Do higher wages improve service quality? Evidence from foster care in California using a regression discontinuity design*

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Abstract

Experience in foster care is common among youth in the United States and high quality foster care is critical to ensure their safety and long-term well being. This paper studies the effectiveness of higher payments to foster care providers as a quality improvement tool. I leverage a federal court case that induced a 21-37% increase in monthly payments to foster families and relatives recruited and managed by California counties, but did not extend to those recruited and managed by private agencies. Additionally, some relatives who established guardianship over a child in California were retroactively eligible for the pay raise based on the date their guardianship was established. I use a regression discontinuity design and two decades of administrative data to identify the short term effects of the court case on the safety of children in foster care, as well as its long term effects on the safety of children in a related guardian's care. I find that higher monthly payments did not immediately affect the safety of children in the care of foster families or relatives recruited by the county, but the long-term receipt of higher payments by related guardians reduced the likelihood that a child was maltreated within five years of their guardianship start by 68%. Further, despite no change in pay, the announcement of the raise for county-recruited homes immediately decreased the monthly likelihood of maltreatment while placed in private agency homes by 34%. These findings are consistent with changes in provider effort and investments on the intensive margin and contrast with the scarce evidence to-date that suggests wages are an ineffective quality improvement tool for foster care.

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1 Introduction

Five percent of children in the US will spend time in foster care by their 18th birthday (Yi et al., 2020). Foster care is a temporary home-like environment for children who have been removed from their homes due to abuse, neglect, or exploitation. Providing high quality foster care is challenging given the physical, mental, and emotional demands of full-time care but critical to ensuring the safety of foster youth. From 2000 to 2010, 6% of children who entered foster care in California were maltreated while in care[1] despite quality assurance processes in place, including training, licensing, and monitoring.

This paper studies the effectiveness of higher wages as an alternative quality improvement tool in foster care. Although foster care payments are a non-taxable benefit received by a foster parent on behalf of a child in their care, the exchange of these payments for care giving services functions identically to wages in models of labor supply and demand. Higher wages could improve quality through the extensive margin, by attracting foster families and relatives[2] who deliver higher quality care, or the intensive margin, by improving the quality of care provided by foster families and relatives. I study a large shock in monthly payments to foster care providers using two decades of administrative data and a regression discontinuity design and find quality of care, defined in terms of child safety, improves on the intensive margin.

Past research demonstrates that placement in foster care improves outcomes for children on the margin in some contexts (Bald et al., 2019; Roberts, 2019; Gross and Baron, 2020), while worsening outcomes in others (Doyle, 2007*b*, 2008). This points to important differences in both the threshold of maltreatment that warrants removal and the quality of foster care across different settings and over time. However, our understanding of foster care quality and effective quality improvement interventions is limited[3]. To my knowledge, the Doyle (2007*a*) study of a 1995 policy that lowered pay to relatives in Illinois offers the only causal evidence of the effect of a payment level change on quality. Doyle found quality of care was unaffected. My study is the first to examine the effect of an increase in pay to both relatives and foster families and document improved outcomes.

The debate around public service wages typically focuses on the extensive margin. Although neoclassical models of labor supply predict higher paying jobs would recruit more productive workers with a higher opportunity cost of time, motivation crowding theory posits that higher pay would recruit workers who are less intrinsically motivated to perform (Delfgaauw and Dur, 2007, 2008). Georgellis et al. (2011) lent some evidence to the crowd out theory by showing extrinsic rewards reduced employment of intrinsically motivated workers in the UK public sector[4] and argued reducing earnings could improve performance. In the child welfare system, fears of motivational crowd out are echoed in the long held public sentiment that foster care providers should not be extrinsi-

[1] Maltreatment refers to substantiated allegations of abuse or neglect that started after the placement in care.

[2] California law requires the social worker to assess all adult relatives for placement within 30 days of removal and give preferential treatment to any who request placement of the child in their care. However, such requests are voluntary.

[3] See reviews by Doyle and Aizer (2018) and Bald et al. (2022).

[4] Georgellis et al.'s finding is limited to education and healthcare industries.

cally motivated. In a nationally representative survey, one-third of respondents believed foster care providers are "in it for the money", and foster family focus groups unanimously reported stigma associated with this perception (Leber and LeCroy, 2012; Hochman et al., 2004).

Alternatively, there are three ways higher pay could improve the quality of foster care on the intensive margin. First, higher pay may increase provider effort or their investment in children by alleviating resource constraints, including material and non-material resources. Resource limitations are especially relevant in cases of neglect, which make up the majority of maltreatment allegations, and to relatives, who have lower socio-economic status[5] on average compared to non-related foster care providers (Sakai et al., 2011). Higher pay could improve one's emotional capacity to provide high quality care by boosting morale (Akerlof, 1982)[6]. Alternatively, higher pay may induce an income effect whereby a relaxed budget constraint increases a family's demand for normal goods, including direct investments in child development and others. Over time, the accumulation of material resources could translate into more temporal, cognitive, and emotional resources that further facilitates high quality care giving. This hypothesis is supported by the robust literature on cash transfers to low-income families (Dahl and Lochner, 2012; Hoynes et al., 2015; Bastian and Michelmore, 2018; Manoli and Turner, 2018; Bailey et al., 2020).

Second, higher pay may improve the ability of low-skilled workers. Over time, higher pay could facilitate on-the-job skill formation by reducing turnover of providers (Salop, 1979). This theory is empirically supported by recent evidence from nursing home facilities affected by minimum wage increases (Ruffini, 2022).

Third, higher pay may increase effort among low performers by increasing the cost of providing poor quality care. Social workers are responsible for matching children with foster care providers and monitoring those matches but only partially observe quality. In this context, low performers could lose out on income by losing their license to operate or being passed up by a social worker in favor of another care provider. Shapiro and Stiglitz (1984) argue higher pay reduces the incentive to shirk workplace responsibilities by increasing the cost of unemployment in the form of foregone wages. In addition, higher pay could reduce the incentive to shirk by increasing the risk of unemployment. If higher pay attracts more workers to supply their labor and demand for their services is unchanged, more competition among workers would result and low performers would be more likely to lose out on employment.

This paper leverages a federal court case that induced an unprecedented 21-37%[7] increase in California's foster care payments. The case created multiple plausibly exogenous changes in payment policy. On August 30, 2010, the court ruled that California Department of Social Services (CDSS) must raise foster care payments to be in compliance with federal law. The ruling functioned to announce a large and imminent pay raise. After nine months of inaction, a second court order

[5]Compared to foster care providers, relatives tend be single, older, less educated, in poor health, low-income, and unemployed. They also receive fewer support services.

[6]Akerlof (1982)'s theory of efficiency wages likens the offer of wages that exceed the market clearing level to a gift exchange.

[7]Varies by age group. Pay increased by by 37% for age 0-4, by 36% for age 5-8, by 34% for age 9-11, by 27% for age 12-14, by 21% for age 15-17.

compelled CDSS to bypass state legislature, if necessary, to raise foster care payments. Immediately thereafter, a raise was implemented on May 31, 2011, for foster families recruited by county agencies (henceforth, "county homes") and approved relative homes providing temporary care, but did not extend to foster families recruited and managed by private agencies (henceforth, "agency homes") and only partially to legal guardians. Typically, relatives who established legal guardianship over a child would also receive the county home rate through the child's 18th birthday[8]. However, CDSS retroactively determined the higher rates would only apply to relatives who established a guardianship on or after May 1, 2011, while those who established guardianship before would receive the pre-policy county home rates in perpetuity.

In this study, I define quality outcomes in terms of safety in foster care using two decades of administrative data from California. In a partially de-identified extract of the Child Welfare Services/Case Management System (CWS/CMS), I observe substantiated allegations of abuse or neglect and assign the incidence of maltreatment to the date on which the allegation was received.

To estimate the effect of the pay increase on maltreatment, I use three regression discontinuity designs. First, I leverage the timing of the pay increase (May 31, 2011) to estimate the immediate impact of receiving 21-37% higher payments on the safety of children in county and relative homes. Using a panel of child-placements[9] by the month of care provision, I treat the month as the running variable and find no immediately measurable impact of the pay raise on the safety of children placed in county homes or relative homes. Notably, this design would only detect an effect that was realized beginning from the first month of implementation and not one that results from cumulative exposure over many months of implementation.

Second, I leverage the guardianship start date criterion (May 1, 2011) to estimate the long-term effect of the pay increase on children placed in guardianships with a relative. The criterion ensures children experience their entire guardianship under one payment regime and allows me to measure safety outcomes for each child over many years. Using a sample of children placed with relative guardians, I treat the guardianship start date as the running variable and find that higher pay reduced the likelihood of being maltreated within five years of the guardianship start by 68% and reduced the likelihood of reentering foster care over the same time horizon by 36%. There is no evidence of threshold manipulation or selection. Further, turnover and competition hypotheses are implausible in this setting as legal guardianships are meant to be one-time, permanent placements. Rather, the improvement in safety and reentry for children with relative guardians is consistent with a change in effort or investment due to an income effect.

Last, I leverage the timing of the initial court ruling (August 30, 2010) to estimate the immediate effect of announcing a pay raise on the safety of children placed in agency homes, who were excluded from the raise and, thus, precluded from an income effect, morale boost, and other individual-level mechanisms. Rather, this design most closely tests the hypothesis that higher wages create a

[8]This benefit is conferred by the Kinship Guardian Assistance Program (KinGAP). Additional eligibility criteria detailed in Appendix A1.

[9]A child may have multiple placements in one month and a placement by involve multiple children. Here, I am interested in each unique match between a care provider.

disincentive to shirk at the market-level by heightening competition among agency homes and increasing the risk of non-placement for low performers. Agencies are contracted by California counties to care for children with needs that cannot be met by relative or county homes and were paid more to do so[10] during the decade that preceded the court rulings. I document evidence that this pay differential buffered agency homes from the real pay stagnation and decline that characterized the period; the share of child-placements in county homes fell while that of agency homes grew consistently. In *California State Foster Parent Association v. Wagner, et al.*, the Association argued higher pay was needed to correct the shortage of county homes and prevent agency homes from further absorbing the unmet demand for foster care[11]. For agency homes, the anticipation of such a demand shock and the heightened competition that would ensue reduces the incentive to shirk in the present.

Similar to my first design, I use a panel of child-placements by the month of care provision and I treat the month as the running variable. I find the ruling immediately reduced the monthly likelihood a child was maltreated while placed in an agency home by 34%. As in my first design, this design would not detect an impact that results from cumulative awareness of the court ruling over time. I find no evidence of threshold manipulation, selection, a change in turnover, or a change in monitoring by social workers. Instead, improved child safety in agency homes is consistent with a change in effort due to anticipated competition among agency homes.

Together, these findings show that payments can be an effective tool to keep children in foster care safe. I find evidence that the announcement of a competitor's raise and the long-term receipt of higher monthly payments improved the safety of children in foster care. I rule out selection on the extensive margin, increased monitoring intensity, reduced turnover, and changes in the quantity or composition of children in care as mechanisms for these improvements. Rather, these findings suggest that providers responded on the intensive margin by increasing their effort or investment of resources. For children entering a guardianship with a relative, I estimate that the raise saved the state of California 1,327 USD on average per child over five years through reductions in foster care reentry alone[12].

This study adds to the small literature on foster care payments and quality. This is the first study, to my knowledge, that estimates the effect of higher foster care payments on the quality of foster care, as well as the first to estimate the effect on quality of care provided by competitors who would not benefit from the raise. Whereas Doyle (2007*a*) found no impact of a decrease in payments on the quality of care provided by relatives, I find an increase in payments improved the

[10]For each month a child is in care, an agency home receives a basic rate and child increment while the agency receives a fee to cover social work and administrative costs. Through 2009, the basic rate and child increment paid to agency homes was over 25% higher than the basic rate paid to county homes. Thereafter, the discrepancy narrowed (10-25%). This compares the minimum payment to each home. In some cases, county homes are also eligible for a Specialized Care Increment. These are supplemental monthly payments set by some California counties for the care of children with a health and/or behavior problem.

[11]While the court ruling centered on the lack of proof that cost categories had been considered in setting rates, the plaintiff's argumentation had centered on the shortage of county homes that had been created by low payment rates.

[12]See Appendix Section A2 for details of calculation.

quality of care provided by relative guardians who received the raise as well as agency homes who did not.

This study also contributes to our understanding of efficiency wage mechanisms and the return on cash transfers in childhood. Shapiro and Stiglitz's seminal theory of efficiency wages posits that higher pay increases the cost of shirking. Although Shapiro and Stiglitz (1984) argue the cost of unemployment affects the incentive to shirk, the response by agency homes to the announcement suggests the risk of unemployment also affects shirking. A large literature demonstrates the benefits of means-tested cash transfers to poor families (Dahl and Lochner, 2012; Hoynes et al., 2015; Manoli and Turner, 2018; Bastian and Michelmore, 2018). Relative guardians' response to the raise suggests cash transfers can have an income effect in the absence of means testing when targeted to non-biological parents and benefiting children who are disproportionately affected by adverse childhood events.

Lastly, I contribute to the literature on public service motivation, which has long demonstrated high rates of intrinsic motivation among employees in the public sector and argued for its importance to public sector performance (Ritz et al., 2016). Motivation crowding predicts low pay in the public sector would attract the most intrinsically motivated service providers who are the highest performers (Delfgaauw and Dur, 2007, 2008; Georgellis et al., 2011). In contrast, my findings suggest that historically low payment rates in California attracted some providers who did not act in the child's best interest due to constrained resources and suppressed competition. This finding holds regardless of familial relation to the child, which could proxy for intrinsic motivation in this setting.

The rest of this paper continues as follows. Section 2 describes California's foster care system and *California State Foster Parent Association, et al. v. Wagner, et al.*. In Sections 3 and 4 I describe the administrative data and regression discontinuity design, respectively. In Section 5, I present the effect estimates. I conclude with policy recommendations and implications for future research in Section 6.

2 Foster care

2.1 Foster care system

When a child is removed from their home due to abuse, neglect, or exploitation, a social worker is responsible for placing the child with a substitute care provider temporarily through foster care. These stays are a temporary form of out-of-home placements (henceforth, "placements"). In temporary placements, the care provider is paid a monthly rate that is meant to cover the material needs and supervision of each child in their care. The payment is a non-taxable benefit that the care provider receives on behalf of the child. The base payment rate varies by the type of placement and, in the period we study, the age of the child[13]. Care providers also undergo monitoring in

[13]In 2017, age-based rates were discontinued.

the form of monthly caseworker visits and mandated reporting[14]. A child may exit a temporary placement to family reunification, a permanent placement (e.g., adoption, legal guardianship), another temporary placement, adulthood[15], or emancipation.

This paper focuses on three types of placement homes providing temporary care: relative homes, county homes, and agency homes. State and federal law requires social workers to place children in the least restrictive and most family-like environment, with priority given to relatives. County governments directly recruit both foster families (henceforth, "county homes") and willing relatives (henceforth, "relative homes"), who are compensated as if the child was placed in a county home. Children with needs that cannot be met by a relative or county home may be placed with a private foster family agency (henceforth, "agency home") that maintains it's own network of specialized homes. Agency homes receive higher base compensation than county homes to reflect the specialized services delivered by care providers and overhead costs borne by the agency. For each month that a child is placed in an agency home, the care provider receives a basic rate and child increment while the agency receives a fee to cover social work and administrative costs. Through 2009, the basic rate and child increment paid to agency homes was over 25% higher than the basic rate paid to county homes. Thereafter, the discrepancy narrowed (10-25%).

I also study relative guardians. Through the Kinship Guardian Assistance Program (KinGAP), relatives who establish legal guardianship over a child in foster care may receive monthly payments as if the child were in a county home through the child's 18th birthday [16]. These placements may receive initial monitoring visits, but are exempt from the requirement for monthly in-home visits by a social worker.

2.2 *California State Foster Parent Association v. Wagner, et al.*

In 2007, the California State Foster Parent Association filed a civil complaint against officials at California Department of Social Services (CDSS) (see Figure 1). In the six years leading up to the filing, foster care payments had not been adjusted in California. In real terms, this led to historically low foster care payments (see Table 2a) and several counties reported acute shortages of county homes. Over 2001 to 2007[17], the share of children placed in county homes fell from 19% to 13% while the share of children placed in agency homes rose from 27% to 34%.

The plaintiffs alleged that CDSS violated federal law by failing to pay providers enough to cover the cost categories required of states receiving federal funding. The Child Welfare Act (Title IV-E of the Social Security Act) requires states to provide foster care maintenance payments on behalf of each child that cover the cost of food, clothing , shelter, daily supervision, school supplies, a child's personal incidentals, liability insurance with respect to the child, and reasonable travel to a child's

[14]Since 1974, the Child Abuse and Prevention Treatment Act (Public Law 93-247) has required certain professionals, like medical providers and teachers, to report suspected cases of abuse or neglect.

[15]Foster care typically ends at age 18. However, foster youth may remain in foster care while attending high school up until age 19 (Reed and Karpilow, 2009). Since January 2012, foster youth in California may enter Extended Foster Care (under AB12) if they meet at least one of five participation criteria.

[16]Additional eligibility criteria detailed in Appendix A1.

[17]Point-in-time measurements taken from July of each year.

home for visitation. A federal District Court issued a summary judgement in favor of the plaintiffs in 2008. The Ninth Circuit of Appeals affirmed the decision on August 30, 2010. By the time the case was decided in 2010, agency homes had surpassed relative homes to make up the largest share of child-placements in California, 39% (see Table 3b).

Although the court allowed CDSS latitude in correcting its payment rates for county homes, court documents at the time estimated that a 30-40% increase was necessary to be substantially in compliance with federal law. By May 2011, CDSS had designed new payment levels but had not implemented them. A federal court order on May 27, 2011, explicitly granted policy makers at CDSS the authority to bypass the state legislature, if necessary, to implement the new payment levels. CDSS implemented a 21-37% raise on May 31, 2011 (see Table 2a). The pay raise applied to county homes and relative homes, but excluded agency homes. Four months after the pay raise was enacted, CDSS announced that KinGAP recipients who established legal guardianship before May 1, 2011 were also ineligible from the pay raise. Instead, they would receive the pre-policy payment levels in perpetuity (see Figure 2b).

3 Data

The data for this study comes from California's Child Welfare Services/Case Management System (CWS/CMS)[18]. I use administrative data spanning CWS/CMS's inception in 1998 through 2020. The CWS/CMS data extract contains detailed information on every child's placements and services received through California's child welfare system, as well as their care providers. In particular, I observe the start and end date of each placement, type of placement, and any familial relationship between the care provider and child, which allows me to distinguish between the types of placements relevant to this study.

To create each analytic sample, I first restrict placements to those lasting longer than one day and supervised by any of California's 58 County Welfare Departments. I further restrict to placement periods wherein the child was age 0 to 17. Then, I create two samples for this study.

First I create a panel of child-placements by month of care provision. I restrict to placements in county homes, relative homes, and agency homes. In a given month, a child may be placed in the care of multiple providers and a provider may have multiple children placed in their home. I use the child-placement to retain all information about each unique match between child and provider over time. To measure maltreatment in care, I create an indicator for the month in which a substantiated allegation was received for the child during a placement. If the allegation included an estimated abuse start date, the placement start date must precede it.

Second, I create a sample of children in KinGAP. I do not directly observe KinGAP participation in CWS/CMS, but can infer participation from eligibility. I define child participants in KinGAP to

[18]The Committee for the Protection of Human Subjects at the Office of Statewide Health Planning and Development approved the use of this data for the purpose of this study in 2020. The CDSS approved the same in Attachment A of Agreement 18-6010 between CDSS and the Regents of the University of California, Berkeley, California Policy Lab on March 23, 2020.

be those placed with a guardian to whom they are related. Then, I estimate the guardianship start date to be the start date of a child's earliest placement with the relative guardian. To measure maltreatment in the context of KinGAP, I create an indicator for maltreatment within five years of the guardianship start. As in the panel, I consider only substantiated allegations of maltreatment that contain an abuse start date that follows KinGAP start, where applicable. As a secondary outcome, I create an indicator for reentry to foster care within five years.

In both samples, I measure several time-invariant characteristics of the child, including race, and gender. I also measure age on the first of the month in the child-placement panel and at the guardianship start in the sample of children in KinGAP. I capture several aspects of the child's history of abuse at the time the placement or guardianship started, respectively. These include the type of maltreatment a child previously experienced, the reason for their current removal, whether they previously spent time in foster care, and the number of previous episodes of foster care.

For all primary care providers, I observe their gender. For a subset of providers, I also observe other socio-economic characteristics. These are optional fields in CWS/CMS and response patterns likely reflect the assigned social worker's tendency to report. I observe age for 71% of providers, Hispanic ethnicity for 69%, marital status for 63%, employment for 15%, and educational attainment for 14%.

4 Empirical strategy

The *California State Foster Parent Association v. Wagner, et al.* case created three plausibly exogenous changes in payment policy: 1) the announcement of a pay raise on August 30, 2010, 2) the enactment of a pay raise on May 31, 2011, and 3) the retroactive exclusion of KinGAP recipients who established guardianship before May 1, 2011, from the pay raise in perpetuity. I use a regression discontinuity design to study the effect of each policy on the safety of children in the relevant sector of foster care. I discuss these strategies in turn below.

4.1 Immediate effect of the implementation of a pay raise on safety

I estimate the immediate effect of the 2011 pay raise on the safety of children placed in county homes and relative homes. County homes and relative homes received a 21-37% raise depending on the age of the child in their care. I study this temporal shock using a sharp regression discontinuity design. In this setting, receipt of the pay raise is a deterministic function of the month of care provision. Because the raise was enacted on the last day of May 2011, I treat June 2011 as the first month of implementation. Assuming average monthly safety outcomes would have been continuous in the absence of the pay raise, I estimate the causal effect by comparing safety outcomes just before and after June 2011.

I limit the panel of child-placements by month to placements in county and relative homes during the post-announcement period from September 2010 to June 2012 (bandwidth -9 months/+13

months) and estimate the following linear probability model

$$y_{cpt} = \beta_0 + \beta_1 \mathbf{1}(m_t \geq c) + \beta_2 (m_t - c)\mathbf{1}(m_t \geq c) + \beta_3 (m_t - c)(1 - \mathbf{1}(m_t \geq c)) + \beta_4 X_{cp} + S_t + \epsilon_{cpt} \quad (1)$$

where y_{cpt} is outcome for child c in placement p and month t, m_t is the month of care provision, and c is the June 2011 threshold. I center the running variable at the threshold, $m_t - c$, and control for linear trends before and after June 2011. I control for a vector of child characteristics measured at the placement p start, X_t, to improve precision, including age, race, sex, past maltreatment, and reason for removal. In regression discontinuities that use time as the running variable, its important to include multiple years of data to demonstrate that effects are not driven by seasonal trends in outcomes. Here, I include quarter fixed effects, S_t, to proxy for seasons. β_1 captures the effect of the pay raise on the monthly likelihood of maltreatment in care at June 2011 implementation. Standard errors are clustered at the child level.

The continuity assumption would be violated if the policy induced manipulation in the number of placements, type of children placed, or type of provider matches. I formally test for changes in the composition by estimating a similar model to Equation 1, wherein I regress child and care provider characteristics on an indicator for months of care provision on or after the June 2011 threshold and control for linear trends before and after the threshold. Additionally, I test for a discontinuity in the density of child-placements at the threshold using a McCrary test.

4.2 Long-term effect of the implementation of a pay raise on safety

Next, I estimate the long-term effect of the May 1, 2011 eligibility threshold on the safety of children placed with relative guardians under KinGAP. Relatives who established legal guardianship on or after May 1, 2011, received the 21-37% pay raise. Here, the pay raise is a deterministic function of the guardianship start date and I again study the shock using a sharp regression discontinuity design. Assuming the likelihood of maltreatment within five years would have been continuous on May 1, 2011, I estimate the causal effect by comparing safety outcomes before and after May 1, 2011.

I limit the sample of guardianships to those established by relatives within 18 months of May 1, 2011 and estimate a linear probability model:

$$y_{ct} = \beta_0 + \beta_1 \mathbf{1}(d_t \geq c) + \beta_2 (d_t - c)\mathbf{1}(d_t \geq c) + \beta_3 (d_t - c)(1 - \mathbf{1}(d_t \geq c)) + \beta_4 X_c + S_t + \epsilon_{ct} \quad (2)$$

where y_{ct} is outcome for guardianship c that started on date t, d_t is the date on which the guardianship was established, and c is the May 1, 2011 eligibility threshold. As in Equation 1, I control for linear trends before and after the threshold, a vector of child characteristics measured at the guardianship start, and quarter fixed effects. In this model, β_1 is the effect of the pay raise on the likelihood a child is maltreated within five years for guardianships established on May 1, 2011. The outcome will be censored for children who enter guardianships with relatives after age 12 but will not bias our effect estimates.

As described in Section 3, I use the start date of the earliest placement with a relative guardian to proxy for the guardianship start date. This may create measure error in the running variable which could attenuate my effect estimates. To mitigate these concerns, I re-estimate my model using a "donut" regression discontinuity wherein I exclude children who started a guardianship within one month of the May 1, 2011, threshold.

Here, the continuity assumption would be violated if the policy induced manipulation in the number of guardianships established, type of children entering into those guardianships, or type of relative establishing guardianship. Importantly, the raise eligibility criteria for KinGAP was announced retroactively in September 2011. This minimizes the potential for eligibility manipulation. I formally test for changes in composition of guardianships established by re-estimating a model similar to Equation 2, wherein I regress a child and relative characteristics on an indicator for guardiansips established on or after the May 1 eligibility threshold and control for linear trends before and after the threshold. As in Section 4.1, I test for a discontinuity in the density of guardianships established at the threshold using a McCrary test.

4.3 Immediate effect of the announcement of a pay raise on safety

To test the hypothesis that higher wages create a disincentive to shirk, I estimate the immediate effect of the announcement of a pay raise for county homes in August 2010 on the safety of children placed in agency homes. In August 2010, the Ninth Circuit of Appeal's ruling affirmed that California must increase it's payments to county homes to comply with federal law. At the time, agency homes were paid more than county homes to provide specialized services and had enjoyed an increasing share of foster care placements for the decade leading up to the Ninth Circuit's ruling. Importantly, agency homes would be excluded from the court ordered pay raise. The anticipation of the pay raise for county homes was determined by the August 2010 announcement, which I study using a sharp regression discontinuity design.

My empirical strategy closely mirrors that described in Section 4.1. Here, I limit the panel of child-placements-month to placements in agency homes during the pre-implementation period from January 2009 to May 2011 (bandwidth -8 months/+9 months) and estimate the following linear probability model:

$$y_{cpt} = \beta_0 + \beta_1 \mathbf{1}(m_t \geq c) + \beta_2 (m_t - c)\mathbf{1}(m_t \geq c) + \beta_3 (m_t - c)(1 - \mathbf{1}(m_t \geq c)) + \beta_4 X_{cp} + S_t + \epsilon_{cpt}. \quad (3)$$

The key difference from Equation 1 is the threshold, defined here to be September 2010. Because the court released its ruling on the second to last day of August, I treat September 2010 as the first month of anticipation. Similar to 4.1, I cluster standard errors at the child level, estimate a model similar to Equation 3 to test for discontinuities in the composition of children and care providers at the September 2010 threshold, and use a McCrary test to test for a discontinuity in the density of child-placements established at the threshold.

5 Results

5.1 Foster care labor market 2001-2011

Before estimating the impact of the court case, I investigate foster care market dynamics leading up to the 2011 raise and observe three patterns. First, the demand for foster care placements was in decline for the decade preceding the 2011 raise. Figure 3a illustrates the declining stock of child-placements that begins in 2001 and accelerates in 2007. Between January 2001 and September 2010, the stock of placements supervised by the 58 Child Welfare Departments had decreased by 39%[19]. Second, agency homes enjoyed a growing market share of foster care placements through the September 2010 announcement. Figure 3b tracks the composition of child-placements by placement type over time. In the decade before the 2010 announcement, the share of child-placements in agency homes grew consistently, while that of relative homes oscillated and county homes declined until 2007 and then plateaued. Third, placements in both relative homes and agency homes reach an inflection point in 2010. The share of child-placements in agency homes begins to decline, while the share of child-placements in relative homes begins to grow.

5.2 Assumption of continuity

The assumption of continuity has three empirically testable implications. First, placements in foster care and guardianships should not be able to manipulate the policy thresholds. For foster care placements, an incentive to manipulate is unlikely as receipt of monthly payments in the pre-policy does not preclude one from receiving receipt of monthly payments after the raise took effect. For guardianships, manipulation is unlikely because the policy was announced retroactively. I formally test for a discontinuity in the density functions of the running variables using the method originally developed by McCrary (2008) and adapted by Cattaneo et al. (2020). I find no evidence of manipulation of county and relative home placements in June 2011 (p-value = 0.5707), KinGAP guardianships starting in May 2011 (p-value = 0.5053), or agency home placements in September 2010 (p-value = 0.8873). Figures 4a-c illustrate the sample size by month for each of the three running variables.

Second, there should be no simultaneous policies affecting placements in foster care and guardianships at the three policy thresholds: September 2010 announcement, June 2011 implementation, and May 1, 2011, guardianship start. Aside from monthly payments, the most salient policy affecting placements in county homes, agency homes, and relative homes are monthly in-home monitoring visits from a social worker. Guardianships are exempt from these monitoring requirements and often have little contact with the child welfare system outside of KinGAP. I re-estimate Equations 1 and 3 with in-home social worker visits as the dependent variable. There are no economically and statistically significant discontinuities in monitoring intensity at the time of the announcement or implementation of the pay raise. I find a one percentage point increase in the monthly likelihood of an in-home social worker visit for county homes and relative homes in June 2011 and a 0.6

[19] Comparing the number of child-placements in January 2001 (77,400) to September 2011 (49,133).

percentage point reduction for agency homes in September 2010. Figures 5a and 5c illustrate the share of child-placements who received a in-home visit by a social worker overtime.

Last, covariates that are related to safety outcomes should not be affected by the policy. To test whether child and care provider characteristics are balanced across the thresholds, I re-estimate Equations 1-3 omitting the vector of child characteristics and quarter fixed effects as independent variables and replacing the dependent variable with a characteristic of interest. Tables A1-A4 demonstrate no economically and statistically significant imbalances in the types of children and care providers at the thresholds. Over the 60 tests presented, three differences (one in 20) are expected to be statistically significant by chance using an $\alpha = 0.05$ significance level. I find six significant differences at the 5% level, but the magnitudes of these differences are not economically meaningful and the differences are concentrated in the largest sample that is most powered to detect small differences. Among agency homes, I detect less than 1 percentage point changes in the likelihood a care provider is Hispanic, a child is age 0-4 years, a child is age 15-17 years, and a child is black, as well as a less than 1 month change in the time since a child was first alleged a victim of maltreatment. Figures A1a-e demonstrate that these small differences are driven by non-linear trends away from the threshold, rather than a discontinuity at the threshold. Further, I demonstrate in Sections 5.3 and 5.5 that the main findings are robust to the exclusion of the vector of child characteristics (X_c) from Equations 1-3.

5.3 Immediate effect of the implementation of a pay raise on safety

In the period before the June 2011 implementation, 0.3% of children placed in county and relative homes were maltreated monthly. Here, each substantiated allegation of maltreatment is assigned to the month in which it was reported to CDSS. Both assigning maltreatment to a single period and conditioning on reporting will underestimate the true prevalence of maltreatment in care.

I find no evidence that the implementation of 21-37% higher monthly payments in June 2011 immediately affected the likelihood of maltreatment in county homes and relative homes (see Table 2, Panel B, Column 2). I measure a small positive effect (0.0004) that is equivalent to a 11% increase, but is not statistically significant at conventional levels (SE 0.0005). This null result is robust to the exclusion of child characteristics from Equation 1 (see Table 2, Panel B, Column 1)), restriction to county homes (Table A5, Panel B, Column 2), and restriction to relative homes (Table A5, Panel B, Column 4).

Figures 6a-b display the share of child-placements that were maltreated across the June 2011 threshold, as well as the residualized share that accounts for child characteristics and seasonality. While I do not observe a discontinuity in maltreatment levels in June 2011, I do observe a kink in maltreatment trends. Between the announcement of the raise in September 2010 and its implementation in June 2011, maltreatment trended downward. After June 2011, maltreatment trended upward. The short time window between the federal court order on May 27th and the enactment of new rates on May 31st could have delayed the delivery of higher payments to foster families and

relatives in unprepared counties[20]. If implementation was delayed in some counties, part of this kink could be attributed to an immediate response to receiving higher pay that is not captured at the June 2011 threshold. Alternatively, this kink could be explained by a response to accumulating exposure to the pay raise; in every month past June 2011, the average child in care has spent a larger portion of their placement under the the new payment levels. However, I am unable to disentangle either response from time trends that would have been observed in the absence of the pay raise.

5.4 Long-term effect of the implementation of a pay raise on safety

Among the children who entered a guardianship with a relative before the May 1, 2011, 15% were maltreated within five years of the guardianship start and 26% reentered foster care. On a monthly basis, this is comparable to the risk faced by children placed in county homes and relative homes[21].

The implementation of a pay raise for guardianships established on or after May 1, 2011, decreased the likelihood of maltreatment within five years by 10 percentage points (SE 0.0266). This is a equivalent to a 68% reduction in risk and is statistically significant at the 1% level. Further, the pay raise decreased the likelihood of reentry to foster care by 9 percentage points (SE 0.0342) or 36% for guardianships established just after May 1st. These results are robust to a "donut" specification that omits guardianships with an estimated start date within one month of the May 1, 2011, eligibility criteria to address potential measurement error (see Table A6).

The impact of the pay raise on the safety of children placed with relative guardians just after May 1st is consistent with change in effort or investments on the intensive margin. Multiple factors minimize the possibility for higher quality care providers to select into guardianship just after the May 1st eligibility threshold. First, the pay raise was implemented on May 31st, 2011, and the May 1st eligibility date was determined retroactively. Second, becoming a guardian is a multi-month process that involves seeking legal assistance, giving notice to and seeking consent from all relevant parties, interviewing with a court investigator, and passing a formal home study, before a court hearing can be scheduled. Those becoming guardians immediately following May 1, 2011, had started the process to become a legal guardian months earlier. Indeed, Section 5.2 demonstrates no statistically significant and economically meaningful evidence of care provider selection on observable characteristics at the May 1st threshold.

Relative care providers often face resource limitations and the 21-37% increase in monthly KinGAP payments could have a substantial income effect. In a national survey, relatives tended to be single, older, less educated, in poor health, low-income, and unemployed compared to other care providers. They also received fewer support services (Sakai et al., 2011). Although CWS/CMS only contains partial information on these characteristics of care providers, I find a similar pattern in my sample (see Table A1, Panels B and C). A larger budget constraint could directly benefit foster

[20] While CDSS program managers do not recall specific implementation delays in June 2011, they confirm that this is often the case with pay policies that are implemented on short notice.

[21] Conservatively assuming each child is maltreated at most once within five years, 0.1508/(12*5)=0.0025 or 0.25% of children are maltreated per month within the first five years in a guardianship with a relative.

youth through investments in material resources or indirectly by alleviating temporal, cognitive, and emotional resource constraints. This is plausible for relative guardians in particular because KinGAP provides a consistent and predictable source of supplemental income through the child's 18th birthday.

5.5 Immediate effect of the announcement of a pay raise on safety

In the period before the September 2010 announcement, 0.5% of children placed in agency homes were maltreated monthly. As discussed above, these estimates will underestimate the true experience of maltreatment in care due to measurement limitations.

In September 2010, the announcement of a large and imminent raise for county homes immediately decreased the monthly likelihood of maltreatment in agency homes by 0.18 percentage points (see Table 2, Panel A, Column 4), equivalent to a 34% reduction. This is statistically significant at the 1% level and robust to the exclusion of child characteristics from Equation 3 (see Table 2, Panel A, Column 3).

Figure 6d illustrates the impact of the announcement on safety in agency homes by mimicking Equation 3. I plot means of residualized outcomes and overlay linear trends on either side of the September 2010 threshold. The magnitude of the discontinuity is represented by the distance between the intersection of each linear trend and the vertical line marking the September 2010 announcement. Figures 6c-d show maltreatment in agency home placements trended upward before the announcement of the raise in September 2010 and follows the same trend thereafter.

The immediacy of the impact of the announcement on the safety of children residing in agency homes is consistent with quality improvement on the intensive margin. Similar to relative guardians, prospective foster families must undergo a multi-step licensing process that includes an orientation, application, home study, first aid certification, and psycho-social assessment. This greatly diminishes the possibility for higher quality providers to select into agencies and have children placed in their homes immediately after the announcement. To isolate the effect on the intensive margin, I further restrict the panel to children placed in agency homes with providers who had cared for foster children before September 2010 and re-estimate Equation 3. The findings are remarkably robust to the main sample; the September 2010 announcement of a raise for county homes reduced the likelihood of maltreatment in agency homes with pre-existing care providers by 32% (A7, Panel A, Column 4).

This response is consistent with anticipated labor market competition creating a disincentive for agency homes to shirk. In the decade prior to the announcement, agency homes enjoyed a growing market share despite falling demand and relative and county homes being prioritized for placements (see Section 5.1). This suggests relatives and county homes were less willing to supply their labor amid the real pay decline and stagnation that characterized the period from 2001 to 2010 (see Figure 2). Although agency homes also faced pay decline and stagnation, their payment level for a single child was substantially higher than that for county homes and plausibly allowed them to absorb the increasing unmet demand. Forward looking agency homes would expect a pay

raise for relatives and county homes to reverse these trends. Relatives and county homes would be more willing to supply their labor, thereby reducing the demand for agency homes and heightening competition for placements among them.

6 Conclusion

In this paper, I provide the first evidence of how foster families and relatives respond to an increase in monthly foster care payments. To study this, I leverage the largest increase in payment levels in California's history and two decades of administrative data. My findings suggest care providers increased their effort or investment in the children placed in their care, which resulted in safer stays in foster care. Specifically, I find receipt of a 21-37% increase in pay reduced the risk of maltreatment within five years of a relative establishing guardianship by 68%. In response to the announcement of an impending raise for county and relative homes, children placed in agency homes also benefited. The announcement reduced the likelihood that a child would be maltreated while placed in an agency home by 34%. These changes cannot be explained by a change in turnover, care provider selection, monitoring, or the quantity or composition of children in care. Rather, the observed effect on guardianships is consistent with the alleviation of resource constraints and the observed effect on agency homes is consistent with anticipated competition that disincentivizes low quality care.

This paper also documents cost savings associated with higher foster care payments. Among children placed with a relative guardian, the reduction in risk of maltreatment is accompanied by a similar magnitude of reduction in risk of foster care reentry. The receipt of higher pay reduced the risk of reentry to foster care within five years by 9.43 percentage points. This yields $1,327 in savings on average per child over five years by avoiding more expensive placements (i.e., agency homes and group homes) upon reentry. The estimated savings do not capture those generated by reducing the long term consequences of child maltreatment, including but not limited to unemployment, criminal activity, and poor health (Currie and Widom, 2010; Currie and Tekin, 2012; Widom, 1989).

Our understanding of foster care quality is limited by the lack of process measurements and infrequent observation by the social worker. In this paper, I measure quality outcomes in terms of safety in care. Although maltreatment in care is a rare outcome, it is frequently assessed due to mandated reporting laws that require professionals beyond the social worker (e.g., teachers, clinicians) to actively monitor the safety of children. However, evidence of maltreatment in care only captures the lower tail of the distribution of foster care quality. Improvements in safety may by symptomatic of a broader shift in quality or reflect a local change among the lowest performers. In supplemental analyses, I find no statistically or economically significant effect of the raise or its announcement on medical or dental exams received by children placed with foster families or relatives (See Appendix Table A8). However, I detect a negative impact on the likelihood a child had contact with their biological family that could indicate either better care provider judgement over risk exposure or reduced care provider effort. At present, child welfare data systems lack

sufficient quality measurements to understand changes in quality beyond the lowest performers.

This paper shows foster care payments can be an effective tool to keep children in foster care safe. Higher pay is logistically simple policy lever that may yield substantial cost savings by avoiding the most expensive placements while keeping children in home-like environments that maintain family ties where possible. Understanding other ways to alleviate resource constraints and maintain a large supply of foster families are important areas for future research.

Figure 1: Timeline of California State Foster Parent Association, et al. v. Wagner, et al.

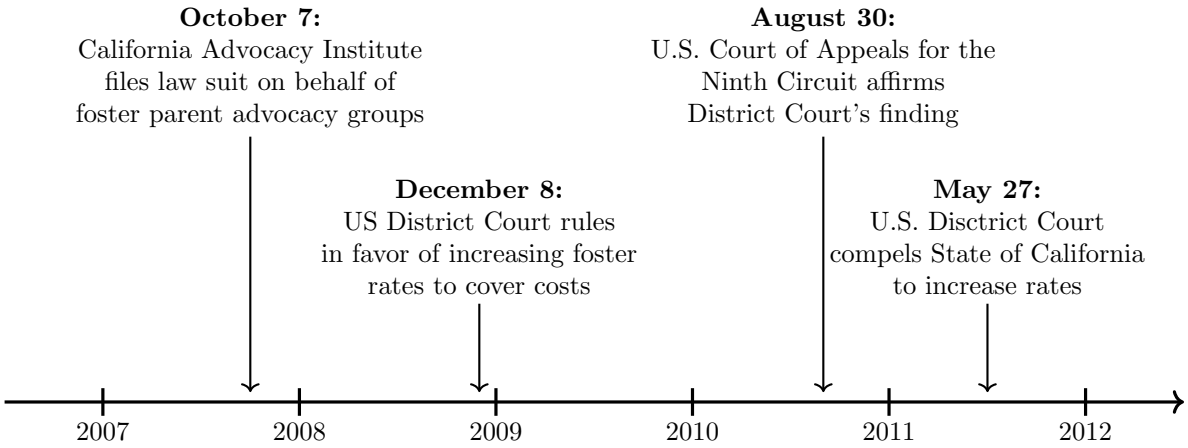
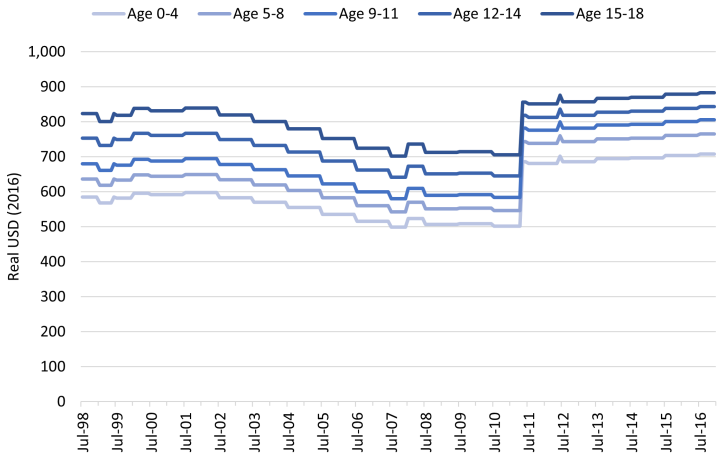


Table 1: Payment policy change by placement type

Placement type	Policy change
Temporary	
Relative homes	21-37% increase in monthly payments for care provided in May 2011 and thereafter
County homes	21-37% increase in monthly payments for care provided in May 2011 and thereafter
Agency homes	No change
Permanent	
Relative guardians	21-37% increase in monthly payments through age 18 for guardianships established May 1, 2011 and thereafter

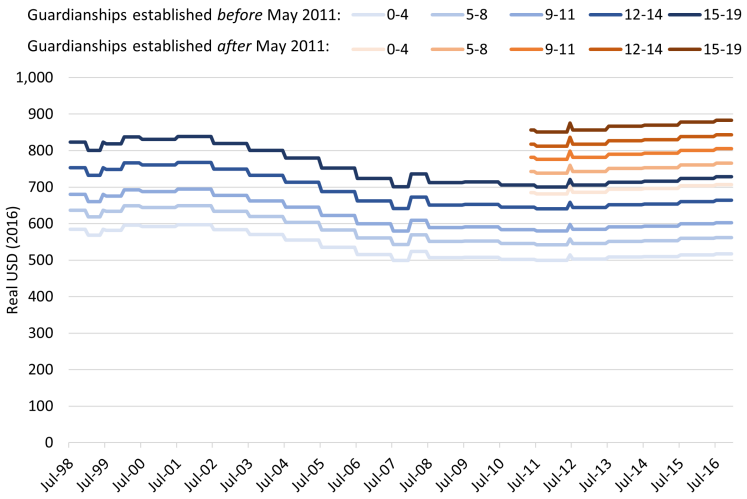
Figure 2: Substitute care providers' monthly compensation per child in their care

(a) County and relative homes



Note: Exceptions for San Diego County through 1999, Santa Mateo County through 2001, Los Angeles County through 2011, Orange County through 2011, Santa Clara County through 2011, and and Marin County through 2011. Source: All County Letters 1998-2016.

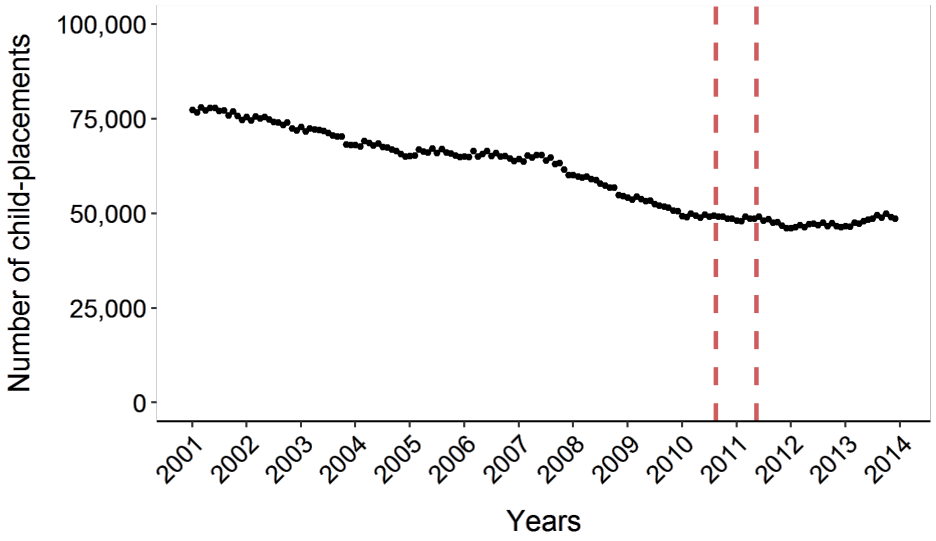
(b) Relatives who become legal guardians



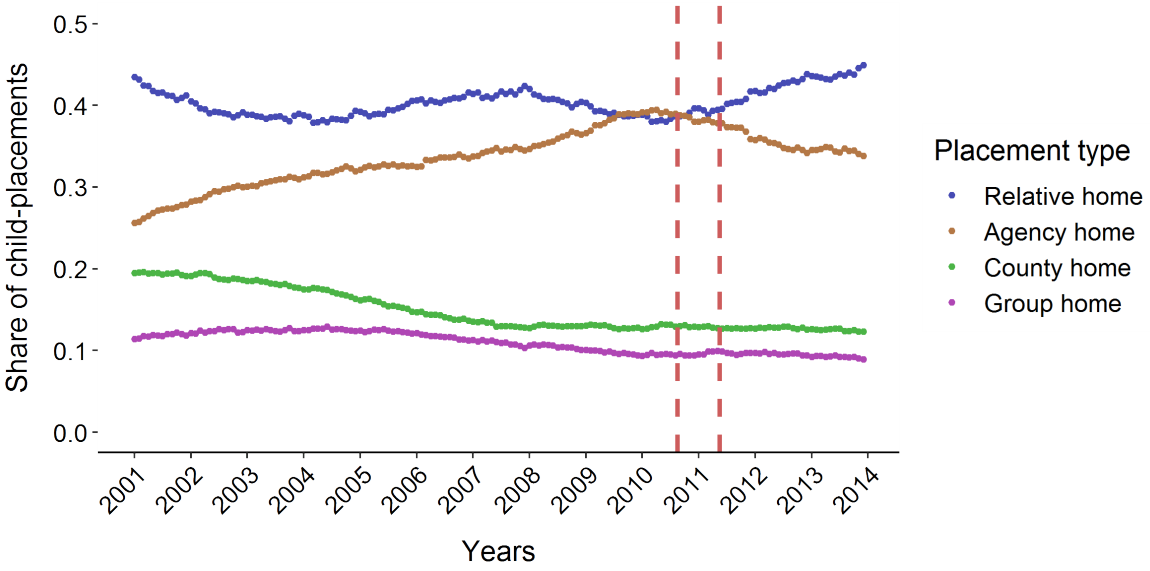
Source: All County Letters 1998-2016.

Figure 3: Child-placements in foster care by month and placement type

(a) Number of child-placements in foster care by month of care



(b) Placement type as a share of child-placements by month of care

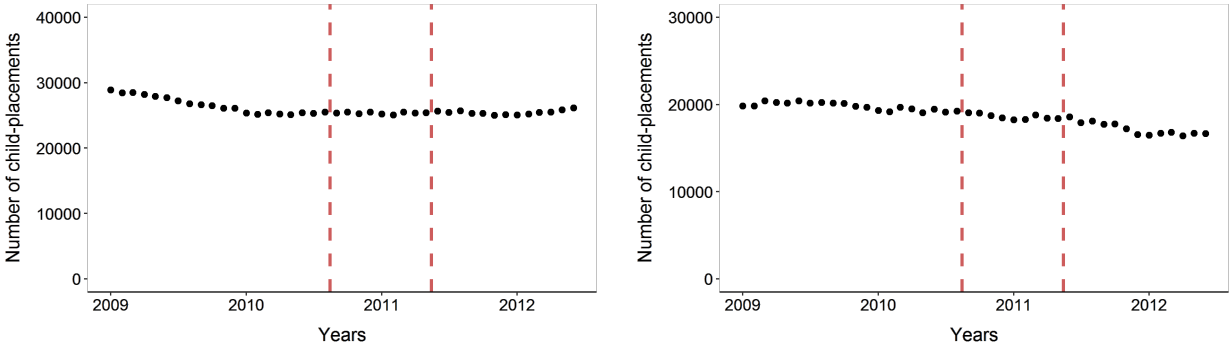


Note: Vertical lines reflect the August 30, 2010 announcement and May 31, 2011 implementation. All active placements are reflected in monthly counts in Panel A and the denominator in Panel B, including new, exiting, and continuing placements.

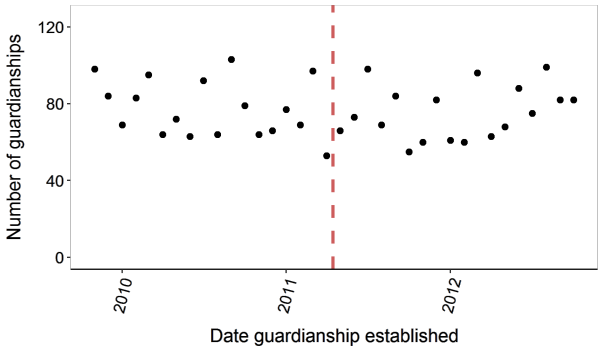
Figure 4: Sample size

(a) Number of child-placements in county and relative homes by month of care

(b) Number of child-placements in agency homes by month of care



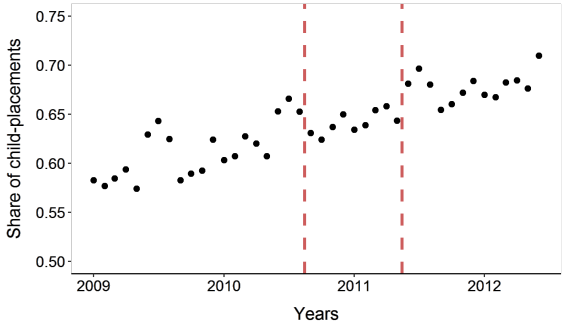
(c) Number of relative guardianships by start month



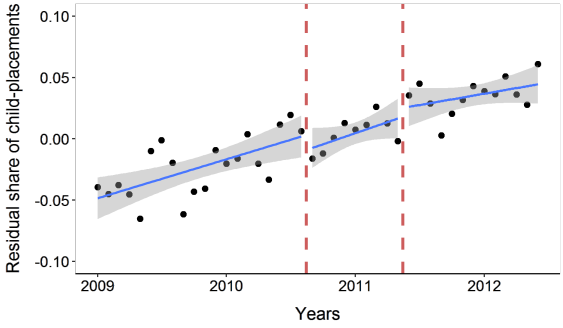
Note: Vertical lines reflect the August 30, 2010 announcement and May 31, 2011 implementation. In Panels A and B, all active placements are reflected in monthly counts including new, exiting, and continuing placements.

Figure 5: In-home visit by social worker by month

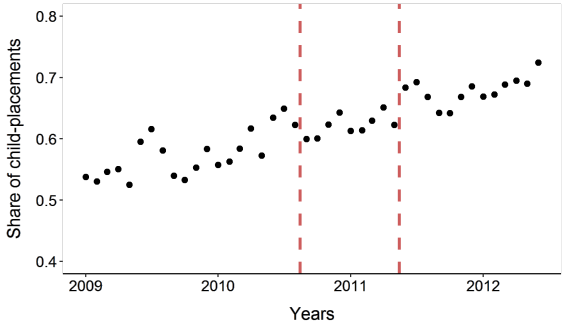
(a) Share of child-placements in county and relative homes



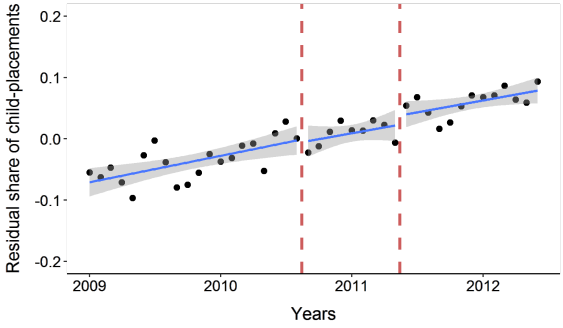
(b) Residualized share of child-placements in county and relative homes



(c) Share of child-placements in agency homes



(d) Residualized share of child-placements agency homes



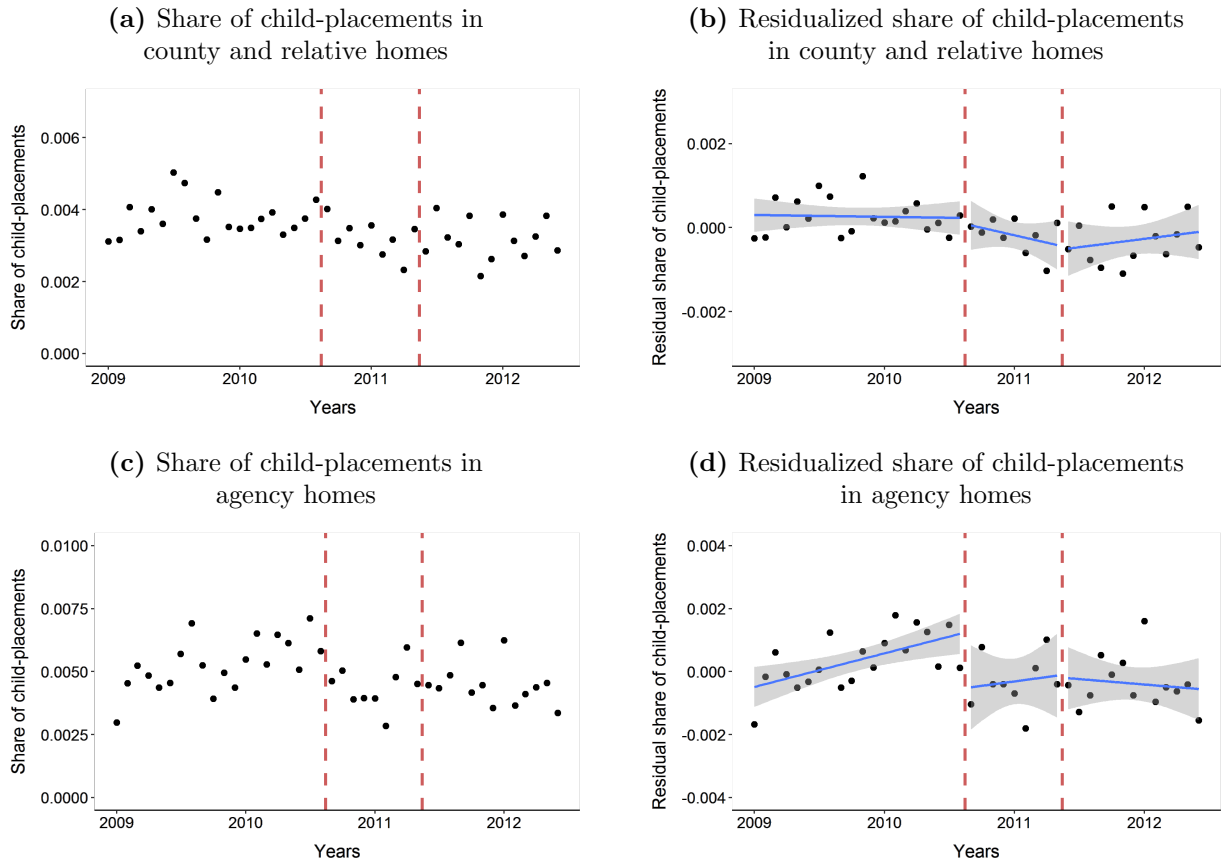
Note: Vertical lines reflect the August 30, 2010 announcement and May 31, 2011 implementation.

Table 2: Effect of announcement and implementation of a pay raise on maltreatment in foster care using regression discontinuity design

	Maltreatment in care			
	(1)	(2)	(3)	(4)
Panel A: Announcement				
September 2010	-0.0003	-0.0003	-0.0022***	-0.0018***
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
N	757,024	757,024	560,146	560,146
Pre-policy mean	0.0038	0.0038	0.0053	0.0053
Relative effect	-7%	-7%	-41%	-34&
Bandwidth	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)
X_c	No	Yes	No	Yes
Panel B: Implementation				
June 2011	0.0005	0.0004	0.0003	-0.0001
	(0.0003)	(0.0005)	(0.0005)	(0.0007)
N	556,837	556,837	388,854	388,854
Pre-policy mean	0.0032	0.0032	0.0044	0.0044
Relative effect	15%	11%	0.6%	-2%
Bandwidth	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)
X_c	No	Yes	No	Yes
Sample	County and relative homes	County and relative homes	Agency homes	Agency homes

Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client-placement-month. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates in Panels (a) and (b) were obtained using Equations 1 and 3, respectively. Columns (2) and (3) omit the vector of child characteristics (X_c). Maltreatment in care refers to substantiated allegations received during a placement. If an estimated abuse start date is provided, it must fall after the placement and guardianship start, respectively.

Figure 6: Maltreatment in care



Note: Vertical lines reflect the August 30, 2010 announcement and May 31, 2011 implementation.

Table 3: Effect of KinGAP pay raise on maltreatment and reentry to foster care using regression discontinuity design

	Maltreatment within five years (1)	Reentry within five years (2)
May 1, 2011	-0.1026***	-0.0943***
	(0.0266)	(0.0342)
Sample	2,753	2,753
Pre-policy mean	0.1508	0.2586
Relative effect	-68%	-36%
Bandwidth	+/- 18 months (Nov. 2009 - Oct. 2012)	+/- 18 months (Nov. 2009 - Oct. 2012)

Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates were obtained using Equation 2. Maltreatment within five years refers to substantiated allegations received within five years of the guardianship start date and for which the abuse started after the guardianship start. Reentry within five years of exit refers to a new episode care that begins within five years of the guardianship start date.

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Appendix

Table A1: Balance test for providers socio-demographic characteristics using regression discontinuity design

	Female	Hispanic	Age in years	Unemployed	High school education or less	Married	First placement
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: County and relative homes							
June 2011	0.0019	0.0032	0.0152	0.0055	-0.0063	0.0028	0.0036
	(0.0018)	(0.0025)	(0.0642)	(0.0045)	(0.0049)	(0.0027)	(0.0024)
Sample	555,563	448,287	453,477	145,542	129,200	387,789	556,837
Pre-policy mean	0.8281	0.3105	38.9933	0.1064	0.1055	0.4185	0.6046
Relative effect	<1%	1%	<1%	5%	-6%	1%	1%
Bandwidth	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012
Panel B: KinGAP							
May 1, 2011	-0.0149	-0.0352	1.7531***	-0.0283	-0.0609	-0.0176	NA
	(0.0146)	(0.0245)	(0.6261)	(0.0477)	(0.0504)	(0.0236)	
Sample	2,753	2,753	2,753	2,753	2,753	2,753	
Pre-policy mean	0.8359	0.4955	48.8159	0.4956	0.5279	0.6859	
Relative effect	-2%	-7%	4%	-6%	-12%	-3%	
Bandwidth	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	
Panel C: Agency homes							
September 2010	0.0008	-0.0074**	-0.0727	0.0012	-0.001	-0.0021	-0.002
	(0.0021)	(0.0036)	(0.0781)	(0.008)	(0.008)	(0.0033)	(0.0025)
Sample	560,079	402,271	409,875	76,080	82,586	422,181	560,146
Pre-policy mean	0.8711	0.324	34.1142	0.0534	0.0639	0.531	0.1858
Relative effect	<1%	-2%	<1%	2%	-2%	<1%	-1%
Bandwidth	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011

Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client-placement-month in Panels A and C and the client in Panel B. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates were obtained using Equation 1-3.

Table A2: Balance test for child age group using regression discontinuity design

	Age 0-4 (1)	Age 5-8 (2)	Age 9-11 (3)	Age 12-14 (4)	Age 15-17 (5)
Panel A: County and relative homes					
June 2011	-0.0014	-0.0011	-0.0006	0.0000	0.0029*
	(0.0023)	(0.0020)	(0.0016)	(0.0016)	(0.0016)
Sample	556,837	556,837	556,837	556,837	556,837
Pre-policy mean	0.4259	0.197	0.1138	0.1166	0.1427
Relative effect	<1%	-1%	-1%	<1%	2%
Bandwidth	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012
Panel B: KinGAP					
May 1, 2011	0.0106	-0.0191	0.0243	-0.0179	0.0021
	(0.0350)	(0.0318)	(0.0302)	(0.0305)	(0.0284)
Sample	2,753	2,753	2,753	2,753	2,753
Pre-policy mean	0.2730	0.2205	0.1753	0.1846	0.1466
Relative effect	4%	-9%	14%	-10%	1%
Bandwidth	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012
Panel C: Agency homes					
September 2010	0.0072**	0.0001	-0.0007	-0.0001	-0.0067***
	(0.0029)	(0.0026)	(0.0022)	(0.0023)	(0.0025)
Sample	560,146	560,146	560,146	560,146	560,146
Pre-policy mean	0.2985	0.1843	0.1337	0.1651	0.2155
Relative effect	2%	<1%	-1%	<1%	-3%
Bandwidth	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011

Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client-placement-month. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates were obtained using Equations 1-3.

Table A3: Balance test for child race/ethnicity using regression discontinuity design

	White	Black	Latin	Asian	Native American
	(1)	(2)	(3)	(4)	(5)
Panel A: County and relative homes					
June 2011	0.0001	-0.0030*	0.0029	-0.0003	0.0003
	(0.0019)	(0.0017)	(0.0022)	(0.0007)	(0.0005)
Sample	556,837	556,837	556,837	556,837	556,837
Pre-policy mean	0.2456	0.2262	0.4901	0.0265	0.0116
Relative effect	<1%	-1%	1%	-1%	2%
Bandwidth	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012
Panel B: KinGAP					
May 1, 2011	0.0155	0.0345	-0.0358	Omitted	Omitted
	(0.0307)	(0.0338)	(0.0393)		
Sample	2,753	2,753	2,753	-	-
Pre-policy mean	0.1649	0.2480	0.5568	-	-
Relative effect	9%	14%	-6%	-	-
Bandwidth	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012	-	-
Panel C: Agency homes					
September 2010	0.0042	-0.0074***	0.0048*	-0.0011	-0.0005
	(0.0025)	(0.0024)	(0.0029)	(0.0009)	(0.0007)
Sample	560,146	560,146	560,146	560,146	560,146
Pre-policy mean	0.2414	0.2202	0.4988	0.0268	0.0128
Relative effect	2%	-3%	1%	-4%	-4%
Bandwidth	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011

Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client-placement-month. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates were obtained using Equations 1-3. Models in which than less than 50 instances of the outcome were observed are suppressed to protect personally identifying information.

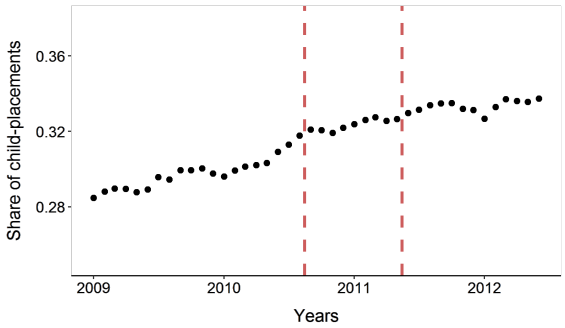
Table A4: Balance test for child gender and previous child welfare experience using regression discontinuity design

	Female (1)	Previous stay in foster care (2)	Episode count (3)	Years since first allegation (4)
Panel A: County and relative homes				
June 2011	0.0000 (0.0022)	0.0015 (0.0018)	0.0016 (0.0027)	0.0356* (0.0199)
Sample	556,837	556,837	556,837	556,837
Pre-policy mean	0.5005	0.227	1.2969	4.7573
Relative effect	<1%	1%	<1%	1%
Bandwidth	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012
Panel B: KinGAP				
May 1, 2011	0.0648* (0.0392)	0.0059 (0.0279)	N/A	0.3344 (0.3726)
Sample	2,753	2,753		2,753
Pre-policy mean	0.5072	0.8750		4.4365
Relative effect	13%	1%		8%
Bandwidth	Nov. 2009 - Oct. 2012	Nov. 2009 - Oct. 2012		Nov. 2009 - Oct. 2012
Panel C: Agency homes				
September 2010	0.0016 (0.0029)	-0.0020 (0.0026)	0.0015 (0.0042)	-0.0765*** (0.0282)
Sample	560,146	560,146	560,146	560,146
Pre-policy mean	0.5127	0.2962	1.408	5.8955
Relative effect	<1%	-1%	<1%	-1%
Bandwidth	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011	Jan. 2009 - May 2011

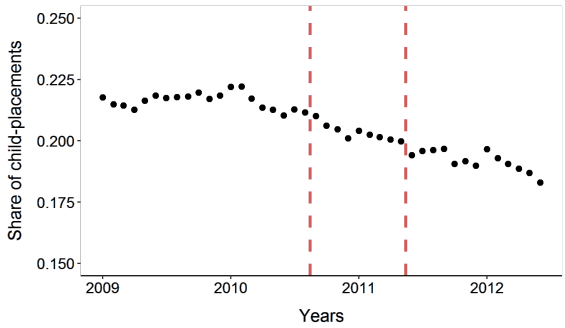
Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client-placement-month. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates were obtained using Equations 1-3. Episode count is not applicable to KinGAP.

Figure A1: Select child and care provider characteristics by month, among placements in agency home

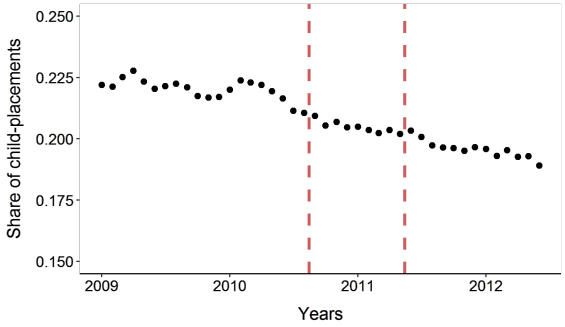
(a) Child age 0-4 years



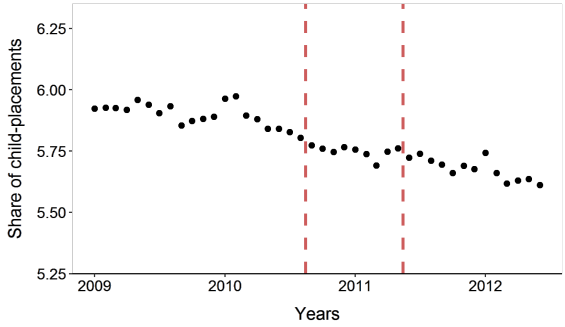
(b) Child age 15-17 years



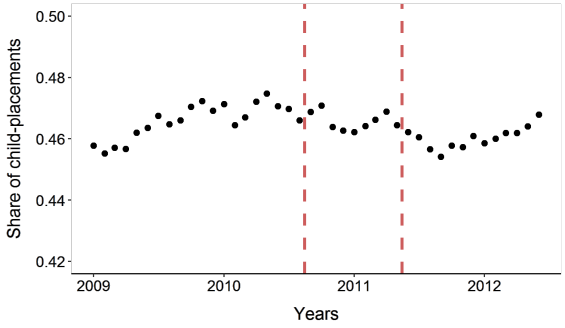
(c) Child's race is Black



(d) Years since first allegation



(e) Care provider's ethnicity is Hispanic



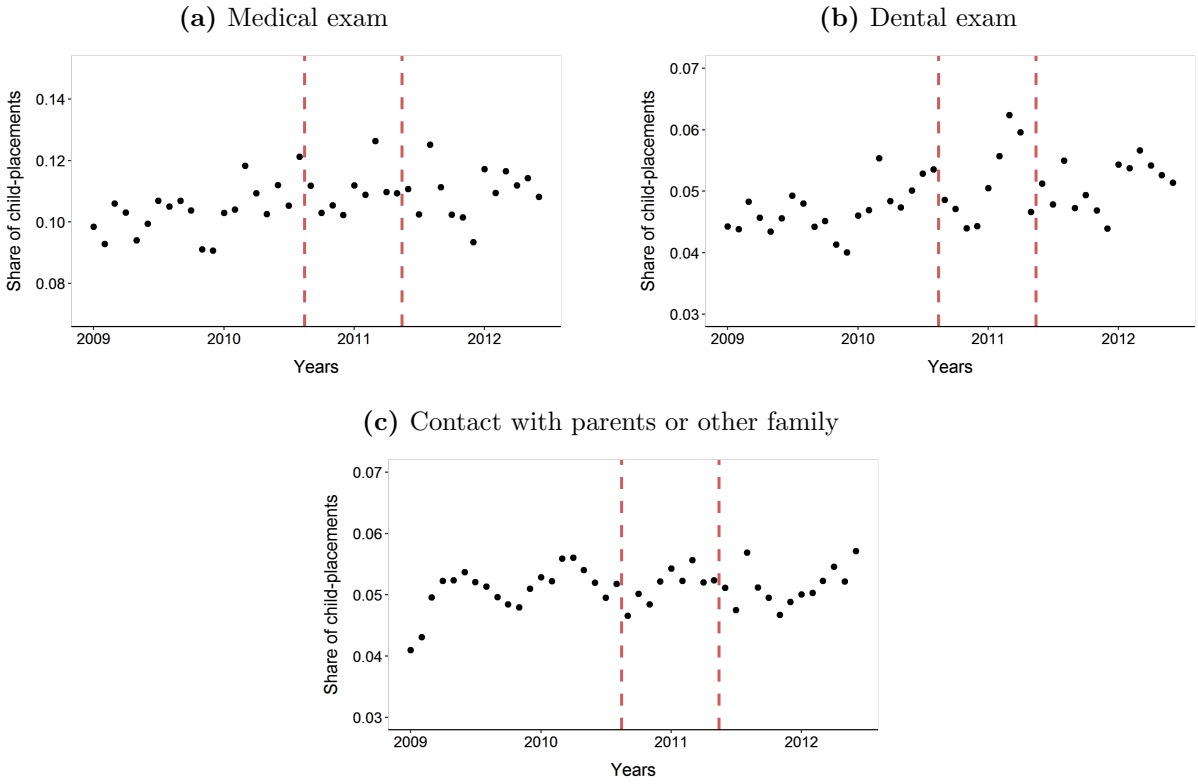
Note: Vertical lines reflect the August 30, 2010 announcement and May 31, 2011 implementation.

Table A5: Effect of announcement and implementation of a pay raise on maltreatment in foster care using regression discontinuity design, among county homes and relative homes

	Maltreatment in care			
	(1)	(2)	(3)	(4)
Panel A: Announcement				
September 2010	0.0002	0.0002	-0.0004	-0.0004
	(0.0007)	(0.0007)	(0.0004)	(0.0004)
N	188,695	188,695	568,329	568,329
Pre-policy mean	0.0044	0.0044	0.0036	0.0036
Relative effect	5%	5%	-12%	-12&
Bandwidth	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)
X_c	No	Yes	No	Yes
Panel B: Implementation				
June 2011	0.0006	0.0006	0.0003	0.0003
	(0.0011)	(0.0011)	(0.0005)	(0.0005)
N	134,481	134,481	422,356	422,356
Pre-policy mean	0.0042	0.0042	0.0029	0.0029
Relative effect	14%	15%	10%	10%
Bandwidth	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)
X_c	No	Yes	No	Yes
Sample	County homes	County homes	Relative homes	Relative homes

Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client-placement-month. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates in Panels (a) and (b) were obtained using Equations 1 and 3, respectively. Columns (2) and (3) omit the vector of child characteristics (X_c). Maltreatment in care refers to substantiated allegations received during a placement. If an estimated abuse start date is provided, it must fall after the placement and guardianship start, respectively.

Figure A2: In-care services for placements in county homes and relative homes by month



Note: Vertical lines reflect the August 30, 2010 announcement and May 31, 2011 implementation.

Table A6: Effect of KinGAP pay raise on maltreatment and reentry to foster care using donut regression discontinuity design

	Maltreatment within five years (1)	Reentry within five years (2)
May 1, 2011	-0.1229***	-0.0970**
	(0.0301)	(0.0382)
Sample	2,634	2,634
Pre-policy mean	0.1538	0.2614
Relative effect	-80%	-37%
Bandwidth	+/- 18 months (Nov. 2009 - Oct. 2012)	+/- 18 months (Nov. 2009 - Oct. 2012)

Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates were obtained using Equation 2. Observations within one month of the May 1, 2011, threshold are excluded to estimate a "donut" regression discontinuity. Maltreatment within five years refers to substantiated allegations received within five years of the guardianship start date and for which the abuse started after the guardianship start. Reentry within five years of exit refers to a new episode care that begins within five years of the guardianship start date.

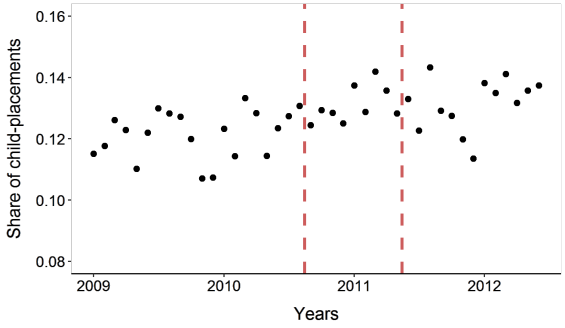
Table A7: Effect of announcement and implementation of a pay raise on maltreatment in foster care using regression discontinuity design, among providers who cared for foster children before September 2010

	Maltreatment in care			
	(1)	(2)	(3)	(4)
Panel A: Announcement				
September 2010	-0.0005*	-0.0005	-0.0020***	-0.0017***
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
N	705,111	705,111	544,457	544,457
Pre-policy mean	0.0038	0.0038	0.0053	0.0053
Relative effect	-14%	-14%	-38%	-32%
Bandwidth	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)	Pre-implementation (Jan. 2009 - May 2011)
X_c	No	Yes	No	Yes
Panel B: Implementation				
June 2011	0.0003	-0.0002	0.0001	0.0002
	(0.0004)	(0.0005)	(0.0005)	(0.0007)
N	323,381	323,381	313,949	313,949
Pre-policy mean	0.0032	0.0032	0.0042	0.0042
Relative effect	10%	-6%	1%	6%
Bandwidth	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)	Post-announcement (Sept. 2010 - June 2012)
X_c	No	Yes	No	Yes
Sample	County and relative homes	County and relative homes	Agency homes	Agency homes

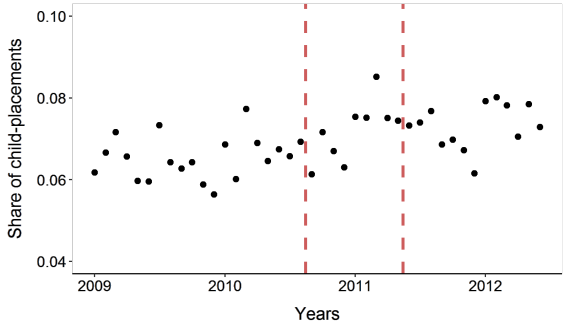
Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client-placement-month. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates in Panels (a) and (b) were obtained using Equations 1 and 3, respectively. Columns (2) and (3) omit the vector of child characteristics (X_c). Maltreatment in care refers to substantiated allegations received during a placement. If an estimated abuse start date is provided, it must fall after the placement and guardianship start, respectively.

Figure A3: In-care services for agency homes by month

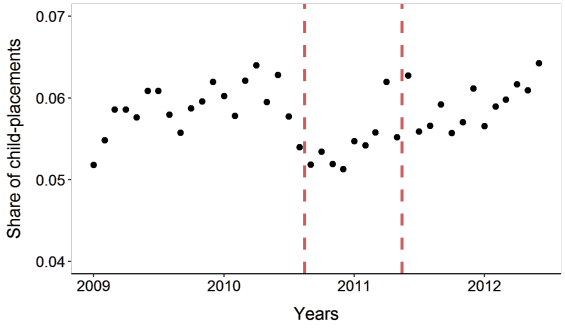
(a) Medical exam



(b) Dental exam



(c) Contact with parents or other family



Note: Vertical lines reflect the August 30, 2010 announcement and May 31, 2011 implementation.

Table A8: Effect of announcement and implementation of a pay raise on services in foster care using regression discontinuity design

	In-home visit by social worker (1)	Physical exam (2)	Dental exam (3)	Contact with family (4)
Panel A: County and relative homes				
June 2011	0.0182***	-0.0011	-0.0025	-0.0042***
	(0.0035)	(0.0025)	(0.0018)	(0.0014)
Bandwidth	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012
Sample	556,837	556,837	556,837	556,837
Pre-policy mean	0.6412	0.1098	0.0510	0.0516
Relative effect	3%	-1%	-5%	-8%
Panel B: Agency homes				
September 2010	-0.0068**	0.0039**	-0.0014	-0.0095***
	(0.0031)	(0.0019)	(0.0014)	(0.0018)
Bandwidth	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012	Sept. 2010 - June 2012
Sample	560,146	560,146	560,146	560,146
Pre-policy mean	0.5743	0.1215	0.0654	0.0588
Relative effect	-1%	3%	-2%	-16%

Note: Standard errors in parentheses. * denotes significant at the 10% level, ** denotes significant at the 5% level, and *** denotes significant at the 1% level. Unit of observation is the client-placement-month. Sample includes all clients age 0-17 and placements longer than 1 day that were supervised by the Child Welfare Department in California. Regression discontinuity estimates were obtained using Equation 3.

A1 KinGAP eligibility

In the State of California, All of the following criteria must be met in order to be eligible for the Kin-GAP program:

- The child is a dependent or ward of the juvenile court.
- The child has been placed in the approved home of the prospective relative guardian for at least six consecutive months while under the jurisdiction of the juvenile court, probation department, Title IV-E agreement tribal agency or a voluntary placement agreement.
- The relative guardian has entered into a written binding agreement with the county welfare agency, probation department, or Title IV-E agreement tribal agency prior to the establishment of the guardianship.
- The juvenile court has ordered legal guardianship and appointed a legal guardian.
- Dependency or wardship is terminated.

A2 Cost saving

The average age of a child who entered a guardianship with a relative in May 2011 was 9.38 years. In the five years following May 2011, the total monthly rate to agencies exceeded that to county homes and relatives by $939 (2011 USD) and monthly placements in agency homes averaged 34.81%. Meanwhile, the total monthly rate to group homes exceeded that to county homes and relatives by at least $2,118 - $695 = $1,423 (2011 USD) and monthly placements in group homes averaged 10%.

I assume a uniform distribution of reentry over five years, so the average child would reenter foster care two and half years from the guardianship start date. Assuming failed reunification with biological parents, I assume the remaining two and a half years (or 30 months) would be spent in foster care.

Thus expected cost savings are:

$$2.5 \text{ years} * 0.5 * 12 \text{ months per year} * 0.0943 * (0.3481 * \$939 + 0.1 * \$1,423) = \$1,327$$