THREE ESSAYS ON ANTIPOVERTY PROGRAMS AND REDUCTIONS IN CHILD MALTREATMENT

by

Neil McCray

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Committe

Priyanka Anand
___________________________ Dr. Priyanka Anand, Chair
Associate Professor, Department of Health Administration and Policy, GMU

Len M. Nichols
___________________________ Dr. Len Nichols, Committee Member
Non-resident Fellow, Urban Institute
Professor Emeritus, GMU

Denise Hines
___________________________ Dr. Denise Hines, Committee Member
Associate Professor, Department of Social Work, GMU

Hongyan
___________________________ Dr. Y. Alicia Hong, Acting PhD Program Director
Department of Health Administration and Policy

PJ Maddox
___________________________ Dr. PJ Maddox, Department Chair
Department of Health Administration and Policy

Germaine Louis
___________________________ Dr. Germaine Louis, Dean
College of Health and Human Services

Date: 4/13/2022

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George Mason University
Fairfax, VA

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by

Neil McCray
Master of Public Policy
George Mason University, 2017
Bachelor of Specialized Studies
Cornell College, 2013

Director: Priyanka Anand, Associate Professor
Department of Health Administration and Policy
College of Health and Human Services

Spring Semester 2022
George Mason University
Fairfax, VA

DEDICATION

To my husband Andrew, whose support has been limitless and whom I love dearly, and to my parents Jeanette and Mark, who have encouraged me my entire life.

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LIST OF ABBREVIATIONS

American Community Survey .... ACS
American Reinvestment and Recovery Act .... ARRA
Broad-based categorical eligibility .... BBCE
Callaway-Sant’Anna .... C-S
Centers for Medicare and Medicaid Services .... CMS
Child Abuse Prevention and Treatment Act .... CAPTA
Child Protective Services .... CPS
Current Population Survey Annual Social and Economic Supplement .... ASEC
Current Population Survey Food Security Supplement .... FSS
Data generating process .... DGP
Difference-in-difference .... DiD
Doubly-robust .... DR
Earned Income Tax Credit .... EITC
Federal poverty level .... FPL
Fourth National Incidence Study on Child Abuse and Neglect NIS-4
Fragile Families and Child Wellbeing study .... FFCW
Instrumental variable IV
Maintenance of Effort .... MOE
National Child Abuse and Neglect Data System .... NCANDS
National Federation of Independent Businesses .... NFIB
National Health Interview Survey .... NHIS
Patient Protection and Affordable Care Act .... ACA
Public Use Microdata Areas .... PUMA
Socioeconomic status .... SES
Special Supplemental Nutrition Program for Women, Infants, and Children .... WIC
Supplemental Nutrition Assistance Program .... SNAP
Temporary Assistance for Needy Families .... TANF
Two-way fixed effects .... TWFE

ABSTRACT

THREE ESSAYS ON ANTIPOVERTY PROGRAMS AND REDUCTIONS IN CHILD MALTREATMENT

Neil McCray, Ph.D.

George Mason University, 2022

Dissertation Director: Dr. Priyanka Anand

Millions of children are reported maltreated in the United States each year. In addition to the costs imposed on victims, maltreatment also imposes costs on society at large, including short and long-term medical care, reductions in education and workforce productivity, and increased criminality, among other costs. Finding ways to reduce maltreatment risk is a critical concern of public policy. Because poverty is a primary risk factor for child maltreatment risk, researchers have considered whether antipoverty programs might reduce child maltreatment.

This dissertation consists of three papers discussing and assessing the effects of antipoverty programs on child maltreatment. The first paper discusses theories that explain the relationship between poverty and child maltreatment generally – primarily family stress and family investment models – and then considers literature on the relationship between child maltreatment and several antipoverty programs and policies including the Earned Income Tax Credit, the Supplemental Nutrition Assistance Program

(SNAP), Medicaid, Temporary Assistance for Needy Families (and its predecessor, Aid to Families with Dependent Children), and the minimum wage. Findings from the literature suggest strong theoretical reasons to expect that antipoverty programs should reduce maltreatment risk, that there are correlations between antipoverty program increases and reductions in child maltreatment, and some more recent causal studies demonstrate policies can reduce maltreatment risk. The second and third papers each consider a different antipoverty program (Medicaid and SNAP, respectively) and use variation in policy decisions at the state-level to assess effects on child maltreatment outcomes.

Paper two considers Medicaid's effects on child maltreatment. First, the paper discusses why Medicaid might reduce maltreatment risk, both via the proposed theoretical models relating to socioeconomic status and via changes in health care utilization for both adults and children. Because the Patient Protection and Affordable Care Act's (ACA) Medicaid expansions, which were originally mandatory, were rendered optional by the U.S. Supreme Court's decision in *National Federation of Independent Business v. Sebelius*, state selection to expand or not expand Medicaid functions as a sort of natural quasi-experiment. This paper exploits variations in state selections to expand or not expand Medicaid to identify the causal effect of Medicaid expansion on child maltreatment outcomes. Prior to the ACA, a number of states had state-funded programs expanding Medicaid similar to ACA Medicaid expansions. Some states also chose to expand Medicaid early or to partially expand their programs. Due to these and other variations, precisely defining which policy changes constitute "Medicaid expansion" can be complicated; this paper considers several different definitions of expansion to examine whether inclusion or exclusion of some states affects results. The paper finds that January 2014 Medicaid expansions led to reductions in child neglect, but the robustness of that result is sensitive to which states are included in the sample in terms of when they expanded and the generosity of their prior Medicaid coverage.

The third paper considers the relationship between SNAP and child maltreatment. The paper first considers theoretical reasons why SNAP might affect child maltreatment risk, including family stress and family investment models, and then considers additional factors relating specifically to food insecurity. Then the paper turns to empirically assessing whether SNAP leads to reductions in child maltreatment. It exploits variation in state decisions regarding broad-based categorical eligibility (BBCE) in SNAP, a policy which increases the number of people eligible for SNAP and can also simplify application processes. State selection of BBCE leads to reductions in neglect and sexual abuse, and some findings indicate BBCE may also reduce physical abuse and medical neglect, though those findings are sensitive to model specification.

This dissertation discusses several theoretical reasons why antipoverty programs should reduce maltreatment risk, assesses literature on several programs, and empirically assesses the causal effect of two programs – Medicaid and SNAP – on child maltreatment outcomes. Findings suggest Medicaid expansions may reduce neglect, though the results are sensitive to how Medicaid expansion is defined, and that SNAP broad-based categorical eligibility reduces neglect and sexual abuse. Results support the proposed theoretical models and more generally support the idea that antipoverty policies and programs may reduce child maltreatment.

1. ANTIPOVERTY PROGRAMS AS MITIGATING FACTORS FOR CHILD MALTREATMENT RISK

Abstract

Over a third of children in the United States are reported as maltreated during their childhoods, and about 12.5 percent have reports that are substantiated by authorities. Behaviors included in maltreatment include physical abuse, neglect, sexual abuse, medical neglect, and other types of maltreatment. Maltreatment imposes substantial costs onto victims, and onto society at large, both immediately and in the future. Given both the pervasiveness of child maltreatment and the costs it imposes on society, researchers have examined whether a variety of policies might reduce its incidence. One risk factor for maltreatment is poverty. Some aspects of poverty may be remediable via policy intervention from antipoverty programs. This paper discusses theoretical models that explain the relationships between poverty and child maltreatment, examines how those theories might predict reductions in child maltreatment stemming from antipoverty programs, and assesses empirical literature on antipoverty programs and their effects on child maltreatment. Available associative empirical literature suggests that antipoverty programs are correlated with reductions in maltreatment, and recent causal analyses find antipoverty programs lead to reductions in maltreatment.

Introduction

Despite child maltreatment being at one point considered "extreme[ly] socially deviant" and of "low prevalence" (National Research Council, 1993, p. 106), 37.4 percent of children in the United States are subject to investigations by child protective services during their childhood (H. Kim et al., 2017) and 12.5 percent of children experience a substantiated case of maltreatment (Wildeman et al., 2014). Given that many instances of maltreatment go unreported (Sedlak et al., 2010) and that children with unsubstantiated maltreatment reports are at similar risk of future maltreatment as those with substantiated cases (Hussey et al., 2005; H. Kim & Drake, 2019; Kohl et al., 2009), these represent conservative estimates of the pervasiveness of child maltreatment in the United States.

Beyond maltreatment's immediate harms are significant future costs imposed on victims, including higher risk of behavioral health problems like anxiety and depression (Felitti et al., 1998; J. Kim & Cicchetti, 2006; Thompson & Tabone, 2010); criminal behavior and violence (Cuadra et al., 2014; Elklit et al., 2013; Grogan-Kaylor & Otis, 2003; Jonson-Reid et al., 2012); chronic diseases (Gilbert et al., 2015); and reduced levels of education, employment, and earnings as adults (Currie & Widom, 2010). Expanding the scope of costs incurred to consider society at large, one study estimates that each year the United States incurs between $124 and $585 billion in lifetime lost economic productivity from child maltreatment, depending on what types of cases are counted (Fang et al., 2012).

Considering both the extent of child maltreatment and its costs, both to survivors and society, there is substantial interest in understanding both the causes of maltreatment and what might be done to prevent it. While many contributing factors have been identified that influence maltreatment risk in complex and interactive ways, one factor consistently identified as increasing the risk of maltreatment is low socioeconomic status (SES; Jonson-Reid et al., 2009; Sedlak et al., 2010). Among risk factors for maltreatment, some implications of low SES may be remediable via policy intervention; to that end, researchers have focused particular attention on how policies intended to address low socioeconomic status might also affect child maltreatment (Bullinger, Lindo, et al., 2021; Maguire-Jack et al., 2021).

Background

What is child maltreatment

The federal government, via the Child Abuse Prevention and Treatment Act (CAPTA), establishes minimum definitions of child abuse and neglect, though states are free to expand upon federally established minimum definitions (Child Welfare Information Gateway, 2019). CAPTA defines child abuse and neglect as "any recent act or failure to act on the part of a parent or caregiver that results in death, serious physical or emotional harm, sexual abuse, or exploitation, or an act or failure to act that presents an imminent risk of serious harm" (*CAPTA Reauthorization Act of 2010*, 2010).

Child maltreatment encompasses a wide variety of caregiver behaviors. Broad categories for those behaviors include physical abuse, which constitutes 17.5 percent of all maltreatment reports; neglect, 74.9 percent of all reports; medical neglect, a subset of neglect, 2.3 percent of all reports; sexual abuse, 9.3 percent of all reports, and emotional/psychological abuse, 6.1 percent of all reports (U.S. Department of Health & Human Services, Administration for Children and Families, Administration on Children, Youth and Families, Children's Bureau, 2021).

Physical abuse is understood to be "'any nonaccidental physical injury to the child' and can include striking, kicking, burning, or biting the child, or any action that results in a physical impairment of the child" (Child Welfare Information Gateway, 2019, p. 2).

Neglect is considered failure of a caregiver "to provide needed food, clothing, shelter, medical care, or supervision to the degree that the child's health, safety, and well-being are threatened with harm" (Child Welfare Information Gateway, 2019, p. 2). Another broader interpretation of neglect is "the presence of certain deficiencies in caretaker obligations… that harm the child's psychological and/or physical health. Child neglect covers a range of behaviors including educational, supervisory, medical, physical, and emotional neglect, and abandonment" (National Research Council, 1993, pp. 59–60).

Medical neglect, the subset of child neglect related specifically to medical care, is generally understood as caretaker failure to provide requisite medical treatment, though what constitutes requisite treatment may vary by state (Child Welfare Information Gateway, 2019).

As with other types of maltreatment, specific definitions of sexual abuse vary by state – some include general definitions and others specify explicit acts that constitute sexual abuse (Child Welfare Information Gateway, 2019). One general definition "includes incest, sexual assault by a relative or stranger, fondling of genital areas, exposure to indecent acts, sexual rituals, or involvement in child pornography" (National Research Council, 1993, p. 59).

The *Child Maltreatment* series of reports released annually by the Children's Bureau counts substantiated cases of maltreatment in the United States, by type. In 2019, there were 656,243 children confirmed victims of child maltreatment. That includes 491,710 children confirmed neglected; 115,100 children confirmed physically abused; 60,927 children confirmed sexually abused; 44,595 children with confirmed "other" types of maltreatment; 39,824 children confirmed psychologically maltreated; and 15,092 children confirmed medically neglected (U.S. Department of Health & Human Services, Administration for Children and Families, Administration on Children, Youth and Families, Children's Bureau, 2021). These numbers undercount the real incidence of maltreatment in several ways: 1) they count children confirmed maltreated, but not instances of maltreatment; if one child were maltreated in the same way multiple times they would be counted only once, 2) they count only the subset of reports found to be substantiated, and substantiation does not adequately measure risk to children (Hussey et al., 2005; Kohl et al., 2009), and 3) only instances of maltreatment which are reported in the first place can be investigated and found substantiated or not; if incidents of maltreatment are not reported, they are not counted by definition.

Etiology of child maltreatment

Unravelling the factors that influence child maltreatment risk is complicated. Early research attempted to identify individual causal factors like child or parent characteristics, but in more recent decades perspectives have shifted to ecological models that consider interactions of a wider array of risk factors (Austin et al., 2020; MacKenzie et al., 2011; National Research Council, 1993). The Fourth National Incidence Study on Child Abuse and Neglect (NIS-4), considered “the largest epidemiological study to date designed to measure actual child maltreatment in the United States” (Drake & Jonson-Reid, 2013), found a number of factors had significant associations with child maltreatment incidence (Sedlak et al., 2010). Parental employment was associated with reduced child maltreatment rates (p. 135). Household low SES, which is jointly determined by income, antipoverty program participation, and parental education, was significantly associated with higher incidence of maltreatment and maltreatment severity (p. 142). Children living with one parent and an unmarried partner had 8 times greater rate of maltreatment compared to children living with two married biological parents (p. 151) and children in larger families had consistently higher rates of maltreatment than those in smaller families (p. 175). Parental alcohol and drug use were also associated with increased risk of maltreatment (p. 207). These findings are generally supported throughout child maltreatment literature.

Factors of particular interest in this paper are poverty and low socioeconomic status. There are two commonly used models in the child welfare literature which explain how “economic factors produce maladaptive outcomes” (Warren & Font, 2015, p. 16). One is the family stress model (Conger, 1994; Conrad et al., 2020; Maguire-Jack et al., 2021; Warren & Font, 2015), which posits that economic stress harms caregiver mental and behavioral health, which can lead to inhibited capacity for caregiving. Another is the family investment model (Conrad et al., 2020; Maguire-Jack et al., 2021; Warren & Font, 2015), which posits that family receipt of economic support, such as from antipoverty programs, allows caregivers to invest additional resources into their families, reducing maltreatment risk. Taken together, the family stress and family investment models offer both an explanation for how poverty can contribute to increased risk of child maltreatment and how antipoverty programs might help to alleviate that economic stress and curb maltreatment risk.

One critical observation is that no one factor successfully predicts child maltreatment incidence, but a variety of factors at individual, family, and higher levels interact in ways that do influence maltreatment risk (MacKenzie et al., 2011). An important implication is that while factors like low SES and poverty have been identified as contributing to maltreatment risk, that does not imply that all families experiencing poverty or low SES maltreat their children. Rather, these factors contribute to increased risk, but they also interact with other individual, family, and environmental factors that might make maltreatment more or less likely.

Socioeconomic status

Low socioeconomic status and poverty are linked to risk of being reported for child maltreatment. Families in poverty are overrepresented in the child welfare system relative to families not in poverty (Jonson-Reid et al., 2009). One critical question is whether that overrepresentation is due to real higher risk of maltreatment or if it is instead due to systemic bias against families living in poverty.

Researchers and policymakers alike have commonly assumed that the overrepresentation is due, at least in part, to bias (H. Kim et al., 2018). Class-based visibility bias would occur mostly with professional reporters of maltreatment because families are assumed to have increased exposure to mandatory reporters. That increased exposure, due to contact with a range of service providers, would mean higher likelihood of reporting for maltreatment even if real maltreatment risk were not elevated. However, class-based bias in child maltreatment reporting has been thoroughly assessed and the theory is not supported empirically (Drake & Zuravin, 1998; Jonson-Reid et al., 2009; H. Kim et al., 2018). The child welfare literature is converging on the conclusion that the overrepresentation of children from poor families in the child welfare system is not driven by artificial inflation due to bias in maltreatment reporting, but instead is being driven by real elevated risk of maltreatment incidence (Drake et al., 2021; Drake & Zuravin, 1998; Jonson-Reid et al., 2009; H. Kim et al., 2018). Poverty does make maltreatment reports more likely, but that appears to be because poverty makes maltreatment more likely.

Poverty and low socioeconomic status might be linked to heightened risk for child maltreatment for a variety of reasons. Poverty might make parents less able to provide a family's material needs (Berger & Waldfogel, 2011, p. 8). This applies primarily to neglect. Families in poverty may not have the resources they need to give their children adequate food, clothing, housing, medical care, or other things they need. Lacking those resources could lead, even unintentionally, to child neglect. A recent report from *Child Trends* contends that every state in the U.S. includes at least one factor related to income in child maltreatment definitions and only half of the states provide any sort of exemption related to financial inability to provide for children (Williams et al., 2022). Family stress and family investment theories offer further explanations for how poverty might lead to maltreatment, including types other than just neglect. Fewer resources mean higher levels of stress and lower capacity for investment in the family, each of which are associated with increased maltreatment risk.

The NIS-4 identified low SES as a risk factor for maltreatment generally, and especially for neglect (Sedlak et al., 2010). Other studies reach similar conclusions. Berger (2004) used data from the National Longitudinal Survey of Youth, which includes observations of the same set of children in 1986, 1988, 1992, 1994, 1996, and 1998, to assess the relationship between family income and a range of child health measures and child maltreatment risk. That study found that as family income increases overall child maltreatment risk declines. One prospective longitudinal study tracked several hundred families over 10 years and found those with lower levels of maternal education had higher levels of maltreatment report risk (Dubowitz et al., 2011). Reviewing developments in child maltreatment research since the 1990s, Pelton (2015) noted that literature has consistently found associations between income and child maltreatment. One recent study found poverty does not just operate at the family level – poverty in surrounding areas also has implications for child maltreatment risk (H. Kim & Drake, 2018).

Identifying the causal effect of income on child maltreatment requires moving beyond examining associations between income and maltreatment. One strategy exploits various types of tax changes as shocks to family incomes, or to consumption of particular types of goods related to child maltreatment, such as alcohol. McLaughlin (2019) examined the association between state-level alcohol taxes and state-level child maltreatment rates and found an inverse relationship: increasing alcohol excise taxes are associated with reductions in child maltreatment. While in general taxes would be considered to be effectively reducing income and would thus be expected to increase maltreatment (family stress theory), in this case alcohol taxes increase the cost of a good with positive associations with child maltreatment (more alcohol consumption is associated with more child maltreatment), decreasing consumption of alcohol. McLaughlin (2017) used variation in gas taxes to measure the impact of exogenous shocks to family income on child maltreatment referral risk. This study used state-level panel data from 2000-2010 and included data on several socioeconomic covariates in addition to gas price data. The study assessed the association between gas prices and child maltreatment referrals and found that a $1 increase in the state average gas price is associated with an increase of between 5.32 and 6.42 children per thousand reported as victims of child maltreatment. Gas taxes increase the cost of a good whose demand is relatively less elastic than alcohol, and those costs are pushed disproportionately onto people with lower incomes; as a result, gas tax increases are associated with increases in multiple types of child maltreatment. Income tax effects are more ambiguous compared to alcohol or gas taxes, potentially because families with very low incomes may not be subject to income taxation (Mclaughlin, 2018).

Assessments of associations between poverty and child maltreatment are not limited just to income. Studies have also assessed impacts separately of additional factors related to low SES, including housing insecurity and food insecurity. Warren and Font (2015) found an association between housing insecurity and abuse and neglect. Marcal (2018) used propensity score matching to identify the impact of exposure to housing insecurity on child maltreatment and find a statistically significant but small increase in maltreatment reporting risk. Food insecurity is also associated with heightened maltreatment risk, primarily due to food insecurity's strong associations with both low SES and higher levels of psychological distress. Jackson et al., (2018) found households experiencing food insecurity have six times higher predicted probability of childhood exposure to violence. Helton et al., (2019) also found food insecurity is associated with increased risk of psychological and physical aggression from caregivers to children. Yang (2015) examined the association between material hardship and child maltreatment and found higher levels of material hardship are strongly correlated with child maltreatment reports, including both neglect and physical abuse. These results support the theory that low SES and poverty are risk factors for maltreatment and that the family stress model is a good explanation for that observed relationship.

Antipoverty programs

Many studies that have examined the relationship between child maltreatment risk and income, poverty, and SES have found correlations but until recently tended to lack causal estimation strategies (Bullinger, Lindo, et al., 2021; Drake et al., 2021; Pelton, 2015). A burgeoning subset of literature on maltreatment and poverty utilizes causal estimation strategies and public policy changes in a variety of areas to identify not just associations but also causal effects. Maguire-Jack et al., (2021) reviewed literature on the impact of programs such as Temporary Assistance for Needy Families (TANF), child care subsidies, the Supplemental Nutrition Assistance Program (SNAP), and the Earned Income Tax Credit (EITC) on child maltreatment. That review's general finding was that antipoverty programs that offer cash or near-cash assistance to families may promote healthier families and reduce child maltreatment risk, though they also noted causal analysis of the impact of economic support programs is difficult due to concerns with omitted variable bias.

EITC

The EITC is a large federal antipoverty program considered to be "the most important means-tested transfer program in the United States…. [I]t has grown to be one of the largest and least controversial elements of the U.S. welfare state, with 26.7 million recipients sharing $63 billion in total federal EITC expenditures in 2013" (Nichols & Rothstein, 2016, p. 137). Between 2017 and 2020, the number of recipients has consistently hovered around 25 million workers and families and total benefits have varied between $60-63 billion (Internal Revenue Service, 2022). The EITC is a tax credit for people who are working and have low to moderate incomes. For workers with higher incomes but below the maximum threshold, it can reduce federal tax burdens. At lower income levels, the EITC can eliminate federal tax burden or, if the EITC is greater than tax liability, yield a tax refund. Working parents with children are the group targeted primarily by EITC, though working adults without children may also qualify for smaller benefits (Marr et al., 2015).

In addition to the federal EITC, states and some localities have also established their own versions of the EITC (Internal Revenue Service, 2021). One benefit of the existence of state EITC programs is that variation in those programs can be used as a natural policy experiment to measure the impact of EITC on child maltreatment. Prior literature has examined the effect of the EITC on child maltreatment as well as EITC's impact on a variety of other outcomes of interest.

Berger et al., (2017) used the EITC to measure how income affects child maltreatment risk in families with unmarried parents. The paper implemented a causal estimation strategy by using variation in state EITC generosity as an instrument for incomes in order to identify the effects of exogenous increases in incomes on child maltreatment. Drawing data from the Fragile Families and Child Wellbeing Study (FFCW), limited to children and families who could be impacted by EITC, the researchers considered both self-reported involvement with Child Protective Services (CPS) and "behaviorally approximated measures of child abuse and child neglect" (p. 1346). The paper found "an exogenous increase in income is associated with a modest reduction in behaviorally approximated child neglect and relatively large reduction in CPS involvement, particularly among low-income single-mother families" (p. 1346). This study represents a massive leap forward in literature on the causal relationship between income and child maltreatment. EITC as an instrument is a novel approach that offers a viable causal estimation strategy with less potential for confounding variables.

The underlying assumption to this approach is that EITC only affects maltreatment via income effects and no other direct pathways.

Klevens et al. (2017) assessed the relationship between EITC and hospital admissions due to abusive head trauma in children under two years old. They aggregated admissions to the state-level and analyzed the relationship via difference-in-difference analysis of state-level panels from 1995 to 2013. The paper found that the effect of state EITC programs varies by EITC status: refundable EITCs are associated with reductions in abusive head trauma (3.1 admissions per 100,000 children), but nonrefundable EITCs were not statistically significant.

One issue closely tied to child maltreatment is foster care. Foster care offers temporary or permanent alternative placement for children separated from their families (Font & Gershoff, 2020). Biehl and Hill (2018) assessed the impact of the 2009 American Recovery and Reinvestment Act (ARRA) temporary EITC benefit expansion on state-level foster care entry rates. They used state-level panel data from 2004 to 2014 and a difference-in-difference strategy that compares states with EITC programs after the federal expansion (which, because states typically set their benefits as a percentage of the federal benefit, also led to state benefit expansions) to themselves pre-expansion and to states without EITC programs before and after the federal expansion. That study found EITC expansions led to reductions in foster care entry rates. Another similar study found state refundable EITC is associated with reductions in foster care caseloads of about 11 percent (Rostad et al., 2020).

Those studies empirically test whether EITC has an impact on child maltreatment and related outcomes. Other research also supports the proposed theoretical models linking EITC and child maltreatment. One study used individual-level Pregnancy Risk Assessment Monitoring System data from 1990-2017 to identify impacts of state EITC policy variation on depression and alcohol consumption of birthing persons. That study found increases in state EITC generosity are associated with reductions in maternal alcohol consumption (Morgan et al., 2022).

Averett and Wang (2018) considered how the 1993 expansion of EITC impacted several factors including child health, home environment quality, and child noncognitive skills. The paper used data from the 1979 National Longitudinal Survey of Youth. This survey sampled 12,686 people and followed those people longitudinally through annual and then bi-annual interviews through 2010. The sample frame included data before and after the 1993 EITC expansion, enabling researchers to see the impact of EITC expansion on family incomes for the same families before and after the expansion. The paper found EITC expansion led to improvements in home environmental quality for some children and improved child health rating. EITC is associated with reduced financial distress, also.

One small sample study (n=314) of rural low-income mothers found that non-participation in EITC is associated with greater levels of financial stress (Gudmunson et al., 2010). Another study used Behavioral Risk Factor Surveillance System survey records from 1993-2016 to assess the relationship between state EITC generosity and health indicators (Morgan et al., 2020). Findings indicated increased benefit generosity is associated with reductions in frequent mental distress and reductions in frequent poor physical health (both which might reasonably be expected to reduce maltreatment risk). Another study assessed a pilot program in Chicago – the Earned Income Tax Credit Period Payment Pilot – and its relationship with food insecurity for low-income families (Andrade et al., 2019). That pilot paid out portions of family EITCs periodically over the course of a year, rather than in single lump sums. Periodic payments are hypothesized to reduce food insecurity by helping families to have more consistent incomes; study results indicate the pilot did reduce food insecurity. An earlier study used the National Health and Nutrition Survey to estimate effects of EITC on health outcomes and found it reduced food insecurity and smoking (Rehkopf et al., 2014).

Literature on the EITC indicates that it is associated with improved child and adult health, better family environments, reduced alcohol consumption, reduced food insecurity, and reduced financial stress. EITC receipt directly lifts millions of families above the federal poverty line (Nichols & Rothstein, 2016). Causal studies indicate the EITC reduces child maltreatment. Both theoretical models, family stress and family investment, are supported by the literature, giving good reason to expect the EITC would reduce child maltreatment. Empirical results support that theoretical expectation and show EITC does lead to reductions in child maltreatment.

SNAP

The Supplemental Nutrition Assistance Program (SNAP), previously known as the Food Stamp Program, cost approximately $65 billion in 2019, making it one of the largest means-tested transfer programs in the United States (Congressional Budget Office, 2019). SNAP disburses funds via "largely unrestricted vouchers" to enable recipients to “purchase most foods at grocery stores or other authorized retailers” (Hoynes & Whitmore Schanzenbach, 2016, p. 220). While SNAP does allow for some policy variation by states, the program is fully federally funded (except for administrative costs, of which states pay half), has “universal eligibility” so that anyone who qualifies based on need is eligible, and is countercyclical in that the number of recipients rises during economic downturns (Hoynes & Whitmore Schanzenbach, 2016).

SNAP should reduce maltreatment and especially child neglect risk via both family stress and family investment models, even considering it just as a cash transfer program. Beyond those perspectives, SNAP may have additional implications due to its emphasis on reducing food insecurity and improving nutrition: food insecurity contributes to family stress and increases in family resources probably increase investment in the family. Benefits specifically from increasing food expenditures extend further: families having more resources for food would be expected to lead to them consuming more and better foods (Carlson & Keith-Jennings, 2018), improving parental and child health. Improved parent and child health may decrease maltreatment risk, and increased access to food should also decrease food-related neglect.

Lee and Mackey-Bilaver (2007) wrote the first paper assessing a relationship between SNAP/WIC benefit receipt and child maltreatment. Prior research on WIC and SNAP benefits did examine the effect that WIC and SNAP had on a variety of children’s health outcomes, but theirs was the first paper to specifically look at child maltreatment. That paper used individual-level services records from the Integrated Database on Children’s Services in Illinois 1990 and 1996 and found participation in SNAP was associated with reductions in both child abuse and neglect. That study offers excellent foundational research on the question of whether there is an association between SNAP and reductions in child maltreatment. However, its limited sample – just children in Illinois in the early to mid-1990s – limits the study's generalizability, and the study did not offer a causal identification strategy.

Another study examined the effect of retailers accepting SNAP, rather than SNAP receipt itself. Bullinger, Fleckman, and Fong (2021) examined census-block data in Connecticut from 2011-2015 and found that in large rural areas each additional store accepting SNAP is associated with 4.4 percent reductions in the child maltreatment report rate and 11.3 percent reductions in substantiated child maltreatment, with most of that change driven by reductions in neglect. Beyond its contribution to literature on SNAP and maltreatment generally, which is substantial, one massive contribution from this paper is its identification of a way to see effects of SNAP without considering SNAP recipients or benefits themselves. More SNAP retailers increases the value of SNAP benefits without explicitly increasing SNAP receipt or spending. Changes in the number of SNAP retailers are also less subject to the standard identification problem of increased SNAP benefits/recipients being closely tied to economic downturns.

That identification problem is neatly illustrated by a study of correlations between social service availability/receipt and child maltreatment (Maguire-Jack & Negash, 2016). That study used a sample (n=1053) of parents in one county in Ohio and found that availability of services is negatively associated with maltreatment, but that actual receipt of services is positively associated. One limitation of this study is that its design did not allow the researchers to find the causal effect of receipt of services. Parents seek services because they need them, and so if the factors that create that need cannot be adequately controlled for, the effect of the services may be obscured by the effect of the factors that contribute to need for services. This is an identification problem: if services might have an effect in reducing maltreatment, but factors that contribute to receipt of services are also correlated with maltreatment, then the problem is identifying the effect of services separate from the factors that make families need the services.

In addition to the approach utilized in the SNAP retailer study, another solution is to use policy changes as exogenous shocks to benefit receipt. One policy which might be used to identify causal effects of changes in SNAP enrollment is broad-based categorical eligibility (BBCE), under which “households may become categorically eligible for SNAP because they qualify for a non-cash Temporary Assistance for Needy Families (TANF) or State maintenance of effort (MOE) funded benefit,” (United States Department of Agriculture, 2018, p. 1). SNAP BBCE varies both by state and over time. This approach solves the identification problem outlined above: in the case of BBCE implementation, SNAP receipt may have grown not because more people were necessarily seeking SNAP benefits, but because eligibility criteria expanded. Thus, SNAP receipt growth may have been due largely to an exogenous policy shift – many people qualified who did not previously – rather than shifts in SES or associated need. Because the enrollment change was due to an exogenous factor rather than a socioeconomic one, this makes it feasible to examine the effect of SNAP specifically rather than SES, which SNAP usually correlates with.

While such an approach has not been used before to examine child maltreatment as the dependent variable, taking such an approach is supported by research looking into the effects of state SNAP policy variation on SNAP enrollment, parent and child health, and economic factors (Andrews & Smallwood, 2012; Breck, 2018; Ganong & Liebman, 2018; Gregory, 2014; Han, 2016; Klerman & Danielson, 2011; Mabli et al., 2009; Pender et al., 2015; Pinard et al., 2017; Ratcliffe et al., 2008).

Miller and Morrissey (2017) examined the impact of SNAP on health outcomes for children and adults. Data were drawn from restricted National Health Interview Survey (NHIS) from 2008 to 2014. The paper used an instrumental variables (IV) approach to assess the causal effect of SNAP receipt, instrumenting SNAP receipt with state policy variations and the temporary benefit expansion from the ARRA. It found that SNAP benefit receipt leads to improved health and reductions in foregone medical care. Breck (2018) used state SNAP policy variation and IV techniques, with NHIS data paired with Medicaid claims and expenditure data. One finding was that SNAP receipt leads to lower Medicaid spending and that the effect is strongest in the year benefits are received and one year later (2018).

Two additional papers assessed the impact of SNAP on material hardship and whether SNAP benefits lead to changes in non-food spending. Both articles offer critical insights both supporting the theoretical model for how SNAP might reduce child maltreatment and indicating ways to structure models assessing that relationship. Han (2016) examined the effect of SNAP on material hardships, specifically looking at the impact of broad-based categorical eligibility expansions. The paper used data from the

Survey of Income and Program Participation (SIPP) and the Current Population Survey's Food Security Supplement and difference-in-difference and triple difference approaches. The paper offered several critical results: BBCE increases SNAP enrollment, it reduces non-food hardships, and it reduces food insecurity in households with children. That SNAP decreases nonfood hardships and leads to improvements in food security in households with children support the theory that SNAP might reduce child maltreatment. Reductions in food insecurity and nonfood hardships support the idea that SNAP decreases family stress and increases family investment, and that SNAP might directly reduce child neglect by providing resources for food. Kim (2016) also found SNAP increases spending on food and frees up resources for mortgage payments, rent, utilities, and transportation.

SNAP also reduces food insecurity. While this is intuitive given that SNAP provides resources to purchase food to people with low incomes, supporting that conclusion empirically is valuable. Ratcliffe and McKernan (2010), using data from the SIPP in 1996, 2001, and 2004 and an IV approach, found SNAP receipt reduces the probability of being food insecure by 30 percent and very food insecure by 20 percent. Food insecurity may be directly related to child maltreatment insofar as food insecurity leads to food neglect, a subtype of neglect. Beyond that relationship, food insecurity may also contribute to child maltreatment risk more generally by contributing to family stress.

Multiple studies suggest food insecurity contributes to child maltreatment. One study analyzed Early Childhood Longitudinal Study-Birth Cohort data and found "the predicted probability of early childhood exposure to violence and/or victimization in the home is nearly 6 times greater in persistently food-insecure households" (Jackson et al., 2018, p. 756). Another study, analyzing data from the Longitudinal Studies on Child Abuse and Neglect, found increases in household food insecurity were associated with increased risk of physical abuse (Helton, 2018). Analyses of data from the FFCW also showed that food insecurity is associated with increased psychological and physical aggression (Helton et al., 2019).

In summary, findings from the literature support the theoretical models behind the relationship between SNAP and child maltreatment. Food insecurity contributes to maltreatment risk and SNAP directly reduces food insecurity. SNAP also reduces material hardships and family stress, increases nonfood spending, and improves adult and child health. These support both family stress and family investment models theoretically linking SNAP to reductions in child maltreatment. While studies on the relationship between SNAP and child maltreatment are more limited, available analyses show SNAP is associated with reductions in child maltreatment, especially neglect. Future research should utilize causal methods as implemented in other studies on the impact of SNAP to identify whether SNAP leads to reductions in child maltreatment.

Medicaid

Like EITC and SNAP, Medicaid is a means-tested program. It provides government funded health insurance to people with low incomes and certain other qualifying conditions. Medicaid is financed jointly by the federal and state governments and is administered by the states. In 2019, Medicaid provided coverage to about 72 million people and cost about $593 billion, of which 62.5 percent was federally funded and 37.5 percent was state funded (Rudowitz et al., 2019). Medicaid's antipoverty effects are "comparable to the combined effect of all nonhealth social insurance programs and greater than the effects of means tested benefits and of refundable federal tax credits" (Remler et al., 2017), suggesting Medicaid may be the largest and one of the most effective antipoverty programs in the United States. When considering effects specifically for children, Medicaid reduces the poverty rate by 5.3 percentage points, comparable to other means tested programs but slightly smaller than the effect of tax credits (Remler et al., 2017).

Medicaid is hypothesized to reduce child maltreatment in several ways. First, as an effective antipoverty program (Remler et al., 2017; Zewde & Wimer, 2019), it may operate via both family stress and family investment models. Since consumption of health care is somewhat inelastic by both price and income (Ringel et al., 2002), Medicaid should offset the cost of medical care (Levy et al., 2019), which would be expected to de facto increase family discretionary incomes (Coleman et al., 2002; Zewde & Wimer, 2019). Higher discretionary income should both reduce financial stress and allow caregivers to invest additional resources in the family.

Beyond those income effects, Medicaid also increases access to and consumption of health care (Mazurenko et al., 2018). For children, more visits to health care providers means increased opportunity for caregivers to be referred to preventive programs or other resources (Flaherty & Stirling, 2010; Mayo Clinic, 2015), as well as increased exposure to mandatory reporters of child maltreatment, so instances of maltreatment may be detected earlier and referred to preventive services or to Child Protective Services as appropriate (Child Welfare Information Gateway, 2015; Flaherty et al., 2000, 2006, 2008). For parents, greater access to health care might mean more behavioral health treatment or better mental health (Baicker et al., 2013; Mark et al., 2015), and/or improved substance abuse treatment (Gertner et al., 2020; Grooms & Ortega, 2019; Wells, 2009).

A few papers have empirically tested the relationship between health insurance and child maltreatment. Miyamoto et al. (2017) examined individual level CPS cases from a county in northern California. Their data did not include income or socioeconomic status, so eligibility for Medi-Cal, California's Medicaid program, was used as a proxy for poverty. The paper found that children who were eligible for Medi-Cal had lower odds of being seriously maltreated compared to children who were not eligible and noted that Medi-Cal might improve health care access for recipients. Thurston et al. (2017) used a similar dataset from a single large county in California and found Medi-Cal eligibility shows a negative association with risk of becoming a child maltreatment case. The paper suffers from the same limitation as Miyamoto et al. with regard to SES: that is, the paper uses Medi-Cal eligibility as a proxy for poverty, and as such cannot separate the effect of Medi-Cal eligibility from its socioeconomic implications. McCray (2018) assessed the correlations between health care coverage, including Medicaid and private coverage, and child physical abuse. The paper used state-level panel data and linear regression methods and finds an association between both private health insurance and Medicaid coverage and reductions in physical abuse. That paper also did not include a causal estimation strategy.

Several papers assessed the degree to which children involved in the child welfare system maintain health care coverage (Raghavan et al., 2008, 2009, 2016). These papers used a variety of approaches to examine the extent of health care coverage for child welfare-involved children and whether children maintain health care coverage over time. Another important line of research has identified an association between access to health care for children and child maltreatment. Stockwell, Brown, Chen, and Irigoyen (2007) found that children who are two years old or younger and lacking a primary care provider are 4 times more likely to be maltreated compared with children with a provider and Stockwell, Brown, Chen, Vaughan, and Irigoyen (2008) found that underimmunization was associated with a four times greater likelihood of being abused physically. The results from these two papers offer additional support to the argument that factors that determine a child's likelihood of having access to health care may also impact the likelihood that the child is maltreated, though it is important to note that the studies are not causal and do not establish the direction of the observed relationship. As such, reverse causality might well be involved: children who are maltreated may be less likely to be taken to the doctor, potentially for fear of detection of the maltreatment.

As with EITC and SNAP, studying the impact of Medicaid on child maltreatment outcomes is complicated because factors that influence eligibility for Medicaid, such as low SES, also influence child maltreatment risk. Estimating the impact of Medicaid on child maltreatment separately from correlations with SES requires an identification strategy. One strategy used in a number of studies on Medicaid is to exploit variation in state decisions to expand Medicaid eligibility via the Patient Protection and Affordable

Care Act (ACA). The ACA originally required all states to expand Medicaid, but a U.S. Supreme Court decision in the case of the *National Federation of Independent Business v. Sebelius* made Medicaid expansions optional for states. Because some states elected to expand and others did not, this resulted in a natural quasi-experiment with some treated and some not treated states. While often considered primarily to be expansions for childless adults, ACA Medicaid expansions also affected parents – one estimate suggests reductions between 4.5 and 9.8 percent in uninsured rates for low income parents (McMorrow et al., 2017). Medicaid expansion for adults also appears to have had strong welcome mat (aka woodwork) effects on coverage for children (Hudson & Moriya, 2017).

Several recent studies examined the impact of Medicaid expansions on child maltreatment. Pac (2019) examined the relationship between Medicaid coverage for adults and child maltreatment, assessing the impact of California's early Medicaid expansion on child maltreatment cases using county-level monthly panel data. That approach included a causal estimation strategy and found that the early expansion led to an 11 percent decrease in child physical abuse, though it detected no effect on neglect. Because the paper considered only California's early Medicaid expansion, and Medicaid programs varied substantially by state, the study's generalizability to Medicaid expansions in other states may be limited. Brown et al. (2019) examined the impact of state Medicaid expansion decisions on child physical abuse and neglect and found an association between expansion and reductions in neglect, though not physical abuse. That paper used state-year level maltreatment data from 2010 through 2016 and performed difference-in-difference (DiD) analysis to compare expansion states to themselves pre-expansion and to non-expansion states. McGinty et al. (2022) used state-year panel data (2008-2018) to examine the impact of state Medicaid expansion decisions on child physical abuse and neglect reports, and found an association between expansion and reductions in neglect reports and a statistically insignificant reduction in physical abuse. Assini-Meytin et al., (2022) used a similar dataset and analytical approach as McGinty et al., (2022) and found no association between ACA Medicaid expansions and child sexual abuse.

Medicaid has strong direct antipoverty effects. It reduces uncertainty related to health care expenditures, decreases family health care spending, increases family discretionary incomes, and reduces family financial stress and increases family investment. It leads to greater access to and utilization of health care by both adults and children, which might prevent child maltreatment for several reasons. These findings in the literature support the hypothesis that Medicaid reduces child maltreatment risk. Available empirical analyses of that hypothesis support the conclusion that Medicaid leads to reductions in child maltreatment.

TANF

Temporary Assistance for Needy Families (TANF) replaced Aid to Families with Dependent Children (AFDC) after the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (commonly referred to as welfare reform). AFDC was a countercyclical entitlement program whose “federal open-ended obligation to states… rose and fell with the health of the state’s macroeconomy” funded via federal-state matching grant (Ziliak, 2016, p. 313), similar to Medicaid. AFDC "provided income support in the form of cash assistance to families with children" (Center on Budget and Policy Priorities, 2022, p. 1). In contrast to AFDC, TANF is funded via federal block-grants to states and states can choose how to design and operate their programs using those funds. TANF established work requirements and attempts to incentivize two-parent families, among other changes. In 2019, total federal and state TANF and MOE spending was about $30.9 billion, of which 21.1 percent was for basic assistance, 10.5 percent for job training, and 16.3 percent for child care (Office of Family Assistance, 2020).

As a form of income support, AFDC/TANF would be expected to reduce maltreatment risk via both family stress and family investment theories. If TANF successfully encouraged employment or two-parent families, there might be additional effects in reducing maltreatment risk, as both unemployment and single-parent families have been identified as risk factors for child maltreatment (Sedlak et al., 2010). In a study of the effect of income and related programs on child maltreatment, Berger (2004) found an association between more generous AFDC/TANF and SNAP benefits and increased dental care and reduced spanking of children.

However, since TANF is a much more restricted program compared to AFDC, the transition from AFDC to TANF presents an opportunity for researchers to assess the effect of reductions in benefit generosity, as does TANF's more substantial by-state policy variation (Maguire-Jack et al., 2021). Examinations of state-level panel data suggest more generous benefits are associated with lower foster care rates (Paxson & Waldfogel, 2002, 2003), and additional restrictions on benefit receipt are associated with increases in maltreatment (Paxson & Waldfogel, 2003). Another study using data from the FFCW found that increases in TANF benefits are associated with reductions in physical abuse and time limits for TANF receipt are associated with increased physical abuse (Spencer et al., 2021).

Analyses of TANF programs in individual states and localities find similar results. Shook (1999) found that reductions in welfare grants for people in Chicago, Illinois who were unemployed led to increased risk of involvement with the child welfare system, compared to people who were employed. Another study in Illinois found TANF sanctions increase child neglect reports (Slack et al., 2007). In Delaware, single parents subject to welfare reform changes (compared to randomly assigned cases not subject to welfare reform) had increased risk of child neglect (Fein & Lee, 2003). In Maryland, the Life After Welfare study observed 8900 families exiting TANF; that study found longer history of welfare receipt among families leaving TANF to be associated with increased risk of substantiated child maltreatment report after exiting TANF (Ovwigho et al., 2003). In Wisconsin, applicants to TANF in 1999 had higher rates of CPS investigations compare to applicants to AFDC in 1996 (Courtney et al., 2005); explanations offered by researchers include: substantial declines in public assistance after welfare reform meant people applying in 1999 faced substantially higher levels of family stress compared to earlier applicants, or potentially that welfare reform "made life more difficult for very low-income families" (p. 150). In Ohio, a study using administrative data of public benefit receipt and child welfare found families forced to exit TANF had increased risk of substantiated child maltreatment (Beimers & Coulton, 2011). In Missouri, a study examined duration of time of families on AFDC/TANF and Medicaid controlling for neighborhood poverty level and race and found increased time on AFDC/TANF and Medicaid is associated with increased risk of child maltreatment report (H. Kim & Drake, 2017). In Arizona, reductions in the amount of time families can receive TANF benefits is associated with increased risk of child neglect (Albert, 2017).

Cancian, Yang, and Slack (2013) assessed how changes in TANF policy and associated child support payment changes influence probability of maltreatment. In Wisconsin in the late 1990s, the state TANF program was structured to randomly assign some unmarried mothers to receive a full pass-through of child support payments and for other mothers to have TANF payments reduced based on child support payments – a partial pass-through. Random assignment of treatment and control groups allowed researchers to assess the causal impact of the full pass-through of child support payments on a variety of outcomes, including child maltreatment. This paper tracked 13,062 mothers for two years from the start of the experiment and found that full-pass through of child support payments reduces the risk of child maltreatment.

Available empirical research examining the relationship between TANF and child maltreatment shows that there is a strong and consistent association between more generous TANF benefits and reductions in maltreatment, including both reports and substantiated cases. The transition from more generous AFDC to less generous TANF is associated with increased child maltreatment risk.

Minimum wage

While not a means tested transfer program, another policy with implications for family incomes that might reasonably be expected to also influence child maltreatment is the minimum wage. The federal government establishes a minimum wage, though states and localities can set minimum wages higher than the federal one.

If the minimum wage is an effective antipoverty policy, then it might reduce child maltreatment risk via family stress and family investment theories. However, whether the minimum wage has an antipoverty effect is debated both theoretically and empirically. Theoretical economic models suggest that minimum wage increases the price of labor and correspondingly should decrease the employment of low wage workers. Empirical analyses of the relationship between minimum wages and employment are less conclusive. Studies from Card and Kreuger find, contrary to both standard economic theory and prior empirical work, that increases in minimum wages led to either no impact on employment or slight increases (Card & Krueger, 1994, 1995b, 1998). Other research in that series found that increases in the minimum wage led to increases in lower percentile wages, lower income inequality, and reductions in poverty (Card & Krueger, 1995a). More recent studies continue that empirical ambiguity. Neumark et al., (2014) found that the minimum wage does impose some level of reductions in employment, whereas another study, reviewing research post-2000, concluded that "the weight of evidence [on the minimum wage] points to little or no employment response to modest increases" (Schmitt, 2015, p. 547). Elaborating on this controversy, another paper pointed out that estimating aggregate effects of the minimum wage across the United States masks treatment effect heterogeneity by labor market concentration (Azar et al., 2019). Setting aside the empirical question of whether the minimum wage affects labor market outcomes, additional evidence suggests it does increase wages for low SES workers and may also increase incomes enough to raise some workers out of poverty (Addison & Blackburn, 1999; Bernstein & Shierholz, 2014; Stevans & Sessions, 2001).

If the minimum wage raises incomes and does not reduce employment of low wage workers, it would be reasonable to conclude the program has antipoverty effects and that the minimum wage should decrease family financial stress and increase resources available for family investment; both would suggest increases in the minimum wage might decrease child maltreatment risk, especially child neglect.

Three studies assess whether the minimum wage might affect child maltreatment or relevant parenting behaviors. Raissian and Bullinger (2017) considered quarterly state-level panel data from 2004 to 2013 to assess how state minimum wage policies affect child maltreatment rates. They found that "a $1 increase in the minimum wage implies a statistically significant 9.6% decline in neglect reports" (Raissian & Bullinger, 2017, p. 60). A paper using data from the FFCW found no statistically significant association between changes in minimum wages and self-reported child maltreatment, though it did find a small effect of reductions in self-reported neglect in one specification (Livingston et al., 2021). A separate study using the same dataset found increases in the minimum wage lead to reductions in spanking by mothers and fathers and reductions in physical and psychological aggression from mothers (Schneider et al., 2021).

Limitations

The greatest limitation to the theory that antipoverty programs should lead to reductions in child maltreatment risk is that most research into these topics has historically tended to examine statistical associations but not offer causal identification strategies. While many studies have suggested correlations between increases in antipoverty programs and reductions in child maltreatment risk, those studies have tended to be subject to the identification problem described above: that there are often confounding factors – like economic downturns – that predict both increased antipoverty program enrollment or benefit receipt and increased child maltreatment risk. Available non-causal research suggests increases in income are generally associated with reductions in maltreatment risk, especially neglect. When considering specifically antipoverty programs, the literature suggests that policies which increase family discretionary incomes are associated with reductions in family stress and increases in resources available for family investment; both would imply decreased child maltreatment risk. Understanding whether these observed correlations indicate a causal effect of antipoverty programs on child maltreatment is more complicated.

However, a growing body of recent research exploits natural policy variation, newer methods allowing for causal identification, and some instances of random assignment to identify causal effects of antipoverty programs. Available causal research finds increased incomes leads to reductions in child maltreatment. Future research should continue the recent trend of applying causal identification strategies to find causal effects of antipoverty programs on child maltreatment.

Discussion and conclusion

Two theories explain why poverty might increase the risk of child maltreatment: family stress and family investment. In these theories, factors that reduce family incomes should increase family stress and diminish resources available for investment in the family; both should increase child maltreatment risk. Insofar as programs have successful antipoverty effects, they should diminish those effects and reduce child maltreatment risk. Studies into particular antipoverty programs, including the EITC, SNAP, Medicaid, and TANF, and other policies including the minimum wage and tax law, provide support to those predictions, though only more recent studies estimate robust causal effects rather than correlations.

The preponderance of the evidence, including both associative and causal results, suggests that socioeconomic status and poverty are inextricably linked to child maltreatment risk. Antipoverty programs can raise family discretionary incomes, reduce family stress, and increase resources available for family investment. Particular programs may have additional effects, including reducing food insecurity, reducing uncertainty related to family health care expenditures, increasing access to health care, and generally making families physically and emotionally healthier. As a result, there is good reason to expect that more generous antipoverty programs can reduce child maltreatment.

2. THE IMPACT OF AFFORDABLE CARE ACT MEDICAID EXPANSIONS ON CHILD MALTREATMENT

Abstract

Hundreds of thousands of children each year are maltreated in the United States, according to counts of substantiated cases; millions more children are subject to reports of physical abuse, neglect, or other types of maltreatment; and further children experience unreported and uncounted cases of maltreatment. One factor that may reduce risk for maltreatment is health care coverage, either by improving socioeconomic status or by increasing exposure to health care providers. This study uses variation in state Medicaid expansion decisions to identify the causal effect of publicly funded health insurance on child maltreatment outcomes through event study and difference-in-differences frameworks. While this paper finds some evidence of a reduction in child neglect from January 2014 Medicaid expansions, in line with prior literature on this topic, these findings do not hold when early and late Medicaid expansion states are included. Results also show reductions in physical abuse and increases in medical neglect that are consistent across expansion specifications but which are both imprecisely estimated and sensitive to model specification.

Introduction

Child maltreatment is an umbrella term which encompasses improper treatment of children by caretakers, typically understood to include physical abuse, sexual abuse, neglect, and psychological or emotional maltreatment (National Research Council, 1993, Chapter 1). The federal government establishes certain minimum thresholds for definitions of maltreatment categories, and states can expand their own definitions beyond those federal minimums. In 2018, there were an estimated 3,960,823 reports of child maltreatment in the United States; of those, 677,529 children were found to be victims of substantiated cases of child maltreatment, including 411,969 cases of neglect and 72,814 cases of physical abuse (U.S. Dept. of Health and Human Services, Administration for Children and Families, Administration on Children, Youth and Families, Children's Bureau, 2020). Substantiated cases are almost certainly an underestimate for actual incidence of maltreatment, because this count includes only cases which were both reported to Child Protective Services (CPS) and found to be substantiated, but many cases of maltreatment are not reported (Sedlak et al., 2010) and are not counted in this measure. In addition to the immediate pain and suffering that every case of maltreatment represents, there are substantial long-term costs as well. Children who suffer maltreatment will spend the rest of their lives at a higher risk for a host of adverse health effects and chronic diseases including, but not limited to, heart disease, obesity, high blood pressure, and cancer (Gilbert et al., 2015; Danese et al., 2009; Felitti et al., 1998). The deleterious consequences go beyond physical symptoms: children who suffer maltreatment are also at higher risk for low academic achievement, abuse of illicit substances, alcoholism, juvenile and adult criminality, and a variety of psychological disorders (Chapman et al., 2004; Felitti et al., 1998; Kisely et al., 2018; Lansford et al., 2002; Silverman et al., 1996). The average lifetime cost associated with each case of child maltreatment, when considering long-term impacts, amounts to hundreds of thousands of dollars in economic losses for society (Fang et al., 2012).

Understanding the precise causal mechanisms behind child maltreatment can be difficult for a variety of reasons, including its relatively uncommon and deviant nature, the way many complex factors interact to influence risk, and variations in understandings of what constitute child maltreatment across both time and place (National Research Council, 1993, Chapter 4). To address these issues, researchers have developed the etiological-transactional model (ET), which “suggests that a broad set of causal and contributing factors is involved, including not only the presence of certain risk factors, but also the absence of protective or positive assets that can prevent the occurrence of abuse and neglect” (Chalk, 2012, p. 148). Socioeconomic status (SES) is a contributing risk factor for child maltreatment (National Research Council, 1993, Chapter 4) in the ET model, which will be described in more detail below. According to the fourth National Incidence Study on Child Abuse and Neglect (NIS-4), which was “the largest epidemiological study to date designed to measure actual child maltreatment in the United States” (Drake & Jonson-Reid, 2013, p. 133), low SES is associated with 3 times greater risk for abuse and 7 times greater risk for neglect (Sedlak et al., 2010).

Medicaid is a means-tested program which provides government funded health insurance to more than 66 million people in the United States (Centers for Medicare &

Medicaid Services, 2018). The program, jointly funded by the federal and state governments, cost over $592 billion in federal fiscal year 2018 (Kaiser Family Foundation, 2019). As a program which provides health insurance to those below certain income thresholds, Medicaid might be considered an antipoverty program (Zewde & Wimer, 2019). As such, Medicaid might thus function as a protective factor within the etiological-transactional model, reducing the likelihood of child maltreatment. This paper first presents a theoretical model for a relationship between Medicaid and child maltreatment. Second, it attempts to assess that causal relationship empirically by using state variation in Medicaid expansion decisions to estimate difference-in-difference and event study models with county-level administrative data from the National Child Abuse and Neglect Data System (NCANDS).

Theoretical model of health insurance and child maltreatment

The etiological model of child maltreatment considers four levels of factors that might influence maltreatment risk: individual, family, community/environment, and culture (National Research Council, 1993, Chapter 4). Individual factors include child and parent factors like personality, disability, alcohol/drug consumption, and age. Family factors include family structure, relationships, income/poverty, and unemployment. Community factors would include factors like neighborhood characteristics, which could also include socioeconomic characteristics. Culture would include factors like broader cultural values. This paper considers the effect of health insurance on maltreatment at primarily the family level. Family SES, including income, poverty, unemployment, and low educational attainment, is considered a risk factor for maltreatment, and Medicaid is proposed as a protective factor that might mitigate maltreatment risk. Two theories explain why low SES would lead to increased child maltreatment risk: family stress and family investment. The family stress model (Conger, 1994; Conrad et al., 2020; Maguire-Jack et al., 2021; Warren & Font, 2015) posits that economic stress harms caregiver mental and behavioral health, which can lead to inhibited capacity for caregiving. The family investment model (Conrad et al., 2020; Maguire-Jack et al., 2021; Warren & Font, 2015) posits that family receipt of economic support, such as from antipoverty programs, allows caregivers to invest additional resources into their families, reducing maltreatment risk.

As will be discussed in the methods section, this paper's primary focus is on Medicaid expansions for adults. This theoretical model will primarily consider adult coverage, though additional corollary effects on child coverage will also be discussed. There are a variety of reasons that adult Medicaid coverage could be expected to reduce child maltreatment risk. First, coverage improves SES by increasing discretionary incomes, which in turn may reduce child maltreatment risk. Because demand for health care is relatively inelastic, both by price and by income (Ringel et al., 2002), families consume some health care regardless of their incomes. Health care coverage offsets the cost of care, freeing up resources and increasing discretionary income. This is borne out empirically: "families with uninsured members are more likely to have high health expenditures as a proportion of family income than are insured families" (Coleman et al., 2002, p. 1). Medicaid's effect of increasing discretionary incomes is also confirmed by Zewde and Wilmer, who found that "the program's antipoverty impact grew over the past decade independent of expansion, by shielding beneficiaries from growing out-of-pocket spending" (2019, p. 132). Increased discretionary incomes should correspond with reduced family financial stress, which could mean reductions in child maltreatment risk.. Note that while there are good reasons to think Medicaid would effectively increase family incomes, there are some factors that might temper that expectation: Medicaid is not cash-equivalent. While the program may protect families from uncertainty of health care costs, unless families are actively using care and Medicaid offsets spending on that care, the income effect may not be very strong. There is also good reason to believe many people who received Medicaid via state expansions were receiving uncompensated care in pre-expansion periods (Callison et al., 2021; Dranove et al., 2016; Moghtaderi et al., 2020), which might also reduce the income effect presented here.

Second, health care coverage increases access to and affordability of health care (Nyman, 1999), which increases health care utilization (Buchmueller et al., 2005; Larson et al., 2016). Medicaid coverage for adults can lead to increases in parents using health care services and improvements to parent mental and physical health, each which might decrease stress (Currie & Madrian, 1999) or improve parenting in ways that make maltreatment less likely. It could also lead to increased substance abuse treatment, which would address a strong risk factor for maltreatment (Wells, 2009); each of these components could be included in the health care access side of the theoretical model, though they might also have implications for SES as well.

Medicaid expansion for adults also appears to have had strong welcome mat (aka woodwork) effects on coverage for children (Hudson & Moriya, 2017). Given that expanded coverage for adults also increases coverage for children, consideration of effects of child coverage is also appropriate. Similar income effects would be expected, and increases in utilization of health care for children are also expected.

Higher levels of coverage for children would mean great utilization of child health care resources, in addition to greater utilization of health care for adults. Increased utilization for children implies greater exposure to health care providers and, given that providers are trained to identify cases of maltreatment and children at risk of future maltreatment, could mean parents get more education on child development and referrals to preventive programs when appropriate (Flaherty & Stirling, 2010; Fussell, 2011; Gwirtzman Lane, 2014; Mayo Clinic, 2015; National Association of Children’s Hospitals and Related Institutions, 2011). Further, because all health care providers are mandatory reporters of child maltreatment (Child Welfare Information Gateway, 2015), greater exposure to the health care system means children are also exposed to more mandatory reporters of child maltreatment. This could address maltreatment retroactively, because providers can identify and report cases of maltreatment and those cases can be investigated and dealt with as necessary. They could also proactively prevent future maltreatment: if providers detect maltreatment that has occurred previously, parents can be referred to preventive services to make future maltreatment less likely, or children can be removed from the home if necessary (Brenzel et al., 2007; Flaherty et al., 2000, 2006, 2008; Herendeen et al., 2014; National Research Council, 1993, Chapter 4).

This theoretical model advances the perspective that Medicaid, by reducing financial stress and increasing access to and utilization of a variety of health care services, should operate as a protective factor reducing maltreatment risk. However, it is important to keep in mind that the different types of maltreatment may each have their own separate etiologies. For example, SES impacts child neglect risk to a higher degree than it impacts child physical abuse risk. The differences in these factors will be considered, as appropriate, in the discussion of effect sizes and significance in the results. It is also important to acknowledge that SES is not just associated with actual risk for maltreatment, but may also be associated with increased risk of reporting suspected maltreatment or with increased risk of substantiating maltreatment that has been reported; both issues would complicate the observed relationship between SES and maltreatment.

Methods

Empirical approach

Understanding a potential causal relationship between Medicaid and child maltreatment requires more than checking for an association between Medicaid enrollment and reductions in maltreatment. Such associations are subject to an identification problem: factors that influence Medicaid enrollment will also tend to impact child maltreatment risk. One example is economic fluctuations: in recessions, with rising unemployment there will be surges in Medicaid enrollment. Rising unemployment also implies higher levels of stress for families, which may make maltreatment more likely as well. Economic booms might have opposite effects: reduced Medicaid enrollment as employment increases and reduced family stress and thus maltreatment risk. The goal of causal analysis is to solve this identification problem by finding a way to measure the impact of Medicaid on maltreatment independent of the other factors that might influence both simultaneously.

One solution to this approach is to find a change in Medicaid enrollment that would not be expected to have any impact on maltreatment, other than via the proposed theoretical model. In the case of Medicaid, such exogenous variation may be found in the form of Medicaid expansion decisions made possible by the Patient Protection and Affordable Care Act of 2010 (ACA) and the subsequent Supreme Court decision in the case of the *National Federation of Independent Business (NFIB) v. Sebelius*. Prior to the passage of the ACA, states set their own income thresholds for Medicaid eligibility, and the ACA originally required all states to expand their Medicaid income thresholds to 138 percent of the federal poverty line. The *NFIB v. Sebelius* decision rendered Medicaid expansion effectively optional for states. Because states could choose to expand, or not, this set the ground for a natural experiment comparing the effects of expanded Medicaid eligibility: expansion states saw a sudden increase in Medicaid enrollment as a new group of low-income adults gained eligibility, while non-expansion states did not show such increases (Courtemanche et al., 2017; S. Miller & Wherry, 2017).[1]

Using variation in state Medicaid expansion decisions in this way requires the assumption that expansion decisions are exogenous to child maltreatment outcomes; i.e. that expansion decisions are not influenced by maltreatment outcomes or unobserved factors that influence maltreatment outcomes. While there is no single statistical test that

[1] Difference-in-differences analyses with the rate of uninsurance for adults under 138 FPL as the dependent variable find that Medicaid expansion led to between 5.5 and 7.5 percentage point reductions in uninsurance for adults in expansion counties post-expansion.

can conclusively demonstrate exogeneity, there are good reasons to believe Medicaid expansion is plausibly exogenous. The strongest concern with exogeneity in this case regards state socioeconomic factors. As established previously, socioeconomic factors have strong implications for maltreatment outcomes. If they also influence selection into expansion, for example if richer states choose to expand and poorer states choose not to expand, then it might be the case that the detected effect due to expansion might actually be due to state economic factors. However, there are three reasons why this is not a substantial concern in this analysis. First, the federal government bears the lion's share of the financial burden with regard to Medicaid expansion. Initial expansions were generally covered by the federal government at 100 percent, with decreases to 90 percent coverage by 2020. Second, the factor that most strongly explains Medicaid expansion decisions has been identified as political leaning of state governments, rather than state economic factors (Barrilleaux & Rainey, 2014; Henley, 2016; White, 2021).[2] Third, the primary concern would be if the characteristics that influence expansion decisions and maltreatment are unobserved; in this case, state economic and political factors are observed characteristics and thus can be included in models;[3] if included explicitly, their effects should not be included in the error term.

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[2] While not the primary focus of this paper, cursory examination of determinants of expansion decisions via logistic regression (where the dependent variable is the decision to expand Medicaid in 2014) do not show a statistically significant association between state expansion decisions and state unemployment, poverty rate, or gross state product. Strongest single factor predicting Medicaid expansion was Democratic control of the lower house of the state legislature (1% higher control = 0.68% higher probability of expansion, $p < .1$).

[3] This paper's preferred estimator (doubly-robust Callaway-Sant'Anna difference-in-difference) features both stabilized inverse probability weighting and outcome regression adjustment. Because propensity scores close to zero may inhibit performance of probability weighting, only a small number of variables are included in this approach. Additional controls, including a wide array of economic,

This paper will use state Medicaid expansion decisions as a source of exogenous variation to identify the impact of Medicaid on child maltreatment. Because states did not all expand Medicaid simultaneously, this is an example of staggered treatment timing, which has been identified recently in econometric literature as challenging standard difference-in-difference approaches in some situations. Specifically, standard two-way fixed effects (TWFE) models with staggered treatment timing are a weighted average of all possible 2x2 DiDs based on treatment timing. Standard approaches may include a number of inappropriate 2x2 DiDs and also yield negative weights; these and other findings challenge traditional interpretations of TWFE with staggered treatment timing (Baker et al., 2021; Borusyak et al., 2021; Caetano et al., 2022; Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfoeuille, 2021; Gardner, 2021; Goodman-Bacon, 2021; Roth et al., 2022; Sant'Anna & Zhao, 2020; Sun & Abraham, 2020).

To address this issue with TWFE, this paper's preferred estimator is the doubly-robust Callaway-Sant'Anna (C-S) difference-in-difference with both stabilized inverse probability weights and outcome regression adjustment and using both never- and not-yet-treated units as controls (Callaway & Sant'Anna, 2021; Rios-Avila et al., 2021; Sant'Anna & Zhao, 2020). The C-S approach checks for which 2x2 DiDs are appropriate to run and then estimates average treatment effects for units grouped by timing of first treatment. When covariates are included, the approach 1) calculates a time-varying propensity to treatment conditional on base-period values of included covariates and uses

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demographic, and political covariates, are included in alternative estimators (Gardner two-stage DiD, TWFE, synthetic controls) which are used as robustness checks.

that propensity score to create a stabilized inverse probability weight, then 2) calculates a residual based on the outcome in each time period, adjusted with an outcome regression, and 3) uses the stabilized inverse probability weights from (1) to weight the residuals from (2). The average treatment effect for each group-time can then be aggregated to either one average treatment effect (for all groups over all post-treatment times, analogous to a standard DiD term) or to one treatment effect for each time period relative to treatment (an event study).

This study will consider aggregated DiD and event study results from C-S DiD to compare differences in child maltreatment counties in Medicaid expansion states to differences in counties in both non-expansion states and counties in states that had not yet expanded.

Child maltreatment outcomes are measured as maltreatment in each county-quarter where maltreatment includes the log-transformed count of reports for physical abuse, neglect, medical neglect, and sexual abuse. Primary specifications of C-S DiD include four covariates – county poverty rate, percent of county population that is white, percent of adults with high school education or higher in each county, and county child population count. Additional checks using alternative methods – two-stage DiD (Butts & Gardner, 2021; Gardner, 2021; Thakral & Tô, 2020) and standard TWFE – also include a wider array of covariates, including the percent of county population below federal poverty level (FPL), county median income, percent of adults in the county who are married, percent of adults in the county with high school education or higher, county unemployment rate, county income inequality as measured by the Gini coefficient, rate of primary care providers per 10,000 people in the county, percent of the county population that is white, and the county child population. State-year variables in those additional checks include whether the governor is a Democrat, the percent of state upper and lower legislative chambers which are Democratic, state unemployment rate, state poverty rate, and gross state product.

Maltreatment counts are log-transformed to reduce the influence of outliers and to ease interpretation of resulting coefficients. While both reports and substantiated cases could be used as measures for incidence of child maltreatment, reports are preferable for three reasons: first, children subject to maltreatment reports are at a similar risk for future incidence of maltreatment regardless of substantiation of the current case (H. Kim et al., 2017; Chalk, 2012; Fallon et al., 2010; Kohl et al., 2009; Hussey et al., 2005). Second, substantiation can vary for reasons unrelated to risk in a particular case (Jones & Finkelhor, 2001). Third, reports are often used as a better measure of the actual incidence of maltreatment than are substantiated cases (H. Kim et al., 2017). While reports do include cases which are ultimately found not to constitute substantiated cases of maltreatment, this metric avoids issues with arbitrariness in substantiation standards and may be a more accurate reflection of actual maltreatment incidence (Bullinger, Lindo, et al., 2021).

Determining which states are considered to have expanded Medicaid, and which states have not, is a critical question. Resources such the Kaiser Family Foundation list out states which have formally expanded Medicaid via the ACA (Kaiser Family Foundation, 2020); however, formal acceptance of the ACA's expansion provisions is an incomplete accounting of the complexity of Medicaid expansion. Twenty-four states, including the District of Columbia, formally expanded Medicaid via the ACA on or before January 2014; three other states expanded later that year, and twelve more have expanded since. The ACA also gave states flexibility to expand Medicaid eligibility prior to 2014. Specifically, eighteen of the twenty-seven 2014 expansion states had some form of expanded eligibility prior to 2014, including ten states with Medicaid eligibility above 100 percent of the FPL or state programs that covered people over that threshold (Anand et al., 2019; Courtemanche et al., 2017). Selecting which states to consider as expanders, and when, has important implications for analysis.

Inclusion of early expansion decisions as treatments could capture the effect of early Medicaid expansions, but assigning a bright line distinguishing which early expansions were sufficiently large to count as treatments for this study, and which were not, could be somewhat arbitrary. If the study does not count early expansions as treatments, early expansion states can either be left in the control group (considered as non-treated) or excluded from the analysis altogether. Leaving them in the control group means the analysis would only consider the impact of 2014 and later expansions, and any detected effect size would not include the effects of early expansions, which might mean underestimation.

An additional complication to the question of how to measure the impact of Medicaid expansions relates to the generosity of their Medicaid benefits. States with more generous Medicaid benefits pre-expansion might be considered to have relatively lesser impact from Medicaid expansions relative to states with less generous benefits which also expanded Medicaid. I conducted additional specifications of my model (not shown) that assessed the impact of Medicaid expansion in a triple-differences framework where counties with higher levels of uninsurance are compared to counties with lower levels of uninsurance. The results from the triple differences analysis produce the same conclusions as the primary specification.

While this study does not account explicitly for Medicaid benefit generosity, Also, while some states may have more generous programs, all states must have benefits that meet certain federal minimum standards, including covering pediatric and family nurse practitioners, federally qualified health centers, inpatient and outpatient services, and labs (Medicaid and CHIP Payment and Access Commission, 2022). Many of the primary benefits from Medicaid improving access to care should fall under primary care services, which is one such mandatory benefit. Further benefit generosity might increase the value of Medicaid to enrollees, but also probably has diminishing returns when considering specifically reductions in child maltreatment risk.

This paper will consider several different specifications of Medicaid expansion and consider implications of results for each. Specifications considered in this paper are adapted from several prior papers assessing Medicaid expansion effects, including Courtemanche (2017), Miller and Wherry (2017), Anand et al., (2019) and McGinty et al. (2022). Table 2.1 outlines six specifications of Medicaid expansion, including which states are included in the treatment or control groups, or excluded, in each specification. The first specification, derived from Courtemanche et al., includes all states and defines expansion states as those who expanded Medicaid in 2014 or later. The second, adapted from Miller and Wherry, excludes five early expander states. The third, also adapted from Miller and Wherry, also excludes California as an early expander.[4] The fourth, adapted from Anand et al., excludes early expanders, states with state programs offering similar coverage to Medicaid for the expansion population, and states with programmatic difficulties at the time of expansion. The fifth, also from Anand, also excludes late expanders; this approach features only one treatment period (Q1 2014), and the pre- and post-periods are identical for all treated states. The sixth, from McGinty et al., includes all states that the McGinty paper considers to have expanded Medicaid in 2014 and excludes late expanders. All specifications except the one based on McGinty et al. include the same control group of non-expansion states.

One critical requirement that must be satisfied is the common trends assumption. The theory underlying these analyses is that, absent the policy intervention in question, the untreated group and the treated group would continue to behave similarly. When a treatment occurs, the untreated group is considered a counterfactual example for what would have happened to the treated group had it gone untreated. Confirming that the treated and control groups are behaving similarly prior to treatment is critical; if they are not similar, and especially if they have divergent trends or are behaving very differently prior to treatment, then any post-treatment difference between the two groups may not be due just to the treatment in question.

[4] California began its ACA Medicaid expansions early, in 2012, but implementation varied by county and over time. Given that implementation and the size of the state, considering how inclusion/exclusion of California impacts results may be important.

As recommended in Roth et al., (2022), pre-treatment trend commonality will be assessed by examining pre-treatment differences in estimated coefficients between the treated and control groups using an event study approach, with trend commonality held conditional on covariates. In addition to assessing the common trends assumption, the event study approach also shows how treatment effects change dynamically over time. While this event study approach is recommended for assessing common trends pre-treatment, such tests may be underpowered to detect certain violations of common trends (Bilinski & Hatfield, 2020; Roth, 2018, 2018, 2020; Roth et al., 2022). This paper's primary specification – doubly-robust C-S DiD – includes both stabilized inverse probability weighting and outcome regression adjustment, two alternative approaches to address/assess common trends (Callaway & Sant'Anna, 2021; Roth et al., 2022; Sant'Anna & Zhao, 2020).

Assuming satisfaction of the common trends assumption, results from the post-treatment periods will be assessed from the event study and DiD analyses. Event study results will show dynamic quarterly effects of expansion (checking for treatment effect heterogeneity over time) and DiD results will show estimated effects over the post-treatment study period.

Data

Child maltreatment data in this study are drawn from the National Child Abuse and Neglect Data System (NCANDS) Child File, which is comprised of compiled child maltreatment reports from all states, with some exceptions in certain years when some states did not submit data. The span of data is 2009-2018, which should provide sufficient time to examine trends during the pre-Medicaid expansion period and to see the impact of the 2014 expansions. Two factors of the NCANDS Child File require connection to secondary datasets: first, the NCANDS files include data only on children subject to maltreatment reports, so by definition children not subject to reports are excluded in NCANDS data. Second, NCANDS data are deidentified and cannot be connected to other data sources to show relevant covariates at the individual level.

Including information about children who are not subject to reports of maltreatment is critical to a study whose intent is to measure the effect of a policy intervention on maltreatment risk; by collapsing data to the county-level and merging on population counts and other data, children whose information does not appear in CPS can be accounted for. To that end, county-quarter report counts are created from the NCANDS Child Files. Those data, which vary by county-quarter, are log-transformed and paired with population count data from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program (which vary by county-year) and county-year rates on health insurance and other socioeconomic variables extracted from ACS and the Small Area Health Insurance Estimates program, each of which vary by county-year. ACS uses rolling five-year averages in order to create reliable county estimates (adding together multiple years increases the size of the sample for each county), which may limit useful variation in the independent socioeconomic variables.[5] State-year covariates are

[5] While other units in ACS, such as the Public Use Microdata Area (PUMA), can avoid that issue, mapping counties from the NCANDS child file to PUMA is impractical. The Child File includes county codes only for counties which have over 1000 observations in a year; counties with fewer observations are coded into a composite county within their particular states. Counties that are compiled into that composite vary by year based on the number of reports; thus, while counties that are included or compiled can be

drawn from the National Welfare Dataset (University of Kentucky Center for Poverty Research, 2022); and state median income data is drawn from Federal Reserve Economic Data (St. Louis Fed, 2022).

The study's sample includes all counties in the United States for which maltreatment data were submitted and available, on a quarterly basis, from 2009 to 2018, for an *n* of 33,825 county-quarters in the full, unrestricted sample. Aggregated county rates represent 36,770,158 individual maltreatment reports from across the United States over ten years.

Results

Table 2.2 shows descriptive statistics comparing non-expansion counties and pre-expansion counties by Medicaid expansion specification. Total child population represented by each specification varies from 19.5 million (specification 1) to 8.1 million (specification 5). Expansion counties show higher physical abuse and neglect report rates relative to non-expansion counties and lower medical neglect. Non-expansion counties show higher uninsured rates for adults, higher poverty rate, and lower median income, education, unemployment, and primary care provider rate. The starkest contrast between treated and control counties is in state political characteristics: expansion states have markedly higher percentages of Democratic control of the governorship and both houses of the state legislature. Beyond variation in levels of dependent and independent variables prior to expansion, further examination of trends in event study analyses will elucidate

observed and replicated in other county-level data sources, they cannot crosswalk consistently into PUMAs. The counties which would be part of given PUMAs would change over time and thus the PUMAs would not be comparable to themselves over time.

any relevant differences between treated and control groups before Medicaid expansions occurred.

Event study and DiD results by Medicaid expansion specification are shown for physical abuse reports (Figure 2.1), neglect reports (Figure 2.2), medical neglect reports (Figure 2.3), and sexual abuse reports (Figure 2.4), respectively. Pre-treatment trends commonality can be assessed in these results before considering post-treatment effects. Doubly-robust Callaway-Sant'Anna DiD includes both stabilized inverse probability weighting and outcome regression adjustment; including both approaches and holding trends common on covariates may help to improve pre-treatment trend commonality (Roth et al., 2022).

Event study results for physical abuse show slight divergence in individual quarters in the pre-treatment period, depending on specification, but no long-term divergent trends. Neglect reports show very slight divergence in periods immediately before expansion, with expansion states showing small decreases compared to non-expansion states; this is not considered a significant violation of the common trends assumption, for two reasons: 1) the size of the divergence, even when statistically significant, is small compared to estimated post-treatment effects, and 2) there may be some minor anticipation effect due to pre-expansion welcome mat effects (aka woodwork effects; Blewett, 2012), as previously-eligible members signed up for coverage due to increased public conversation about Medicaid expansion (Guth et al., 2020). Similarly, medical neglect and sexual abuse show relatively consistent pre-treatment trends with any significant divergence varying around zero.

Estimated treatment effects vary by type of maltreatment and by Medicaid expansion specification. Physical abuse results show reductions across expansion specifications that begin about 5 quarters after the expansion: while effect sizes vary between about 4 and 10 percent, the direction and magnitude of effects is consistent after 5 quarters post-expansion. This indicates that inclusion or exclusion of particular states in expansion specifications does not substantially affect estimated treatment effects. Results are also imprecisely estimated and include wide confidence intervals. Medical neglect results show increases, varying between 5.2 and 16.6 percent. As with physical abuse, medical neglect results are imprecisely estimated and do not reach statistical significance at $p < .05$. Sexual abuse results show small estimated effects with large confidence intervals, and estimated effects vary from 4.7 percent to -5.3 percent, depending on expansion specification.

Estimated treatment effects of Medicaid expansion on neglect are not consistent across expansion specifications. The most restrictive specifications – that is, specifications 5 (only January 2014 expansions with no partial, early, or late expanders) and 6 (only 2014 expansions with no late expanders, slightly different specification) – show reductions in the post-expansion period between 5.6 and 7.1 percent. However, more inclusive specifications 1 (all states), 2 (excluding just early ACA expansions), 3 (additionally excluding California), and 4 (additionally excluding states with programs similar to Medicaid expansion) show either no effect or increases in neglect in the post-expansion period, between 0.8 and 5 percent.

In addition to C-S DiD, models were estimated using additional empirical approaches (some not shown) that allow for inclusion of additional covariates (the full array of county and state covariates listed in Table 2.2): Gardner two-stage DiD (another solution to the problem of variation in treatment timing) using all expansion specifications (Butts & Gardner, 2021; Gardner, 2021; Thakral & Tô, 2020); two-way fixed effects models using expansion specification five, which has no variation in treatment timing, with both the full sample and restricted to border-county pairs (Peng et al., 2020); and synthetic controls (Abadie, 2021; Abadie et al., 2010, 2015; Galiani & Quistorff, 2017). Results for neglect are generally consistent across alternative estimation strategies: expansion specifications 5 and 6 (the most restrictive) show reductions in neglect reports, with either no effect or slight increases observed in other specifications. Approaches other than C-S DiD also tended to have large pre-treatment trend divergence, especially in neglect results; doubly-robust C-S DiD appears to best account for that pre-treatment trend divergence out of all tested approaches. Results for physical abuse, medical neglect, and sexual abuse are less consistent across alternative estimation strategies, indicating these results may be sensitive to model specification.

Discussion

Results of this paper in part support and in part run counter to previous findings in the literature (Assini-Meytin et al., 2022; Brown et al., 2019; McCray, 2018; McGinty et al., 2022; Pac, 2019). McCray (2018), using state-year panel data from 2000-2015, finds a correlation between increases in health care coverage for children and reductions in physical abuse. Pac (2019), using county-month panel data from 2010 to 2013, found statistically significant reductions in physical abuse reports for children under 6 in California following that state's early Medicaid expansion in 2012, and no statistically significant effect on neglect reports. While this paper can partially replicate Pac's results for physical abuse, results are imprecisely estimated and not statistically significant at $p < .05$ in most specifications.

Brown et al., (2019), using state-year panel data from 2010 to 2016 and including all expansions in the treated group (similar to this paper's specification 1 in Table 2.1) finds a statistically significant reduction in neglect for children under age 6. This paper can replicate that result using Medicaid expansion specifications 5 and 6, but other specifications of Medicaid expansion do not show a reduction in neglect following Medicaid expansion; instead, they show an increase. Results also vary depending on whether analyses are run using state- or county-level data.

McGinty et al. (2022) considers the impact of Medicaid expansions on child maltreatment using a state-year panel from 2008-2018 with log-transformed child physical abuse report and child neglect report rates per 100,000 children. Because treatment timing varies, that paper uses the Callaway-Sant'Anna DiD approach, and includes as controls the percent of each state's population that is Black, poverty rate, percent of adults who did not graduate from high school, and the age-adjusted drug overdose death rate. Because they found states that expanded after 2014 had non-parallel trends, that paper excludes late (post-2014) expanders, though it includes Michigan, which expanded later in 2014. That paper also includes several states in its preferred specification which this paper's preferred specification excludes as early expanders, including: California, Hawaii, Iowa, Maryland, Minnesota, and Oregon. McGinty et al. also exclude West Virginia due to data reporting complications during the study period. That paper's preferred specification is detailed in Specification 6, Table 2.1. Assini-Meytin et al., (2022) use a similar analytical approach as McGinty et al. to examine an association between Medicaid expansion and child sexual abuse.

This paper replicates the finding from Assini-Meytin et al., (2022), finding no consistent effect of Medicaid expansion on child sexual abuse reports. This paper also attempts to replicate neglect results from McGinty et al., (2022) using county-level data in Figure 2.2 and state-level data in Figure 2.5. County-level results, discussed above, show reductions in neglect reports following Medicaid expansion only in the most restrictive specifications of expansion, and increases in other specifications. Of the county-level specifications that show reductions, only number 5 shows any periods with effects significant at $p < .05$; estimates in 6 are less precisely estimated. State-level results show either no effect or very imprecise reductions in specifications 1-4, but statistically significant reductions in neglect reports in specifications 5 and 6. These results are supported by replications using Gardner two-stage DiD (Figure 2.6), which included a much wider array of covariates.

While McGinty et al., (2022) find reductions in child neglect reports following Medicaid expansion, based on the results in this paper at the county-quarter and state-quarter level, that finding appears to be sensitive to how Medicaid expansion is specified and the selected units of analysis. As has been noted previously, “State-level analyses may mask important variation in both child maltreatment and macroeconomic conditions that occur within a state" (Bullinger, Lindo, et al., 2021, p. 12). Analyses that are more aggregated by time, such as annual vs. quarterly, may have similar effects. Collapsing data by year smooths out substantial temporal variation that is more clearly observed in quarterly data and collapsing by state smooths substantial geographic variation more clearly observed at the county-level.

While this paper can, to an extent, replicate prior results, some nuance in interpretation is required. C-S DiD results show reductions in physical abuse and increases in medical neglect following Medicaid expansion, but they are imprecisely estimated and alternative approaches (two-stage DiD, TWFE, county-pair TWFE, and synthetic controls) do not have similar findings, so these appear to be sensitive to model specification. Results for both sexual abuse and neglect vary depending on how Medicaid expansion is specified, and county-level analyses actually show increases in neglect in some specifications, while state-level analyses show either no effect, statistically insignificant reductions, or statistically significant reductions.

Limitations

This paper faces several limitations that should be acknowledged. First, an ideal dataset for this research question would allow for identification of individual level records to be linked to other datasets in order to compare the effect of expansion, including actual Medicaid enrollment, on maltreatment outcomes. One major limitation of this paper is the inability to link individual level records; instead, this paper collapses data to the county-level. While this is a useful workaround and it yields a substantially larger sample than a state-level dataset, the result is that valuable individual level variation is lost or not available.

Second, maltreatment reporting can be a problematic proxy for maltreatment incidence. While reporting is commonly considered a good proxy for maltreatment incidence, reporting can vary for reasons other than variations in incidence, such as the hypothesized effect in this study: that when parents are given health insurance, their children might be more likely to see the doctor more regularly. Such an effect might both increase reports – but not incidence – and change the rate at which reports are substantiated. This available explanation of observed increases in medical neglect reports in counties with Medicaid expansion: that expanded access to care for adults (via expansion and the woodwork effect) and expanded access to care for children (via the woodwork effect) led to increased exposure to health care providers. Another potential explanation is that other mandatory reporters such as teachers or social workers might have become more likely to report medical neglect after Medicaid expansions. Future research should assess the effect of Medicaid expansion on detection of maltreatment by health care providers and attempt to assess whether Medicaid expansion affected reporting separate from incidence.

Another important consideration is whether maltreatment reports are reliably reported by counties – i.e. to what extent does measurement error potentially impact this analysis. Some states did report data quality issues during the span of the study, some states also did not submit data in every year of the study, and some counties with small numbers of reports were aggregated together. The primary concern is whether measurement error would systematically bias report counts or rates in ways that would influence results. First, there is little reason to expect systematic measurement errors in maltreatment reporting that are tied to state Medicaid expansion decisions. While inconsistent reporting by counties or states is possible, that reporting is probably not influenced by or related to Medicaid expansion. As long as errors are not related to treatment assignment, that should not provide undue concern in this analysis. While measurement error cannot be identified herein, this paper did consider whether inclusion of data from very small counties (whose numbers are aggregated) might influence results; inclusion or exclusion did not influence results and so they were left in the data.

Third, though this paper uses county-level estimates, its sample is limited because counties with fewer than 1000 maltreatment reports each year are aggregated to prevent identification of individuals in smaller counties. If analyses could be replicated with a full sample that did not aggregate small counties, results could be broken down by county population to check for treatment effect heterogeneity by county size. If a non-aggregated sample were used, analyses could also map counties to PUMAs, as noted above, avoiding the problem of using five-year averages from ACS data. This would mean more accurate annual changes per observation and greater variation in covariates.

Fourth, this study does not control for variation in state policies defining child maltreatment. While models including unit fixed effects (such as Gardner two-stage DiD) should absorb any inter-state policy variation (if time invariant), unit and time fixed effects would not account for changes to state definitions of child maltreatment. This would be of particular concern if in-state maltreatment definition variations also interacted with Medicaid expansion – for example if definitions of neglect changed and that change interacted with changes from Medicaid expansion of what mandatory reporters are likely to identify and report maltreatment. Future research should consider approaches to account for variation of state maltreatment policies over time.

Conclusion

This paper provides the first nationally representative estimates of the effect of Medicaid expansion on county-level child maltreatment for children ages 0-17, with quarterly data from 2009 through 2018 (though McGinty et al. (2022) does so at the state-year level). It finds that ACA Medicaid expansions may have reduced child physical abuse reports and increased medical neglect reports, though these findings are sensitive to model specification and are imprecisely estimated. It also finds that January 2014 Medicaid expansions appear to have reduced child neglect, but that inclusion of partial, early, and late expansions reverses that observed relationship. The finding that neglect results vary by expansion specification is robust to alternative model specifications.

If the reason we care about measuring the effect of Medicaid expansion is because we want to know exclusively about the effect of a past policy change, it might be reasonable to conclude that results indicate January 2014 Medicaid expansions did lead to reductions in child neglect reports. If, however, we also care to generalize findings to consider what effect we might expect from future expansions in states that have not yet expanded Medicaid, then consideration of just January 2014 Medicaid expansions (which show reductions in neglect) would be improper. Rather, if results are to be generalized to states which have not yet expanded, then including results from states that expanded late (as all not-yet-expanded states would be late expanders, if they expanded) would be necessary. Those specifications, depending on whether considered at the county- or state-level, show either statistically insignificant decreases, no effect, or even in some cases significant increases in neglect reports.

These findings are practically applicable in several ways. First, they introduce additional nuance to the literature on the impact of Medicaid expansion on child maltreatment outcomes – specifically, that prior findings of statistically significant reductions in neglect post-expansion appear to be sensitive to which states are considered to have expanded Medicaid. Exclusion of early and late expanders from analysis shows reductions in neglect, but inclusion of early and late expanders shows a more complicated relationship. Results also partially support prior findings of reductions in physical abuse, though the reduction is sensitive to estimation approach. These findings also yield some support to the theory that antipoverty programs may have effects on child maltreatment outcomes.

Beyond informing the question of child maltreatment and Medicaid expansion, this also contributes to the literature relating to Medicaid expansion’s externalities more broadly. Last, these findings illustrate the important role that methodological decisions can make for a study’s results (Huntington-Klein et al., 2020) and show that adjusting analyses to account for variation in treatment timing and divergent pre-treatment trends may yield different results from analyses that do not.

Tables

Table 2.1 Medicaid expansion specifications

State	Expansion date	Expansion scenarios						Notes
		1	2	3	4	5*	6	
Alabama		0	0	0	0	0	0	
Alaska	2015-Sep	1	1	1	1	.	.	Expanded after January 2014
Arizona	2014-Jan	1	1	1	1	1	1	
Arkansas	2014-Jan	1	1	1	1	1	1	
California	2014-Jan	1	1	.	.	.	1	Early ACA expansion in some counties via Low Income Health Program. Pre 2014 eligibility over 100 FPL, not capped, in some counties. Waiver effective 11/1/2010, county programs started 7/1/2011.
Colorado	2014-Jan	1	1	1	1	1	1	Not counted as early expander, but did have pre-2014 expansion to adults <=10% FPL effective 4/1/2012.
Connecticut	2014-Jan	1	1	1	1	1	1	Not counted as early expander, but did have pre-2014 expansion to adults <=56% FPL effective 4/1/2010.
Delaware	2014-Jan	1	.	.	.	.	.	Pre 2014 eligibility via state funded program over 100 FPL, not capped.
District of Columbia	2014-Jan	1	.	.	.	.	.	Early ACA expansion. Pre 2014 eligibility over 100 FPL, not capped, in some counties. ACA option 7/1/2010 (133% FPL), Waiver 12/1/2010 (200% FPL)
Florida		0	0	0	0	0	0	
Georgia		0	0	0	0	0	0	
Hawaii	2014-Jan	1	1	1	.	.	1	Pre 2014 eligibility via state funded program over 100 FPL, not capped.
Idaho	2020-Jan	0	0	0	0	0	0	No expansion during study period, left in controls.
Illinois	2014-Jan	1	1	1	1	1	1	
Indiana	2015-Feb	1	1	1	.	.	.	Expanded after Jan 2014.
Iowa	2014-Jan	1	1	1	.	.	1	Pre 2014 eligibility via state funded program over 100 FPL, not capped.
Kansas		0	0	0	0	0	0	
Kentucky	2014-Jan	1	1	1	1	1	1	
Louisiana	2016-Jul	1	1	1	.	.	.	Expanded after Jan 2014.
Maine	2019-Jan	0	0	0	0	0	.	No expansion during study period, left in controls. McGinty et al. exclude Maine as a late (July 2018) expander.
Maryland	2014-Jan	1	1	1	.	.	1	Pre 2014 eligibility via state funded program over 100 FPL, not capped.
Massachusetts	2014-Jan	1	.	.	.	.	.	Pre 2014 eligibility via state funded program over 100 FPL, not capped.
Michigan	2014-Apr	1	1	1	1	.	1	Expanded after Jan 2014
Minnesota	2014-Jan	1	1	1	.	.	1	Pre 2014 eligibility over 100 FPL, not capped. ACA option effective 3/1/2010

								(133% FPL), Waiver 8/1/2011 (250% FPL)
Mississippi		0	0	0	0	0	0	
Missouri	2021-Oct	0	0	0	0	0	0	No expansion during study period, left in controls.
Montana	2016-Jan	1	1	1	1	.	.	Expanded after Jan 2014.
Nebraska	2020-Oct	0	0	0	0	0	0	No expansion during study period, left in controls.
Nevada	2014-Jan	1	1	1	1	1	1	
New Hampshire	2014-Sep	1	1	1	1	.	.	Expanded after Jan 2014.
New Jersey	2014-Jan	1	1	1	1	1	1	Not counted as early expander, but did have pre-2014 expansion to adults <=23% FPL effective 4/14/2011.
New Mexico	2014-Jan	1	1	1	1	1	1	
New York	2014-Jan	1	.	.	.	.	.	Pre 2014 eligibility via state funded program over 100 FPL, not capped.
North Carolina		0	0	0	0	0	0	
North Dakota	2014-Jan	1	1	1	1	1	1	
Ohio	2014-Jan	1	1	1	1	1	1	
Oklahoma	2021-Jul	0	0	0	0	0	0	No expansion during study period, left in controls.
Oregon	2014-Jan	1	1	1	.	.	1	Difficult open enrollment.
Pennsylvania	2015-Jan	1	1	1	1	.	.	Expanded after Jan 2014.
Rhode Island	2014-Jan	1	1	1	1	1	1	
South Carolina		0	0	0	0	0	0	
South Dakota		0	0	0	0	0	0	
Tennessee		0	0	0	0	0	0	
Texas		0	0	0	0	0	0	
Utah	2020-Jan	0	0	0	0	0	0	No expansion during study period, left in controls.
Vermont	2014-Jan	1	.	.	.	.	.	Pre 2014 eligibility via state funded program over 100 FPL, not capped.
Virginia	2019-Jan	0	0	0	0	0	0	No expansion during study period, left in controls.
Washington	2014-Jan	1	1	1	1	1	1	Not counted as early expander, but did have pre-2014 expansion to adults <=133% FPL effective 1/3/2011.
West Virginia	2014-Jan	1	1	1	1	1	.	McGinty et al. note complications with child maltreatment reporting during the study period.
Wisconsin		0	0	0	0	0	0	
Wyoming		0	0	0	0	0	0	
Treat		32	27	26	19	14	20	
Control		19	19	19	19	19	18	
Excluded		0	5	6	13	18	13	
Total included		51	46	45	38	33	38	

Lists all U.S. states (plus DC), along with the date the state expanded Medicaid via the ACA. Specifications 1-6 show whether state is included as a treatment state (1), control (0), or excluded (.). Notes list details on expansion decision and/or why some states are excluded. Spec. 1 = all states (Courtemanche et al.). 2 = exclude early expanders (Miller and Wherry). 3 = also exclude CA (Miller and Wherry). 4 = also exclude partial early expanders (Anand et al.). 5 = also exclude late expanders (only consider Jan. 2014 expansions; Anand et al.). 6 = McGinty et al. spec.; exclude post-2014 expanders and others. 1-5 have same controls; 6 varies slightly. Four end rows show total number of treated, control, excluded, and total states included in analysis for each specification.

Table 2.2 Descriptive statistics – control and pre-treatment means

		Non-expansion	Expansion states					
			Spec. 1	Spec. 2	Spec. 3	Spec. 4	Spec. 5	Spec. 6
Dependent variables (vary by county-quarter)	Overall maltreatment report rate	12.29	12.33	11.90	12.12	11.94	11.27	12.17
	Physical abuse report rate per	2.45	2.74	2.83	3.04	3.33	2.91	3.08
	Neglect report rate	6.84	8.12	7.52	7.71	7.39	7.01	7.53
	Medical neglect report rate	0.24	0.13	0.15	0.17	0.20	0.20	0.17
	Sexual abuse report rate	0.78	0.71	0.75	0.79	0.74	0.82	0.70
County-level covariates (vary by county-year)	Uninsured rate for adults under 138 FPL, %	40.09	35.84	38.26	37.32	37.50	39.57	38.86
	Poverty rate, %	11.87	10.50	10.47	10.43	10.37	10.24	10.24
	Median income, in thousands	53.50	57.33	56.34	55.58	55.57	56.78	57.93
	Marriage rate, %	49.42	48.33	48.74	48.82	49.04	49.31	48.92
	Education rate (adults with high school ed. or higher), %	85.70	86.22	86.36	87.30	87.39	86.90	86.02
	Unemployment rate, %	7.84	9.14	9.30	9.25	9.53	9.18	9.56
	Income inequality (Gini coefficient)	44.91	45.00	44.79	44.64	44.82	45.12	44.87
	Portion of population that is white, %	73.20	72.57	73.23	75.06	76.69	75.84	71.66
	Primary care providers per 10,000 people	7.46	9.67	9.25	9.27	9.27	9.39	9.43
State-level covariates (vary by state-year)	Democratic state governors, %	12.95	51.77	41.13	42.30	43.96	51.28	45.03
	State legislature lower house Democrats, %	34.77	56.45	53.07	51.45	51.68	53.85	56.06
	State legislature upper house Democrats, %	34.37	52.10	50.36	48.41	48.53	52.22	53.23
	State unemployment rate	3.77	5.63	5.75	5.43	5.67	5.83	6.14
	State poverty rate	14.62	14.54	14.52	14.23	14.06	14.35	14.51
	Gross state product (millions)	724.79	672.12	592.38	384.41	421.71	408.10	679.81
N		14,293	19,532	16,581	15,001	11,074	8,111	11,995
Child pop		28,779	44,366	38,344	29,187	21,973	16,354	30,643

Means, pre-treatment, weighted by county child populations. Rates are reports per 1000 children per quarter. N shows number of county-quarters in control and treated groups (before and after expansion). Child populations show total number of children represented in each specification in Q4 2013, in thousands. Spec. 1 = all states (Courtemanche et al.). 2 = exclude early expanders (Miller and Wherry). 3 = also exclude CA (Miller and Wherry). 4 = also exclude partial early expanders (Anand et al.). 5 = also exclude late expanders (only consider Jan. 2014 expansions; Anand et al.). 6 = McGinty et al. spec.; exclude post-2014 expanders and others.

Figures

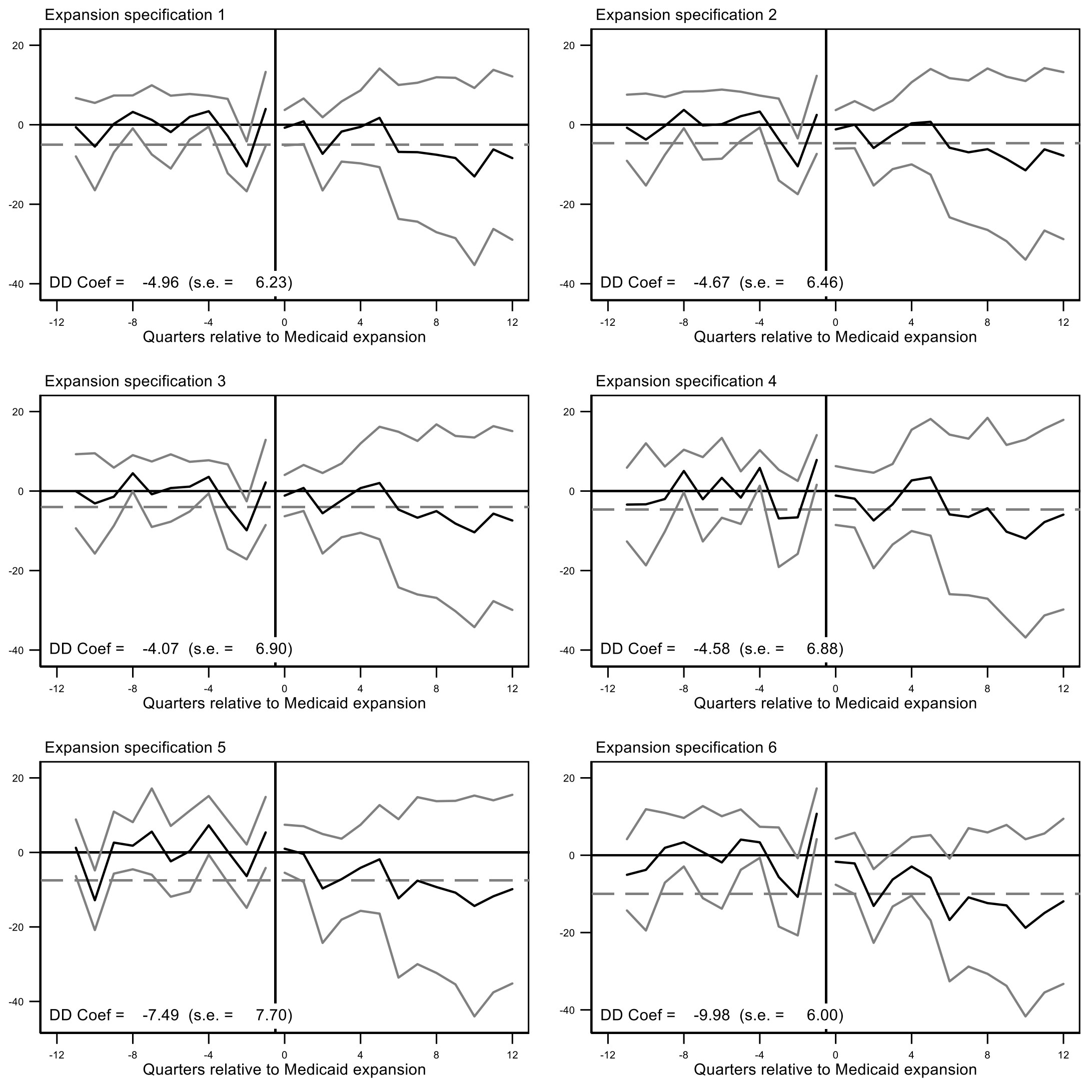

Figure 2.1 Event study, physical abuse reports (log-transformed), C-S DiD, by Medicaid expansion specification, county-level

Graphs display differences in estimated coefficients of the log of child physical abuse reports between treated and control counties from 12 quarters before to 12 quarters after Medicaid expansion. Solid black line shows estimated coefficient in each quarter; solid gray lines show upper and lower confidence intervals; horizontal gray dashed line shows estimated post-treatment DiD coefficient. All models include county covariates such as poverty, education, race, and child population. Wild bootstrapped standard errors included, clustered at state-level. Spec. 1 = all states. 2 = exclude early expanders. 3 = also exclude CA. 4 = also exclude partial early expanders. 5 = also exclude late expanders (only consider Jan. 2014 expansions). 6 = McGinty et al. spec.; exclude post-2014 expanders and others. 1-5 have same controls; 6 varies slightly (see Table 2.1 for details).

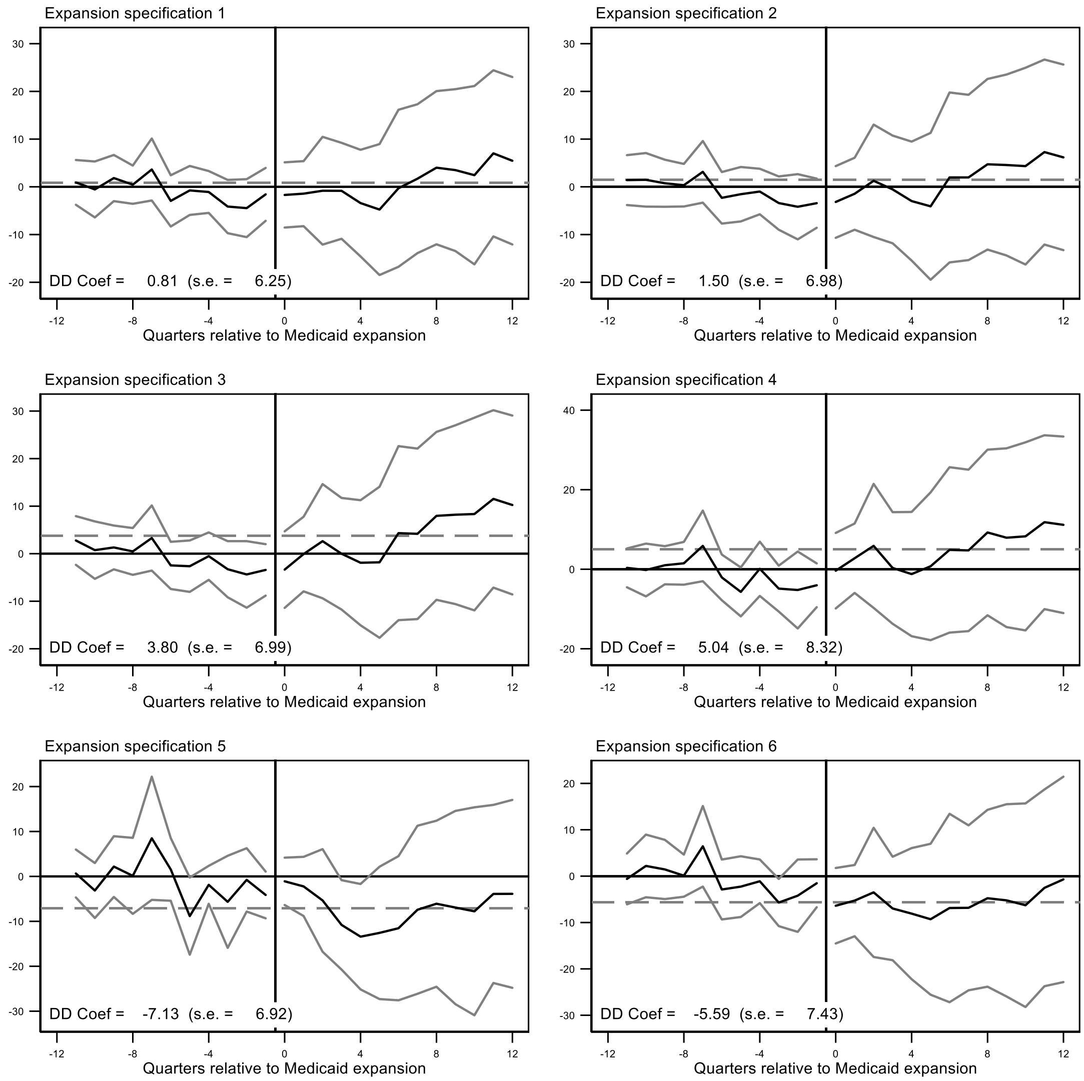

Figure 2.2 Event study, neglect reports (log-transformed), C-S DiD, by Medicaid expansion specification, county-level

Graphs display differences in estimated coefficients of the log of child neglect reports between treated and control counties from 12 quarters before to 12 quarters after Medicaid expansion. Solid black line shows estimated coefficient in each quarter; solid gray lines show upper and lower confidence intervals; horizontal gray dashed line shows estimated post-treatment DiD coefficient. All models include county covariates such as poverty, education, race, and child population. Wild bootstrapped standard errors included, clustered at state-level. Spec. 1 = all states. 2 = exclude early expanders. 3 = also exclude CA. 4 = also exclude partial early expanders. 5 = also exclude late expanders (only consider Jan. 2014 expansions). 6 = McGinty et al. spec.; exclude post-2014 expanders and others. 1-5 have same controls; 6 varies slightly (see Table 2.1 for details).

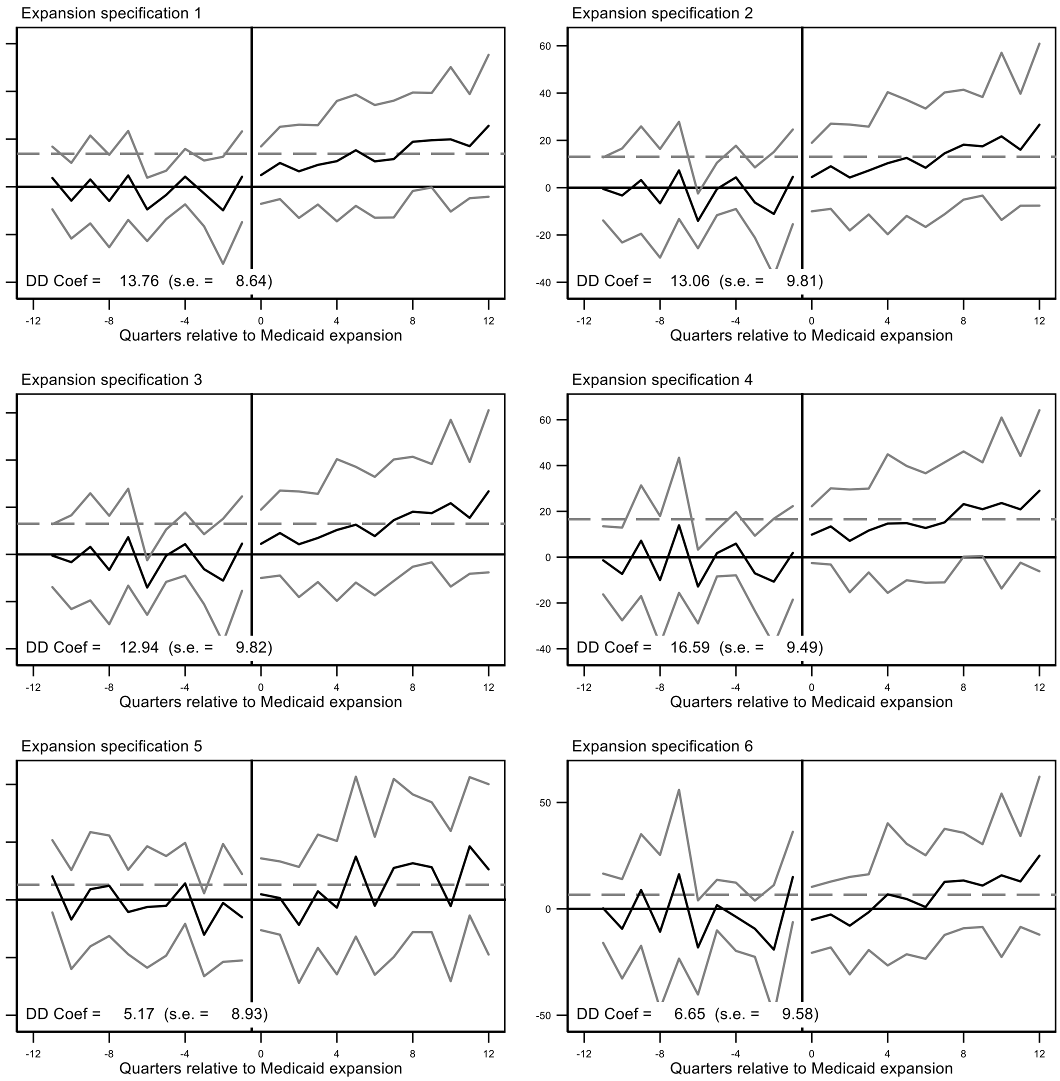

Figure 2.3 Event study, medical neglect reports (log-transformed), C-S DiD, by Medicaid expansion specification, county-level

Graphs display differences in estimated coefficients of the log of child medical neglect reports between treated and control counties from 12 quarters before to 12 quarters after Medicaid expansion. Solid black line shows estimated coefficient in each quarter; solid gray lines show upper and lower confidence intervals; horizontal gray dashed line shows estimated post-treatment DiD coefficient. All models include county covariates such as poverty, education, race, and child population. Wild bootstrapped standard errors included, clustered at state-level. Spec. 1 = all states. 2 = exclude early expanders. 3 = also exclude CA. 4 = also exclude partial early expanders. 5 = also exclude late expanders (only consider Jan. 2014 expansions). 6 = McGinty et al. spec.; exclude post-2014 expanders and others. 1-5 have same controls; 6 varies slightly (see Table 2.1 for details).

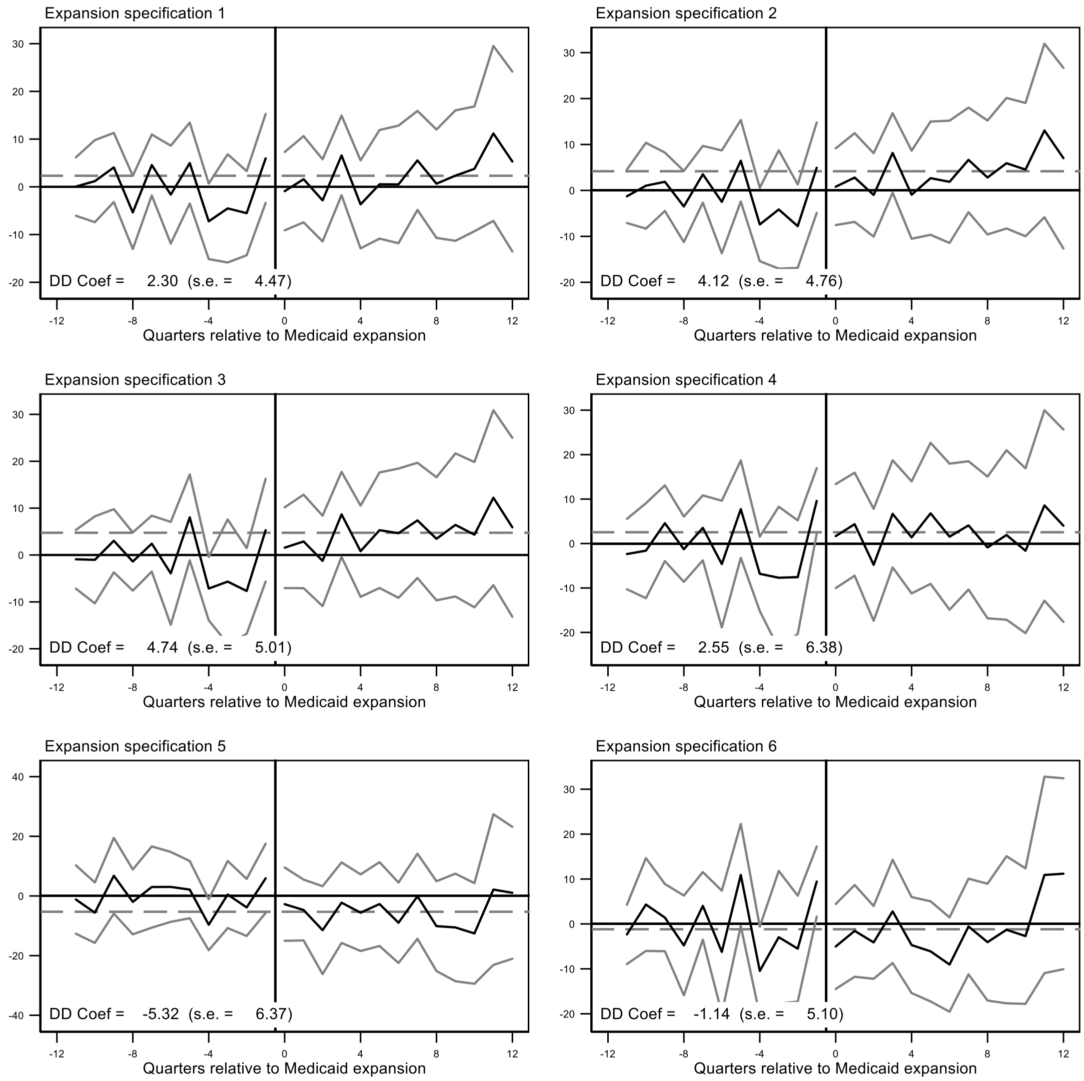

Figure 2.4 Event study, sexual abuse reports (log-transformed), C-S DiD, by Medicaid expansion specification, county-level

Graphs display differences in estimated coefficients of the log of child sexual abuse reports between treated and control counties from 12 quarters before to 12 quarters after Medicaid expansion. Solid black line shows estimated coefficient in each quarter; solid gray lines show upper and lower confidence intervals; horizontal gray dashed line shows estimated post-treatment DiD coefficient. All models include county covariates such as poverty, education, race, and child population. Wild bootstrapped standard errors included, clustered at state-level. Spec. 1 = all states. 2 = exclude early expanders. 3 = also exclude CA. 4 = also exclude partial early expanders. 5 = also exclude late expanders (only consider Jan. 2014 expansions). 6 = McGinty et al. spec.; exclude post-2014 expanders and others. 1-5 have same controls; 6 varies slightly (see Table 2.1 for details).

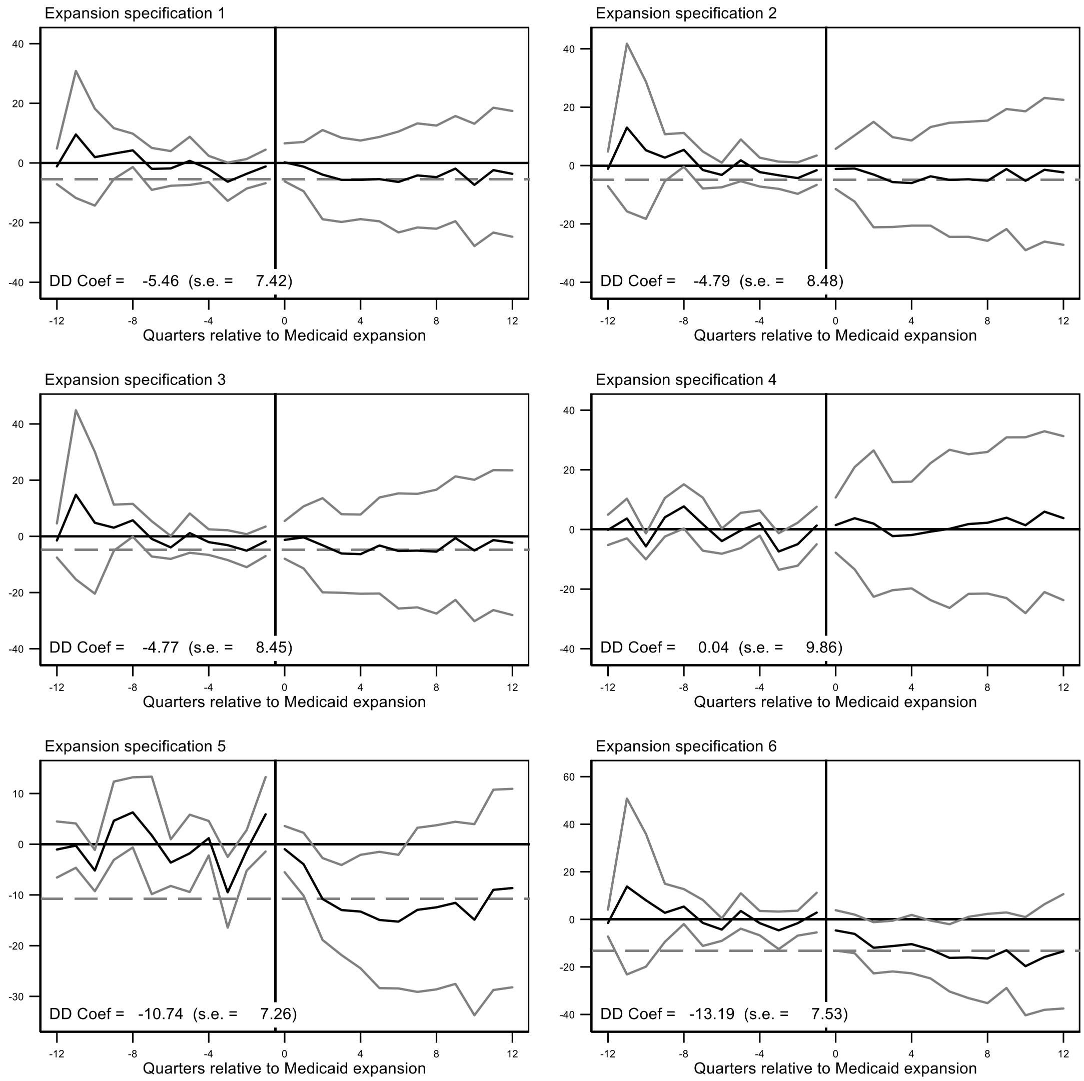

Figure 2.5 Event study, neglect reports (log-transformed), C-S DiD, by Medicaid expansion specification, state-level

Graphs display differences in estimated coefficients of the log of child neglect reports between treated and control states from 12 quarters before to 12 quarters after Medicaid expansion. Solid black line shows estimated coefficient in each quarter; solid gray lines show upper and lower confidence intervals; horizontal gray dashed line shows estimated post-treatment DiD coefficient. All models include state covariates such as poverty, education, race, and child population. Wild bootstrapped standard errors included. Spec. 1 = all states. 2 = exclude early expanders. 3 = also exclude CA. 4 = also exclude partial early expanders. 5 = also exclude late expanders (only consider Jan. 2014 expansions). 6 = McGinty et al. spec.; exclude post-2014 expanders and others. 1-5 have same controls; 6 varies slightly (see Table 2.1 for details).

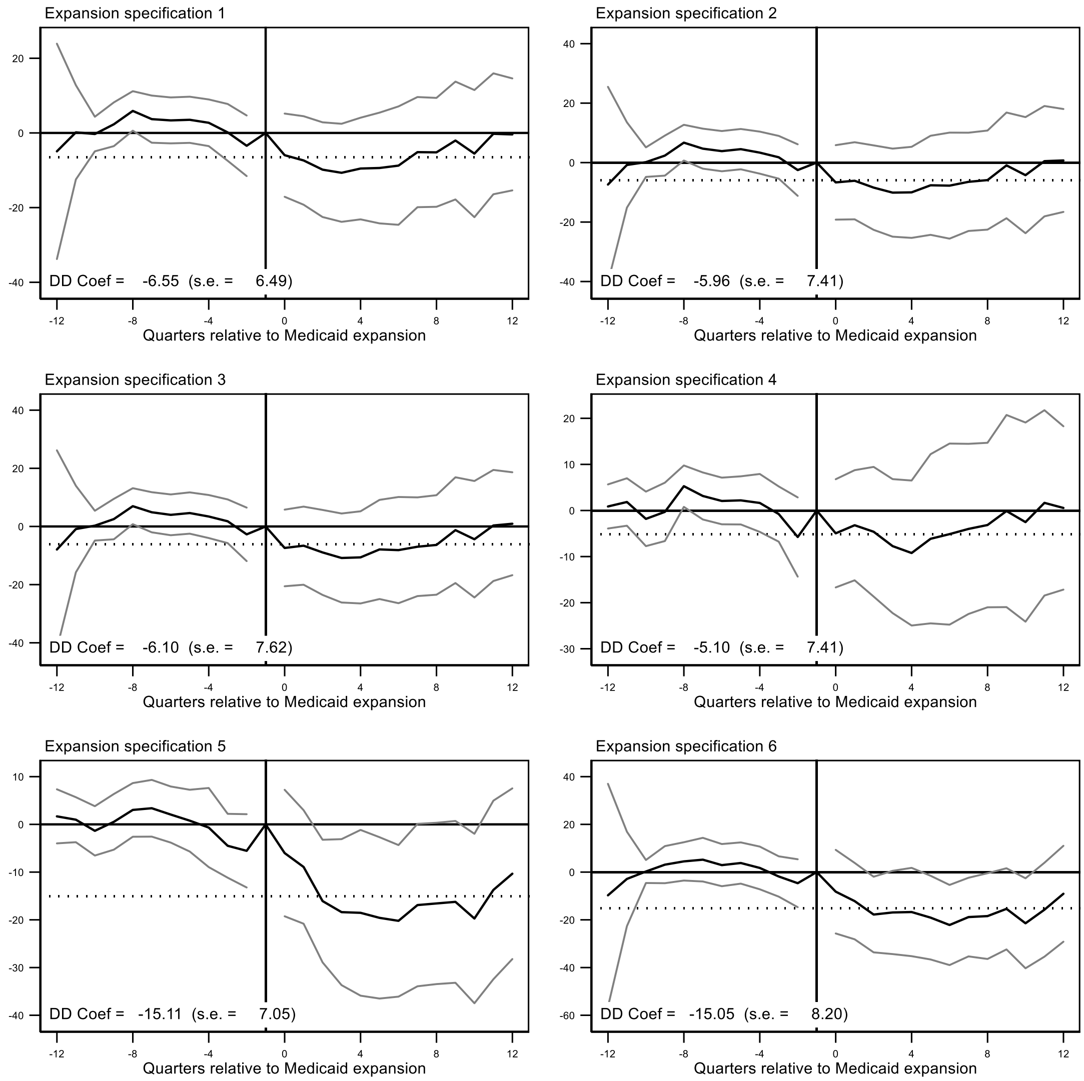

Figure 2.6 Event study, neglect report rate (log-transformed), Gardner two-stage DiD, by Medicaid expansion specification, state-level

Graphs display differences in estimated coefficients of the log of child neglect reports between treated and control states from 12 quarters before to 12 quarters after Medicaid expansion. Solid black line shows estimated coefficient in each quarter; solid gray lines show upper and lower confidence intervals; horizontal gray dotted line shows estimated post-treatment DiD coefficient. All models include the full array of state covariates outlined in Table 2.2. Eicker-Huber-White robust standard errors included, clustered at state-level. Spec. 1 = all states. 2 = exclude early expanders. 3 = also exclude CA. 4 = also exclude partial early expanders. 5 = also exclude late expanders (only consider Jan. 2014 expansions). 6 = McGinty et al. spec.; exclude post-2014 expanders and others. 1-5 have same controls; 6 varies slightly (see Table 2.1 for details).

3. DOES THE SUPPLEMENTAL NUTRITION ASSISTANCE PROGRAM REDUCE CHILD MALTREATMENT?

Abstract

Child maltreatment is prevalent in the United States, and its costs, both to children who experience maltreatment and to society at large, are both immediate and long-lasting. Understanding how public policies might reduce maltreatment risk is critical. Poverty and low socioeconomic status (SES) are large risk factors for child maltreatment. One antipoverty program that has the potential to reduce child maltreatment is the Supplemental Nutrition Assistance Program (SNAP). This paper utilizes state SNAP policy variation in the form of broad-based categorical eligibility (BBCE) to assess the causal effect of SNAP on child maltreatment outcomes via both difference-in-differences and event study frameworks. Results indicate BBCE leads to reductions in child neglect of between 8 and 16 percent, depending on model specification. Additional findings suggest BBCE may also lead to reductions in medical neglect and sexual abuse, though those findings are sensitive to model specification.

Introduction

Over a third of children in the United States are subject to an investigation for child maltreatment by the time they reach 18 years old (H. Kim et al., 2017), and one estimate suggests each year the United States incurs hundreds of billions of dollars in economic losses from lifetime costs associated with child maltreatment (Fang et al., 2012). Given the prevalence of child maltreatment in the United States, and the cost of that maltreatment, both to the children and families themselves and also to communities and society as a whole, understanding how maltreatment might be prevented is critical.

Poverty is perhaps the strongest individual risk factor for child neglect (Sattler, 2022; Sedlak et al., 2010) and one of several strong risk factors for maltreatment more generally (Drake & Jonson-Reid, 2013; H. Kim & Drake, 2018), and the Supplemental Nutrition Assistance Program (SNAP) is one of the largest antipoverty programs in the United States (Bartfeld et al., 2015). The program specifically attempts to reduce food insecurity, which may decrease child neglect especially as related to food neglect, and also may reduce family financial and other stressors more generally. There is some limited evidence of SNAP's effectiveness at reducing child maltreatment risk. One study examines SNAP receipt and child maltreatment records in Illinois and finds SNAP receipt is associated with reductions in abuse and neglect reports (Lee & Mackey-Bilaver, 2007); another finds that neighborhoods in Connecticut with greater proximity to retailers accepting SNAP have reduced neglect risk (Bullinger, Fleckman, et al., 2021). While neither of these studies offers causal interpretation of results, SNAP receipt is associated with reduced maltreatment risk, especially neglect.

This paper will outline a theoretical framework for a relationship between SNAP receipt and child maltreatment and estimate that relationship empirically by exploiting state decisions related to SNAP broad-based categorical eligibility (BBCE, a policy which expands SNAP eligibility and can simplify enrollment processes) as plausibly exogenous shocks to SNAP receipt. In contrast with prior literature on this topic, data from the entire country will be utilized, making results more generalizable, and to the extent that exogeneity and parallel trends assumptions are satisfied, results may be interpreted causally.

Background and theoretical model of SNAP and child maltreatment

SNAP is a program targeted to low-income households whose goal is to "'alleviate hunger and malnutrition' by 'permit[ing] low-income households to obtain a more nutritious diet through normal channels of trade'" (Food and Nutrition Act of 2008, p. 1-2, as cited in Caswell & Yaktine, 2013, p. 28). The program, originally conceived in 1963, provided benefits to "more than one in seven Americans… at a cost of nearly $80 billion in FY2013, making it the second largest program in the safety net in terms of recipients and fourth largest in terms of expenditure" (Bartfeld et al., 2015, p. 1).

SNAP's potential impacts on child maltreatment, including overall maltreatment, physical abuse, neglect, sexual abuse, and medical neglect, are considered at the level of the family. While there are multiple levels of factors that influence maltreatment risk, including individual, family, community, and cultural factors (MacKenzie et al., 2011; National Research Council, 1993; U.S. Dept. of Health and Human Services, Center for Disease Control and Prevention, National Center for Injury Prevention and Control, Division of Violence Prevention, 2017), SNAP operates mostly on the family level. Two theories explain why low SES would lead to increased child maltreatment risk: family stress and family investment. The family stress model (Conger, 1994; Conrad et al., 2020; Maguire-Jack et al., 2021; Warren & Font, 2015) posits that economic stress harms caregiver mental and behavioral health, which can lead to inhibited capacity for caregiving. The family investment model (Conrad et al., 2020; Maguire-Jack et al., 2021; Warren & Font, 2015) posits that family receipt of economic support, such as from antipoverty programs, allows caregivers to invest additional resources into their families, reducing maltreatment risk. Just as risk factors can influence maltreatment risk, protective factors may potentially mitigate risk. The primary risk factors for child maltreatment considered in this paper are poverty and low socioeconomic status, which influence risk at the family level; SNAP is considered a protective factor (Sattler, 2022) that might mitigate risks to which poverty and low SES contribute.

SNAP benefits are hypothesized to affect child maltreatment in several ways. The first is specific to neglect: one form of neglect occurs when caretakers have insufficient resources to adequately provide for their children's nutrition. If SNAP helps parents to provide food for their children, reducing food insecurity (Lombe et al., 2009), this would de facto make neglect less likely. There is currently a paucity of research examining the extent to which food neglect reports contribute to overall child neglect reporting (Helton, 2016). Estimates of the portion of maltreatment reports attributable specifically to food neglect include three percent (North and South Carolina; Theodore et al., 2007), eight percent (Hawaii; Duggan et al., 2004), and 11 percent (United States, 1990s; Straus et al., 1998). Even studies that attempt to identify neglect sub-types do not typically assess specifically food neglect; studies more often consider physical neglect as its own sub-type, and physical neglect typically includes food but also housing and potentially clothing and medical care. One study using the Maltreatment Classification System found a 50 percent correlation between a food neglect measure and neglect reports to Child Protective Services (Dubowitz et al., 2005).

Related, though worthy of distinction from food insecurity's de jure relationship with neglect, is the relationship between food insecurity and maltreatment more generally, including abuse. Food insecurity is a unique source of stress for families, and stress is a critical contributor to child maltreatment risk (Maguire-Jack & Negash, 2016; McBride et al., 2002; Rodriguez-JenKins & Marcenko, 2014; Warren & Font, 2015; Whipple & Webster-Stratton, 1991) Evidence suggests food insecurity is associated with increased risk of child maltreatment (Jackson et al., 2018), adverse childhood experiences (Jackson et al., 2019), and intimate partner violence, which is itself associated with child maltreatment (Breiding et al., 2017; Hatcher et al., 2019). Even in cases where parents can shield their children from food insecurity – for example, by going without food themselves and providing what they can for their children, which might mean avoiding instances of food neglect (Helton, 2016; McIntyre et al., 2003) – parents may still experience depression and stress, both which have negative impacts on children (Bronte-Tinkew et al., 2007). Helton, Moore, and Henrichsen (2018) used a small sample (n=129) study of mothers at risk for child protective service involvement in Missouri to assess the relationship between food security and risk for child maltreatment, and find that food insecurity may be “a potentially modifiable antecedent to abuse and neglect” (p. 263); another study contends food insecurity is a “potentially malleable risk factor… in relation to violence and victimization during childhood” (Jackson et al., 2018, p. 760). Providing resources for nutrition is a direct way to address this substantial source of family stress.

Three additional pathways are considered that might reduce family stress, and thus impact abuse and neglect more generally. First, SNAP benefits are a direct way to address the issues of low SES, poverty, and financial stress. Though they might not necessarily raise a family up out of poverty, SNAP benefits are a way to mitigate or “alleviate the effects of poverty on low-income children” and their families (Lee & Mackey-Bilaver, 2007, p. 505). One specific effect of poverty that SNAP may alleviate is parental financial stress, which is another reason the program might make abuse and neglect less likely (Maguire-Jack & Negash, 2016). This might be explained by an income effect; SNAP benefit receipt increases discretionary incomes and changes consumption patterns (Castellari et al., 2017; Cotti et al., 2016)

Lee and Mackey-Bilaver (2007) offer two additional reasons that SNAP might reduce family stress: SNAP benefits improve child health outcomes and have positive effects on child temperament. Children who are less healthy may be more likely to be abused, and children with difficult temperaments are also at greater risk (Brayden, Altemeier, Tucker, Dietrich, & Vietze, 1992 as cited in Lee & Mackey-Bilaver, 2007). Gundersen and Kreider (2009), using advanced nonparametric bounding methods to approach causality for food security and child health, find that “food security has a statistically significant positive impact on favorable general health and being a healthy weight" and that "previous research has more likely underestimated than overestimated the causal impacts of food insecurity on health" (971).

Food deserts are one limitation to the theory that SNAP ought to reduce food insecurity. While SNAP benefits should improve family food access, if food choices are inhibited (as in food deserts) then increased access might not substantially alter consumption behavior. One study does suggest that food security for SNAP recipients in a food desert improves when new supermarkets open in their neighborhood (Cantor et al., 2020), but if choice remains limited then SNAP receipt might not affect food security. However, there are three reasons this objection does not substantively diminish the theoretical relationship between SNAP receipt and improvements in food security: first, only about 12.8 percent of people in the U.S. live in a food desert (The Annie E. Casey Foundation, 2021). Second, SNAP recipients in both rural and urban areas tend to do food shopping in areas outside their own neighborhoods to access desired foods (Mantovani, 1996; U.S. Dept. of Agriculture, Economic Research Service, 2009), so living in food deserts may not substantially inhibit improvements to food security from SNAP. Given that most SNAP recipients probably live in non-food desert areas and that even those who do may be able to travel to non-food desert areas, it should still be reasonable to expect SNAP receipt should improve food security. And, even if in food deserts SNAP does not substantially decrease food insecurity or lead to consumption of healthier foods, it may still reduce family stress and increase family investment by allowing families to dedicate resources previously allocated to food to other spending categories (shelter, clothing, parental time, etc.).

Because SNAP should reduce food insecurity and family stress, it should function as a protective factor mitigating maltreatment risk. This paper will attempt to assess the causal relationship between SNAP and child maltreatment outcomes.

Methods

Empirical approach

This study's identification strategy uses variation in state SNAP BBCE as an exogenous shock to SNAP enrollment to identify the impact of SNAP enrollment on child maltreatment outcomes. States with BBCE are compared to themselves pre-BBCE and to states which have never chosen to implement BBCE, in both difference-in-differences and event study frameworks.

Under BBCE, "households may become categorically eligible for SNAP because they qualify for a non-cash Temporary Assistance for Needy Families (TANF) or State maintenance of effort (MOE) funded benefit," (United States Department of Agriculture, 2018, p. 1). BBCE allows states to change their SNAP rules, including raising income eligibility thresholds and adopting less restrictive asset tests (Rosenbaum, 2019). As of 2018, 40 states plus DC had broad-based categorical eligibility (Aussenberg & Falk, 2018; Falk & Aussenberg, 2014). Estimates of increases in SNAP receipt due to BBCE range from around 3.5 percent (Ganong & Liebman, 2018) to about 6 percent (Andrews & Smallwood, 2012; Klerman & Danielson, 2011; Mabli et al., 2009); administrative and microsimulation data indicate around 8 percent of SNAP recipients qualify due to BBCE (Lauffer, 2019; Rosenbaum, 2019). Federal estimates put the number of households receiving SNAP due to BBCE at 914,000, which equates to 4.9 percent of eligible households and 1.7 million people (Food and Nutrition Service, 2019). While precise estimates vary, that BBCE increases SNAP receipt is well established, both because BBCE increases the number of people eligible for SNAP and because it can simplify the process to receive SNAP benefits for people who might be eligible even without BBCE.

Two assumptions critical to this paper's analysis are that state BBCE decisions are influenced neither by child maltreatment outcomes, nor by unobserved characteristics that might also influence child maltreatment outcomes (the exogeneity assumption) and that non-BBCE states constitute valid counterfactuals for states with BBCE – i.e. that absent the policy change BBCE states would continue to behave similarly to non-BBCE states (the parallel trends assumption).

While there are no formal tests that can conclusively prove the validity of exogeneity and parallel trends – they always remain assumptions – their validity can be supported. Regarding exogeneity, recent research suggests state BBCE decisions may have been influenced by state economic conditions such as unemployment. While some states had BBCE prior to the Great Recession, in 2009 USDA recommended states begin utilizing BBCE and 41 had adopted the policy change by 2011 (Ganong & Liebman, 2018; Shahin, 2009). Given that state economic conditions might influence both state BBCE decisions and child maltreatment, this could be a source of concern regarding endogeneity if state economic conditions remained unobserved characteristics. However, this paper controls for state economic conditions, including median income, unemployment, poverty, and gross state product, to minimize these types of endogeneity concerns.

A variety of approaches have been used previously to assess the validity of the parallel trends assumption, including explicitly modeling a linear trend difference in the pre-treatment period between the treated and control groups and then checking for that trend's statistical significance, and using event studies to identify whether pre-treatment "treatment effects" are statistically significant. However, recent research indicates a number of problems to such approaches. For example, depending on specification, such tests are often underpowered to identify economically significant parallel trend violations and may at other times identify statistically significant but economically insignificant trend violations (Bilinski & Hatfield, 2020; Roth, 2018, 2018; Roth et al., 2022). Another issue is that conditioning research based on passing such a parallel trends test may bias results away from representative real-world data generating processes (DGPs) and towards the subset of such DGPs that produce stable pre-treatment trends (Roth, 2020; Roth et al., 2022). Such approaches also turn the nature of hypothesis testing upside-down: instead of assuming a violation exists and attempting to find evidence it does not, such tests assume no violation and look for evidence that a violation might exist.

One proposal to address such issues, adapted from medical research, is non-inferiority testing (NIT; Bilinski & Hatfield, 2020; Roth et al., 2022). This approach recommends that researchers present results of two sets of models: one base model that assumes parallel trends, and another more complex model with divergent pre-treatment trends explicitly included. If the basic and more complex models show similar treatment effect coefficients in the post-treatment period, that provides evidence that any non-parallel trends have little effect on results; if coefficients of the models vary, the degree of variation also allows the researcher to identify the degree to which non-parallel trends may be influencing results, and the degree to which such non-parallel pre-trends call into question the parallel trends assumption. Additionally, Roth et al. (2022) recommend checking for parallel trends conditional on covariates, using event studies, and using estimation methods which incorporate regression adjustment and inverse probability weighting, such as are included in the doubly-robust Callaway-Sant'Anna difference-in-differences (C-S DiD; Callaway & Sant'Anna, 2021; Sant'Anna & Zhao, 2020).

Because state BBCE decisions vary over time, using an estimation approach that accounts for variation in treatment timing is critical (Borusyak et al., 2021; Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfoeuille, 2021; Gardner, 2021; Goodman-Bacon, 2021; Roth et al., 2022; Sant'Anna & Zhao, 2020; Sun & Abraham, 2020). One class of solutions to problems with TWFE with variation in treatment timing is to use imputation estimators (Borusyak et al., 2021; Gardner, 2021; Liu et al., 2021; Wooldridge, 2021), which "fit a TWFE regression… using observations only for units and time periods that are not-yet-treated. They then infer the never-treated potential outcome for each unit using the predicted value from this regression" (Roth et al., 2022, p. 16). One version of the imputation estimator is Gardner's two stage DiD. The outcome of interest is regressed on unit and time fixed effects (and, optionally, covariates) only for periods where treatment has not yet occurred – including both never and not-yet treated units; and then in a second stage, regressing the residual of the first stage on the treatment variable, which can either take the form of a standard DiD term (0 for never treated, 0 for treatment units pre-treatment, 1 for treatment units post-treatment) or be additionally interacted with time to generate event-study estimates. While this paper uses Gardner's two-stage DiD (Butts & Gardner, 2021; Gardner, 2021; Thakral & Tô, 2020) as its preferred specification, results from the Callaway-Sant'Anna DiD (Callaway & Sant'Anna, 2021; Rios-Avila et al., 2021; Sant'Anna & Zhao, 2020) are also presented.

This paper's preferred specification takes the following form. E_s indicates the quarter when a state s begins BBCE. $K_{sq} = q - E_s$ indicates quarters relative to states beginning BBCE. For treatment states, negative values of K_{sq} indicate quarters prior to BBCE and positive values indicate quarters since BBCE. For never-treated states, $K_{sq} = -1$. $D_{sq} = 1\{q \geq Es\}$ indicates states are treated; $D_{sq} = 0$ indicates either not-yet or never treated states.

First, the maltreatment rate Y (maltreatment rate per 1000 children, by type) in state s and quarter q is regressed on state and quarter fixed effects (β_s and ρ_q, respectively) and a vector of state-year specific demographic, economic, and policy covariates X and error term ϵ for periods where $K_{sq} < 0$, i.e. only including never-treated and not-yet treated states.

Equation 1: Gardner two-stage DiD (stage 1)

$$Y_{sq} = \alpha + \beta_s + \rho_q + \theta X_{sy} + \epsilon_{sq} \quad \{s, q : K_{sq} < 0\}$$

For difference-in-difference estimates, the second stage regresses the residuals from the first stage ($\hat{\epsilon_{sq}} = Y_{sq} - \hat{Y}_{sq}$) on the treatment variable D_{sq} with error term υ using the full sample.

Equation 2: Gardner two-stage DiD (stage 2, DiD)

$$\hat{\epsilon}_{sq} = \delta D_{sq} + \upsilon_{sq}$$

Alternatively, for event study estimates, the second stage is adjusted to include *k*, a measure of quarters relative to state selection into BBCE. Treatment states have values of *k* between -20 (five years before BBCE) to 20 (five years after BBCE), excluding *k* = -1 as the reference group. Never-treated states are set to *k* = -1. δ for *k* = -20 through *k* = -2 show pre-BBCE "treatment effects", the difference between treatment units and never-treated units in the pre-treatment period. δ for *k* = 0 through *k* = 20 show the difference between treatment and never-treated units in the treatment period; these are the estimated dynamic treatment effects.

Equation 3: Gardner two-stage DiD (stage 2, event study)

$$\hat{\epsilon}_{sq} = \sum_{k=-20}^{20} \delta k\{K_{sq} = k; k \neq -1\} + v_{sq}$$

Covariates included in *X* in the first stage include economic, demographic, and policy covariates. Economic and demographic covariates include the poverty rate, percent of population that is white, percent of adult population with high school education or higher, gross state product, binary variable for whether the state governor is Democrat, percent of state upper and lower legislative houses that are Democrat, median household income, employment rate, and marriage rate. Policy covariates include both non-SNAP policies and SNAP policies other than BBCE. Non-SNAP policy covariates include percent of population receiving Women, Infants, and Children (WIC) benefits, percent of children in low-income families who are uninsured (a proxy for State Children's Health Insurance Program coverage, which changed substantially in the early 2000s), percent of state population receiving Temporary Assistance for Needy Families (TANF) benefits, and the state minimum wage. SNAP policy covariates include whether states are operating call centers for SNAP, whether states have streamlined application processes for SNAP and SSI, the median SNAP certification periods for households with and without earnings, whether states allow telephone interviews for initial certification and recertification, whether states require fingerprinting, whether states allow online applications, total SNAP outreach spending, whether states have simplified reporting options, and whether states have SNAP auto deductions higher than standard.

Data

Child maltreatment outcome data are drawn from the National Child Abuse and Neglect Data System's (NCANDS) Child Files from 2000 through 2019. These data, which include individual report-level records of all child maltreatment reports made to Child Protective Services in each state and subsequently reported to the federal government by the states (with some missing data from certain states in certain report years), are converted to quarterly state population-level rates using annual population estimates from the Surveillance, Epidemiology, and End Results (SEER) Program. Some prior research using NCANDS data has log-transformed counts of reports, or in some cases log-transformed report rates, in order to make data closer to normally distributed and reduce the influence of outliers. In this study, visual inspection of report rate distributions via histogram (Figure 3.5) show approximately normal distribution with slight right skew; log-transformation either of counts or of rates moves distributions of state-level data away from normal. Figure 3.7 replicates the study's preferred specification using log-transformed maltreatment report counts, by-type, to show that this decision does not substantially influence results.

In addition to child maltreatment reports, NCANDS also includes substantiation for each report, and substantiated reports could be considered alongside overall reports. However, assessing impact on reports is preferable for a number of reasons, including that substantiation has been found to vary due to factors other than actual maltreatment risk (Jones & Finkelhor, 2001), because children subject to reports are at similar risk of future cases of maltreatment regardless of substantiation of initial reports (Chalk, 2012; Fallon et al., 2010; Hussey et al., 2005; H. Kim et al., 2017; Kohl et al., 2009), and reports are considered a more accurate measure of maltreatment incidence than substantiated cases (H. Kim et al., 2017).

State-year demographic, economic, and non-SNAP policy covariates are drawn primarily from the National Welfare Data state panel dataset (University of Kentucky Center for Poverty Research, 2022). Education is drawn from Current Population Survey Annual Social and Economic Supplements (CPS ASEC, Flood et al., 2021). Household median incomes are from Federal Reserve Economic Data (St. Louis Fed, 2022). Both BBCE treatment and other SNAP policy covariates are drawn from the United States Department of Agriculture's SNAP Policy Database (Tiehen et al., 2019).

Table 3.1 compares mean values of the dependent variables and covariates between control (never-BBCE) states and treatment (pre-BBCE) states using two-sample t-tests (significance of the difference between groups shown in with p-values). One item of general concern might be that treatment states are different from control states, with lower levels of SNAP receipt, food insecurity, and overall maltreatment, higher levels of neglect, and lower levels of medical neglect and sexual abuse. However, this is only a concern if that difference is also time-variant; that is assessed in event study results (which indicate differences appear to be time invariant). Despite some variation in levels of dependent variables, for the most part they show stable parallel trends (with some exceptions discussed below).

Variation in levels of SNAP receipt should also be of lesser concern because SNAP policies other than BBCE (represented by the SNAP policy covariates) in general became more liberal over time (even in non-BBCE states), increasing eligibility and reducing transaction costs and stigma associated with SNAP (Tiehen & Marquardt, 2020). Inclusion of those policy variables, which changed in some cases after states adopted BBCE, explains some of the difference between control and pre-treatment states. Beyond SNAP receipt, a more general observation is that inclusion of the rich set of economic, political, and demographic variables will explain large portions of variation between control and pre-treatment states (treatment and control states are more similar when also conditioned on covariates).

Results

Figure 3.1 shows results of event studies examining the impact of BBCE on SNAP receipt and food insecurity (all log-transformed counts, rather than rates) – mechanisms by which portions of this paper's theoretical model function. Results indicate increases in SNAP receipt of about 5 percent post-BBCE. While the DiD coefficient for the entire post-treatment period is not statistically significant at $p < .05$, event study results show increasing enrollment starting about 3 quarters after BBCE starts and increasing consistently for the next 5 years; the increase is large enough to reach statistical significance at $p < .05$ within about 20 quarters. Results for food insecurity measures show reductions for 8-12 quarters after starting BBCE, with effect sizes of about 15 percent over 5 years. Taken together, these results provide some evidence that broad-based categorical eligibility leads to increases in SNAP receipt (which are in the range of previously identified treatment effects) and reductions in marginal food insecurity, supporting this paper's proposed theoretical model.

Figure 3.2 shows the event study and DiD results for maltreatment report rates, by type, using Gardner two-stage DiD and the full array of covariates from Table 3.1. Results show reductions in neglect report rates (1.55, 23 percent) and sexual abuse report rates (0.08, 11 percent) which are statistically significant at $p < .05$. Reductions in these report rates are consistent for about the first three years post-BBCE and they level off after. Results of Bilinski and Hatfield non-inferiority testing (NIT), in which divergent pre-treatment trends are explicitly modeled via restricted linear splines, are shown in Figure 3.6. While estimated treatment effects are smaller in Figure 3.6 (the coefficient on neglect is -0.59, which is equivalent to an 8.8 percent decrease; the coefficient on sexual abuse is -0.06, which is equivalent to an 8.6 percent decrease), trends remain consistent and effects are significant at $p < .05$ within five years. NIT results indicate that divergent pre-treatment trends may explain part of the observed effects in Figure 3.2, but effects are still observed after modelling divergent trends. Physical abuse and medical neglect show decreases in this specification as well, but their effects are less precisely estimated, and as shown in the following figures, are sensitive to model specification.

Results from Gardner two-stage DiD with a smaller array of covariates including just poverty rate, percent of state population that is white, percent of adults with high school education or higher, and state child population are shown in Figure 3.3, along with results from Callway-Sant'Anna doubly-robust DiD using the same covariates. This shows that both physical abuse and medical neglect results are sensitive to model specification: physical abuse is consistent between Gardner and C-S in Figure 3.3, but estimates are inconsistent with those from Figure 3.2. Medical neglect results are not consistent across estimators, with C-S showing a statistically significant (and large) decrease, but with no observed treatment effect in this version of the Gardner DiD. Estimated reductions in neglect are both statistically significant at $p < .05$ for most of the study period and are practically significant, with reductions similar to those observed in Figure 3.2. Sexual abuse results are also consistent between Gardner and C-S DiD in Figure 3.3, though results are less precisely estimated and are statistically insignificant at $p < .05$.

The doubly-robust C-S estimator includes both stabilized inverse probability weighting and outcome regression adjustment – two additional approaches recommended when considering parallel trend assumptions (Roth et al., 2022). Figure 3.4 replicates the doubly-robust C-S approach from Figure 3.3, but uses county-level rather than state-level data. Because distributions vary when considering county-level data, these use log transformed counts rather than rates. County-level results indicate a 16 percent reduction in neglect and a 12 percent reduction in sexual abuse, though both are imprecisely estimated and do not reach statistical significance at $p < .05$ during the study period. Medical neglect shows an imprecisely estimated and inconsistent effect. Physical abuse shows a reduction of 32 percent, large enough that even with wide confidence intervals it remains statistically significant for most of the post-treatment period.

Figure 3.7 replicates Figure 3.2 using log transformed counts of state-level child maltreatment reports, rather than rates per 1000 children. Results for neglect and sexual abuse show consistent reductions. Physical abuse shows a 15 percent reduction with very wide confidence intervals including zero, indicating imprecise estimation and no statistical significance at $p < .05$, and medical neglect shows a reduction of 36 percent, which is not significant at $p < .05$ during the whole period (though statistically significant in 2 individual periods near the end of the sample).

Discussion

This study finds that BBCE increases receipt of SNAP, supporting the proposed identification strategy to exploit BBCE as a shock to SNAP receipt, and reduces multiple measures of food insecurity, supporting the proposed theoretical model that SNAP reduces food insecurity. Gardner two-stage DiD results show reductions in both neglect and sexual abuse, though those have some pre-treatment divergent trends and NIT suggests those divergent trends drive portions of estimated treatment coefficients. C-S DiD results, which include doubly-robust regression adjustment and stabilized inverse probability weighting, correct for those pre-treatment divergent trends and still show reductions in neglect and sexual abuse after BBCE. Reductions in neglect and sexual abuse following BBCE are consistent across estimators, model specification, and units of analysis. Some specifications show reductions in physical abuse and neglect, but those results are sensitive to model specification and units of analysis.

That increases in SNAP receipt could lead to reductions in neglect is in line with previous research: Lee and Mackey-Bilaver (2007) find associations between SNAP receipt and reductions in child abuse and neglect reports, and Bullinger et al., (2021) find proximity to retail locations that accept SNAP is associated with reductions in both child maltreatment reports and substantiated cases of maltreatment. Considering that food insecurity and food neglect are intrinsically linked and that food neglect constitutes some portion of overall neglect reports, this is to be expected: if BBCE reduces food insecurity, it should reduce neglect as well. However, what makes this finding more interesting is that reductions in food insecurity and neglect are of similar sizes: each appear to have decreased, in some specifications, by about 15 percent. Food insecurity and food neglect are probably not perfectly correlated, and even if they were, food neglect constitutes only a portion of overall neglect reports. While some of the estimated reduction in neglect is probably due to reduced food insecurity and food neglect, it would be reasonable to think that some portions of the reduction in neglect are for other forms of neglect, such as clothing, medical care, or supervisory.

It is reasonable that this paper does not find robust estimated effects on physical abuse: child neglect is several times more closely associated with low socioeconomic status than is physical abuse (Sedlak et al., 2010). Reductions in medical neglect in some specifications are interesting, but are less readily interpretable as robust causal effects given their sensitivity to model specification.

This paper's most novel finding is the relationship between BBCE and reductions in child sexual abuse. Findings suggest BBCE led to reductions in child sexual abuse reports of between 8.6 and 17 percent. One reasonable concern with such a large estimated effect is that BBCE is only estimated to lead to increases of SNAP receipt of about 5 percent; sexual abuse is estimated to decrease by an amount twice the gain in SNAP receipt. There are several explanations for this finding. First, socioeconomic status is a risk factor for sexual abuse incidence: the Fourth National Incidence Study finds children in low-socioeconomic status households are around three times more likely to experience sexual abuse than are children not living in low-socioeconomic status households (Sedlak et al., 2010) and other studies also find SES is a risk factor (Assink et al., 2019). Policies that affect low-socioeconomic status households might have an outsize effect on related outcomes. Second, child sexual abuse incidence may be much higher than reported estimates suggest (Harvey et al., 2021; Priebe & Svedin, 2008) and some argue socioeconomic status is a risk factor for child sexual abuse reporting rather than incidence (Finkelhor, 1993). While robust and recent evidence indicates socioeconomic status is a risk factor for incidence, if it were also a risk factor for reporting – i.e. if low SES households were more likely to be reported than higher SES households – then estimated rates would disproportionately be counting cases from low socioeconomic status families and undercounting incidence from families with higher socioeconomic status. In that case, the denominator of overall cases may be much larger (and thus the real percentage reduction in overall cases smaller than estimated here).

Limitations

An ideal dataset for identifying the causal effect of SNAP receipt on child maltreatment outcomes would include individual level data with both child maltreatment and family socioeconomic and demographic characteristics. While the NCANDS data on which this study is based are at the child-report level, they do not include socioeconomic characteristics and are not identifiable in any way that could merge them with other individual-level data. In order to include such additional data, this study collapses counts of reports to the state-level and merges on additional state-level covariates. This aggregation eliminates variation at more granular units of analysis, such as the individual or county-level, and also substantially limits the study's sample size.

This study also uses maltreatment reports, and not substantiated cases, as its dependent variables. While this is probably a more accurate way to assess incidence of maltreatment, this also means that reports of maltreatment which have been found not to meet standards sufficient to qualify as substantiated are being counted. Another issue with using child maltreatment reports is that this study can only assess the relationship between BBCE and cases of maltreatment that are reported. If maltreatment cases are not reported, they would not be considered in this study.

Using reports as a proxy for incidence might be problematic if there is systemic bias in child maltreatment reporting, especially insofar as that bias might also interact with SNAP BBCE. While SES is commonly understood to be a risk factor for child maltreatment, SES might also be a risk factor for child maltreatment reporting (Finkelhor, 1993); some research indicates families of lower socioeconomic status may be more likely to be reported for child maltreatment by laypeople (Calheiros et al., 2020) and by pediatricians (Laskey et al., 2012). However, reviews of bias in child maltreatment reporting have found "that the overrepresentation of poor children is driven largely by the presence of increased risk among the poor children that come to the attention of child welfare rather than high levels of systemic class bias" (Jonson-Reid et al., 2009, p. 1) and that there is little empirical evidence that disproportionate representation of low-SES families in child maltreatment is due to bias (Drake & Zuravin, 1998). Disentangling report risk from incidence is beyond the scope of this research, but if any systematic bias in maltreatment reporting also has an interaction with SNAP receipt, that could complicate interpretation of study findings.

This study's preferred specification includes an array of state economic factors, including gross state product, poverty rate, median household income, and employment rate, and several political factors including Democratic control of each house of state legislatures and the governorship. To the extent that inclusion of these variables does not sufficiently control for factors that influence state decisions regarding BBCE, there might be some concern about endogenous selection into treatment, which would undermine the identification strategy and weaken causal interpretation of the results.

Last, while this study controls for variation in additional SNAP policy variables, it does not control for variation in state policies defining child maltreatment. While models including unit fixed effects should absorb any inter-state policy variation (if time invariant), unit and time fixed effects would not account for changes to state definitions of child maltreatment. This would be of particular concern if in-state maltreatment definition variations also interacted with SNAP BBCE variation. Future research should consider approaches to account for variation of state maltreatment policies over time.

Conclusion

This study provides some of the first nationally representative estimates of the causal effect of SNAP receipt on child maltreatment reports. Results indicate BBCE led to reductions in neglect reports and sexual abuse reports. Some results showed reductions in physical abuse and medical neglect, though those findings were sensitive to model specification.

This study's findings have practical applications. They contribute to the literature on externalities associated with the Supplemental Nutrition Assistance Program and improve our understanding of benefits the program provides beyond its effect of reducing food insecurity and improving health. The findings expand on our knowledge relating to the impacts specifically of broad-based categorical eligibility for SNAP. The findings also contribute to our understanding of the etiology of child maltreatment and how antipoverty programs may reduce child maltreatment risk, across multiple types of maltreatment.

Tables

Table 3.1 Pre-treatment descriptive statistics, t-test

	Controls (non-BBCE)	Treatment (pre-BBCE)	p-value
Dependent variables			
SNAP receipt	9.82	8.98	0.00
Marginally food insecure, %	23.66	22.13	0.00
Food insecure, %	13.86	13.04	0.00
Very low food security, %	4.62	4.21	0.00
Overall maltreatment report rate per 1000 children	11.88	10.92	0.00
Physical abuse report rate per 1000 children	2.28	2.24	0.55
Neglect report rate per 1000 children	5.81	6.73	0.00
Medical neglect report rate per 1000 children	0.24	0.19	0.00
Sexual abuse report rate per 1000 children	0.95	0.70	0.00
Covariates			
Population below FPL, %	12.05	12.29	0.18
Population that is white, %	84.50	80.63	0.00
Population with HS education or higher, %	82.81	81.22	0.00
Gross state product (thousands)	159,274	295,869	0.00
State governors, % Democrat	37.65	44.91	0.01
State legislature lower house, % Democrat	38.37	55.82	0.00
State legislature upper house, % Democrat	35.89	55.30	0.00
Median household income	60,913.30	61,893.10	0.06
Employment rate, %	62.74	61.80	0.00
Marriage rate, %	54.84	52.54	0.00
Population	3,662,007	6,519,721	0.00
Non-SNAP policy covariates			
WIC receipt, %	7.34	7.52	0.15
Uninsured children in low-income families, %	4.92	6.01	0.00
Population receiving TANF benefits, %	1.11	1.34	0.00
State minimum wage	6.01	6.11	0.13
SNAP policy covariates			
Operating SNAP call centers, %	32.69	32.19	0.84
Streamlined application for SNAP/SSI, %	10.55	19.46	0.00
Certification period (months), HH with earnings, median	8.54	7.96	0.00
Certification period (months), HH without earnings, median	9.06	8.65	0.01
Allowing telephone interviews for initial certification, %	41.18	10.63	0.00
Allowing telephone interviews for recertification, %	51.47	27.84	0.00
Requiring fingerprinting of SNAP recipients, %	0.00	12.43	0.00
Allowing online SNAP application, %	44.85	25.00	0.00
SNAP outreach spending dollars (thousands)	10.81	36.20	0.00
Simplified reporting option, %	70.43	82.78	0.00
SNAP auto deductions higher than standard, %	8.06	11.08	0.06

Table shows t-test comparing means for treated (pre-treatment) and control states (control mean, treatment mean, and p-value of difference between the two).

Figures

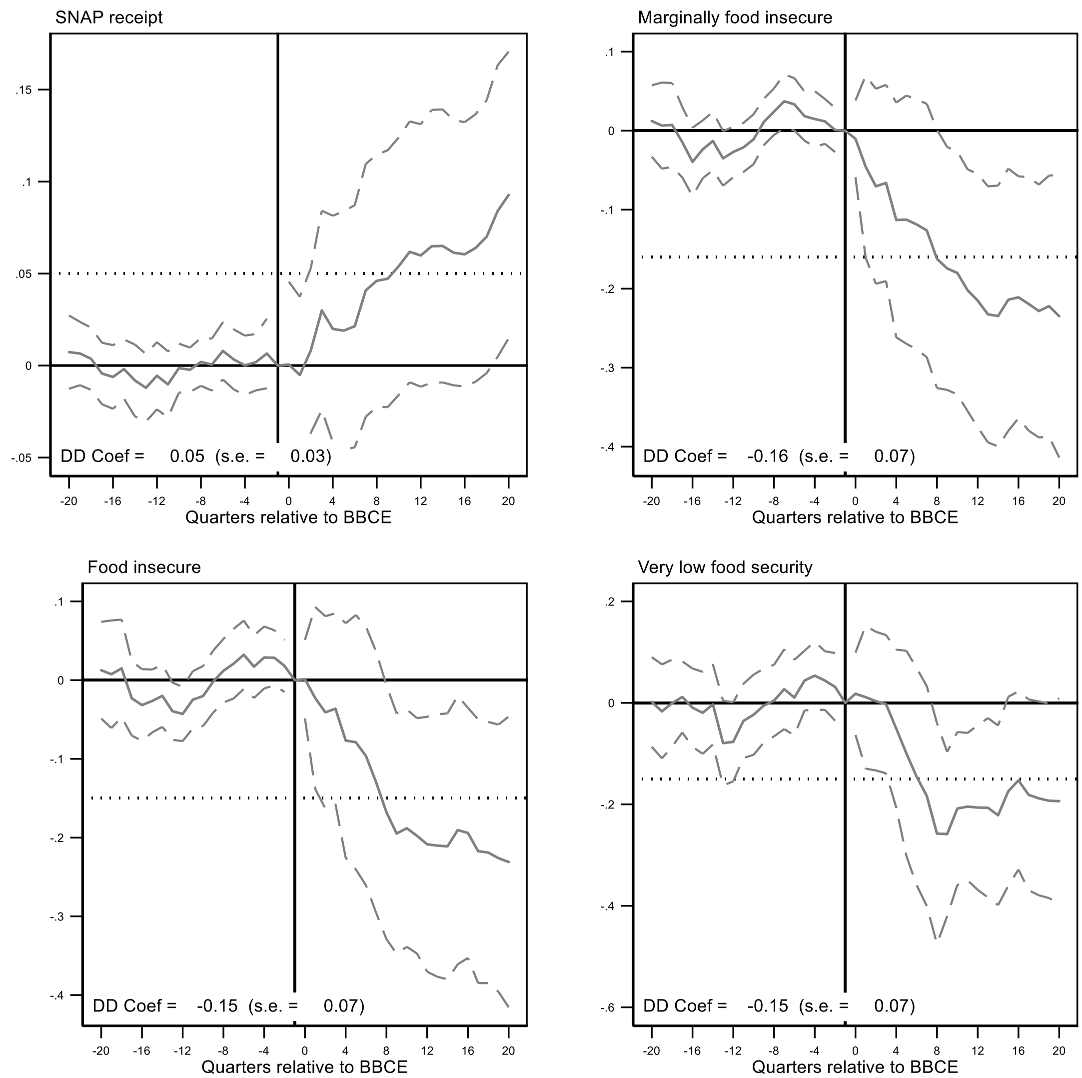

Figure 3.1 Event study, SNAP receipt and food security, Gardner DiD, state-level
Graphs display differences in estimated coefficients of SNAP receipt and three levels of food insecurity (all log-transformed) between treated and control states from 20 quarters before to 20 quarters after BBCE. Solid gray line shows estimated coefficient in each quarter; dashed gray lines show upper and lower confidence intervals; horizontal gray dotted line shows estimated post-treatment DiD coefficient. All models are estimated using Gardner two-stage difference-in-difference and include state and quarter fixed effects and the full array of demographic, economic, political, and policy covariates. Eicker-Huber-White robust standard errors included, clustered at state-level.

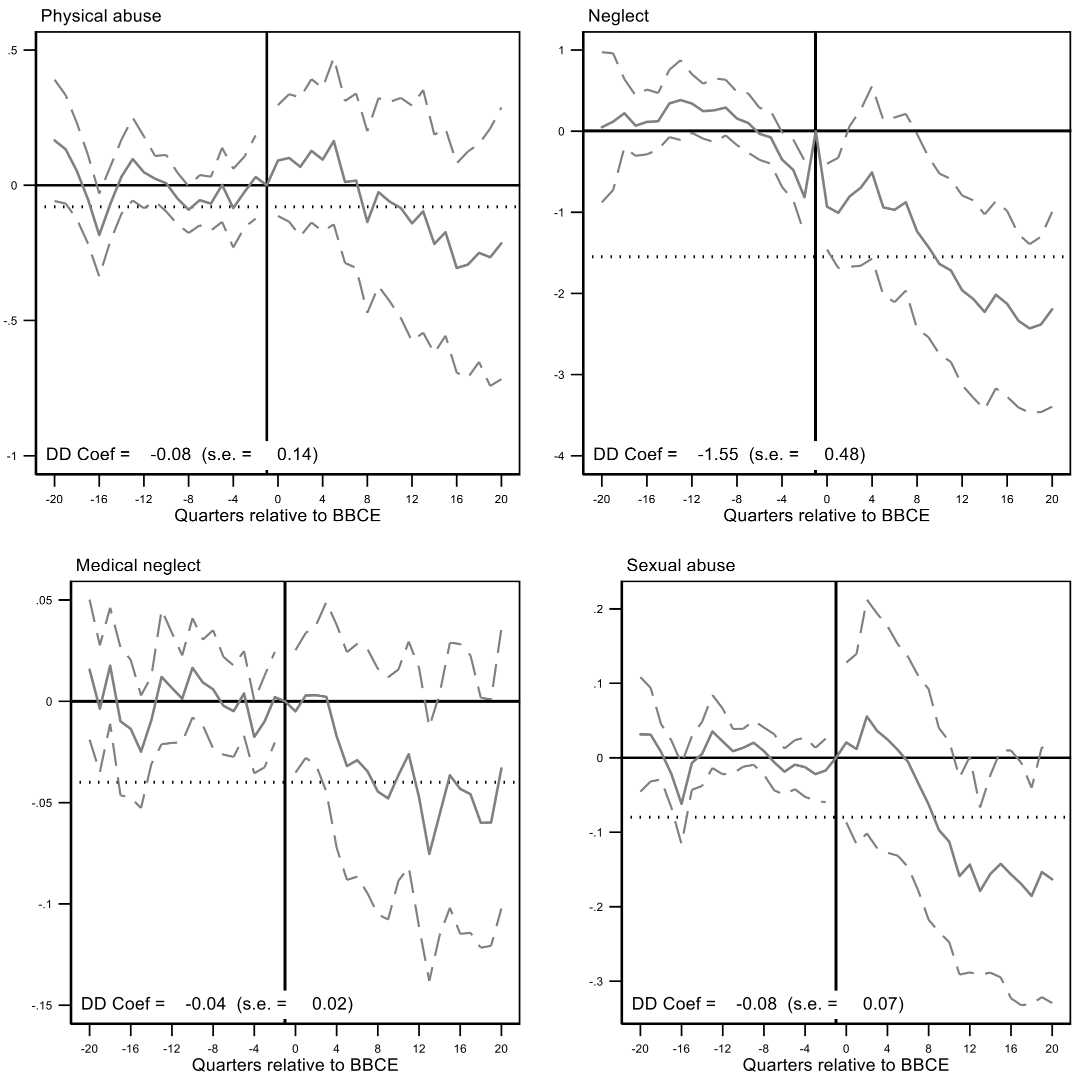

Figure 3.2 Event study, child maltreatment report rates, Gardner DiD, state-level
Graphs display differences in estimated coefficients of child maltreatment report rate (by-type) for all children between treated and control states from 20 quarters before to 20 quarters after BBCE. Solid gray line shows estimated coefficient in each quarter; dashed gray lines show upper and lower confidence intervals; horizontal gray dotted line shows estimated post-treatment DiD coefficient. All models are estimated using Gardner two-stage difference-in-difference and include state and quarter fixed effects and the full array of demographic, economic, political, and policy covariates. Eicker-Huber-White robust standard errors included, clustered at state-level. Compare to Figure 3.6 for Bilinksi and Hatfield non-inferiority test.

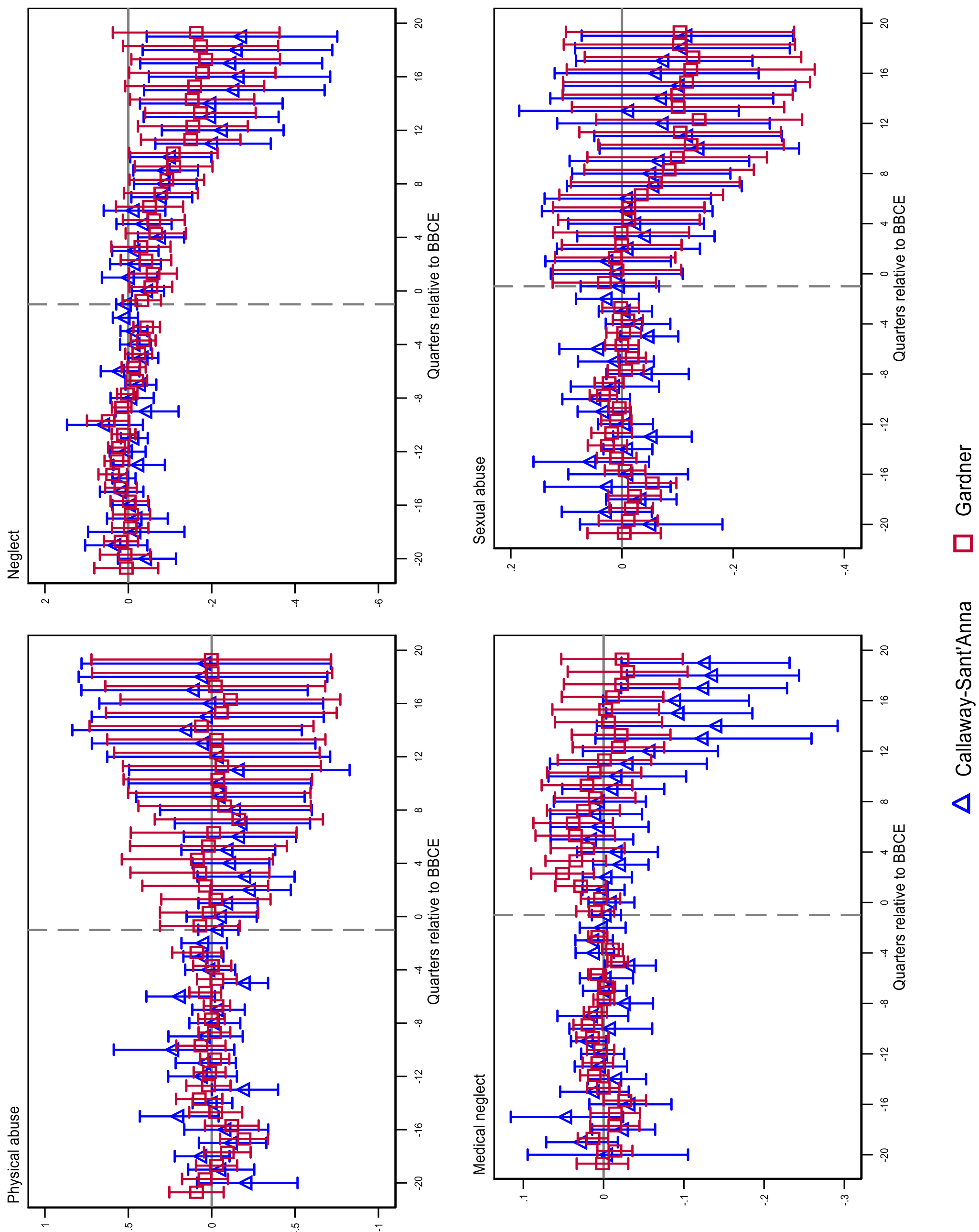

Figure 3.3 Event study, child maltreatment report rates, Gardner and C-S DiD, state-level
Graphs display results from Gardner two-stage DiD and doubly robust Callaway-Sant'Anna DiD to estimate differences in coefficients of child maltreatment report rate (by type) between treated and control states from 20 quarters before to 20 quarters after BBCE. All models include poverty rate, percent of population that is white, percent of adults with high school or higher education, and county child population, with cluster-robust standard errors.

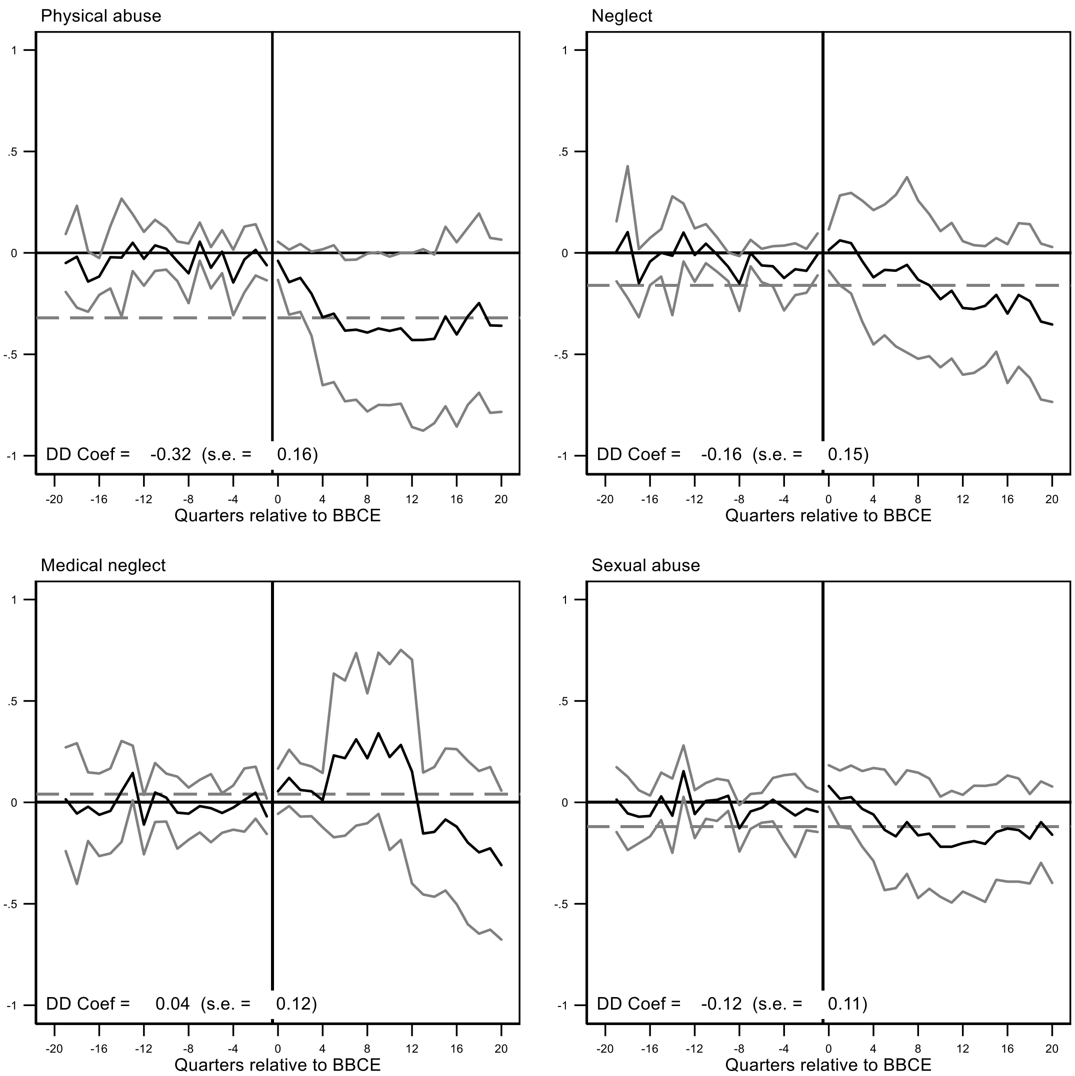

Figure 3.4 Event study, child maltreatment reports (log-transformed), C-S DiD, county-level
Graphs display differences in estimated coefficients of the log of child maltreatment reports (by-type) between treated and control counties from 20 quarters before to 20 quarters after BBCE. Solid black line shows estimated coefficients in each quarter; solid gray lines show upper and lower confidence intervals; horizontal gray dashed line shows post-treatment DiD coefficient. All models are estimated using doubly robust Callaway-Sant'Anna difference-in-difference and include county child population, county unemployment rate, percent of the county population that is Black, and percent of adults in the state with high school education or higher. Standard errors clustered at state-level.

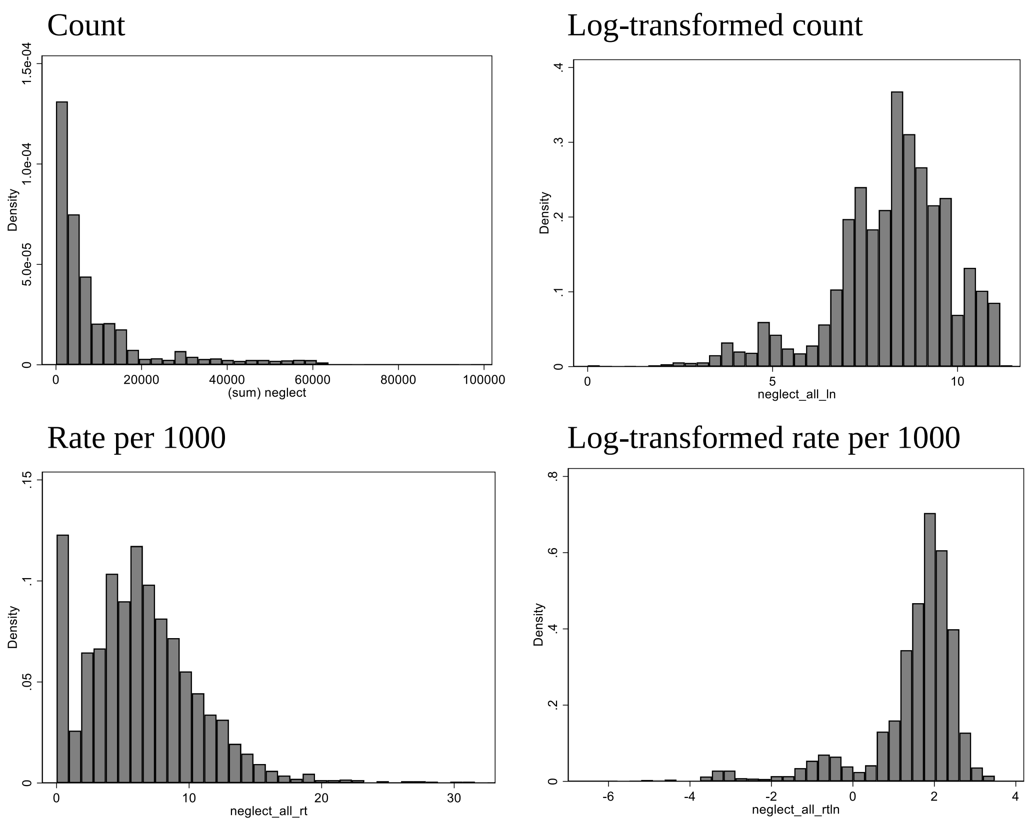

Figure 3.5 Histogram, distributions of child neglect reports by measure
Histograms show distributions of child neglect reports by count, log-transformed count, rate per 1000 children, and log-transformed rate per 1000 children.

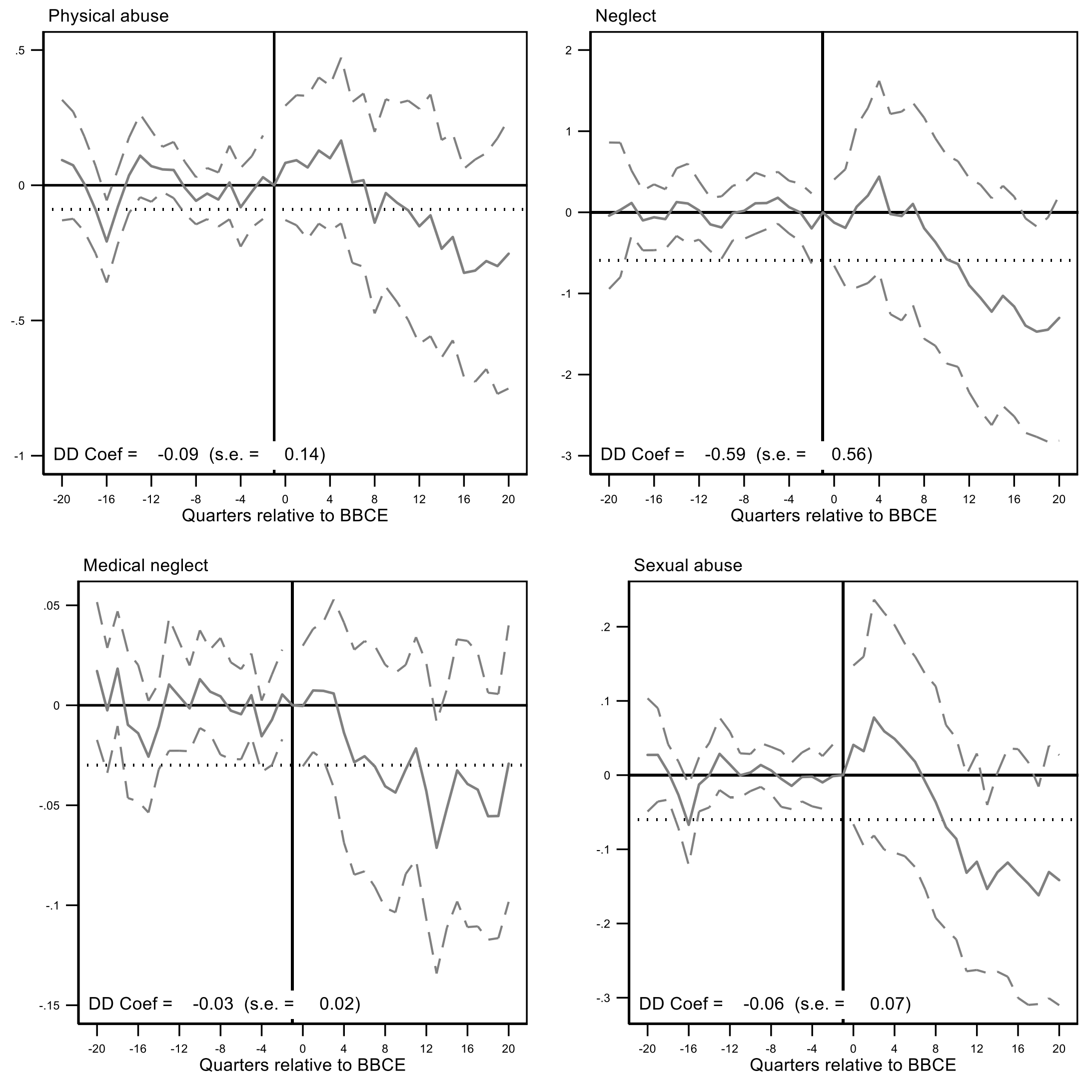

Figure 3.6 Event study, child maltreatment report rates, Gardner DiD, state-level, NIT
Graphs display differences in estimated coefficients of child maltreatment (by-type) for all children between treated and control states from 20 quarters before to 20 quarters after BBCE. Solid gray line shows estimated coefficient in each quarter; dashed gray lines show upper and lower confidence intervals; horizontal gray dotted line shows estimated post-treatment DiD coefficient. All models are estimated using Gardner two-stage difference-in-difference and include state and quarter fixed effects and the full array of demographic, economic, political, and policy covariates. Models also include linear spline trend differences between treated and control states in the pre-treatment period to explicitly model non-parallel trends (compare to Figure 3.2 for Bilinksi and Hatfield non-inferiority test). Eicker-Huber-White robust standard errors included, clustered at state-level.

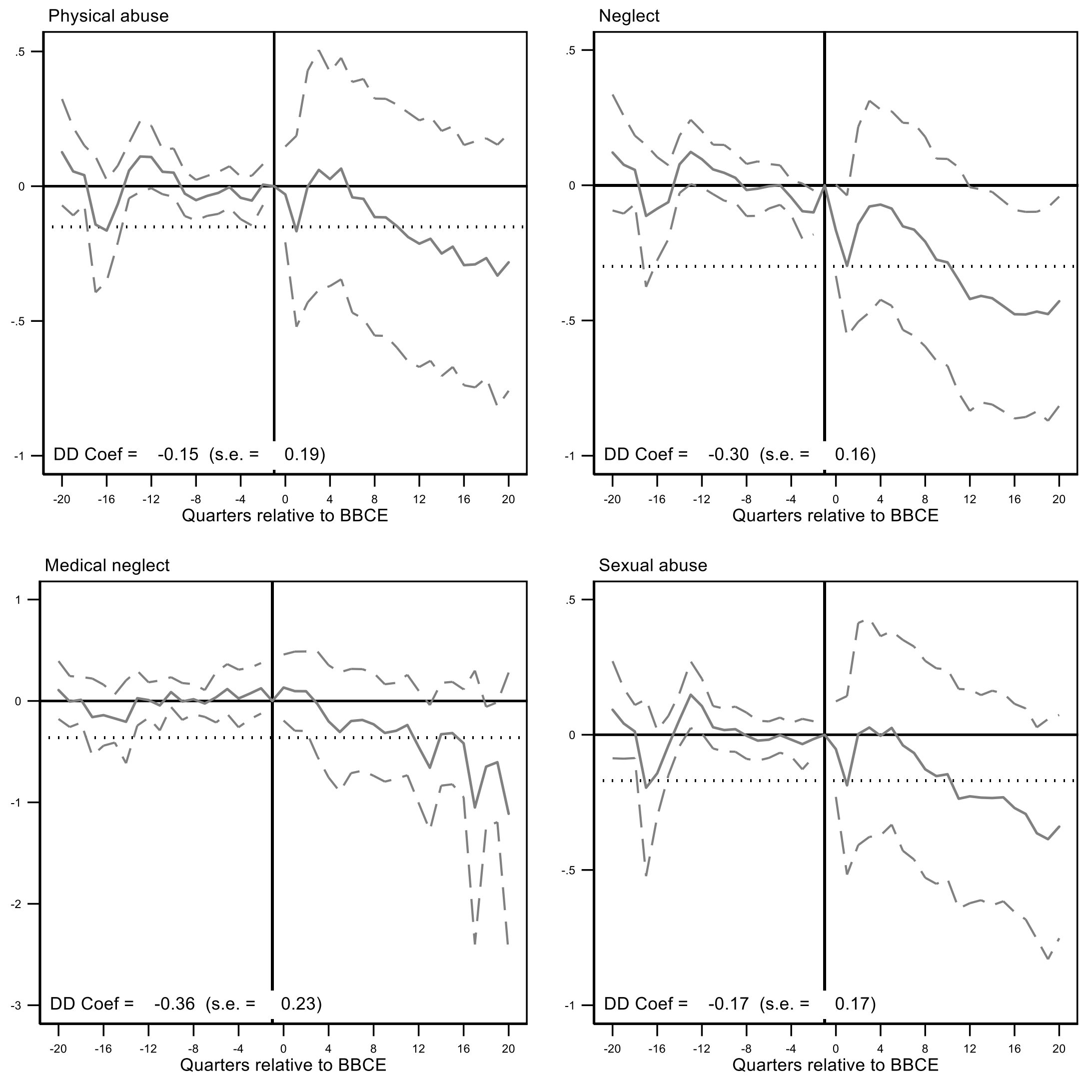

Figure 3.7 Event study, child maltreatment reports, log-transformed, Gardner DiD, state-level
Graphs display differences in estimated coefficients of log-transformed child maltreatment reports (by-type) for all children between treated and control states from 20 quarters before to 20 quarters after BBCE. Solid gray line shows estimated coefficient in each quarter; dashed gray lines show upper and lower confidence intervals; horizontal gray dotted line shows estimated post-treatment DiD coefficient. All models are estimated using Gardner two-stage difference-in-difference and include state and quarter fixed effects and the full array of demographic, economic, political, and policy covariates. Eicker-Huber-White robust standard errors included, clustered at state-level.

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BIOGRAPHY

Neil McCray graduated from Hallsville High School in Texas in 2010. He received his Bachelor of Specialized Studies in Philosophy and Politics from Cornell College in 2013 and his Master of Public Policy from George Mason University in 2017. He worked as a Senior Research Analyst and Research Consultant at The Lewin Group from 2014 through 2019. He was also a research assistant, teaching assistant, and instructor at GMU from 2017 through 2021. Starting in 2020 he was a Senior Researcher with Virginia's Department of Medical Assistance Services. He received his Ph.D. in Health Services Research with a concentration in Health Systems and Policy in 2022.