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

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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; Miller & Wherry, 2017).[1]

[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.

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 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.

[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, demographic, and political covariates, are

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 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

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

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 (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 (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 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 1.1 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

[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.

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.

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 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).

[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 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.

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 1.2 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 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 1.1), neglect reports (Figure 1.2), medical neglect reports (Figure 1.3), and sexual abuse reports (Figure 1.4), 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 1.2): 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 1.1) 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 1.1. 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 1.2 and state-level data in Figure 1.5. 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 1.6), 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 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 1.1 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 1.2 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

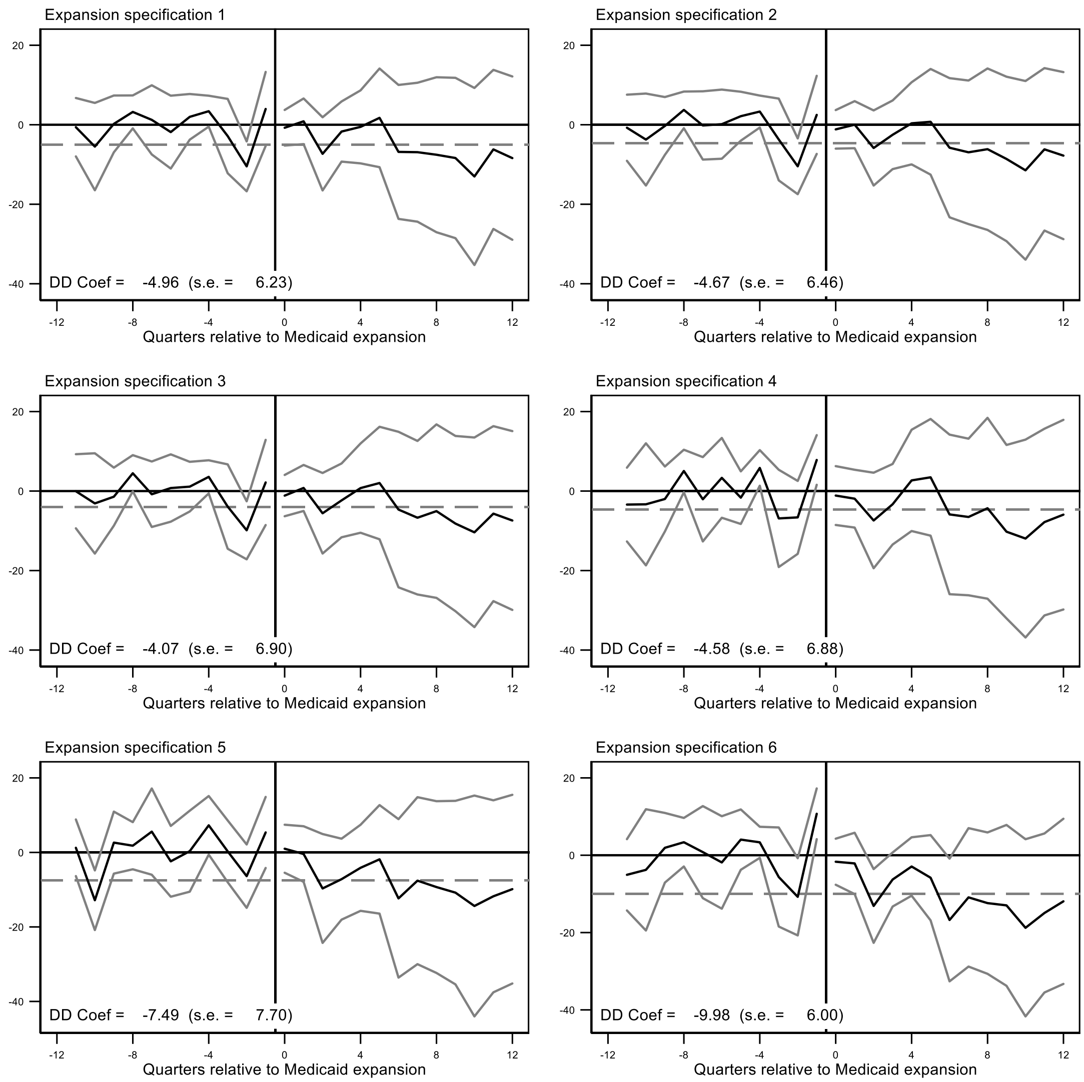

Figure 1.1 Event study, physical abuse reports (log-transformed), C-S DiD, by Medicaid expansion specification, county-level

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 1.1 for details).

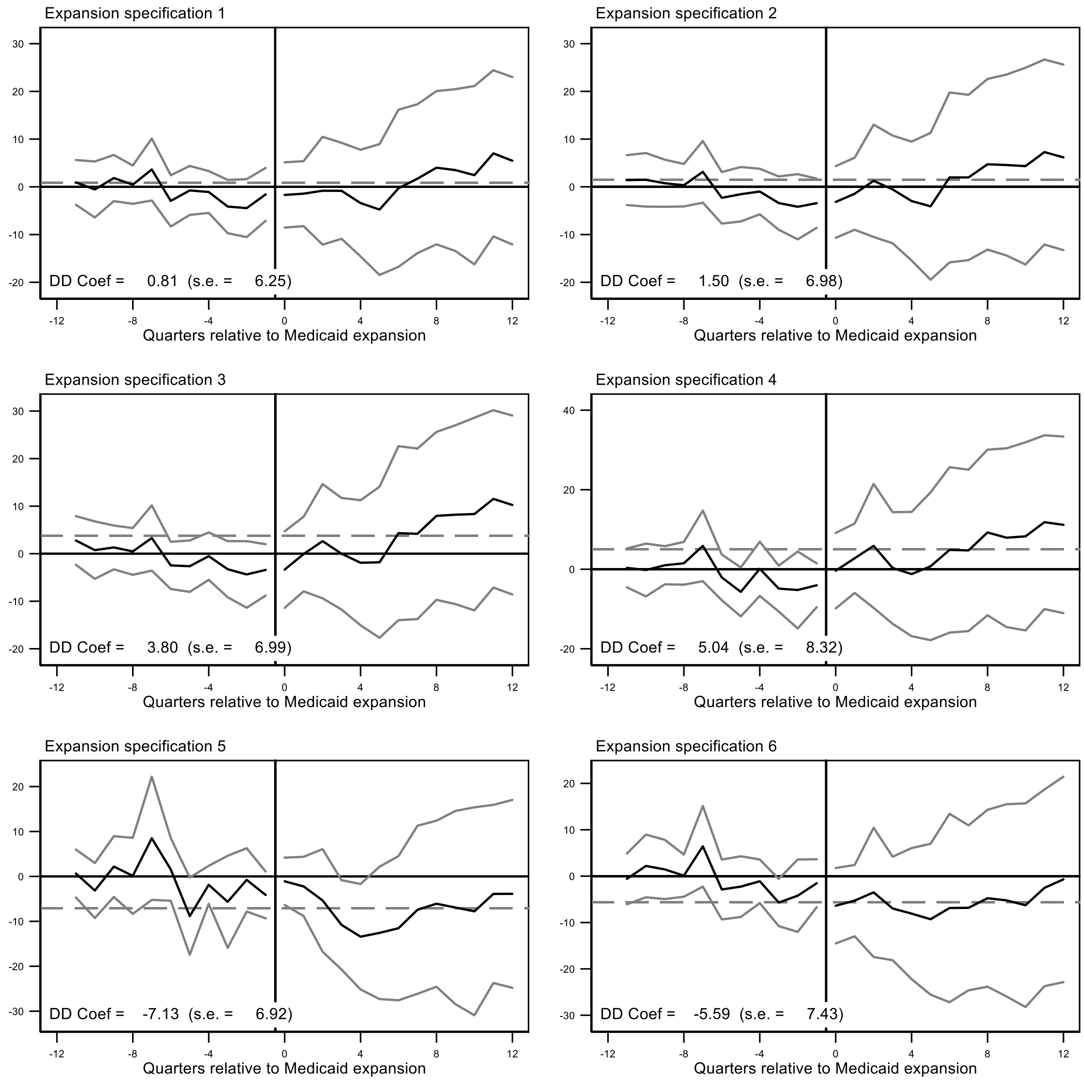

Figure 1.2 Event study, neglect reports (log-transformed), C-S DiD, by Medicaid expansion specification, county-level
Graphs display differences in estimated coefficients of the log of child neglect reports between treated and control counties from 12 quarters before to 12 quarters after Medicaid expansion. 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 1.1 for details).

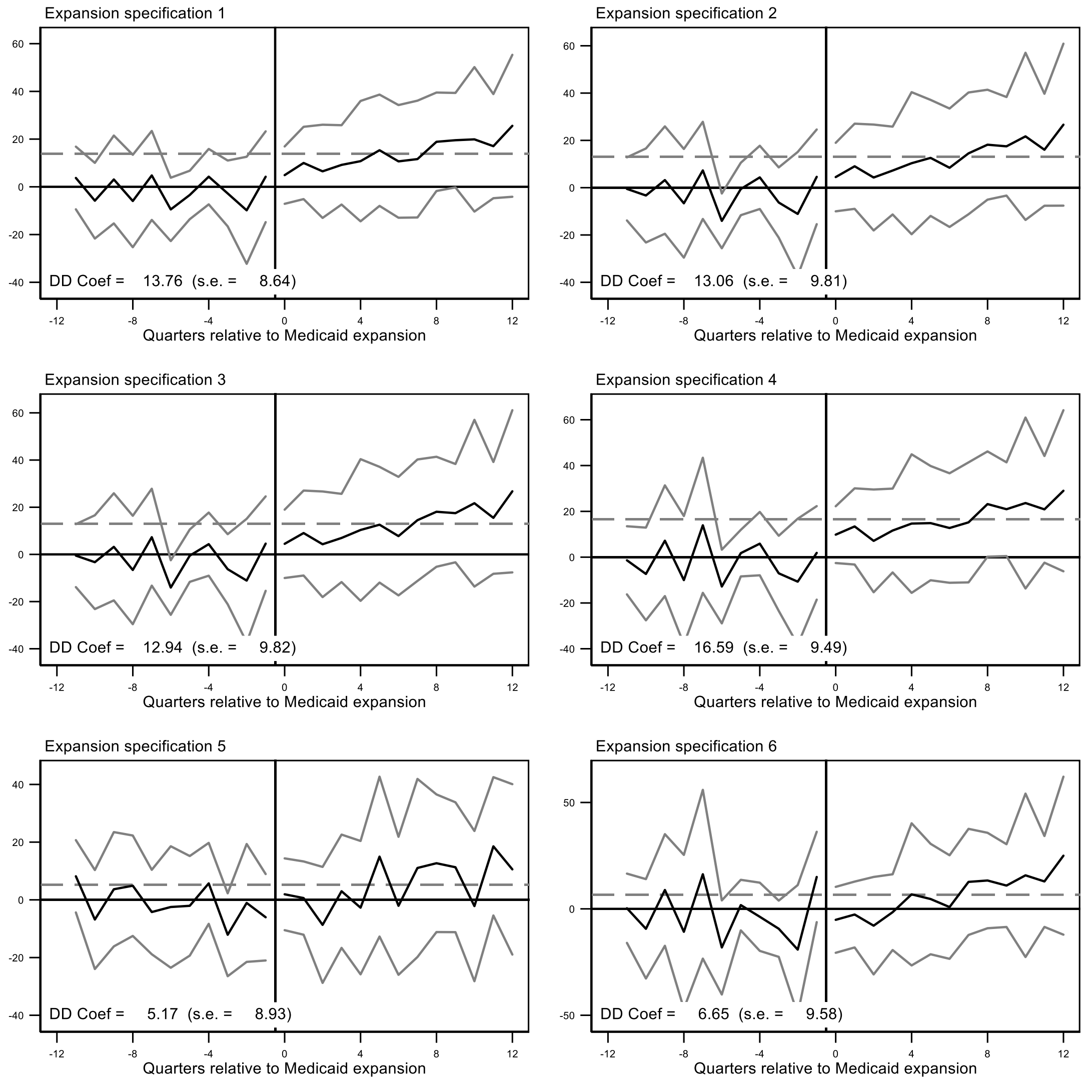

Figure 1.3 Event study, medical neglect reports (log-transformed), C-S DiD, by Medicaid expansion specification, county-level

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 1.1 for details).

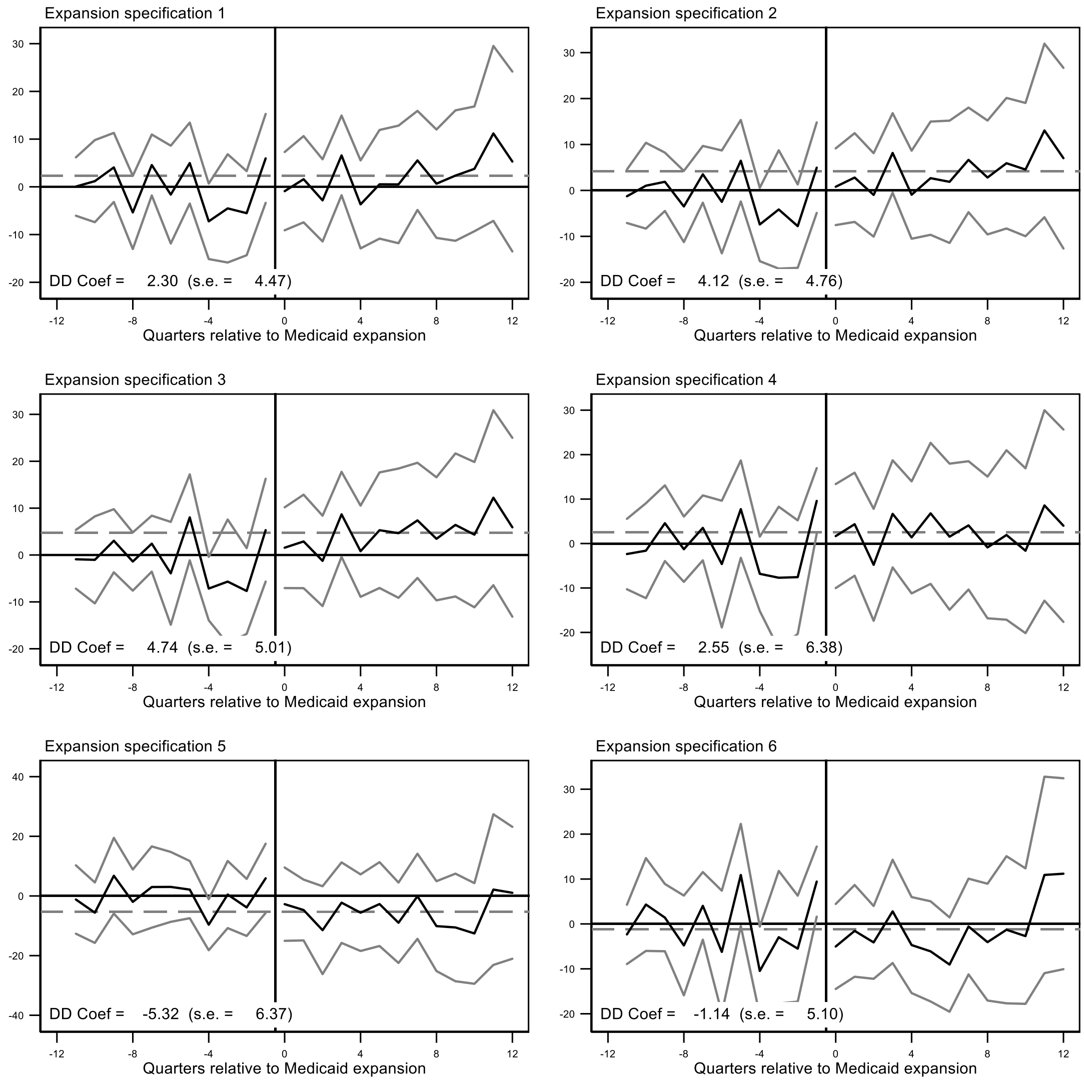

Figure 1.4 Event study, sexual abuse reports (log-transformed), C-S DiD, by Medicaid expansion specification, county-level

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 1.1 for details).

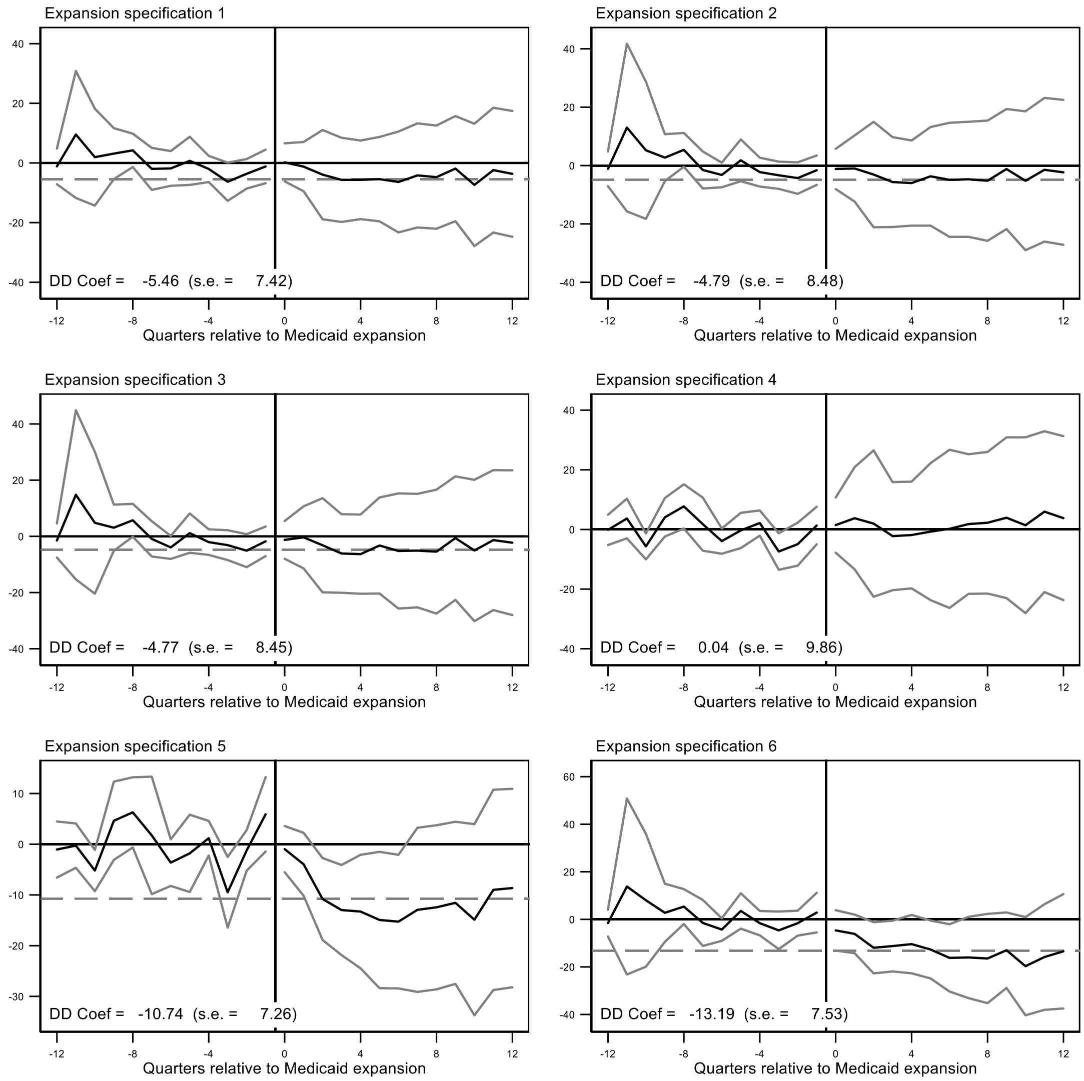

Figure 1.5 Event study, neglect reports (log-transformed), C-S DiD, by Medicaid expansion specification, state-level
Graphs display differences in estimated coefficients of the log of child neglect reports between treated and control states from 12 quarters before to 12 quarters after Medicaid expansion. 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 1.1 for details).

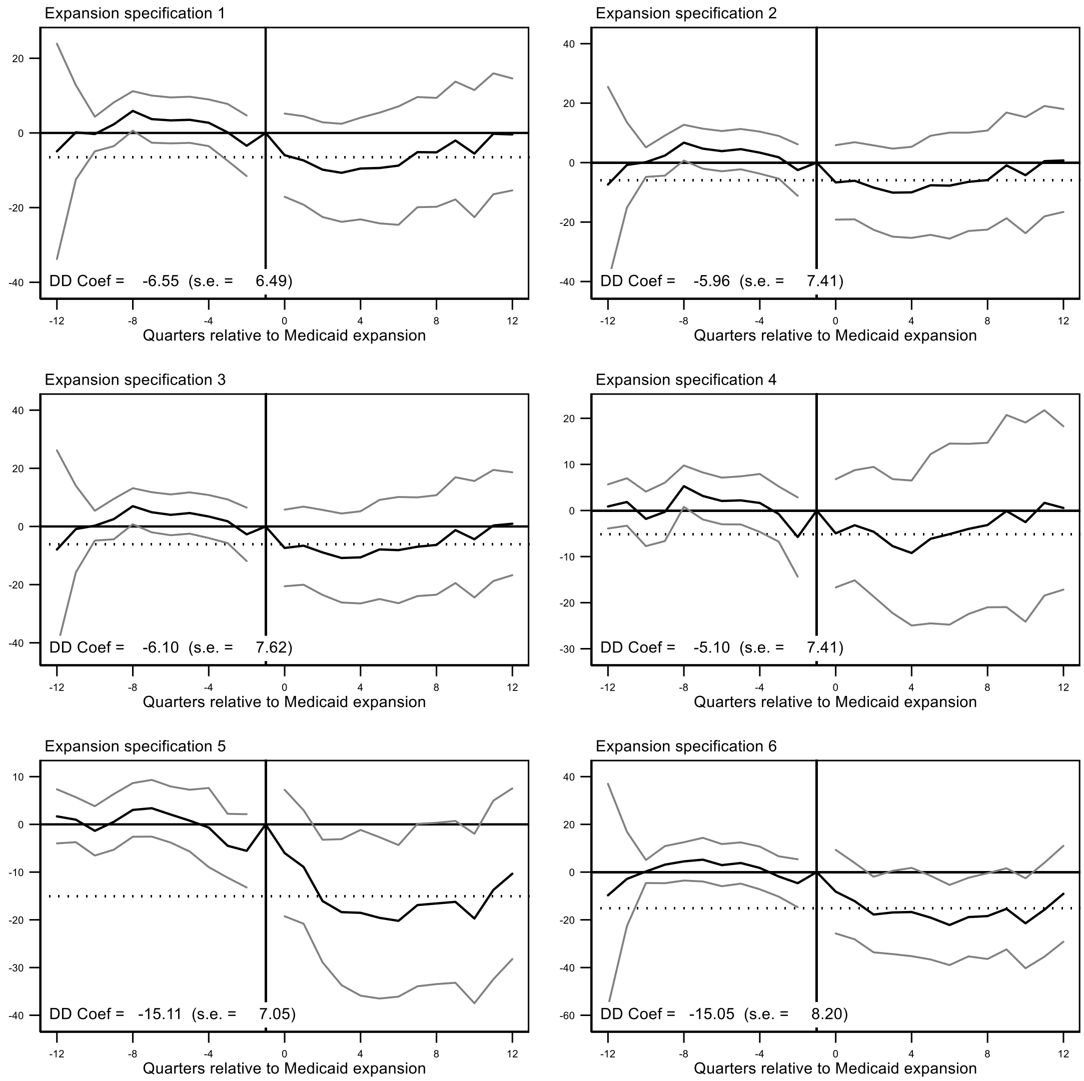

Figure 1.6 Event study, neglect report rate (log-transformed), Gardner two-stage DiD, by Medicaid expansion specification, state-level

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 1.2. 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 1.1 for details).

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