh sanctions against welfare reliant families.

Finally, I believe that my research has also highlighted the need for a standardized measure of TANF sanction severity. The majority of research focusing on TANF sanctions does so without taking into account the heterogeneity that exists within sanction policy across states and time. Now that over twenty years have passed since TANF's implementation, it is necessary for a severity measure to be developed and used consistently across TANF sanction studies. Not only could such a measure help to more effectively detect variation in sanction policies, but it would also allow scholars to more effectively build from and further contribute to previous researchers' work.

## Tables

Table A1: Heterogeneity Testing

	1	2
	Weighted	Weighted
Treated	0.104 (0.090)	-0.384* (0.163)
Constant	1.660*** (0.243)	0.810** (0.293)
Controls	X	X
$\widehat{Kincare}$	2.134	2.360
Observations	632526	631422

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Note: Regression output from (1) reported as percentage point change. Sample for Model 1 includes only children with predicted outcome percentages below the predicted median. Sample for Model 2 includes only those with predicted outcome percentages above the predicted median.

Table A2: Event Study Results

	1	2	3
	Unweighted	Weighted	Weighted
Pre-5 Years	-0.0098 (0.113)	-0.218 (0.149)	-0.224 (0.148)
Pre-4 Years	-0.000311 (0.127)	-0.00116 (0.166)	-0.00109 (0.165)
Pre-3 Years	-0.147 (0.124)	-0.351* (0.163)	-0.341* (0.162)
Pre-2 Years	-0.027 (0.124)	-0.111 (0.166)	-0.132 (0.165)
Event Year	-0.063 (0.124)	-0.176 (0.00166)	-0.194 (0.165)
Post-1 Year	-0.083 (0.131)	-0.392* (0.171)	-0.434* (0.170)
Post-2 Years	0.202 (0.136)	-0.104 (0.178)	-0.141 (0.177)
Post-3 Years	-0.0364 (0.136)	-0.366* (0.178)	-0.378* (0.177)
Post-4 Years	-0.128 (0.138)	-0.532** (0.177)	-0.520** (0.176)
Post-5 Years	0.048 (0.116)	-0.337* (0.152)	-0.337* (0.151)
Constant	3.645*** (0.204)	4.092*** (0.242)	2.144*** (0.245)
Controls			X
$\hat{Kincare}$	3.025	3.047	3.010
Observations	1263957	1263948	1263948

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Note: Regression output from (2) reported as percentage point change. Pre-1 Year omitted as a base group.

Table A3: Variation in Treatment Timing

	Year Full-Family Sanction Passed
Alabama	1996
Alaska	2002
Arizona	1996
Arkansas*	1997
California**	N/A
Colorado	1997
Connecticut	1994
Delaware	1995
D.C.***	2016
Florida	1996
Georgia	1997
Hawaii	1999
Idaho	1996
Illinois	1995
Indiana	2003
Iowa	1993
Kansas	1996
Kentucky	1996
Louisiana	1997
Maine	2011
Maryland	1996
Massachusetts	1995
Michigan	1994
Minnesota	2003
Mississippi	1996
Missouri	2015
Montana	2000
Nebraska	1995
Nevada	1996
New Hampshire	2007
New Jersey	1997
New Mexico	1997
New York**	N/A
North Carolina	1999
North Dakota	1996
Ohio	1996
Oregon	1996
Pennsylvania	1997
Rhode Island	2005
South Carolina	1996
South Dakota	1996
Tennessee	1996
Texas	2003
Utah	1992
Vermont**	N/A
Virginia	1995
Washington	2006
West Virginia	1995
Wisconsin	1996
Wyoming	1997

\* Arkansas repealed the sanction in 1997 and re-passed it in 2002.

\*\* These states never passed the full-family sanction.

\*\*\* D.C. repealed the sanction in 2018.

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