The Effect of Medicaid on Child Maltreatment: Evidence from the Expansion

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Abstract

In this paper, I examine the effect of access to Medicaid on child maltreatment rates using administrative data capturing the full census of alleged child maltreatment reports in the U.S. between 2010 and 2013 (N= 4,755,579). To identify the effect of Medicaid, I exploit the exogenous variation in access to Medicaid by the county-level early expansions in California's Low Income Health Program (LIHP) from 2011 – 2012. My preferred estimates suggest that access to Medicaid significantly reduced reports of physical abuse by up to 11 percent. I detect ample effect heterogeneity, with larger effects among children from families with financial hardships and those historically disadvantaged. This paper provides new evidence to inform the Medicaid discussion, providing new evidence suggestive of the potentially costly consequences of a retraction of benefits or generosity.

Introduction

Child maltreatment in the United States is increasingly prevalent. An estimated 37.4 percent of children experience an investigation by Child Protective Services (CPS) before their 18th birthday (Kim, Wildeman, Jonson-Reid, & Drake, 2017). In 2016, states received 4.1 million CPS referrals for 7.4 million children, a 10 percent increase since 2012 (U.S. Department of Health & Human Services Families, Administration on Children Youth and Families, & Bureau, 2016). As many adulthood inequalities appear to manifest in childhood, preventing early maltreatment is paramount (Almond & Currie, 2011; Almond, Currie, & Duque, 2017; Currie & Rossin-Slater, 2015; Heckman & Masterov, 2007). Children under 12 months in particular face the greatest risk of undetected maltreatment, the consequences of which are grim. Not only do the youngest children have the highest victimization rates (24.8 per 1,000 children), they account for half of child maltreatment fatalities (U.S. Department of Health & Human Services Families et al., 2016). If the effects of maltreatment nearly disappear if treated before the age of two, as suggested by a long-running study of orphans in Bucharest, the long-run benefits of early intervention are likely vastly underestimated (Nelson et al., 2007).

One fundamental challenge is to provide early and generous access to preventative services. An estimated 1.9 million children received prevention services in 2016 with another 1.3 million receiving post-investigation services,[1] implying that the majority of victims – and the much larger population of unsubstantiated victims and those not captured by official statistics – never receive stabilizing support.

[1] https://www.childwelfare.gov/pubPDFs/canstats.pdf

The Family First Prevention Services (FFPS) Act of 2018 aims to broaden access to preventative services, allocating funding to States for the title IV-E reimbursement of substance abuse and other mental health services along with parental training. With the exception of home visiting programs, however, there is limited evidence as to the efficacy of most preventative programs (Levey et al., 2017). Further, as these and other preventative services are often prioritized by observed risk, meaning that receipt is contingent upon a prior interaction with CPS, researchers have turned to policy-based universal prevention strategies. Despite the fact that child maltreatment is epidemic in proportion with an estimated cost of up to $30 billion annually – with an additional $124 to $585 billion lifetime cost of each new case (Fang, Brown, Florence, & Mercy, 2012) – relatively little is established in the causal sense about large-scale policy solutions outside of those targeting parenting behavior.

As poverty is the most prevalent risk factor for child maltreatment, a key policy question is whether sweeping improvements to the safety net can reduce the rate of violence against children. Indeed, numerous studies have shown in experimental and quasi-experimental contexts that an increase in income through cash assistance or tax credit programs reduces the odds of maltreatment (Berger, Font, Slack, & Waldfogel, 2016; Cancian, Yang, & Slack, 2013; Paxson & Waldfogel, 1999a, 2002, 2003; Raissian & Bullinger, 2017; Wildeman & Fallesen, 2017). However, hardships do not occur in a vacuum. Multiple studies have shown that one form of hardship often proceeds another, resulting in cumulative and pronounced disadvantage. This explains why the most successful prevention programs – home visiting programs – target multiple hardships at once by heaping parenting interventions upon social interventions, connecting the highest-risk families with additional resources, income, and skillsets (García, Heckman, Leaf, & Prados, 2016; Olds, 2006). Yet, this form of intervention may not be appropriate for the majority of at-risk families with fewer, more tractable hardships.

Medicaid is a similarly holistic safety net program with evidence of important improvements to recipients beyond physical health. Previous studies have documented positive effects on recipients' medical expenses and medical out-of-pocket spending (MOOP), improved mental health, and reductions in payday loans, suggesting that access to medical care might be a stabilizing source in impoverished households (Allen, Swanson, Wang, & Gross, 2017; Boudreaux, Gonzales, & Saloner, 2017; Hu, Kaestner, Mazumder, Miller, & Wong, 2016; Remler, Korenman, & Hyson, 2017; Wherry, Kenney, & Sommers, 2016b). Though 17 states have yet to expand Medicaid, several expansion states are presently considering a retraction of benefits through either imposing work requirements or changing benefit levels. Yet, many of the benefits of Medicaid are not fully understood, so a retraction of benefits (or failure to expand) could have negative externalities not accounted for by benefit-cost analysis. By one estimate, retraction of Medicaid benefits under the Patient Protection and Affordable Care Act (ACA) would result in a loss of coverage for 2.8 million people with a substance use disorder, and 1.2 million with a serious mental health disorder, two of the most prominent risk factors for perpetrators of child maltreatment (Frank & Glied, 2017). As medical reporters are the most prominent reporting source for infants – accounting for nearly one quarter of all reports for children under 12 months (see figure 1) – early prevention hinges on access to medical care. Given the ancillary evidence for Medicaid ameliorating the many contextual factors that put children at high risk for maltreatment, quantifying the direct benefits of Medicaid in a quasi-experimental framework is an important policy imperative.

In this paper, I provide the first evidence on the effect of Medicaid on young child maltreatment rates using the exogenous rollout of California's Low Income Health Program (LIHP). Using detailed administrative data covering the census of CPS reports in 50 states from 2010 through 2013 from the National Child Abuse and Neglect Data System (NCANDS), I compare the child maltreatment rates among children age five and younger in expansion counties to those non-expansion counties within and outside of California. My findings broadly indicate that access to Medicaid indeed reduces child maltreatment, especially physical abuse among traditionally disadvantaged populations of young children. I find that medical abuse skyrockets as the result of access to Medicaid, implying that in the absence of public insurance – even if they are categorically eligible – children are not receiving requisite care.

This paper proceeds as follows. After a brief discussion of child maltreatment and the policies aligned to protect children, I describe the Medicaid expansion and the three potential mechanisms through which I expect Medicaid to impact maltreatment rates. Following a description of the five data sources, I describe my empirical approach and results. I employ a number of robustness checks – discussed in the following section – followed by a discussion and conclusion.

Child Maltreatment

Child maltreatment is broadly defined as "any recent act or failure to act on the part of a parent or caretaker which results in death, serious physical or emotional harm, sexual abuse or exploitation; or an act or failure to act, which presents an imminent risk of serious harm," according to the Child Abuse and Treatment Act of 1988 (CAPTA), reauthorized in 2010 (P.L. 111-320). As states and localities have the autonomy to further define child protection laws and employ the necessary enforcement, there is ample cross-state variation in the criteria for investigation and punishment. States tend to vary with respect to funding and the intensity of their responses as well, though there has been a general shift towards either keeping children in the home or placing them with relatives as opposed to non-relative foster care under Title IV-E of the Social Security Act.[2]

Though there is some causal evidence that maltreatment is harmful to children's short- and long-term outcomes, the effect of maltreatment is difficult to disentangle from other factors such as family and neighborhood environments, socioeconomic disadvantage, prenatal and postnatal health investments, etc. that might differentiate maltreated children from those who are not maltreated. Thus, limited data availability and the absence of randomized studies robust causal inference in the majority of studies. Although attention has long been paid to the consequences of maltreatment across literature within the relevant disciplines, causal studies are rare for these reasons. Even within the studies that are closer to providing a well-identified effect of maltreatment, vast underreporting implies that the calculated effects may overestimate the true effects of maltreatment as only the more severe cases are detected and investigated and ultimately quantified (Currie & Spatz Widom, 2010).

In the early health and development literature, child maltreatment can be theoretically conceptualized both as a shock to early health endowment and as a negative parental investment. For instance, prenatal maltreatment could result in differences in health at birth, as indicated by conditions such as Fetal Alcohol Syndrome (FAS) or Neonatal Abstinence Syndrome (NAS), the prevalence of which has markedly increased due to the opioid crisis. Similarly, child maltreatment can be perceived as a negative parental investment – in other words, that which occurs after birth. Conceived this way, maltreatment affects children across multiple domains of well-being; in the short-term, it has been linked to poorer outcomes in education, cognitive ability and employment, physical and mental health, and adverse behaviors. Two quasi-experimental studies showed that maltreated children showed large and significant deficits in IQ, reading scores, and school performance (Currie & Widom, 2010; Perez & Widom, 1994). Though the cumulative effects of maltreatment are scarcely known or studied, both studies found that the effects persisted into adulthood with the latter study finding a 14 percentage point gap employment and an $8,000 gap in earnings. Maltreatment has also been associated with a host of mental health outcomes, including internalizing behaviors such as anxiety and depression, externalizing behaviors such as aggression and perhaps not surprisingly, nearly all forms of childhood maltreatment are correlated with PTSD (see Gilbert et al., 2009 for a complete review). Finally, maltreatment has also been linked with aggression, violence, and criminality through the lifespan (Currie & Tekin, 2006, 2012).

Policy Background

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[2] https://www.childwelfare.gov/pubPDFs/kinship.pdf

In 2010, California adopted an early Medicaid expansion program, the Low-Income Health Program (LIHP), using its "Bridge to Reform" §1115 Medicaid Demonstration Waiver matched at 50 percent with the Patient Protection and Affordable Care Act's (ACA) early expansion funding. LIHP expanded Medicaid eligibility up to 133 percent of the Federal Poverty Level (FPL), though the effective threshold is 138 percent FPL. After January 2014, participants were auto-enrolled in Medi-Cal, California's Medicaid program (or moved into marketplace insurance due to a change in eligibility status).

Numerous studies have documented enrollment increases following California's Medicaid expansion. Parents and adults who were previously ineligible enrolled at rates 30 percent higher compared to the pre-expansion rates. Although children were previously eligible for insurance coverage through CHIP up to 200 percent of the FPL, a handful of studies have documented boosts in children's enrollments following an increase in the parental eligibility threshold (Hudson & Moriya, 2017; Ku & Broaddus, 2006). There is ample evidence of a 'first stage' effect of Medicaid expansions. In randomized control trials, the Medicaid expansion has been causally linked to an increase in healthcare utilization, an improvement in users' self-reported overall health, and to a 30 percent decline in depression (Baicker et al., 2013a; Finkelstein et al., 2012). Later quasi-experimental studies found that these improvements were most likely relayed through reductions in out-of-pocket medical spending, a reduction in medical debt, and reduced poverty rates (Allen et al., 2017; Boudreaux et al., 2017; Hu et al., 2016; Remler et al., 2017; Wherry et al., 2016b).

Access to Medicaid can benefit children in four primary ways. First, there is ample evidence that access to Medicaid improves parental health care utilization and mental health (Baicker et al., 2013b; Finkelstein et al., 2012). A healthier parent is more likely to work, is less prone to stress, and may be more likely to use and benefit from mental health services (Currie & Madrian, 2000). Medicaid coverage for substance addiction treatment and the many physical and mental health ailments known to afflict maltreating parents could mechanically improve the behavioral aspects of maltreatment as well. Prior to ACA, families with children could lose their own coverage if the child is removed from the home, hampering the parent's ability to both recover and subsequently regain custody (Golden & Emam, 2013).

Second, access to Medicaid might benefit children through the contemporaneous increase in household income or a positive change in the household budget. Prior evidence has linked Medicaid access to reduced out-of-pocket medical expenditures (MOOP), a reduction in payday lending, a reduction in overall medical debt (Allen et al., 2017; Baicker et al., 2013b; Finkelstein et al., 2012; Hu et al., 2016) and lower poverty rates (Wherry, Kenney, & Sommers, 2016a). Children might benefit directly through increased investment as well as through a reduction in household stress. These may be important mechanisms given the established relationship between household income and child maltreatment rates. In a series of path-breaking studies using state-year aggregated data, Paxson and Waldfogel (1999; 2002; 2003) find ample evidence of a relationship between income, poverty and maltreatment. Their findings suggest that a 1-percentage point increase in the fraction of children below 75 percent of the poverty threshold is associated with a 3.8 percent increase in the number of maltreatment cases. A series of randomized control trials found that a boost in annual income decreased maltreatment rates (Cancian et al., 2013; Wildeman & Fallesen, 2017), and a recent study found that the increase in family income by about $1,000 annually from the EITC resulted in a reduction in neglect by 3 – 4 percent and a reduction of CPS reports by 8 – 10 percent among low-income, single mother families (Berger et al., 2016).

Access to Medicaid has consistently been found to directly increase healthcare utilization among children (see E. M. Howell and Kenney 2012). Increasing the eligibility threshold of parents appears to boost child enrollments as well (Hudson & Moriya, 2017; Ku & Broaddus, 2006). Medicaid has been found to reduce avoidable hospitalizations and infant/child mortality, two measures that may be correlated with maltreatment rates (Aizer, 2007; Bermudez & Baker, 2005; Bhatt & Beck-Sagué, 2018; Currie & Gruber, 1996; E. Howell, Decker, Hogan, Yemane, & Foster, 2010; Kaestner, Joyce, & Racine, 2001). Medicaid-spurred improvement in infant and child mortality rates might eventually shift the average population health of children through improved maternal health and prenatal care, leading to a decline in the number of children born with developmental disabilities and other limiting conditions linked to abuse and neglect. Furthermore, as pediatricians are a trusted resource for parents regarding child development and discipline strategies, physician access may directly improve parenting practices as well (Bass et al., 1993; Combs-Orme, Holden Nixon, & Herrod, 2011; Flaherty & Stirling, 2010; MacPhee, 1984; Regalado, Sareen, Inkelas, Wissow, & Halfon, 2004; Taylor, Moeller, Hamvas, & Rice, 2013).

Finally, Medicaid could also directly benefit children through increased time with mandated reporters and through home visiting interventions in early childhood. If mandated reporting among physicians falls after a child reaches their first birthday, it could be that increasing face time with physicians could boost reporting among children 2 – 5. Further, the ACA allocations for home visiting programs could prevent long-term child maltreatment, benefiting children throughout their lifetime. This latter mechanism could conversely result in an increase in reported maltreatment. As physicians are a primary mandated reporting source for young children, increased exposure could increase reporting rates. I consider and test for this possibility, as discussed in the empirical methodology section.

Data

My primary data includes the census of child maltreatment reports for children under six years in the National Child Abuse and Neglect Data System (NCANDS) from 2010 through 2013. These data are collected biannually and administered by the National Data Archive on Child Abuse and Neglect housed at Cornell University. Though contribution is voluntary under the Child Abuse Prevention and Treatment Act of 1988, NCANDS has become the primary source for child maltreatment statistics in the United States. From 2010 through 2016, 51 states contributed data to NCANDS, all of which are included here with the exception of Puerto Rico, though I limit my primary estimations to the three year county-month sample from January 1, 2010 through December 31, 2013 for a total of (N= 4,755,579) children aggregated to N=33,073 county-months. There is substantial variation in report rates across county, as illustrated in the kernel density curve in figure 2.

I use the 2010 Small Area Health Insurance Estimates (SAHIE) from the U.S. Census Bureau to estimate the pre-expansion, county-level insurance rates. Unlike estimates from the American Community Survey, these estimates include counties with populations under 65,000 and incorporate Medicaid enrollment rates to provide the most accurate county-level rates available. The 2010 Small Area Income and Poverty Estimates (SAIPE) provide the weighted and adjusted county-level poverty rates. I use the overall poverty rates, rather than that of children, to account for potential changes in parental eligibility. County population data comes from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER) for the entire period of inquiry. I compile age-specific population estimates into a single estimate for the population of children under six. County-level unemployment rates come from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) for the same period.

The many ways that previous authors have measured child maltreatment require distinction; maltreatment is an overall measure of child abuse and neglect and can be captured either as a behavioral approximation (see e.g. (Berger et al., 2016)), or as a count of screened-in maltreatment reports (Cancian et al., 2013). Though the latter is notoriously prone to underreporting and the former yields a rough approximation of overall maltreatment, both are commonly used to measure overall maltreatment rates in a population. Some studies measure CPS involvement with parental self-reports as well, however these are also prone to bias. It is common to separate neglect and abuse reports to detect treatment heterogeneity, which assists in understanding the pathways between poverty and maltreatment, as neglect -- which is often characterized by being in a state of resource scarcity -- may be more closely tied to income and economic disadvantage than other forms of maltreatment. Finally, several studies use Out of Home care (OOH) rates as an approximation for abuse and neglect. While typically borne out of data limitations, findings from these studies must be interpreted with caution, because differences in state policies, reporting mandates, and intervention methods (i.e. dual track responses) would skew the true distribution of child maltreatment rates.

Accordingly, I use four primary outcome measures which are later disaggregated into maltreatment type. The first captures the raw CPS report rate per 100,000 children. Though this variable is right skewed, I include it for comparison with previous literature alongside a log-transformed variable. In addition, I include two indicators for whether the alleged maltreatment was substantiated and for whether the child was removed from the home and placed into foster care. As both of these may reflect substantive state-, county-, or agency-level differences in policy, the log of CPS reports provides the most robust measure.

I use the American Community Survey from 2007 through 2013 for robustness checks, approximating maltreatment three ways. First, I construct a variable indicating whether a child is living without both parents, then iterate this variable as an indicator for whether the child and siblings are living without both parents. My third measure indicates whether a grandparent is caring for the child.

Empirical Approach

To estimate the causal effect of access to Medicaid on child maltreatment, I require a plausibly exogenous source of variation in Medicaid. If access to Medicaid is random (or quasi-random, as proposed here), the observed outcomes in an OLS model are more likely to be the result of differential access to Medicaid, as opposed to some other driving force. A conventional OLS model would be prone to bias due to selection into Medicaid and the presence of other unobserved factors that might contemporaneously predict Medicaid participation and maltreating behaviors. For instance, disabled or otherwise disadvantaged parents may be more likely to enroll in Medicaid and to maltreat their children. As disability status is unobserved, the effect of Medicaid would be biased away from zero. Therefore, I identify the effect of Medicaid using the temporal and spatial variation in the early California Medicaid expansion. Using a difference-in-difference framework (DD), I compare the pre- and post- expansion abuse and neglect rates of children living in expansion counties to those in non-expansion counties, as characterized by equation 1 below.

$$(1)\quad maltreat_{cm} = \alpha + \beta C_c + \rho M_m + \gamma X_{cm} + \theta POST_{cm} + \varepsilon_{cm}$$

Maltreatment outcomes ($maltreat_{cm}$) for children in county c in month m are measured as the number of reports per 100,000 children, are regressed on county- and month-fixed effects (C and M, respectively), a vector of controls for child and family characteristics (X), and an indicator, POST, set to 1 for California counties beginning in the month of their expansion and thereafter, and 0 for all counties outside of California and those within California prior to expansion. The coefficient of interest θ represents the estimated intent-to-treat (ITT) effect of Medicaid on child maltreatment reports. County-level fixed effects account for static differences across counties and month effects account for temporal changes that affect all counties uniformly. County-specific linear time trends are included in later models to account for other factors that vary across counties over time, such as compositional shifts in Medicaid-eligible populations or differential responses to the Great Recession. All regressions are executed using OLS models with White robust standard errors clustered at the county level and weighted by the county population of children younger than six years of age.

A key assumption underlying the DD methodology is that absent the Medicaid expansion, the trends in maltreatment would have been indistinguishable across treated and untreated counties. Though this assumption – often referred to as the parallel trends assumption -- is inherently untestable, I explore the presence of pre-trends by regressing a trend variable with county-level indicators among the pre-treatment sample, as shown in equation (2) below.

$$(2)\ maltreat_{cm} = \alpha + \gamma X_{cm} + \beta TR_{cm} + \theta TREAT_{cm} + \rho TRxTREAT_{cm} + \varepsilon_{cm}$$

$TREAT$ indicates treatment status, equal to 1 for California counties with expanded Medicaid and 0 otherwise. TR is the linear time trends, and ρ is the coefficient of interest, indicating whether the pre-treatment trend differs between the treated and control counties.

Omitted variable bias is another common threat to causality, where other unobserved factors that predict maltreatment are spuriously related to the error term. Accordingly, the key identifying assumption for causal inference is that access to Medicaid did not plausibly change the demographic characteristics of children prone to maltreatment, nor other factors that might affect reporting, especially among Medical personnel. To test the first part of this assumption, I regress the child demographics on treatment status.

With regard to the latter assumption, medical personnel have been designated mandated reporters in California since 1963, though at the time only physicians were noted and were responsible only for reporting physical abuse. California's Child Abuse and Neglect Reporting Act (CANRA) of 1980, § 11165.7 part (21) expanded the list of responsible medical personnel to include the myriad of reporters mandated today. Two sections of CANRA were enacted in 2011 or 2012, neither of which would have had any bearing on reporting behavior. The first, § 11167 part (f) outlined the type of information collected on a report and the confidentiality rules for reporters. The second, clarified the definitions of substantiation, unfoundedness, and inconclusiveness (§ 1165.12).[3]

To ensure exact counterfactuals and to account for potential bias from omitted variables, I also estimate synthetic control models. To ease interpretation, I collapse the data to the state-level, using the last county-level expansion date as that for the entire state. This method ensures that the most precise counterfactual states are used by creating a synthetic California based on the characteristics and pre-law trends in the outcome variable. The resulting synthetic California is a weighted combination of other untreated states. The weights are selected such that the difference in the characteristics in California and the other counties is minimized. The weights sum to one and are estimated separately for each state. I conduct inference on the state-year aggregated data using DD regression analysis. and employ the cross-validation method for selecting predictor weights (Abadie, Diamond, & Hainmueller, 2015).

Results

Table 1 shows the summary statistics of my sample. The children in the census of maltreatment reports have a mean age of 2.43 and are predominantly white. The vast majority of reports were unsubstantiated, and only 14 percent of reported children were eventually removed from their home and placed into foster care. In line with previous studies, 61.71 percent of the sample is comprised of children who were alleged victims of neglect and nearly 16 percent of physical abuse. 28 percent of the children's caregivers reported having financial hardships at the time of the report, with an even greater percentage reporting receiving any type of public assistance. The majority of the children hail from unknown family structures at the time of the report. Of those for whom the family structure is known, cohabiting is the most common.

Table 2 shows the primary results from DD estimations. In column 1 of panel A, the expansion is credited with 35 fewer reports per 100,000 children in a given county-month, a reduction of nearly 4 percent relative to the pre-expansion mean. When county-specific linear time trends are included, the effect falls to 10 reports per 100,000 children. Though insignificant, the direction and magnitude suggest that the effect is not negligible, and as I discuss later, is likely masked by competing heterogeneous effects. Turning to the reports disaggregated by maltreatment type, access to Medicaid drives down reports of physical abuse by 7.98 reports per 100,000 children per county-month and sexual abuse reports by 1.68 reports per 100,000 children. Panels C and D show the estimates from the same model using the

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[3] *https://leginfo.legislature.ca.gov/faces/codes_displayText.xhtml?lawCode=PEN&division=&title=1.&part=4.&chapter=2.&article=2.5*

log of the reports as an outcome measure. As this variable is normally distributed, I consider these estimates less prone to bias and considerably more reliable. Access to Medicaid appears to reduce Physical abuse CPS reports by 11 percent (panel D, column 2), representing the intent-to-treat effect (ITT), with similarly negligible effects on all other outcomes with one exception. There appears to be a rather large, negative effect on other types of maltreatment (column 6), with my preferred estimates suggesting a significant 78 percent decline in the number of maltreatment reports alleging an 'other' type of maltreatment.

In order to estimate the effect of the treatment-on-the-treated (TOT), I estimate the same model with a subset of counties for whom the exact compliance rate is available (appendix table A2). The average compliance rate in this set of counties was 28.5 percent as of 12/2012, yielding an implied TOT of 42 percent.[4] Though not all CMSP counties are included in my primary sample, the average compliance rate among these counties is 28.8 percent,[5] resulting in a TOT of 39 percent. In other words, every 12,607 new enrollments reduced physical abuse CPS reports by 1 percent.

Due to state- and county-level differences in defining and prosecuting child abuse and neglect, the raw CPS report rate is widely considered the best measure of maltreatment. However, prior studies assessing child maltreatment prevention strategies often use the rate of substantiated reports and removal (foster care rate) as proxy measures. Accordingly, table 3 presents the same models as in table 2, using these two alternative measures. Note that the rate of substantiated reports appears unaffected in the full model (panel B column 1) and in the maltreatment subtype models. Interestingly, the foster care rate falls in most specifications (panel D). This could be an artifact of parental access to Medicaid, which may have resolved any tractable parenting issues that otherwise would have kept children in care. For instance, children whose parents were addicted to opioids would have gained access to treatment under the expansion, along with a laundry list of other physical and health benefits.

I next probe whether my primary effects are being driven by county size. In table 4, I omit the weights and stratify three primary outcomes by an indicator for whether the observed county is above or below the median observed county population (using exclusively the log of the report rate). If small cell sizes were driving these effects, we would expect the small counties to have inflated coefficients. Fortunately, this is not the case, as seen when comparing columns 1-3 to 4-5. In panels B and C, I present the results from models stratified by county poverty rates (B) and 2010 insurance rates (C). I delimit counties in the same way, coding an indicator with "1" if the county is at or above the observed median

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[4] ITT = .12 (column 2 panel B in table A2), compliance = .28. For TOT = ITT/ compliance, the implied TOT = .42.
[5] An estimated 506,660 of the 1,757,000 eligible individuals enrolled = 28.52 % for CA

and "0" if below. If these results are in fact being driven by the Medicaid expansion, we would expect to see larger effects in more disadvantaged counties (panels B and C, columns 4, 5, and 6). My results generally support this assertion, with the exception of all reports having a larger, significant negative effect in counties with high insurance rates (column 1).

Effect Heterogeneity

Prior evidence suggests that a retraction of Medicaid benefits could be particularly detrimental so historically underrepresented subgroups of the population. Under previous expansions, Medicaid was accredited for reducing maternal mortality and infant mortality, increasing overall access to care and healthcare utilization, and reducing the general burden of medical debt. To that end, in this section, I further disaggregate by child characteristics and other markers of disadvantage. For the remainder of the paper, I retain only my preferred specifications with log outcomes and county-specific linear time trends.

Table 5 presents the results from equation (1) on the full sample for major maltreatment type, substantiation, and foster care disaggregated by child race/ethnicity. Though the effects for Black and Other children are relatively larger, up to 19 percent, the effect for White children is relatively large as well. The foster care rate follows the same general pattern, with strongest effects among White children. These results suggest that children of traditionally disadvantaged race/ethnicities face a significant increase of physical abuse by up to 19 percent and around a 2 percent increase in foster care in the counterfactual world without Medicaid. The smaller effects among Hispanic children could be due to the fact that enrollment is conditional upon citizenship, a question verifiable in future research with CPS reports and insurance status in the same data.

As children under three months had access to Medicaid under their mother's pregnancy-based eligibility, it is unsurprising that the effect among infants (< 12 months) is relatively small and insignificant. In California, the income eligibility limits for infants was much higher (200% FPL) compared to that of older children, so this age group in particular would only directly benefit from the expansion by enrollment of older siblings, parents, and relatives.[6] Most other ages appear to benefit, though the one year olds to a much greater degree. All age groups see a decline in foster care rates, following the same pattern as the results in table 2. In table 7, the same models are disaggregated by child gender. Though both genders benefit from Medicaid in terms of significantly reduced physical abuse,

[6] Repeating this analysis without infants yields the same results (available upon request).

females benefit more than males, by nearly 8 percentage points (14 percent relative to six percent). The foster care benefits are evident in these models as well.

At the time of the CPS report, the child's primary caregiver was asked whether they relied on any type of public assistance. Table 8 shows the same DD models stratified by this indicator. Families who answered 'yes' saw a 25 percent reduction in all reports, a 28 percent reduction in physical abuse and a 20 percent reduction in neglect, all significant at 95 percent or higher. Though the results for physical abuse among the more advantaged families retain significance, the magnitude of the effect is nearly half that as the disadvantaged families. Not only do these results give further support to the notion that these effects are being driven by Medicaid, but they reveal a potential direct pathway through which children might benefit – if families are on public assistance with and without Medicaid, then these effects can be attributed to Medicaid alone, as the sole source of variation in these effects. Unlike the previous models, children from disadvantaged families see a small but significant reduction in neglect, suggesting that Medicaid may curtail maltreatment by relaying additional resources and benefits that the absence of which would be classified as neglect.

If children gain access to Medicaid through their caregivers (either directly or indirectly), the surveillance hypothesis posits that increased time with medical personnel resulting from increased medical care access and usage can result in an increase in reports. To test this assertion, I stratify the same models by an indicator for whether the reporter was medical or non-medical, as shown in table 9. The effect on medical reporters is negative, undermining the surveillance hypothesis. However, it is important to note that if the decrease in reports by medical personnel is offset by an increase in non-medical personnel, the spillover effects of the law could only be interpreted as a substitution effect away from medical reporters, rather than an actual change in child maltreatment. Conversely, reports fall among both groups of reporters, giving further credence to an absolute reduction in maltreatment.

Robustness Checks

To further probe the robustness of these results, I turn to the American Community Survey (ACS) approximating for child maltreatment with an indicator for whether a child was living out of the home at the time of the survey in the same time period. As the ACS has more detailed information on household and family characteristics, I similarly specify the sample, retaining only children under six years of age from 2010 through 2013. As the child's maltreatment status is unobservable (as in most major household surveys), I construct an indicator for whether the child is living with either parent, so that children living without a parent is equal to one, and zero otherwise. Parallel measures in the ACS indicate whether all children in the household are living without parents, and whether children are living with grandparents. These are only included as subsidiary outcomes as they exclude children living with other kin or relatives.

I employ the same difference-in-difference model as in equation (1), shown in panel A of table 10, and include county-specific linear time trends as shown in panel B. Though the sample is markedly larger, the effects follow the same general pattern as my primary data, albeit at diminished significance. Children are .04 percentage points less likely to be living without their parents (25.96 percent of the pre-treatment mean) and .03 percentage points less likely to be living with a grandparent (both significant at 10 percent). However, the effects are considerably larger, up to 26 percent of the pre-expansion mean, suggesting that there is likely some additional heterogeneity not captured by these specifications. These results give further credence to my primary findings.

If access to Medicaid is the source of reduction in child maltreatment reports, we would expect to see a similar pattern from the 2014 ACA expansion. Though the rollout was at the state level, 26 states enacted an expansion January 1, 2014, with another five in the following five years. The results, shown in table 11, follow the same general pattern, though in the preferred specification with state-specific linear time trends, only the coefficient on the number of reports remains marginally significant. This could be because uptake rates are not observable in these data, and because other, related policies resulted in a surge of funding in the same period, degrading the variation needed for identification. Further, when I restrict the sample to California alone, the same pattern emerges, albeit with much less power (panel B).

One further possibility that would undermine the DD approach would be if caregivers manipulated their treatment assignment, resulting in an effect that captures both a shift in the eligible population and the effect of access to Medicaid. To rule out this possibility, I regress on the treatment indicator a set of child characteristics. These results (available upon request) suggest that this possibility is weak at best, with marginal significance in one indicator for children's age. Overall, there is no evidence to suggest that treatment group manipulation is problematic.

If pre-trends in the outcome variables vary between Medicaid expansion and non-expansion counties, the credibility of these results would decline, as the effect could be biased by factors other than medicaid. To test this assumption, I estimate equation (2) on the pre-treatment sample, shown in table 12. The first row indicates that there is no significant difference between the outcomes across the top in the treated and untreated counties, implying that the parallel trends assumption is met. However, the results from the dynamic policy effects model I estimate in the following section suggest that this assumption may be violated, urging a conservative interpretation of the results on all reports and neglect reports.

Conversely, physical abuse reports appear to be entirely free of pre-trends, giving further credence to the reliability of my estimates.

One remaining threat to validity is the theoretical construction of a control group. If the control group fails to represent a perfect counterfactual, the observed differences in report rates could be biased away from zero, capturing other nuances in unobserved heterogeneity. To test this possibility, I employ a synthetic control estimation, averaging across the county treatment effects, the difference between California and synthetic California, to generate figure 2 using a lowess estimator. The vertical dashed line represents the average month in which Medicaid was expanded across all California counties. The treatment effect for all CPS reports and neglect reports is positive in the pre-expansion period, followed by negligible effects in the post-expansion period. This pattern suggests that Medicaid may have been expanded in counties with relatively high CPS reports pre-expansion. Alternatively, the null effects in the pre-expansion period followed by negative effects post-expansion for physical abuse reports follows the pattern that emerges from my primary results. Comparing the estimated treatment effects to the reported compliance rate in figure 3, it is clear that high-compliance counties have relatively larger estimated treatment effects in all three outcomes.

Dynamic policy effects

If California's early Medicaid expansions were truly exogenous, we would expect to see little or no discernable change in maltreatment reports before the expansion, and a large, cumulative effect after the expansion. To test this assertion, I implement an event study model, where I allow individual three-month period to enter the model as leads and lags, rather than the post indicator in equation (1). Accordingly, I estimate the following equation:

$$(3)\ maltreat_{cm} = \alpha + \beta C_c + \rho M_m + \gamma X_{cm} + \sum_{j \in J} \theta_j POST_{cm}^{j} + \varepsilon_{cm}$$

Where $POST_{cm}^{j}$ is a series of dummy variables equal to 1 for the counties in which the expansion was in place for j periods, J ={-12, -9, -6, -3, 0, 3, 6, 9, 12} with the three-month of the expansion as the omitted category (0). I estimate models with and without county-specific linear time trends, though only report the latter as these represent my more conservative estimates.

The results for all CPS reports are shown in figure 4. The top panel shows the coefficients for the full sample, followed by physical abuse reports only in the center panel and neglect reports in the last panel. The results in the full sample (top panel) follow the same pattern as the synthetic controls, suggesting that the models with county-specific linear time trends are the most robust, as these are more likely to account for any unobserved heterogeneity. In accordance with the DD model findings, Medicaid appears to have the largest effect on physical abuse rates (bottom panel), especially in the months immediately after the expansion.

Discussion and Conclusion

This study provides the first evidence that access to Medicaid plausibly reduces violence against children. Motivated by the evidence linking the early childhood environment with lifelong health and human capital production, increased early access to Medicaid might ameliorate the economic, cognitive, and mental and physical health disparities associated with childhood abuse and neglect (Berger & Waldfogel, 2011; Currie & Tekin, 2006; Currie & Widom, 2010; Gilbert et al., 2009; Perez & Widom, 1994; Robinson et al., 2012). I identify the effect of access to Medicaid by the quasi-experimental variation due to California's county-level Low Income Health Program in 2011 – 2012. I find that children experienced an 11 percent reduction in reports alleging physical abuse, though these effects are notably larger among more disadvantaged children. Children living in homes with financial hardships see an 28 percent reduction in maltreatment reports, and those who are either Black or Other race/ethnicities benefit by up to 19 percent of the pre-expansion mean. My results are robust to synthetic controls analysis and alternative data samples and specifications.

As Medicaid under ACA was not designed to prevent child abuse, these striking findings inform Medicaid's overall cost-benefit calculation and provide a new strategy for practitioners and legislators seeking to reduce maltreatment and the costs thereof. Though the potential mechanisms remain theoretical, ancillary evidence points to multiple, cumulative facets of disadvantage that may result in a reduction of maltreatment rates, including stability of the household budget, a reduction in stress and depressive symptoms, and an overall improvement in parental and child health. Future researchers should identify ways to test these mechanisms using data that capture both child maltreatment status and insurance status. Though there is a great deal of evidence pointing to income effects, per se, little is known about the cumulative effects of combining income supplementation with insurance, or other holistic improvements to the parenting context. Home visiting programs provide perhaps the best evidence of this type, though due to their high relative cost and general preference for families known to CPS, the vast majority of at-risk children are ineligible for their benefits.

The effects I detect here are well within the range detected in previous studies, though the effects here are concentrated on physical maltreatment. Medicaid remits around $500 per year in terms of financial benefits in addition to the many non-financial benefits I discuss at length above, implying that my effects should align with those from both cash and in-kind programs. Several previous studies detect a reduction in child maltreatment due to safety net and other antipoverty programs. One such study used an existing randomized control trial to identify the effect of additional child support received on child maltreatment rates among a large sample of TANF recipient families in Wisconsin (Cancian, Yang, and Slack 2013). The additional child support was garnered through a full pass-through and disregard, causing an increase in annual family incomes by around $170 compared to the control groups. Though it appears small, the treatment group saw a 10 percent reduction in the odds of being screened-in for a child maltreatment investigation, representing a 2 percentage point reduction over a two year period. A later study using Danish registry data found a similar effect, albeit with a much larger increase in income (Wildeman and Fallesen 2017). Identified by a 2004 law that effectively lowered the welfare payment ceilings, the authors find that the resulting 30 percent reduction in income was associated with a 1.5 percentage point increase in the odds that a child was involved in CPS, or, a 25 percent increase in the annual risk of CPS involvement. The increase in income was equivalent to around $4,800 per year, yet the effect was similar in magnitude, suggesting that these results may not perfectly translate to the US context. Although both studies are limited in generalizability due to their sample of welfare recipients, the overall pattern of causal and observational studies is highly suggestive of a causal effect of income on child maltreatment (see also Berger and Waldfogel 2004; Slack, Lee, and Berger 2007). Paxson and Waldfogel (2002; 2003) find that welfare reform is correlated with child maltreatment as well; benefit reductions appear to increase caseloads, and various measures of generosity – benefit levels, lifetime limits, sanctions, work requirements -- appear to have a similar effect (more generous policies yield lower caseloads). These effects are also relatively large; a 10 percent increase in welfare benefits is associated with a reduction in OOH care by nearly 8 percent. The latter finding suggest that even if welfare reform had competing effects (improved the lives of some and diminished that of others), the net effect on child maltreatment was overall negative. That welfare reforms generated negative externalities in child maltreatment rates was the same conclusion drawn by later authors as well (Wildeman and Fallesen 2017). Other safety net programs appear to affect child maltreatment rates, presumably through the family budget as well. Although Paxson and Waldfogel (2003) failed to detect the effect of EITC state benefit levels on child maltreatment, a later study identified the effect of EITC generosity using an instrumental variable approach that takes advantage of the geographic and temporal variation in state EITC benefit structures (L. M. Berger et al. 2016). Drawn from a longitudinal sample of families from the Fragile Families and Child Wellbeing Study, the authors find that the EITC instrument caused a first-stage increase in family income by about $1,000 annually, resulting in an reduction in neglect by 3 – 4 percent and a reduction of CPS reports by 8 – 10 percent among low-income, single mother families. These results are robust to control function methods and to other samples, indicating that the permanent shifts in income experienced by these families substantially reduced neglect and CPS reports, but not physical abuse. Several studies found that other factors such as foreclosure rates (Frioux et al. 2014; Wood et al. 2012; L. M. Berger et al. 2015), minimum wage (Raissian and Bullinger 2017), and gasoline prices (McLaughlin 2017) appear to predict a reduction in maltreatment as well. The latter two effect sizes are particularly interesting; a $1 increase in the minimum wage appears to reduce neglect reports by 9.6 percent, and a $1 increase in gasoline prices increases the overall maltreatment rate by 6.42 percent.

Despite the growing evidence of the importance of reducing child maltreatment in early childhood, the large-scale strategies for doing so are not well understood outside of parenting interventions and home visiting programs. Additional information is needed on the precise factors that contribute to mandatory reporting, and the conditions under which children benefit the most. One key question from this analysis is why reports among medical personal plummet after a child's first birthday, and why child care providers initiate such a small percentage of reports. Though with this evidence, expanding Medicaid access and eligibility may be a useful strategy for improving the early parenting environment and early child health.

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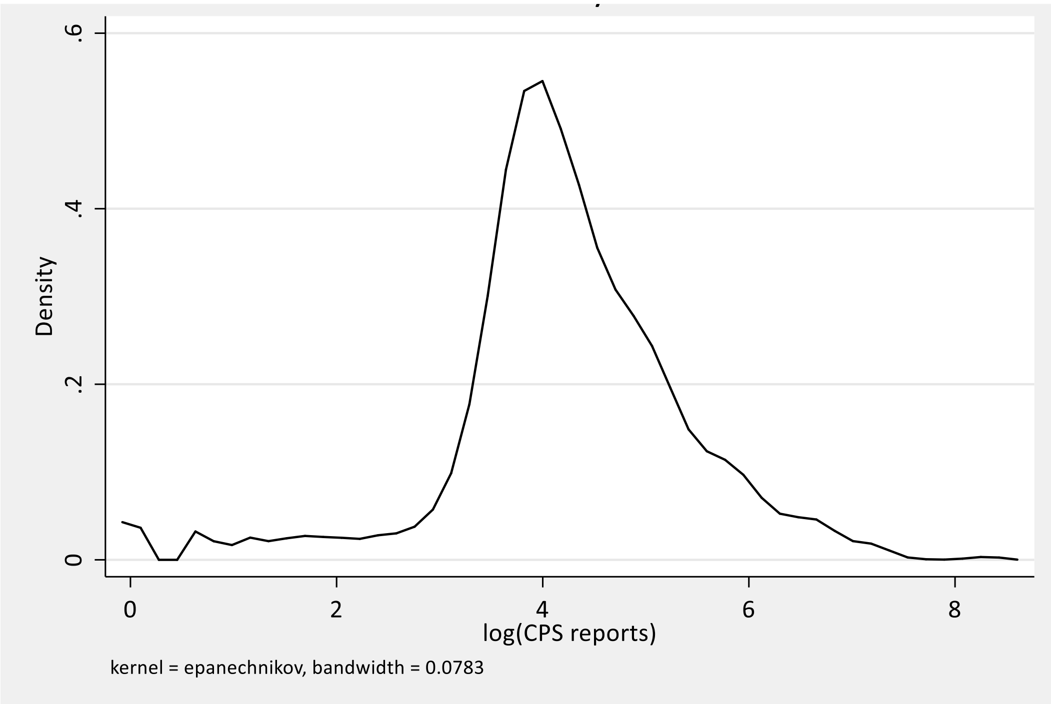
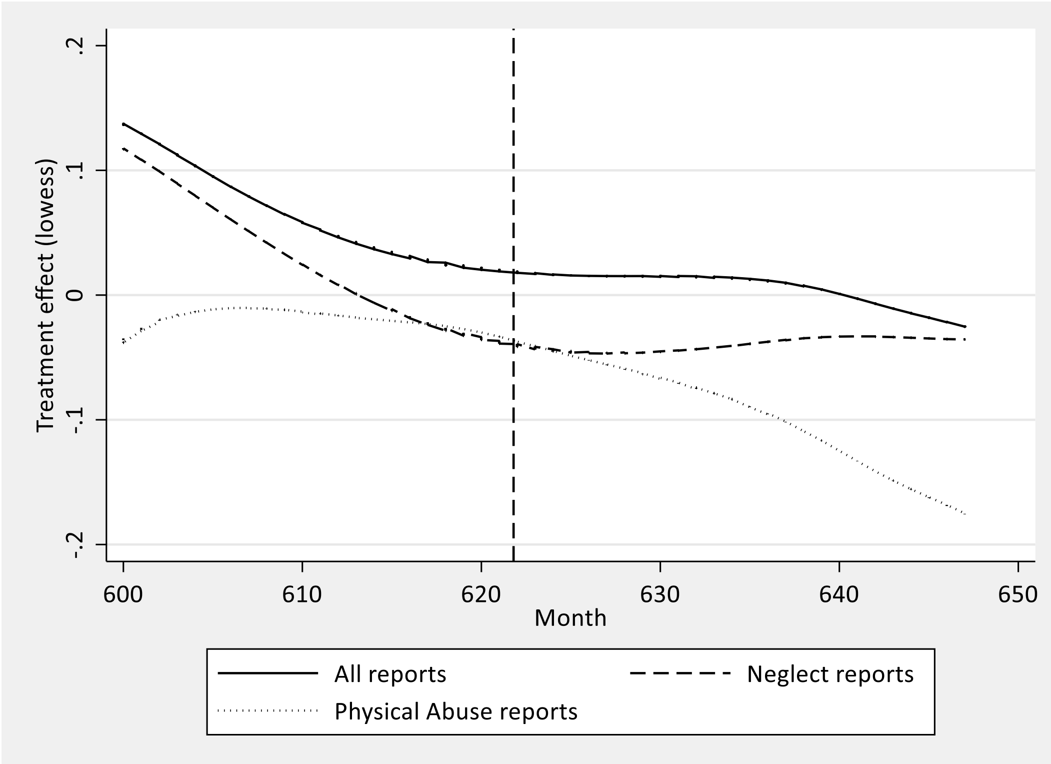
Figures

Figure 1: Spatial variation in CPS Report rates by county



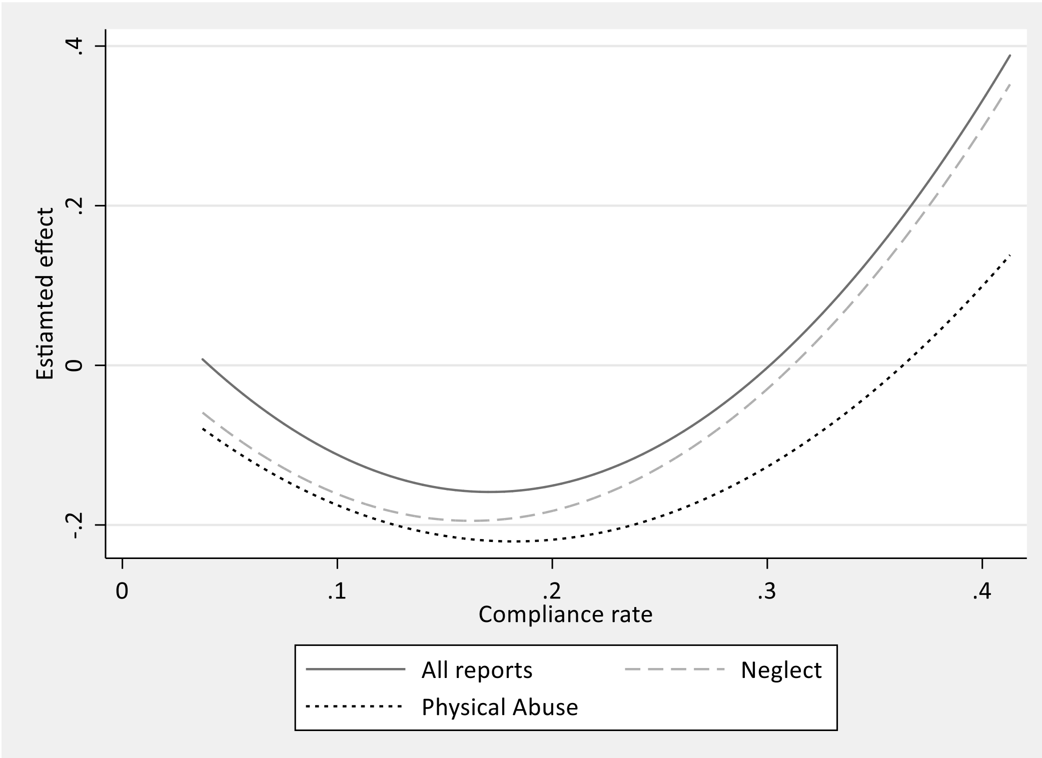
Note: Total alleged maltreatment reports to Child Protective Services (CPS) among children younger than six years from the census of reports drawn from 2010 – 2013 NCANDS administrative data.

Figure 2: Synthetic control effects (lowess smoothed)



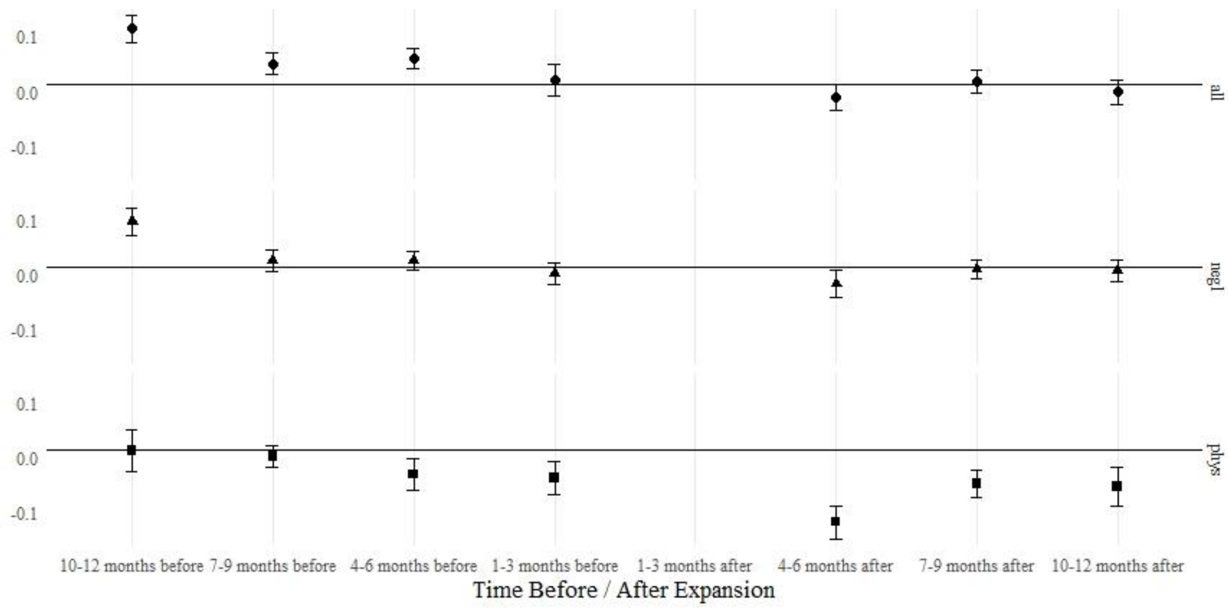
Note: Synthetic control models estimating the effect of Medicaid access on all reports, neglect reports, and physical abuse reports. Lines represent the average effects across counties using a lowess estimator. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in the weighting stage for all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure).

Figure 3: Synthetic control estimates by compliance rate



Note: This figure shows the physical abuse treatment effect by county-level compliance (uptake) rate. Synthetic control models estimating the effect of Medicaid access on physical abuse reports. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in the weighting stage for all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure).

Figure 4: Dynamic policy effects



Note: Event history models are estimated using OLS weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). LTT = linear time trends. State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Tables

Table 1: Descriptive Statistics

	Full Sample	
	Mean/Freq	SD
Age	2.43	1.75
Race		
White	41.77%	
Black	27.84%	
Hispanic	23.64%	
Other	6.75%	
Foster care	0.14	0.35
Substantiated	0.24	0.43
Type		
Physical	15.84%	
Neglect	61.71%	
Sexual Abuse	3.68%	
Psych. Abuse	3.69%	
No Maltreatment	9.27%	
Other	5.80%	
Financial hardships	0.28	0.45
Public Assistance	0.32	0.47
Family Structure		
Married	7.32%	
Cohabiting	24.24%	
Single parent	18.13%	
Kin / OOH	2.86%	
Other / Unknown	47.45%	
N	4,755,579	

Table 2: Difference-in-Difference (DD) estimation

	(1)	(2)	(3)	(4)	(5)	(6)
	All reports	Physical Abuse	Neglect	Sexual Abuse	Psych. Abuse	Other
A: Rate per 100,000						
Treat*Post	-35.22**	-7.98**	-6.18	-1.68*	-1.84	-2.78
	-15.59	-2.80	-9.19	-0.64	-8.43	-2.93
r2	0.89	0.83	0.88	0.64	0.89	0.92
B: Rate per 100,000, with county-specific linear time trends						
Treat*Post	-10.23	-7.44***	-5.21	-1.35**	5.90	-1.53
	-7.04	-1.47	-6.17	-0.42	-3.77	-3.11
r2	0.91	0.86	0.90	0.67	0.92	0.93
C: Log(reports)						
Treat*Post	-0.09***	-0.10*	-0.01	-0.09*	-0.20	-0.26*
	-0.03	-0.04	-0.04	-0.04	-0.18	-0.10
r2	0.95	0.93	0.95	0.88	0.94	0.93
D: Log(reports), with county-specific linear time trends						
Treat*Post	-0.03	-0.11***	-0.01	-0.06	-0.02	-0.78***
	-0.02	-0.03	-0.03	-0.04	-0.07	-0.09
r2	0.97	0.94	0.97	0.88	0.95	0.95
N	33,073	31,284	32,691	26,123	10,583	8,643

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). LTT = linear time trends. State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 3: DD estimation, alternative measures

	(1)	(2)	(3)	(5)	(6)	(7)
	All reports	Physical Abuse	Neglect	Sexual Abuse	Psych. Abuse	Other
A: Rate of Substantiated Reports						
Treat*Post	0.01	-0.01	0.01*	-0.02*	-0.02	0.08***
	-0.01	-0.01	0.00	-0.01	-0.02	-0.02
r2	0.72	0.60	0.78	0.28	0.40	0.61
N	33,073	31,284	32,691	26,123	10,583	8,643
B: Rate of Substantiated Reports, with county-specific linear time trends						
Treat*Post	0.00	0.01	0.00	-0.02	0.02	0.27
	-0.01	-0.01	-0.01	-0.01	-0.03	-0.18
r2	0.77	0.63	0.82	0.31	0.46	0.65
N	33,073	31,284	32,691	26,123	10,583	8,643
C: Foster Rate						
Treat*Post	0.02	0.02	0.01	0.00	-0.01	0.01
	-0.01	-0.02	-0.01	-0.01	-0.01	-0.03
r2	0.95	0.92	0.95	0.91	0.91	0.83
N	29,232	24,261	28,302	17,673	8,602	5,709
D: Foster Rate, with county-LTT						
Treat*Post	-0.02***	-0.02**	-0.02***	-0.02**	-0.01	0.06
	-0.01	-0.01	-0.01	-0.01	-0.01	-0.06
r2	0.97	0.95	0.96	0.93	0.93	0.87
N	29,232	24,261	28,302	17,673	8,602	5,709

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 4: DD estimation, stratified models

	(1)	(2)	(3)	(4)	(5)	(6)
	All reports	Physical reports	Neglect Reports	All reports	Physical reports	Neglect Reports
A: County Size						
	Above. Median County Pop.			Below Median County Pop.		
Treat*Post	-0.12***	-0.18***	-0.03	-0.57*	-0.15	-0.53
	(0.03)	(0.04)	(0.03)	0.32	(0.25)	(0.28)
r2	0.90	0.85	0.91	0.43	0.59	0.60
N	20,674	20,260	20,590	12,399	11,024	12,101
B: County Poverty Rates						
	Above Median Income			Below Median Income		
Treat*Post	-0.08**	-0.07	-0.01	-0.11**	-0.15***	-0.03
	(0.04)	(0.06)	(0.03)	(0.05)	(0.04)	(0.08)
r2	0.97	0.95	0.97	0.91	0.88	0.92
N	16,594	15,497	16,427	16,479	15,787	16,264
C: County Uninsurance Rates						
	Below Median Uninsured Rate			Above Median Uninsured Rate		
Treat*Post	-0.09**	-0.08	-0.03	-0.05	-0.13**	0.09
	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.09)
r2	0.97	0.94	0.96	0.89	0.89	0.92
N	16,741	15,842	16,663	16,332	15,442	16,028

Note: Panel A shows OLS models. Panels B and C show OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 5: DD estimates stratified by child race/ethnicity

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: White					
Treat*Post	0.00	-0.14**	0.01	-0.01	-0.02**
	(0.02)	(0.06)	(0.03)	(0.01)	(0.01)
r2	0.94	0.88	0.93	0.56	0.96
N	32,904	29,370	32,394	32,904	28,322
B: Black					
Treat*Post	-0.03	-0.15**	-0.02	0.01	-0.02**
	(0.03)	(0.06)	(0.03)	(0.02)	(0.01)
r2	0.96	0.91	0.95	0.42	0.94
N	29,527	20,353	27,678	29,527	23,001
C: Hispanic					
Treat*Post	-0.02	-0.07**	-0.01	0.00	-0.02**
	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
r2	0.97	0.93	0.96	0.37	0.93
N	27,828	17,245	24,745	27,828	20,316
D: Other					
Treat*Post	-0.03	-0.19***	0.02	0.02**	-0.02**
	(0.04)	(0.07)	(0.05)	(0.01)	(0.01)
r2	0.92	0.82	0.90	0.29	0.93
N	23,701	12,466	20,341	23,701	17,260

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 6: DD estimates stratified by child age

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: Infant (< 12m)					
Treat*Post	-0.02	-0.02	-0.01	-0.01	-0.02***
	(0.02)	(0.08)	(0.02)	(0.01)	(0.01)
r2	0.96	0.89	0.95	0.63	0.96
N	32,409	21,935	31,505	32,409	26,882
B: One year					
Treat*Post	0.00	-0.21***	0.05	0.00	-0.02***
	(0.02)	(0.08)	(0.04)	(0.01)	(0.01)
r2	0.96	0.87	0.94	0.54	0.96
N	32,276	21,412	31,241	32,276	25,310
C: Two years					
Treat*Post	-0.03*	-0.15***	-0.01	0.01	-0.02***
	(0.02)	(0.06)	(0.04)	(0.01)	(0.01)
r2	0.96	0.87	0.94	0.54	0.96
N	32,421	22,818	31,399	32,421	25,344
D: Three Years					
Treat*Post	-0.02	0.02	-0.01	0.00	-0.02***
	(0.03)	(0.06)	(0.03)	(0.01)	(0.01)
r2	0.96	0.88	0.94	0.54	0.96
N	32,524	23,820	31,406	32,524	25,318
E: Four years					
Treat*Post	-0.02	-0.15***	-0.02	0.01	-0.02***
	(0.02)	(0.05)	(0.04)	(0.01)	(0.01)
r2	0.96	0.89	0.94	0.53	0.95
N	32,582	24,522	31,379	32,582	25,316
F: Five years					
Treat*Post	-0.04**	-0.11**	0.00	0.00	-0.02**
	(0.02)	(0.05)	(0.03)	(0.01)	(0.01)
r2	0.95	0.89	0.93	0.53	0.96
N	32,576	25,316	31,364	32,576	25,183

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 7: DD estimates stratified by child gender

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: Female					
Treat*Post	-0.02	-0.14***	-0.01	0.00	-0.02***
	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
r2	0.97	0.92	0.96	0.71	0.97
N	32,919	28,807	32,380	32,919	28,238
B: Male					
Treat*Post	-0.03*	-0.06*	-0.01	0.00	-0.02***
	(0.02)	(0.04)	(0.03)	(0.01)	(0.01)
r2	0.97	0.93	0.96	0.71	0.97
N	32,930	29,947	32,455	32,930	28,321

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 8: DD estimates stratified by financial hardship

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: No Public Assistance					
Treat*Post	0.01	-0.18 **	-0.06	0.00	-0.01
	(0.08)	(0.07)	(0.08)	(0.03)	(0.01)
r2	0.95	0.95	0.95	0.61	0.94
N	17,227	14,027	16,311	17,227	14,540
B: Public Assistance					
Treat*Post	-0.25***	-0.28**	-0.2**	0.02	0.00
	(0.08)	(0.13)	(0.09)	(0.02)	(0.01)
r2	0.94	0.91	0.93	0.59	0.92
N	14,730	10,065	13,810	14,730	13,539

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 9: DD estimates stratified by reporter type

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: Non-Medical Reporters					
Treat*Post	-0.03	-0.09***	-0.01	0.00	-0.02***
	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
r2	0.97	0.94	0.96	0.75	0.97
N	33,031	30,935	32,642	33,031	28,893
B: Medical Reporters					
Treat*Post	-0.03	-0.16*	-0.01	0.00	-0.01*
	(0.03)	(0.09)	(0.04)	(0.01)	(0.01)
r2	0.93	0.85	0.91	0.57	0.93
N	29,951	19,510	27,122	29,951	23,231

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 10: DD estimates, robustness checks with American Community Survey data

	(1)	(2)	(3)
	Living Out of Home	All Siblings and Children OOH	Lives with grandparent
A: Fixed effects only			
Treat*Post	-0.005*	-0.004*	-0.003**
	(0.00)	(0.00)	(0.00)
r2	0.02	0.03	0.08
B: Fixed effects and county-LTT			
Treat*Post	-0.003	-0.004*	-0.003*
	(0.00)	(0.00)	(0.00)
r2	0.02	0.02	0.04
N	793,734	793,734	793,734

Note: Panel A shows estimates of equation (1) using child-level OLS regressions with White robust standard errors clustered at the county-level. Panel B shows the same model as panel A, however with county-linear time trends included. OLS models weighted by the county population of children under six years of age. American Community Survey data 2010 – 2013. Covariates are included in all models (household head education, child race, child age). All models include state fixed effects and year fixed effects. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 11: DD estimates, robustness checks

	(1)	(2)	(3)
	Log(all reports)	Physical Abuse	Neglect
A: State-level ACA Expansion			
Treat*Post	-0.09*	-0.12*	-0.08
	(0.03)	(0.05)	(0.04)
r2	0.96	0.93	0.96
N	51,783	48,857	51,165
B: California Counties Only			
Treat*Post	-0.04	-0.05*	-0.01
	(0.03)	(0.03)	(0.02)
r2	0.99	0.97	0.99
N	1,727	1,687	1,726

Note: Panel A shows estimates for the 2014 state-level expansion, with White robust standard errors clustered at the level of the state. Panel B shows the estimates with California counties only, omitting all other states, with White robust standard errors clustered at the county level. OLS models weighted by the county population of children under six years of age. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, year fixed effects, and linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Test of pre-trends in outcome variables

	(1)	(2)	(3)	(4)	(5)
	Log(all reports)	Physical Abuse	Neglect	Rate of Subst. Reports	Foster rate
Trend*Treat	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Trend	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat	0.20	-0.60***	0.86***	-0.12***	-0.91***
	(0.13)	(0.08)	(0.12)	(0.02)	(0.02)
Constant	6.83***	3.38***	2.90***	0.19***	0.19***
	(0.49)	(0.24)	(0.31)	(0.05)	(0.05)
N	12,301	11,686	12,176	12,301	10,365
r2	0.96	0.94	0.96	0.80	0.98

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports in the pre-treatment period collapsed to the county level. Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects and month fixed effects. Trend = monthly time trend, treat = indicator for treated county. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Table A1: County expansion dates

County	County FIPS	Population (2017)	Compliance rate	Expansion date	County Medical Services Program (CMSP)	Omitted from data (< 1,000 reports)
Alameda	6001	1,663,190	95.6%	Jul-11		x
Alpine	6003	1,120	28.6%	Jan-12	x	x
Amador	6005	38,626	28.6%	Jan-12	x	x
Butte	6007	229,294	28.6%	Jan-12	x	
Calaveras	6009	45,670	28.6%	Jan-12	x	x
Colusa	6011	21,805	28.6%	Jan-12	x	x
Contra Costa	6013	1,147,439	35.7%	Jul-11		
Del Norte	6015	27,470	28.6%	Jan-12	x	x
El Dorado	6017	188,987	28.6%	Jan-12	x	
Fresno	6019	989,255	.	Jan-14		
Glenn	6021	28,094	28.6%	Jan-12	x	x
Humboldt	6023	136,754	28.6%	Jan-12	x	
Imperial	6025	182,830	28.6%	Jan-12	x	
Inyo	6027	18,026	28.6%	Jan-12	x	x
Kern	6029	893,119	11.0%	Jul-11		
Kings	6031	150,101	28.6%	Jan-12	x	
Lake	6033	64,246	28.6%	Jan-12	x	x
Lassen	6035	31,163	28.6%	Jan-12	x	x
Los Angeles	6037	10,163,507	33.5%	Jul-11		
Madera	6039	156,890	28.6%	Jan-12	x	
Marin	6041	260,955	28.6%	Jan-12	x	
Mariposa	6043	17,569	28.6%	Jan-12	x	x
Mendocino	6045	88,018	28.6%	Jan-12	x	
Merced	6047	272,673	.	Jan-14		
Modoc	6049	8,859	28.6%	Jan-12	x	x
Mono	6051	14,168	28.6%	Jan-12	x	x
Monterey	6053	437,907	.	Mar-13		
Napa	6055	140,973	28.6%	Jan-12	x	x
Nevada	6057	99,814	28.6%	Jan-12	x	
Orange	6059	3,190,400	29.4%	Jul-11		
Placer	6061	386,166	26.0%	Aug-12		
Plumas	6063	18,742	28.6%	Jan-12	x	x

Riverside	6065	2,423,266	16.9%	Jan-12		
Sacramento	6067	1,530,615	3.7%	Nov-12		
San Benito	6069	60,310	28.6%	Jan-12	x	x
San Bernardino	6071	2,157,404	20.4%	Jan-12		
San Diego	6073	3,337,685	24.3%	Jul-11		
San Francisco	6075	884,363	34.4%	Jul-11		
San Joaquin	6077	745,424	4.4%	Jun-12		
San Luis Obispo	6079	283,405	.	Jan-14		
San Mateo	6081	771,410	41.3%	Jul-11		
Santa Barbara	6083	448,150	.	Jan-14		
Santa Clara	6085	1,938,153	29.2%	Jul-11		
Santa Cruz	6087	275,897	14.3%	Jan-12		
Shasta	6089	179,921	28.6%	Jan-12	x	
Sierra	6091	2,999	28.6%	Jan-12	x	x
Siskiyou	6093	43,853	28.6%	Jan-12	x	x
Solano	6095	445,458	28.6%	Jan-12	x	
Sonoma	6097	504,217	28.6%	Jan-12	x	
Stanislaus	6099	547,899	.	Jan-14		
Sutter	6101	96,648	28.6%	Jan-12	x	x
Tehama	6103	63,926	28.6%	Jan-12	x	
Trinity	6105	12,709	28.6%	Jan-12	x	x
Tulare	6107	464,493	.	Mar-13		
Tuolumne	6109	54,248	28.6%	Jan-12	x	x
Ventura	6111	854,223	36.1%	Jul-11		
Yolo	6113	219,116	28.6%	Jan-12	x	
Yuba	6115	77,031	28.6%	Jan-12	x	

Table A2: DD estimation, subsample

	(1)	(2)	(3)	(4)	(5)	(6)
	All reports	Physical Abuse	Neglect	Sexual Abuse	Psych. Abuse	Other
A: Log(reports), with county-LTT						
Treat*Post	-0.03	-0.11***	-0.01	-0.06	-0.02	-0.78***
	-0.02	-0.03	-0.03	-0.04	-0.07	-0.09
r2	0.97	0.94	0.97	0.88	0.95	0.95
N	33,073	31,284	32,691	26,123	10,583	8,643
B: Log(reports), with county-LTT and in CA, excluding CMSP counties						
Treat*Post	-0.02	-0.12***	0.00	-0.05	0.01	-0.78***
	-0.02	-0.03	-0.03	-0.05	-0.08	-0.09
r2	0.97	0.94	0.97	0.89	0.95	0.95
N	32,066	30,316	31,685	25,342	9,606	8,587

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). LTT = linear time trends. State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.