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For the Good of the Kids: Three Essays about the Economics of Child Welfare

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ABSTRACT

FOR THE GOOD OF THE KIDS: THREE ESSAYS ABOUT THE ECONOMICS OF CHILD WELFARE

By

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Millions of children in the United States come into contact with Child Protective Services each year and hundreds of thousands enter foster care. This dissertation uses economics, statistical methods, and national administrative and survey data to identify and address issues related to child maltreatment and evaluate potential solutions, such as extended foster care and mandatory reporting laws.

Chapter 1 estimates the effect of extending foster care support and services from 18 to 21 years old on the transition to adulthood for youth that have grown up in foster care. Over 20,000 youth age-out of foster care each year and lose access to housing, social, and financial support. Subsequently, these youth face various hardships, such as homelessness, incarceration, low educational attainment, and unemployment. In response, over the past decade, states have implemented extended foster care, a program that provides access to housing, social, and financial support beyond 18 years old. I exploit the staggered roll-out of extended foster care to provide some of the earliest nationwide evidence of the causal effects of this program on the transition to adulthood. I find that extended foster care effectively reduces hardships and is cost effective.

Chapter 2 evaluates how state legislation related to mandatory reporters impacts child maltreatment reporting. Child maltreatment is believed to be underreported, so mandatory

reporting legislation may be a feasible and effective way for policymakers to approach the true level of maltreatment. The list of mandatory reporters varies by state and over time. I create a state panel of mandatory reporter job classifications, child maltreatment referrals and reports, and case dispositions from 2004 to 2017. Exploiting legislation changes, I find that increasing the number of jobs classified as mandatory reporters increases reporting by 4 percent. However, this increase is driven by unsubstantiated reports.

Finally, chapter 3 documents the drastic decline in reporting during the pandemic in Colorado as a result of the COVID-19 pandemic, pandemic-induced school closures, and stay-at-home order. This chapter estimates two counterfactuals to quantify the number of maltreatment victims that may have been missed during 2020, so that state agencies can allocate resources appropriately.

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CURRAN ALEXANDRA PRETTYMAN

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

I dedicate this work to all of the children who have suffered hardships, with or without intervention from Child Protective Services or other support groups. No child should be alone when facing abuse or neglect; but often are, at no fault of their own. If you are one of the many that have suffered, I hope you can emerge from the ashes and use your experiences to make the world a better place.

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DATA DISCLAIMER

The data used in this publication, [AFCARS, NCANDS, NYTD], were obtained from the National Data Archive on Child Abuse and Neglect and have been used in accordance with its Terms of Use Agreement license. The Administration on Children, Youth and Families, the Children's Bureau, the original dataset collection personnel or funding source, NDACAN, Cornell University and their agents or employees bear no responsibility for the analyses or interpretations presented here.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

Chapter 1: Happy 18th Birthday, Now Leave: The Hardships of Aging Out of Foster Care

1.1. Introduction

Transitioning to adulthood can be daunting, especially for foster youth who lose access to housing, social, and financial support rather abruptly (Collins, 2001; Osgood et al., 2010). Over 20,000 youth age out of foster care in the United States each year and face various hardships as they transition to adulthood. By the age of 21, 23 percent will have experienced homelessness, 26 percent will have been incarcerated, and only 66 percent will have received a high school diploma or GED (AECF, 2019). Moreover, less than 8 percent will receive a college degree, and 50 percent will still be unemployed by the age of 24 (National Foster Youth Institute, 2017). On one hand, these hardships might stem from the accumulation of adverse childhood experiences, such as neglect and abuse (Gypen et al., 2017). Alternatively, these hardships might stem from losing access to resources at a developmentally young age (Rosenberg & Abbot, 2019). This paper focuses on the latter and evaluates the impact of prolonged access to resources on the transition to adulthood for foster youth.

Recognizing the challenges foster youth face while transitioning to adulthood and the subsequent costs to society, the federal Fostering Connections Act of 2008 incentivized states to extend foster care support and services beyond 18 years old. As a result, between January 2012 and December 2016, 22 states implemented extended foster care (i.e. prolonged access to housing, social, and financial support), potentially impacting over 31,500 youth each year.[1] Extended foster care is associated with increased college enrollment and employment and decreased pregnancy and homelessness at age 19; however, these benefits fade by age 21

[1] Author's calculation based on the number of 17-year-old foster youth (from AFCARS 2011 & 2014) in the 22 states that implemented extended foster care and the 20 states that had extended foster care prior to 2012.

(Courtney et al., 2007; Dworsky & Courtney, 2010a; Dworsky & Courtney, 2010b; Hook & Courtney, 2010).

In this paper, I use a difference-in-differences design that exploits the staggered roll-out of extended foster care to estimate the causal effect of this program on the transition to adulthood for foster youth across the country. In particular, I examine the effect of extended foster care on young adult outcomes, such as homelessness, incarceration, educational attainment, and employment. I also examine heterogeneity by funding source,[2] foster care placement setting, and individual childhood experiences to learn who benefits the most.

I enrich the existing evidence on the effectiveness of extended foster care by providing some of the earliest nationwide causal estimates. Prior studies compare outcomes of foster youth across a handful of states without controlling for individual or state characteristics (Courtney et al., 2007; Dworsky & Courtney, 2010a; Dworsky & Courtney, 2010b; Hook & Courtney, 2010). Alternatively, I link novel individual-level survey data to rich case-level administrative data for two cohorts of foster youth across the country. The survey data come from the National Youth in Transition Database (NYTD), which contains demographic information and outcome measures for foster youth between the ages of 17 and 21. Cohort 1 was surveyed biennially from 2011 to 2015 and cohort 2 was surveyed biennially from 2014 to 2018. The administrative data come from the Adoption and Foster Care Analysis and Reporting System (AFCARS), which contains detailed information about a youth's foster care history. I also construct a state-level panel of economic conditions, safety net generosity, and extended foster care policy changes. Combining these data, I compare outcomes of youth across cohorts within the same state under different extended foster care policies, controlling for individual, cohort, and state characteristics. To

[2] Some states finance extended foster care with federal reimbursements and others use state funding.

establish causality, I argue that the timing of these policy changes is exogenous with respect to individual outcomes after controlling for cohort and state trends.

I find evidence that access to extended foster care reduces homelessness by 18 to 30 percent, incarceration by 36 to 46 percent, and disconnectedness (neither enrolled in school nor working)[3] by 7 to 30 percent. Additionally, youth with access to extended foster care appear to be making a tradeoff between educational attainment and employment. They are 21 percent more likely to be enrolled in school, but 15 percent less likely to be working. Furthermore, extended foster care primarily helps youth that lived with a foster family prior to turning 18 (as opposed to living in a group home) and appears to mitigate the hardships of experiencing homelessness and substance abuse as a minor. However, extended foster care does not overcome the lasting consequences of juvenile incarceration. Lastly, federally-funded extended foster care has stronger effects than state-funded extended foster care. This confirms the hypothesis that the federal program is more effective than the state programs. Understanding how the current program impacts foster youth differentially based on placement setting, experiences, and funding enables better targeting of future resources.

More broadly, this study makes an important contribution to the transition to adulthood literature. While there is abundant research demonstrating that the transition to adulthood has become increasingly difficult over the past several decades (Danziger & Rouse, 2008; Settersten & Ray, 2010; Sironi & Furstenberg, 2012; Benson, 2014) and more so for vulnerable populations (Rapheal, 2008; Osgood et al., 2010), there is less focus on policy intervention and evaluation (Bloom, 2010; Lee & Morgan, 2017). I demonstrate that extended foster care provides resources and incentives that beneficially alter a youth's transition to adulthood, potentially creating long-

[3] Some people may refer to this as "idle" or "NEET" (neither in employment nor education or training), but throughout the paper I use "disconnected." "Disconnected" is commonly used in public policy.

run gains. Back-of-the-envelope calculations suggest that for every one dollar spent on extended foster care, there is at least a two-dollar return. This study provides enriched evidence on the efficacy of a federal program that impacts some of the nation's most vulnerable youth and their transition to adulthood.

1.2. Causal Effects of Foster Care

Foster youth are more likely to drop out of high school, face unemployment and lower incomes, experience homelessness, commit crime, and suffer from substance abuse compared to their non-foster youth peers (Gypen et al., 2017). Moreover, foster youth face various hardships growing up, such as abuse and neglect, mobility and school instability, and enrollment in lower performing schools (Barrat & Berliner, 2013). There is abundant research that shows a negative association between foster care placement and long-run outcomes,[4] but it is unclear how much adverse childhood experiences contribute to foster care placement and poor outcomes. This uncertainty confounds the causal effect of foster care.

Estimating the causal effects of foster care faces many statistical challenges due to the non-random assignment of youth to foster care and lack of an appropriate control group. To deal with these challenges, the economic literature on child welfare often exploits the semi-random assignment of caseworkers when administrative data are available (Doyle, 2007; Doyle, 2008; Aizer & Doyle, 2015; Bald et al., 2019; Gross & Baron, 2021). The main assumption underlying this approach is that youth in these cases experience the same hardships and the only difference is foster care placement, which is quasi-randomly determined via caseworker assignment.

Using caseworker assignment, the causal evidence on the effectiveness of foster care is mixed. Doyle (2007) finds that foster care in Illinois has adverse effects on child development, as

[4] See Gypen et al. (2017) for a summary of 32 studies from 2004 to 2015.

measured by teen pregnancy, delinquency, and adult labor market outcomes. In contrast, Gross and Baron (2021) finds improved attendance and math test scores for children removed from allegedly abusive homes in Michigan. Bald et al. (2019) find differential effects for young boys and girls in Rhode Island; young girls benefit, but there is no effect for young boys. This approach identifies the local average treatment effect in cases where children are on the margin of being admitted to state custody. A key limitation of these studies is that they are unable to address the effects of foster care for older youth who have been in care for multiple years.

This paper contributes to the strand of literature that focuses on estimating the impact of extended foster care; a program for foster youth that provides financial, social, and housing support beyond 18 years old. Existing research estimates the effect of extended foster care on the transition to adulthood by comparing outcomes of youth across states at a single point in time. One study finds that extended foster care is associated with delayed homelessness (Dworsky & Courtney, 2010a). At 19 years old, only 4.5 percent of youth with extended foster care had experienced homelessness versus 12.2 percent of youth without extended foster care. However, by 23 and 24 years old, 28.9 percent of youth with extended foster care experienced homelessness versus 29.9 percent of youth without. Another study finds that extended foster care is associated with an increase in college enrollment and completion of an additional year of school, but it is not associated with an increase in college graduation (Dworsky & Courtney, 2010b). Lastly, Hook & Courtney (2010) find that extended foster care is associated with increased employment from 19 to 21 years old, but not from 21 to 23 years old. These studies use data from the Midwest Survey, a longitudinal survey that followed youth from 17/18 years old to 26 years old in Iowa, Wisconsin, and Illinois in the early 2000s. In these studies, the researchers compare the outcomes of youth in Illinois to those in Wisconsin and Iowa because

Illinois provided extended foster care services and assistance to emancipated youth, whereas Wisconsin and Iowa did not. These cross-sectional analyses do not control for state-level characteristics, so they potentially suffer from omitted variable bias and may be misattributing beneficial outcomes to extended foster care.

A recent national-level analysis conducted by Child Trends finds that extended foster care is associated with better access to services that aid in the transition to adulthood and better adult outcomes, like employment and educational attainment (Rosenberg & Abbott, 2019). This study uses logistic regression models comparing youth in extended foster care to youth not in extended foster care. This analysis may suffer from selection bias since youth in states with extended foster care can choose whether or not to participate. Depending on the reasons youth choose to participate in extended foster care, these results may either overestimate or underestimate the true effect of extended foster care.

Finally, a recent study using California administrative and survey data from 2006 to 2015 finds that extended foster care increases college enrollment by 10 to 11 percent, extends employment by one and one half months for each additional year in extended foster care, and reduces homelessness by 28 percent for young adults (Courtney et al., 2018). The researchers address omitted variable bias by focusing their analysis on one state, rather than making cross-state comparisons. Additionally, they overcome selection bias by exploiting county-level variation in the uptake of extended foster care. They instrument participation with county of residence and argue that county of residence is a good instrument because participation in extended foster care varies across counties and is unrelated to youths' characteristics that may be associated with selection into extended care. The key concern of this study is the extent in which the results are generalizable to the rest of the country.

My analysis enriches the existing evidence of the effectiveness of extended foster care in three ways. First, I control for time-varying state characteristics, such as safety net generosity and unemployment and poverty rates and include state fixed effects to control for time-invariant state characteristics to reduce omitted variable bias. Second, I mitigate selection bias that comes from youth choosing to participate in extended foster care by estimating the intent-to-treat effect of the program. Lastly, I use a national dataset to obtain more generalizable estimates compared to prior research.

1.3. Background on Independent Living Programs and Extended Foster Care

A primary goal of foster care is to safely reunify children with their biological parents. When reunification is not possible, the next best option is adoption. Adoption subsidies targeted to families help children achieve permanency (Hansen & Hansen, 2006; Argys & Duncan, 2013), but subsidies targeted to states for older youth are less effective (Brehm, 2018). In these cases, youth remain in care until emancipation.

Over 20,000 youth age out of foster care each year and are abruptly forced to become self-sufficient overnight. Since foster youth typically lack supportive parental figures, they have to learn many skills quickly and on their own, such as how to apply to college, take out loans, set up bank accounts and manage finances, write resumes and apply to jobs, and obtain health insurance. Alternatively, the average young adult can acquire these skills over various years and receive assistance from their parents (Swartz et al., 2011). In fact, 34 percent of youth aged 18 to 34 still lived at home with their parents in 2015 (Vespa, 2017), and during this time, they received approximately 48,000 dollars[5] in financial support.

[5] This is the inflation adjusted value (2015 USD) for the original estimate of 38,000 dollars (Schoeni & Ross, 2004).

Recognizing the challenges foster youth face while transitioning to adulthood, state and federal agencies have implemented various programs to assist this process. In 1986, the federal government began allocating funds to states for Independent Living Programs (ILPs) to help foster youth live independently and transition to adulthood. ILPs and services vary across and within states and are based on need and availability of funding. Transitioning from state custody to an ILP is not automatic; youth learn about these programs through their caseworker, foster parents, probation officer, ILP coordinator, or self-discovery.

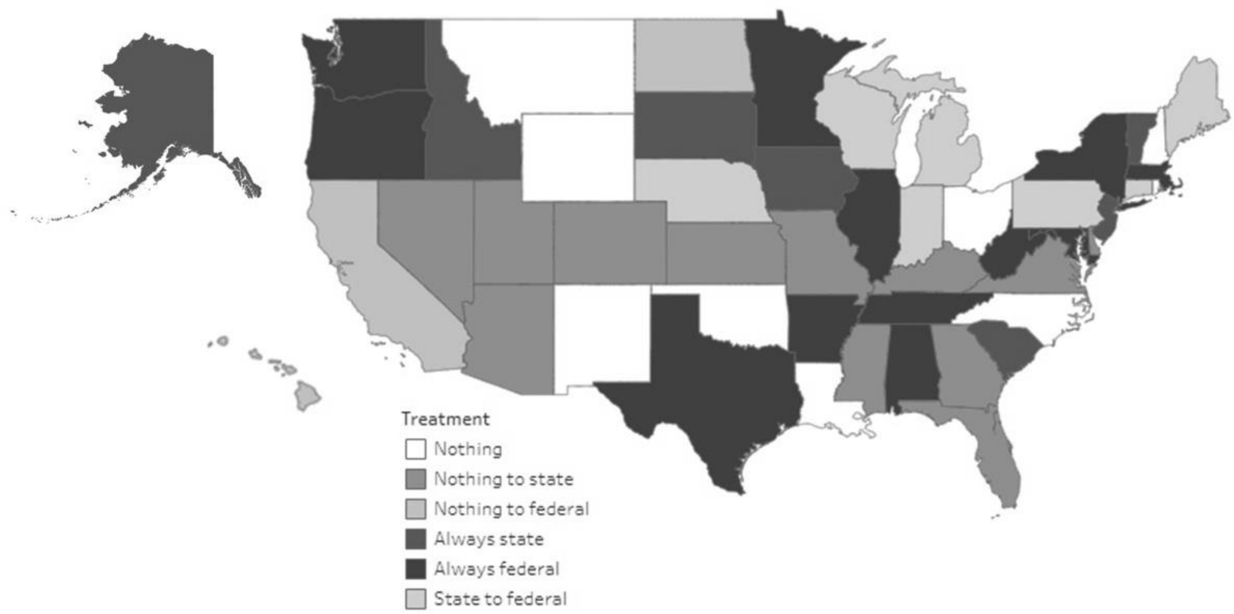
In 1999, the John H. Chafee Foster Care Independence Program (CFCIP) was created to assist current and former foster youth achieve self-sufficiency. This program provides grant-based federal funds up to 140 million dollars to states that submit plans outlining how they will assist foster youth transitioning to adulthood. This program provides education, employment, financial management, housing, and emotional and social support. CFCIP is targeted to 18 to 21 year olds after they have aged out of state custody or 16 to 18 year olds who are or have been in custody. In 2002, the CFCIP was expanded to include the Education Training Voucher Program (ETV) which allocated 5,000 dollars per year to college-going eligible youth. Originally states could request up to 60 million dollars in total each year, which would assist 12,000 youth. As of 2009, states can only request up to 45 million dollars for ETVs each year. Youth can receive college financial assistance for up to five years or until their 23rd birthday.[6] Furthermore, under the CFCIP, the federal government increased accountability measures by requiring that states track their service uptake and outcomes for youth served. As a result, some regions created foster care alumni surveys to follow up with their youth, but the national accountability system was not

[6] FC2S. Education Training Vouchers.

created until 2011. Due to a lack of data, it is difficult to measure the efficacy of these earlier programs.

More recently, the Fostering Connections Act of 2008 (FCA) incentivized states to implement extended foster care. In 2010, nine states implemented extended foster care under the FCA, in 2011, another four states were approved, and as of December 2016, 23 states operate under this federal policy. Additionally, from 2012 to 2016, 12 states enacted their own state-funded extended foster care programs. Figure 1 shows the geographic and timing variation of extended foster care in the United States from 2012 to 2016.

Figure 1: States that Extended Foster Care between 2012 and 2016



Notes: This figure shows the geographic and timing variation of extended foster care in the United States from 2012 to 2016. In this figure, there are six different shades of gray used to identify the treatment and control states. No shading identifies states that had not implemented extended foster care as of 2016 (control 1), light shading identifies states that changed their policy between 2012 and 2016 (treatment), and dark shading identifies states that adopted policies prior to 2012 (control 2). There is variation within the shading level to indicate the difference between federally-funded and state-funded extended foster care. There are 22 states that changed their extended foster care polices between the years 2012 and 2016. Three states (California, Hawaii, and North Dakota) implemented federally-funded extended foster care. Seven states (Connecticut, Indiana, Maine, Michigan, Nebraska, Pennsylvania, and Wisconsin) switched from a state to federal policy. The remaining 12 states (Arizona, Colorado, Delaware, Florida, Georgia, Kansas, Kentucky, Missouri, Mississippi, Nevada, Utah, and Virginia) implemented state-funded extended foster care. Youth in these states across different cohorts live under different policies. Appendix A discusses the data collection process, details for policy changes, a table of the effective policy dates, and a summary table of characteristics for states within each treatment.

In this figure, there are six different shades of gray used to identify the treatment and control states. No shading identifies states that had not implemented extended foster care as of 2016 (control 1), light shading identifies states that changed their policy between 2012 and 2016 (treatment), and dark shading identifies states that adopted policies prior to 2012 (control 2). Additionally, there is variation within the shading level to indicate the difference between federally-funded and state-funded extended foster care. There are 22 states that changed their extended foster care polices between the years 2012 and 2016.[7] Youth in these states across different cohorts live under different policies. I exploit this within state, cross cohort variation to estimate the effect of extended foster care on the transition to adulthood for foster youth.[8]

1.4. Hypothesized Effects of Extended Foster Care

Extended foster care is additional time as a non-minor dependent that helps foster youth between the ages 18 and 21 maintain a safety net of support while experiencing independence in a supervised environment. Youth in extended foster care may be living with foster families, in group homes, institutions, or supervised independent living settings, such as dorms, shared housing, and apartments. Regardless of their placement, youth in extended foster care meet with a caseworker monthly and receive specialized case management appropriate for their

[7] Three states (California, Hawaii, and North Dakota) implemented federally-funded extended foster care. Seven states (Connecticut, Indiana, Maine, Michigan, Nebraska, Pennsylvania, and Wisconsin) switched from a state to federal policy. The remaining 12 states (Arizona, Colorado, Delaware, Florida, Georgia, Kansas, Kentucky, Missouri, Mississippi, Nevada, Utah, and Virginia) implemented state-funded extended foster care.

[8] Appendix A discusses the data collection process, details for policy changes, a table of the effective policy dates, and a summary table of characteristics for states within each treatment.

developmental needs. In some states, foster care maintenance payments are paid directly to the youth.[9] In short, extended foster care provides youth with additional housing, social, and financial resources that should shift their budget constraint outward and ease the transition to adulthood.

To be eligible for these resources, youth must either be enrolled in school, or working at least part-time or participating in training programs to reduce employment barriers, or have a documented medical condition that prevents them from working or attending school. For youth without a documented medical condition, these eligibility requirements increase the marginal benefit of attending school or working, which then incentivizes behaviors that aid in the transition to adulthood.

Assuming optimal policy design, extended foster care should reduce hardships and smooth the transition to adulthood. As a direct effect, extended foster care should reduce homelessness. Reducing homelessness is important as it potentially has spillover effects on other outcomes of interest. For example, youth who experience homelessness between 19 and 21 years old are less likely to go to college or be employed (Kim & Rosenberg, 2017). Additionally, a former foster youth spoke about her experience transitioning to adulthood and said that she was aware of the importance of school and work, but without a safe place to live, she could not invest in these activities.[10] Stable housing may allow youth to better invest time and money in their own human capital accumulation and labor productivity.

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[9] Foster care maintenance payments cover the cost of food, clothing, shelter, daily supervision, school supplies, etc. and average 1,600 dollars per month across the country. As of February 2014, 12 states allowed for direct payment to the youth. (JCYOI, 2014, pg.23).

[10] Eprise Armstrong discussed her experiences in the panel, “Extending foster care to 21: implications to providers and impact on budgets” on May 12, 2011. The video can be found online.

Alternatively, to receive housing support, youth have to meet specific eligibility requirements. These eligibility requirements increase the marginal benefit of school and work; therefore, extended foster care indirectly impacts these outcomes. However, altering one's preferences over school and work may not be enough to induce these behaviors for those who are resource constrained. Foster youth often list "unable to pay for school" as the main reason for not going to college (Courtney et al., 2011). In addition to housing support, extended foster care provides educational aid, mentoring, career preparation, and employment skills training. Educational aid and employment skills training are correlated with connectedness (Rosenberg et al., 2020) and receiving educational aid is the strongest predictor of post-secondary education (Hunter, 2013). The net effect of extended foster care on college enrollment[11] and employment should be positive (i.e. the effect on disconnectedness should be negative). Whether extended foster care has a larger impact on college enrollment or employment is an empirical question and depends on which supports are more beneficial. For example, if extended foster care provides financial stability for youth in college, then there may be a tradeoff between college enrollment and employment.

Lastly, extended foster care should decrease the incidence of incarceration. Incarceration is a result of inadequate resources and/or a low opportunity cost of going to jail. As foster youth age out of care, they may be at an increased risk of committing crime. For example, one-in-five foster youth aging out of care rely on illegal ways of making money (Vaughn et al., 2008). Once arrested, lacking financial resources needed to make bail or afford an attorney may increase the likelihood of incarceration. Extended foster care offers financial resources and social support that can reduce criminal behavior and incarceration. Additionally, as youth acquire more human

[11] I use the term "college enrollment" to refer to any post-secondary enrollment, so this term includes enrollment in community college, 4-year universities/colleges, and technical colleges.

capital, they make better decisions and have a higher opportunity cost of going to jail, so they are less likely to commit street crimes (Lochner, 2004). Similarly, employed youth have a higher opportunity cost of going to jail, so they should also be deterred from committing crime. Regardless of the youth's decision to continue in school or work, the incidence of incarceration should decrease. Extended foster care has the potential to directly and indirectly reduce incarceration.

Extended foster care should both directly and indirectly alter a youth's transition to adulthood, but by how much is the empirical question of interest. The transition to adulthood is a function of both past experiences and current resources (Benson, 2014). Once youth turn 18, past experiences are fixed, although they can differ across youth. Alternatively, governments have the ability to influence current resources through ILPs, CFCIP, and extended foster care, so resources are a function of where the youth lives. At age 17, assume all foster youth have housing, social capital (i.e. case worker and/or foster parents), and financial assistance (via foster care maintenance payments). At age 18, there are three main scenarios. One, youth living in states without extended foster care lose access to these resources.[12] Two, youth living in states with federally-funded extended foster care have continued access to all three resources until age 21. And three, youth living in states with state-funded extended foster care may have access to all or some of these resources, but there is less accountability and scope.

Since the size of the effect of extended foster care relies on where youth live, there are potentially heterogeneous effects by funding source. Extended foster care is hypothesized to be more effective in states with federally-funded extended foster care than states with state-funded

[12] In some cases, youth can remain in their current placement setting until they graduate high school, so they might not lose access to these resources as abruptly. It is also possible that foster parents may let youth remain in care beyond 18 and maintain a relationship, but the foster care maintenance payments end at this age.

programs for two reasons. First, states with federally-funded extended foster care may have increased quality and quantity of resources compared to states with state-funded programs. Second, states with federally-funded extended foster care can plausibly support more youth (even if the youth do not meet eligibility requirements) than states with state-funded extended foster care (GAO, 2019).[13] For example, eligible youth can be funded with Title IV-E funds, which are reimbursed by the federal government, and non-eligible youth can be funded with state funds, which are not reimbursed. One limitation of this paper, is that the specific mechanism cannot be identified.

Finally, there may be heterogeneous effects by placement setting. Despite the general consensus that foster home placements provide higher quality care and better connections to supportive adults than group homes (Dozier et al., 2014; Lo et al., 2015), it is unclear whether youth who lived in foster homes prior to aging out will benefit more or less from extended foster care than youth who lived in group homes. Youth transitioning from a foster home to independence in states without extended foster care might lose access to supportive adults and quality care relative to youth transitioning from a group home to independence in these states. Alternatively, a foster family might maintain a relationship and continue caring for the youth aging out, in which case these youth would lose less than their peers transitioning from a group home. Overall, foster youth living in states with extended foster care should fare better than foster youth in other states without extended foster care and there may be heterogeneous effects by policy and placement setting.

[13] See footnote 40 from this GAO report for an example.

1.5. Data

Data for this analysis come from three main sources; the National Youth in Transition Database (NYTD), the Adoption and Foster Care Analysis and Reporting System (AFCARS), and the University of Kentucky Center for Poverty Research (UKCPR) Poverty and Inequality National Welfare Dataset. NYTD is a national survey that collects demographic information and outcome measures for the universe of foster youth aging out of care, AFCARS is a national dataset that contains rich descriptive information about children in foster care, and the UKCPR Welfare Dataset contains state-level information about the economy and safety net programs in a given year. I link individuals from the two most recent NYTD cohorts to their AFCARS data and control for time-varying state characteristics with the welfare dataset. The first cohort was 17 in fiscal year (FY) 2011 and the second cohort was 17 in FY 2014.

NYTD is the first national survey to collect outcome measures for foster youth aging out of care.[14] States identify and survey all youth in foster care at age 17 and then follow up with these same youth at ages 19 and 21, regardless of their foster care status. Youth answer questions about their educational attainment, employment status, and incidence of homelessness and incarceration, among other outcomes.[15] NYTD also collects i) demographic information, such as date-of-birth, race, gender, and state, ii) report details, such as date-of-report and survey

[14] National accountability of foster youth outcomes began in 2011 as a result of the 2008 accountability mandate proposed by the Administration for Children and Families. States are required to collect and report reliable responses every 6 months and are fined for noncompliance. States must report outcomes for at least 80% of youth in foster care and 60% discharged from care. These numbers were based on research on response rates and reviewing the Office of Management and Budget's guidance on surveys. States are fined up to 5% of their CFCIP funds if they do not comply and meet reporting requirements. For more specific details about NYTD data collection and reporting requirements, visit https://www.childwelfare.gov/cb/research-data-technology/reporting-systems/nytd/faq/.

[15] The college enrollment outcome is derived from the current enrollment and educational attainment questions. Youth that have graduated from high school and are enrolled in school are assumed to be enrolled in college. I use “college enrollment” loosely to include any post-educational program beyond high school such as 2-year, 4-year, and trade school enrollment.

participation (or reason for not participating),[16] and iii) service use, such as foster care status, academic support, career preparation, budgeting, mentoring, health education, and financial assistance. In 2011 and 2014 nationwide, there were approximately 38,000 and 31,000 youth in foster care at age 17, respectively.[17] Just under 32,000 of these youth were eligible[18] to participate in the NYTD surveys. For the remainder of this section, I discuss the analysis sample, and later I discuss the differences between respondents across the different surveys and address potential non-response bias.

I restrict my analysis sample to youth who participated in the survey, had foster care history information from AFCARS, and do not have any missing outcome measures, resulting in 11,120 observations (or one-third of the eligible NYTD participants).[19] Table 1 provides summary statistics for the sample of NYTD participants.[20] Cohort 1 makes up 47 percent and cohort 2 makes up the remaining 53 percent of the analytical sample, 46 percent of the sample is young men, 54 percent is young women, and 42 percent of the sample is Non-Hispanic white, 30 percent is Non-Hispanic black, and 20 percent is Hispanic. Representative of the foster care population, black youth are disproportionately represented compared to the general population (30% versus 14%). More than half of the sample (58%) have been diagnosed with a disability at some point in their life. Of these youth that have been diagnosed with a disability, 80 percent

[16] Reasons for not participating include declined, incarceration, incapacitation, death, not in sample, and missing or unable to locate.
[17] Author's estimate based on the number of 17-year-old foster youth in care at the start of the fiscal year (from AFCARS 2011 & AFCARS 2014 data).
[18] Survey eligibility is based on age, foster care status, and survey completion. Eligible youth must turn 17 during the fiscal year, be in foster care on the day of the survey, complete the survey within 45 days of their 17th birthday, and answer at least one survey question.
[19] About half (n=16,320) of the eligible youth were missing demographic information and foster care history from AFCARS. Another 1,983 youth declined to participate in the survey and another 2,630 youth were missing at least one of the outcome measures.
[20] Refer to Appendix Table C1 for summary statistics with the full list of controls and Appendix Table C2 for summary statistics by treatment.

were diagnosed with an emotional disorder such as ADHD, ADD, anxiety, an eating disorder, or a mood or personality disorder.

Table 1: Summary Statistics for NYTD Participants

	Variable	For 19 Year Olds (N=11,120) Mean	Std. Dev.	For 21 Year Olds (N=8,416) Mean	Std. Dev.
Extended Foster Care Policy	Federal EFC at 18	0.51	0.50	0.51	0.50
	State EFC at 18	0.24	0.43	0.23	0.42
NYTD Cohort	Cohort 1 (17 in FY2011)	0.47		0.46	
	Cohort 2 (17 in FY2014)	0.53	0.50	0.54	0.50
Demographic Characteristics	Female	0.54	0.50	0.57	0.50
	Non-Hispanic White	0.42	0.49	0.41	0.49
	Non-Hispanic Black	0.30	0.46	0.30	0.46
	Non-Hispanic Other	0.08	0.27	0.08	0.28
	Hispanic	0.20	0.40	0.21	0.40
	Ever diagnosed with a disability	0.58	0.49	0.58	0.49
Experiences at 17	Ever been homeless	0.17	0.38	0.17	0.38
	Employed at 17	0.15	0.36	0.15	0.36
	Ever been incarcerated	0.27	0.44	0.26	0.44
	Ever been referred for substance abuse	0.23	0.42	0.22	0.41
Foster Care History	Total removals from home as a child	1.39	0.66	1.39	0.67
	Total placements as a child	7.16	7.15	7.13	6.98
	Cumulative length of stay in foster care as a child (in years)	4.43	3.65	4.44	3.64
	Age at first removal	11.72	4.76	11.72	4.72
	Age at last removal	17.28	1.98	17.27	1.99
First Placement	Kinship Care	0.16	0.37	0.16	0.37
	Foster home	0.49	0.50	0.50	0.50
	Group home	0.29	0.45	0.28	0.45
	Other	0.06	0.23	0.06	0.24
Ever removed for… These do not add up to 100% because a child may be removed for multiple reasons.	Abuse	0.27	0.45	0.27	0.45
	Neglect	0.56	0.50	0.56	0.50
	Parental Incarceration	0.06	0.24	0.06	0.24
	Parental Substance Abuse	0.19	0.39	0.19	0.39
	Inadequate Housing	0.10	0.30	0.10	0.30
	Child-related issue	0.32	0.47	0.31	0.46
Outcomes	Homelessness	0.20	0.40	0.37	0.48
	Enrolled in high school	0.29	0.45	0.06	0.24

Variable	For 19 Year Olds (N=11,120)		For 21 Year Olds (N=8,416)	
	Mean	Std. Dev.	Mean	Std. Dev.
Finished high school/GED	0.56	0.50	0.81	0.40
Enrolled in college/post-secondary education	0.28	0.45	0.27	0.44
Employed	0.38	0.49	0.56	0.50
Incarceration	0.19	0.39	0.28	0.45
Foster Care	0.40	0.49	0.21	0.41

Notes: The sample is restricted to foster youth who completed the NYTD survey at 19 and/or 21 years old and are not missing demographic information, foster care history, nor outcomes. Less than one percent of the observations are missing the indicator for high school graduation at age 19. The summary statistics do not vary much when restricting the sample to the youth that are not missing this variable and so I report the results of the larger sample. The similarity in demographic characteristics and foster care history across ages 19 and 21 indicates similar youth responded to the survey in both years.

On average, this sample of foster youth entered care at 12 years old and have been in care for a cumulative total of about 4.5 years. The most common removal reasons are neglect (56%), child-related issues (32%), and abuse (27%). Most youth were first placed in a foster home (49%), group home (29%), or kinship care (16%). The last placement settings as a minor included foster homes (44%), group homes (29%), kinship care (12%), and other placements (16%), such as supervised independent living, trial home visit, and runaway.[21]

By 17 years old, 17 percent had experienced homelessness, 27 percent had been incarcerated, 23 percent had been referred for substance abuse, and 15 percent were employed. In contrast, the average adolescent has a 3 percent chance of experiencing homelessness (Bassuk et al., 2014) and a 0.15 percent chance of incarceration.[22] By 19 years old, 56 percent of NYTD respondents had graduated from high school or received their GED, 28 percent enrolled in college or some other post-secondary education program, 38 percent were employed, 20 percent had been homeless in the past two years, and 19 percent had been incarcerated in the past two

[21] A "trial home visit" is when a youth returns home under state agency supervision before reunification is complete. "Runaway" is designated for youth who have run away from the foster care setting.
[22] Estimate comes from the Kids Count Data Center provided by the Annie E. Casey Foundation.

years. Finally, only 40 percent were in foster care at age 19, despite 75 percent having access to extended foster care.

Fewer youth responded to NYTD at age 21, resulting in 8,416 observations. The respondents at 21 are similar to those at 19 based on demographic characteristics and foster care history. At 21 years old, 81 percent had graduated from high school, 27 percent were enrolled in college, and 56 percent were employed. Thirty-seven percent have experienced homelessness and 28 percent have been incarcerated by age 21. Only 20 percent were in foster care at age 21. This is unsurprising as many states with extended foster care end care at age 21.

1.6. Empirical Strategy

Participation in extended foster care is a function of youth eligibility and selection. Measuring participation is a function of data availability. Per the NYTD codebook, youth are reported as being in foster care if they are under the responsibility of a qualified agency in accordance with the federal definition of foster care.[23] In practice, foster care status should only be reported "yes" for eligible, participating youth in states with federally-funded extended foster care programs.[24] In the majority of states with federally-funded extended foster care, less than 50 percent of the youth in care are eligible for federal reimbursement (GAO, 2019). In other words, foster care status in NYTD should have been reported "no" for the majority of participants. This practice limits the ability to observe participation for ineligible youth and across all states. Moreover, states with extended foster care cannot mandate participation, so youth can leave at

[23] See 45 CFR 1355.20 for the federal definition of foster care.
[24] According to personal correspondence with the Administration for Children and Families. Some states misunderstood this question giving insight into their state policy. For example, Georgia and Kentucky reported that 20 to 30 percent of youth from the FY2011 and FY2014 NYTD cohorts were in foster care beyond 18 years old, despite not having a federally-funded extended foster care program during this period.

any time for any reason introducing selection bias. For these reasons, I focus on estimating the intent-to-treat effect of extended foster care and leave the treatment-on-treated effect for future research.

To determine the effect of extended foster care on the transition to adulthood, I use a difference-in-differences approach and estimate a two-way fixed effects linear probability model with the following equation:

$$Prob(y_{iasc} = 1) = \beta_0 + \beta_1 FedEFC_{iasc} + \beta_2 StEFC_{iasc} + \boldsymbol{X_{iasc}\beta} + \boldsymbol{S_{sc}\beta} + \gamma_s + \gamma_c \qquad (1.1)$$

Where y is the outcome for individual *i* of age *a* in state *s* and cohort *c*. *FedEFC* is a binary indicator equaling one if federally-funded extended foster care was available in state *s* when individual *i* of age 19 in cohort *c* turned 18 years old and zero otherwise. *StEFC* is a binary indicator equaling one if state-funded extended foster care was available in state *s* when individual *i* of age 19 in cohort *c* turned 18 years old and zero otherwise. These extended foster care indicators are mutually exclusive, and they are derived using the effective date of the policy and the youth's birthday. **X** is a vector of youth demographic characteristics and other individual-level controls, such as race, gender, experiences prior to 17 years old, reason for entry into foster care, length of stay, number of placements in foster care, and first placement setting, that are plausibly correlated with a foster youth's transition to adulthood. **S** is a vector of observable state-level time-varying controls such as the unemployment rate, poverty rate, and measures of safety net program generosity. I calculate the 3-year average for each of these controls to most effectively summarize the economic conditions for cohort *c* in state *s* as they may be correlated with implementation of extended foster care and a youth's transition to adulthood. State fixed effects are included to control for unobservable state time-invariant characteristics that may be correlated with youth outcomes, such as ILPs and the CFCIP. Finally, the cohort fixed effect can also be thought of as a year fixed effect since I am using cross-sectional data for two distinct cohorts.

The coefficients of interest, β_1 and β_2, estimate the intent-to-treat effect of having extended foster care at 18 years old for youth within a state, controlling for state and cohort/year effects and individual characteristics. β_1 estimates the impact of the federal policy and is identified off of three states. β_2 estimates the impact of state policies and is identified off of 11 states. The difference between β_1 and β_2 estimates the impact of changing from a state to federal policy, which happens in seven states.

The validity of this difference-in-differences approach relies on the assumptions that the timing of the policy changes is exogenous to unobservable time-varying cohort characteristics and that the policy is uncorrelated with survey participation.[25] Extended foster care legislation appears to take anywhere from two months to two years to pass, so the effective date of implementation in which my model is identified is arguably random, relative to cohort characteristics. Even if there are non-random differences in the timing of implementation, including the set of individual, state, and cohort controls should alleviate this concern.[26] I test this assumption by excluding different combinations of controls and find that the estimated effects can be attributed to the policy and are not confounded by other factors. These results are discussed in more detail later as well. Second, extended foster care appears to be correlated with

[25] Recent discussion also emphasizes the difficulty in interpreting the difference-in-differences treatment effect for multiple groups with multiple time periods when the timing of the policy varies (de Chaisemartin & D'Haultfoeuille, 2019; Goodman-Bacon, 2019; Callaway & Sant'Anna, 2020). Although timing of the policy varies, since there are only two time periods in this analysis, this is less of a concern.

[26] Furthermore, in Appendix B, I demonstrate that it is difficult to predict which states implement extended foster care, at least based on economic factors and the foster care environment.

survey participation. However, after addressing non-response, this correlation is not driving the results.[27]

To quantify the policy effect, I estimate equation 1.1 for outcomes at age 21. However, instead of *FedEFC* and *StEFC* being binary indicators equaling one or zero, I allow them to take discrete values between zero and four to count the number of years federally-funded and state-funded extended foster care has been available in state *s* for individual *i* in cohort *c*. This new variable takes into account both the youth's age when the policy was implemented, as well as the youth's age when they lose access to extended foster care services. For outcomes measured at age 21, exposure to extended foster care is more flexible and informative than the binary indicator.

To understand who benefits the most from the extended foster care program, I interact extended foster care policies with placement settings and experiences prior to 17 years old (separately). I estimate the following equations:

$$Prob(y_{iasc} = 1) = \delta_0 + \delta_1 FedEFC_{iasc} + \sum_p \delta_{1p}(FedEFC\ x\ p)_{iasc} + \delta_2 StEFC_{iasc} + \sum_p \delta_{2p}(StEFC\ x\ p)_{iasc} + \boldsymbol{X_{iasc}\delta} + \boldsymbol{S_{sc}\delta} + \gamma_s + \gamma_c \qquad (1.2)$$

$$Prob(y_{iasc} = 1) = \delta_0 + \delta_1 FedEFC_{iasc} + \sum_e \delta_{1e}(FedEFC\ x\ e)_{iasc} + \delta_2 StEFC_{iasc} + \sum_e \delta_{2e}(StEFC\ x\ e)_{iasc} + \boldsymbol{X_{iasc}\delta} + \boldsymbol{S_{sc}\delta} + \gamma_s + \gamma_c \qquad (1.3)$$

Where most of the variables are the same as above, and the summation terms are shorthand for the interaction effects.[28] In equation 1.2, p indexes the last placement settings as a child. The three placement settings considered are foster homes, kinship care, and group homes.

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[27] See section 1.9 for a detailed discussion of non-response and techniques employed to address non-response bias.

[28] For example, $\sum_p \delta_{1p}(FedEFC\ x\ p)_{iasc} = \delta_{1fh}(FedEFC\ x\ fh)_{iasc} + \delta_{1kc}(FedEFC\ x\ kc)_{iasc} + \delta_{1gh}(FedEFC\ x\ gh)_{iasc}$ where fh indicates foster home, kc indicates kinship care, and gh indicates group home.

δ_{1p} estimates the effect of federally-funded extended foster care for youth in placement setting p, and δ_{2p} estimates the effect of state-funded extended foster care for youth in placement setting p. In this specification, the **X** vector also controls for placement setting p independently of the interaction term because the quality of care received as a child is plausibly correlated with participation in extended foster care and outcomes as a young adult.

In equation 1.3, e indexes experiences prior to 17 years old. The three experiences considered are incarceration, homelessness, and substance abuse referral. δ_{1e} and δ_{2e} estimate the effect of federally-funded and state-funded extended foster care, respectively, for youth with experiences e. Like in equation 1.1, the **X** vector also controls for experiences prior to 17 years old.

1.7. Results

I estimate equation 1.1 separately for outcomes at ages 19 and 21 to determine the impact of extended foster care on the transition to adulthood for foster youth across the country. Then, I estimate equations 1.2 and 1.3 for outcomes at age 19 to understand who primarily benefits from this program in the short-run. In all analyses, standard errors are clustered at the state level.[29]

1.7.1. Extended Foster Care Smooths the Transition to Adulthood

Table 2 reports results from the intent-to-treat analysis and shows that extended foster care reduces hardships, like homelessness, incarceration, and disconnectedness, and increases educational attainment and employment in the short-run. The effects are often larger and more precisely estimated for the federal policy relative to the state policies, confirming the notion that

[29] Cameron & Miller (2015) and Conley and Taber (2011) caution that models may be inconsistent when there are few treated groups. To alleviate this concern, I also calculated standard errors using clustered bootstrap estimation. Results do not change and are available upon request.

the federal policy is more effective.[30] For this reason, I focus most of the discussion on the federal policy here on out. Finally, impacts persist for all outcomes at age 21, except employment. Overall, this implies that youth continue to benefit through the three years the policy ought to be impacting them.

On average, the probability of experiencing homelessness between the ages 17 and 19 decreases by 22 percent for youth living in states that switched from no policy to federally-funded extended foster care compared to youth living in states without extended foster care. All else equal, an additional year exposed to federally-funded extended foster care decreases the probability of ever experiencing homelessness as an adult by almost 6 percent. Similarly, the likelihood of being incarcerated between the ages 17 and 19 is reduced by 26 percent for youth living in states that switched from no policy to federally-funded extended foster care compared to youth living in states without extended foster care. An additional year exposed to federally-funded extended foster care reduces incarceration by 12 percent, all else equal. Youth living in states that switched from no policy to federally-funded extended foster care were 16 percent less likely to be disconnected at age 19 than youth living in states with no policy. For each additional year with federally-funded extended foster care, disconnectedness is reduced by almost 10 percent.

In most states, federally-funded extended foster care prolongs access to social, housing, and financial support for three years, from age 18 to 21, so it is more policy-relevant to discuss the impact of full exposure, as opposed to marginal effects. There are two ways to estimate the full impact of the policy: assume linear effects and scale the marginal effect by three or directly

[30] The federal policy also appears to yield more homogenous effects. I estimate two additional models, one that omits the state policy, and another that combines the federal and state policy, to demonstrate this point. In these models, for outcomes at age 19, the effect of the federal policy is often less precisely estimated. Results are in Appendix Table C3.

estimate the 3-year effect.[31] These approaches imply that federally-funded extended foster care reduces homelessness by 18 to 30 percent, incarceration by 36 to 46 percent, and disconnectedness by 7 to 30 percent.

Consistent with the reduction in disconnectedness, extended foster care appears to help youth complete high school and enroll in college, at the expense of employment. Approximately three in ten foster youth are enrolled in high school at 19 years old, and they are 19 percent more likely to be enrolled in high school in states with federally-funded extended foster care. Each additional year with federally-funded extended foster care increases the probability of graduating high school by age 21 by 2 percent. The policy effect on high school graduation ranges from an increase of 4 to 6 percent. There is no statistically significant effect of having extended foster care available at age 18 on college enrollment for 19-year-olds, but the sign suggests increased enrollment, conditional on high school graduation or a GED. With each additional year exposed to federally-funded extended foster care, results in Table 2 suggest the probability of college enrollment increases by 21 percent. However, results in Appendix Table C4 suggest the marginal effect is driven by the first year and so the full policy effect is less clear. Finally, for 19-year-olds, employment is 14 to 23 percent higher in states that switched from no policy to extended foster care compared to states that did not switch. Alternatively, at age 21, the effects on employment decrease by 5 percent with each additional year exposed to federally-funded extended foster care, or by 15 percent with full exposure to the policy.

[31] Results from these exercises are provided in Appendix Table C4.

Table 2: Main Regression Results for Youth that Completed the NYTD Survey at 19 and/or 21

	Outcomes at 19 Years Old (N=11,120)					
	Ever been Homeless in Past Two Years	Ever been Incarcerated in Past Two Years	Disconnected	High School Enrollment	College Enrollment	Employment
Federal EFC at 18	-0.048*	-0.053*	-0.043**	0.052*	0.010	0.083***
	(0.025)	(0.029)	(0.020)	(0.029)	(0.038)	(0.028)
State EFC at 18	-0.015	-0.021	0.017	-0.005	-0.041	0.051***
	(0.021)	(0.016)	(0.015)	(0.022)	(0.025)	(0.018)
Mean of Control Group (No EFC at 18)	0.218	0.201	0.264	0.274	0.493	0.366
Adjusted R-Squared	0.088	0.197	0.049	0.041	0.083	0.045
	Outcomes at 21 Years Old (N=8,416)					
	Ever been Homeless in Adult Life	Ever been Incarcerated in Adult Life	Disconnected	High School Graduation	College Enrollment	Employment
Years exposed to Federal EFC	-0.026*	-0.035***	-0.031**	0.014**	0.045**	-0.028***
	(0.014)	(0.007)	(0.013)	(0.006)	(0.019)	(0.010)
Years exposed to State EFC	-0.030***	-0.019**	-0.019	-0.012	0.014	0.009
	(0.010)	(0.008)	(0.012)	(0.009)	(0.013)	(0.012)
Mean of Control Group (No Policy Ever)	0.444	0.295	0.321	0.798	0.214	0.561
Adjusted R-Squared	0.139	0.234	0.062	0.069	0.143	0.066

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects. EFC stands for extended foster care. The college enrollment outcome at 19 years old is conditioned on high school graduation/GED and consists of 6,155 observations.

I further investigate whether the availability of extended foster care influences decision-making by estimating the effect of having extended foster care at age 17. In all states, youth can remain in care until 18 years old regardless of a state's extended foster care policy. Therefore, there should be no difference between having extended foster care at age 17 or 18, unless youth use this information to plan for the future. Results from this exercise are presented in Appendix Table C5. I find evidence that youth may rely on extended foster care policies to experiment living on their own. For example, youth are equally likely to experience homelessness and/or incarceration between the ages of 17 and 19, regardless of extended foster care availability at age 17. This suggests that youth in states with extended foster care may try to live on their own and experience these hardships before deciding to return to care, whereas youth in states without extended foster care may experience these hardships as a result of aging out at 18.

1.7.2. Who Benefits the Most from Extended Foster Care?

Federally-funded extended foster care primarily benefits youth that were living in foster homes prior to turning 18 years old and potentially mitigates some hardships experienced as a foster child. The last placement setting prior to turning 18 for many youth is a foster home (44%), kinship care (12%), or a group home (29%). About one in five NYTD participants experienced homelessness and substance abuse during their childhood. Tables 3 and 4 report results from the specifications that interact extended foster care with placement settings (equation 1.2) and adverse childhood experiences (equation 1.3).

Youth that lived in foster homes prior to aging out in states with extended foster care are less likely to experience homelessness between the ages of 17 and 19 and more likely to be employed at 19 years old. Extended foster care also increases high school enrollment among youth in group homes and kindship care.

Table 3: Interaction between Extended Foster Care Policy and Last Placement Setting as a Child

	Outcomes at 19 Years Old					
	Homelessness	Incarceration	Disconnected	High School Enrollment	College Enrollment	Employment
Fed EFC at 18 x Last placement as a child: foster home (N=2,425)	-0.059** (0.026)	-0.040 (0.025)	-0.033 (0.031)	0.002 (0.031)	0.012 (0.047)	0.049* (0.027)
Fed EFC at 18 x Last placement as a child: group home (N=1,567)	-0.045 (0.028)	-0.038 (0.027)	-0.007 (0.031)	0.062* (0.032)	0.020 (0.040)	0.008 (0.025)
Fed EFC at 18 x Last placement as a child: kinship care (N=802)	-0.019 (0.029)	-0.022 (0.023)	-0.070* (0.036)	0.056* (0.029)	-0.003 (0.064)	0.116** (0.045)
Observations	11,064	11,064	11,064	11,041	6,125	11,064
Adjusted R-squared	0.091	0.207	0.052	0.043	0.084	0.047

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. The number of observations in the interaction term is noted. All regressions control for demographic characteristics, foster care history (including last placement setting), experiences at 17 years old, state controls, and include cohort and state fixed effects. The abbreviation EFC is shorthand for extended foster care. The coefficients on the interaction between the placement setting and state EFC are statistically insignificant for all outcomes and so they are not reported in this table.

Table 4: Interaction between Extended Foster Care Policy and Experiences at 17 Years Old

	Outcomes at 19 Years Old					
	Homelessness	Incarceration	Disconnected	High School Enrollment	College Enrollment	Employment
Fed EFC at 18 x has been incarcerated (N=1,326)	0.047**	0.029	0.086***	-0.023	-0.077*	-0.033
	(0.020)	(0.027)	(0.020)	(0.025)	(0.046)	(0.023)
Fed EFC at 18 x has been homeless (N=894)	-0.044	-0.015	-0.062**	0.021	0.008	0.013
	(0.030)	(0.031)	(0.026)	(0.024)	(0.051)	(0.029)
Fed EFC at 18 x has been referred for substance abuse (N=1,222)	0.010	0.018	-0.055*	0.017	0.073**	0.053**
	(0.028)	(0.022)	(0.032)	(0.025)	(0.035)	(0.026)
Observations	11,120	11,120	11,120	11,097	6,155	11,120
Adjusted R-squared	0.089	0.197	0.051	0.040	0.083	0.046

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. The number of observations in the interaction term is noted. All regressions control for state-funded extended foster care, demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects. The abbreviation EFC is shorthand for extended foster care.

On average, youth living in states with federally-funded extended foster care that experienced homelessness as a child are less likely to be disconnected at age 19 compared to similar youth living in states without extended foster care. Similarly, youth living in states with federally-funded extended foster care referred for substance abuse are also less likely to be disconnected at 19 years old. Furthermore, these youth are more likely to be enrolled in college or employed at 19. Interestingly, extended foster care does not mitigate the hardships of experiencing juvenile incarceration, but instead exaggerates this hardship. One explanation might be that these "trouble-makers" are stigmatized and now have more eyes watching them.

1.8. Additional Analyses

This section discusses alternative specifications, sensitivity analyses, and their implications. In its entirety, this section demonstrates that the results presented in the previous section are robust to changes in models, controls, and samples, with the exception of omitting state fixed effects. Foster care environments vary considerably across states, so it is important to control for unobservable time-invariant differences. In addition, the need to address non-response becomes apparent through these sensitivity analyses.

1.8.1. Alternative Specifications

I consider alternative approaches and models to show that my equation is correctly identified and specified. These results are provided in Appendix Table C6.

First, I employ a triple differences approach which exploits individuals' birthdays from the same cohort and state as the source of variation. The validity of this approach relies on the assumption that states did not choose effective policy dates based on an individual's birthday. Overall, the triple differences estimates are slightly smaller or similar in magnitude relative to the estimates from the main specification, and less precise. One reason for slightly smaller estimates might be that youth within the same cohort and state, that differ in age by a few months, may have similar experiences transitioning to adulthood, attenuating the effects to zero. They may have already made plans to age out before the policy went into effect or the policy may take time to be effective. Alternatively, loss of precision may come from lack of statistical power. This approach has cleaner identification but less statistical power, and so the estimates suffer from imprecision.

I also consider alternative specifications by estimating equation 1.1 using probit and logit models. These models assume different functional forms for the explanatory variables and error term, but usually yield similar results to a linear probability model (Angrist & Pischke, 2009; Hellevik, 2009; Wooldridge, 2010). As expected, the results from these models are comparable to the main specification.

1.8.2. Sensitivity Analyses

This section discusses the findings from various sensitivity analyses. I alter the set of control variables, states, and observations to test the robustness of the results. Overall, results are robust, but the need to address non-response becomes apparent.

1.8.2.1. Changing the set of control variables

I consider alternative analyses by estimating equation 1.1 excluding foster care history, experiences at age 17, and state-level controls. Results are reported in Appendix Table C7. Foster care history is correlated with outcomes as an adult regardless of the policy. Excluding these controls (column 3) and obtaining similar results demonstrates that the estimated effects are in response to the policy and not confounded by one’s experiences in foster care. I exclude experiences at age 17 (column 4) and obtain similar results, which verifies that states did not implement extended foster care based on observable cohort-specific experiences.[32] If policy implementation was correlated with cohort-specific experiences, then removing these controls would have resulted in larger estimates.

Finally, excluding state controls for safety net generosity and economic conditions (column 5) yields slightly smaller estimates at age 19 and slightly larger estimates at age 21, although neither set of results are statistically different from the main results. This observation implies that state controls have more explanatory power over time and are more important to control for to properly isolate the effect of the policy. I also estimate equation 1.1 excluding state fixed effects (column 6). The validity of cross state comparisons relies on the assumptions that the timing of the policy is random across states and that states have similar foster care environments. This exercise yields statistically insignificant results, suggesting that states have considerably different foster care climates and other unobservable time-invariant characteristics that need to be accounted for when trying to identify the impact of the extended foster care policy.

1.8.2.2. Changing the set of states

Next, I change the set of states in the analysis to determine if any are driving the results. Results are reported in Appendix Table C8. First, I restrict the sample to the 22 states that changed their extended foster care policy between 2012 and 2016 (column 2). This analysis excludes the always-taker and never-taker states. Overall, the estimated effects in this sample are similar in magnitude, but less precise compared to the main analysis that includes all 51 states (column 1). This is expected and reassuring since the identification in both models comes from

[32] Additionally, excluding cohort fixed effects (column 7), which controls for unobservable cohort-specific trends, yields statistically similar results.

within state changes. Including the non-switcher states adds to the overall model fit and precision, but does not affect the point estimates on extended foster care.

Second, I exclude the seven states that went from state to federal extended foster care policies (column 3). Even though the main results are not statistically different from the estimates in this exercise, there are a few notable differences worth highlighting. First, these seven states appear to be dampening the effect of federal extended foster care on homelessness, high school enrollment, and college enrollment at age 19. Alternatively, they seem to be driving the effect on employment and incarceration at age 19. To explain this phenomena, recall that the main specification for outcomes at age 19 only estimates the effect of a specific extended foster care policy at 18. In the seven states that switch from state to federal policies, some youth are living under two different policies. For example, youth in cohort one in Connecticut had state extended foster care at 18, and then federal extended foster care starting at 19. In Michigan and Maine, federal extended foster care was implemented less than a year after state extended foster care.

As a third check, I omit the 19 states with state-funded extended foster care to obtain a cleaner effect of federally-funded extended foster care. In this exercise, I have two control groups and one treatment group. One control group is the set of states with no policy. The other control group is the set of states that adopted federally-funded extended foster care prior to 2012 (always-takers). The treatment group consists of the states that adopted federally-funded extended foster care between 2012 and 2016. The results from this exercise show the effect of implementing federally-funded extended foster care without being complicated by the state policy. Overall, results are larger for most outcomes, suggesting that the main results are relatively modest. Two outcomes worth noting are incarceration and employment at age 19. In this exercise, they are smaller suggesting that the effect of the federal policy may be overstated for these two outcomes in the short-run. However, by age 21 the effect size from this exercise is similar to the main results, so whatever differences exist at age 19 do not persist to age 21.

Finally, I repeatedly estimate equation 1.1 omitting one state at a time. Appendix Figures C1 and C2 plot the effect size and the 95 percent confidence interval for the coefficient on federally-funded extended foster care for each regression omitting a state. Each graph displays a different outcome. These results suggest that California drives some, but not all, of the precision of the results. About 22 percent of the NYTD respondents live in California. The next largest states represented are Michigan (4.6%), Texas (4.5%), and Florida (4%).

1.8.2.3. Changing the sample size

As a final robustness check, I consider different analysis samples by letting the sample size vary by outcome measure and restricting the sample to youth that participated in the survey at both 19 and 21 years old. These variations provide more insight into non-response and results are presented in Appendix Table C9.

The first exercise, letting the sample vary by outcome measure (column 2), shows that the impact of extended foster care is similar whether it comes from youth who just answer a specific question or all questions. This exercise alleviates any concern that the construction of my sample may have introduced additional biases.

The second exercise, comparing the estimates in the unrestricted sample to the restricted sample (columns 1 and 3) indicates that some of the impact of extended foster care is plausibly coming from changes in respondents between survey years. At age 19, the estimated effects are slightly larger in the restricted sample compared to the unrestricted sample, but at age 21 the opposite is true. Appendix Table C10 shows that survey drop-outs and returners, meaning they participated in two out of the three surveys, appear to be similar along most characteristics, aside from childhood experiences such as homelessness and incarceration. Survey returners are more likely to have experienced these hardships. In order to observe these patterns, it must be that survey returners benefit more from extended foster care than survey drop-outs.

1.9. Addressing Non-Response

Non-response is a major concern with these data, as indicated from the number of observations dropped when constructing the analytical sample and changes in sample size from year-to-year. The source of non-response can be systematic or selective. One source of systematic non-response comes from the survey design.[33] About one-fourth of the youth are excluded because they were not randomly selected to participate in the follow up surveys at 19 and 21 in their state. As long as states randomized correctly, this non-response is not a threat to the validity of estimated effects. Another source of systematic non-response comes from youth losing eligibility to participate in the survey as a result of incarceration, incapacitation, or death. This information is available so I can assume certain outcomes, like disconnectedness and incarceration, in these cases. Additionally, less than 2 percent of non-response is coming from these cases, so I do not perceive this source of non-response as a threat to my estimated effects. Alternatively, selective non-response comes from eligible youth choosing not to participate in the survey and may bias my results.

Non-response bias arises when the survey respondents are systematically different from the non-respondents leading to results that are not representative of the target population. The summary statistics discussed earlier indicate that NYTD participants have had different

[33] Some states opted to follow a random sample of their Cohort for follow up surveys at ages 19 and 21. There were 12 “samples states” in FY2011 and 15 in FY2014.

experiences with the foster care system than the average foster youth. For example, NYTD participants on average were 12 years old when they entered foster care and averaged about one and one-half placements per year.[34] The average foster child enters care at seven years old and experiences three placements per year (ACF, 2017; Casey Family Programs). Appendix Table C10 further suggests that NYTD participation is positively selected. Participants in all three surveys are less likely to have been removed for child-related issues and more likely to have been employed at 17 versus non-respondents. Survey drop-outs and survey returners are also better off than non-respondents. In general, the more surveys a youth responded to, the better off they appear, providing suggestive evidence for positive selection.

Positive selection could overestimate or underestimate the effect of extended foster care, depending on how response rates vary by treatment. For example, if treated states have higher response rates and respondents are positively selected, then my analysis might overestimate the effect of extended foster care. I find that extended foster care is negatively correlated with non-response (i.e. youth with extended foster care are more likely to respond) and then address this concern. First, I predict the likelihood of non-response using equation 1.1 where the dependent variable is an indicator equaling one if the youth participated in the survey and zero otherwise. The results of this exercise are presented in Appendix Table C11. Youth seem to be more likely to participate in NYTD at age 19 if they had extended foster care at age 18. Each additional year exposed to extended foster care also increases the likelihood of participating at 21. Failing to correct for non-response, may overstate the beneficial effects of extended foster care.

I address non-response bias two ways. First, I estimate equation 1.1 using inverse survey participation weights at the state and individual level. This approach gives states and individuals

[34] The number 1.5 placements per year comes from dividing the average number of placements (7) by the average length of stay (4.5).

with higher response rates less weight in the analysis since I am concerned with overstating the effect of extended foster care. Second, I estimate equation 1.1 using imputed outcomes and control for missing observations. I use mean and regression imputation techniques. Mean imputation assigns missing outcomes the average value of the non-missing observations. This technique preserves the overall mean and increases sample size. Regression imputation assigns missing outcomes a predicted value to preserve the relationship between covariates. In practice, I estimate equation 1.1 omitting the extended foster care variables, and then use the predicted values to impute the missing outcomes. I omit the extended foster care variables because I do not want to preserve the relationship between the outcomes and extended foster care, since I suspect this relationship is biasing my results.

Table 5 shows that overall none of the estimates from these techniques are statistically different from the main results and gives a range of potential effect sizes. Additionally, for most outcomes, the main effects are in the middle of the range of effect sizes. Using this range of estimates, one may conclude that exposure to extended foster care decreases homelessness by 18 to 35 percent, incarceration by 36 to 67 percent, and disconnectedness by 20 to 30. Even after correcting for non-response, extended foster care still appears to provide beneficial effects.

Table 5: Results from Techniques that Address Non-Response

	Outcomes at 19 Years Old				
	(1)	(2)	(3)	(4)	(5)
	Main Results	Inversely Weighted by State Survey Participation Rate	Inversely Weighted by Individual Response Rate	Mean Imputed	Regression Imputed
	Outcome: Homelessness				
Fed EFC at 18	-0.048*	-0.043	-0.046**	-0.034*	-0.030*
	(0.025)	(0.029)	(0.022)	(0.018)	(0.018)
Observations	11,120	11,120	11,120	15,733	15,733
Adjusted R-squared	0.088	0.088	0.087	0.062	0.124

	Outcome: Incarceration				
Fed EFC at 18	-0.053*	-0.059*	-0.046	-0.038**	-0.038*
	(0.029)	(0.029)	(0.028)	(0.016)	(0.022)
Observations	11,120	11,120	11,120	15,733	15,733
Adjusted R-squared	0.197	0.197	0.193	0.155	0.281
	Outcome: Disconnected				
Fed EFC at 18	-0.043**	-0.046**	-0.047***	-0.037**	-0.047***
	(0.020)	(0.021)	(0.016)	(0.014)	(0.015)
Observations	11,120	11,120	11,120	15,733	15,733
Adjusted R-squared	0.049	0.049	0.046	0.035	0.070
	Outcome: High School Enrollment				
Fed EFC at 18	0.052*	0.049	0.054*	0.038**	0.035
	(0.029)	(0.030)	(0.032)	(0.018)	(0.022)
Observations	11,097	11,097	11,097	15,733	15,733
Adjusted R-squared	0.040	0.041	0.039	0.029	0.063
	Outcome: College Enrollment				
Fed EFC at 18	0.010	0.014	0.012	0.011	0.016
	(0.038)	(0.039)	(0.036)	(0.037)	(0.037)
Observations	6,155	6,155	6,155	6,657	6,657
Adjusted R-squared	0.083	0.084	0.081	0.080	0.089
	Outcome: Employment				
Fed EFC at 18	0.083***	0.093***	0.070***	0.050**	0.069**
	(0.028)	(0.033)	(0.023)	(0.021)	(0.027)
Observations	11,120	11,120	11,120	15,733	15,733
Adjusted R-squared	0.045	0.046	0.047	0.036	0.065

Outcomes at 21 Years Old

	(1) Main Results	(2) Inversely Weighted by State Survey Participation Rate	(3) Inversely Weighted by Individual Response Rate	(4) Mean Imputed	(5) Regression Imputed
	Outcome: Homelessness				
Years with Fed EFC	-0.026*	-0.029*	-0.023	-0.052***	-0.055***
	(0.014)	(0.015)	(0.014)	(0.007)	(0.009)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.139	0.140	0.133	0.099	0.214
	Outcome: Incarceration				
Years with Fed EFC	-0.035***	-0.039***	-0.036***	-0.066***	-0.058***
	(0.007)	(0.008)	(0.006)	(0.017)	(0.007)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.234	0.238	0.231	0.206	0.388
	Outcome: Disconnected				
Years with Fed EFC	-0.031**	-0.029**	-0.033**	-0.021*	-0.017
	(0.013)	(0.013)	(0.012)	(0.011)	(0.013)
Observations	8,416	8,416	8,416	15,733	15,733

Adjusted R-squared	0.062	0.062	0.059	0.041	0.088
			Outcome: High School Graduation		
Years with Fed EFC	0.014**	0.014**	0.013**	0.047***	0.049***
	(0.006)	(0.006)	(0.006)	(0.014)	(0.017)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.069	0.069	0.067	0.077	0.100
			Outcome: College Enrollment		
Years with Fed EFC	0.045**	0.043**	0.045**	0.036*	0.035**
	(0.019)	(0.018)	(0.018)	(0.018)	(0.015)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.143	0.142	0.152	0.091	0.148
			Outcome: Employment		
Years with Fed EFC	-0.028***	-0.025**	-0.026**	-0.007	-0.012
	(0.010)	(0.010)	(0.011)	(0.007)	(0.011)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.066	0.067	0.060	0.056	0.100

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects, unless otherwise noted. The abbreviation EFC is shorthand for extended foster care. The first column reports the main results again for easy reference, the second and third columns report estimates weighted by response rate at the state and individual level, respectively. The fourth and fifth columns report results from imputation methods. These regressions also control for missingness.

1.10. Cost-Benefit Analysis

Funding extended foster care programs is a worthy investment. It is estimated that 2 percent of national child welfare expenditures (approximately 582 million dollars)[35] are spent on services and assistance for foster youth aged 17 to 21 years old, even though they make up 10 percent of the foster youth population (ACF, 2015; ACF, 2017). These services potentially provide both private and public returns, making this relatively small investment considerably more valuable. Cost-benefit analyses in California and Washington suggest that a dollar spent on extended foster care yields a return of two to five dollars (Courtney et al., 2009; Burley & Lee, 2010; National Conference of State Legislatures, 2019), and the Annie E. Casey Foundation

[35] Child Trends estimated that in FY2014, 2 percent of the 29.1 billion dollars national child welfare expenditures was spent on services and support for older youth currently or previously in foster care. For more information, see https://www.childtrends.org/wp-content/uploads/2017/09/Transition-Age-Youth_United-States.pdf and Rosinsky & Connelly (2016).

estimates that approximately 4.1 billion dollars could be saved if foster youth graduated high school and experienced homelessness, incarceration, and early parenthood at similar rates to their non-foster youth peers (Future Savings, 2019). Extended foster care provides a potential way to ensure that foster youth have more similar experiences to their non-foster youth peers as they transition to adulthood.

I find that a dollar spent on extended foster care maintenance payments yielded a return of two to four dollars for the NYTD participants in the FY2011 and FY2014 cohorts. Table 6 provides a breakdown of these estimates and calculations. I estimate the cost of extended foster care for the sample of NYTD participants at age 21 using their age of exit from care and monthly maintenance payments obtained from the AFCARS data. I calculate the total cost for a youth in extended foster by multiplying the length of time beyond age 18 that they have been in care by the monthly maintenance payments. The median age of exit is 18 to 18.6 years, with a range from 18 to 22. Based on this sample, the average amount spent on extended foster care maintenance payments is 8,659 dollars per youth in states with a federal policy, 3,469 with a state policy, and 3,413 with no policy.[36] In total, 51.6 million dollars were spent on extended foster care maintenance payments across the country.

[36] States without an extended foster care policy still occasionally support youth who are in high school or have a disability which is why these states still spend money on foster care maintenance payments.

Table 6: Cost-Benefit Analysis

	No EFC	State EFC	Federal EFC
Number of youth	2121	1961	4334
Median age at exit	18.0	18.0	18.6
Average of total foster care maintenance payments received as an adult	$3,413	$3,469	$8,659
Total amount spent on foster care maintenance payments: **$51.6 million**			
Number of youth ever homeless if had similar transition as non-foster youth peers: 337			
Number of youth ever homeless	887	787	1411
Counterfactual if no policy	887	932	1628
Difference in counterfactual versus actual	0	145	217
Cost of being homeless for 7 days per youth: $252			
Cost avoidance: **$91,177**			
Number of youth ever incarcerated if had similar transition as non-foster youth peers: 5			
Number of youth ever incarcerated	654	615	1080
Counterfactual if no policy	654	690	1366
Difference in counterfactual versus actual	0	75	286
Cost of being incarcerated per youth: $52,080 to $334,230			
Cost avoidance: **$18.8 million** to **$120 million**			
Number of youth that graduated high school by age 21 if had similar transition as non-foster youth peers: 7490			
Number of youth that graduated high school by age 21	1742	1568	3560
Counterfactual if no policy	1742	1529	3430
Difference in actual versus counterfactual	0	39	130
Cost of not graduating per youth: $410,659			
Cost avoidance: **$69.5 million**			
Benefit-cost ratio: $1.71/$1 to $3.69/$1			

Notes: The first panel presents the cost of extended foster care using the foster care maintenance payment amounts reported in AFCARS, and panels two through four present the amount of money saved using the costs of homelessness, incarceration, and not graduating high school from the Future Savings report produced by the Annie E. Casey Foundation. All counts of youth are specific to the two NYTD cohorts (FY 2011 and FY 2014), the counterfactual numbers if foster youth had similar experiences as their non-foster youth peers comes from probability estimates from the Annie E. Casey Foundation Future Savings Report, and the counterfactual counts of no extended foster care policy is based on the main results of this paper.

I use the conservative estimates from this paper's main results to avoid overstating the benefits of extended foster care. I compare the actual incidence of homelessness, incarceration, and high school graduation for the NYTD participants at age 21 to the counterfactual outcome of having no policy. All else equal, if no states implemented extended foster care during 2012 to 2016, then 362 more youth might have experienced homelessness, 361 more youth might have been incarcerated, and 169 fewer youth might have graduated high school by age 21. To determine the monetary value of reducing these hardships and calculate the benefits of extended foster care, I use the costs of homelessness, incarceration, and not graduating high school from the 2019 Annie E. Casey Foundation Future Savings report.[37] Specific to the NYTD FY2011 and FY2014 cohorts, extended foster care reduced costs to society by 88.4 million to 190 million dollars, depending on the cost of incarceration.

The benefits may be even larger since this calculation does not include the long-term benefits of reducing homelessness and incarceration at a young age.[38] Additionally, this analysis does not monetize the benefits of being employed at age 19 or being enrolled in college at age 21, nor does it account for nonpecuniary returns. The benefits of extended foster care outweigh the costs and indicate that this program is a worthy investment, with at least a two dollar return on investment.

1.11. Conclusion

To date, much of the existing research shows beneficial associations, not causal evidence, between extended foster care and the transition to adulthood by comparing outcomes of youth across a handful of states. Citing this research, states continue to adopt extended foster care polices. For example, between January 2017 and July 2019, seven states were approved to

[37] The cost of homelessness is a conservative estimate that only takes into consideration the cost of a providing a bed, and not the cost of other services that shelters may provide. The cost of incarceration is based on the cost of a one-day detention placement, costs to society, and recidivism. Finally, the cost of not graduating high school is based on lifetime gross income and societal tax loss.

[38] Reducing youth homelessness and incarceration may prevent future episodes and other costly outcomes (Hodgson et al., 2013; McLaughlin et al., 2016; Barnert et al., 2017; U.S. Department of Health and Human Services, 2017).

implement federally-funded extended foster care, and currently another two are pending approval.[39] With increased uptake of extended foster care, it is important to demonstrate that this program is beneficial and cost-effective.

I estimate the intent-to-treat effect of extended foster care on the transition to adulthood and enrich the existing research by comparing youth within a state under different policy regimes nationwide. The intent-to-treat effect is advantageous over the treatment-on-treated effect because it is more policy relevant and is not biased by selection into treatment. I use a combination of relatively new individual-level survey data, rich administrative case-level data, and state-level data to reduce omitted variable and selection bias. I also provide estimates from a variety of different model specifications, showing that the results are invariant to specification changes, except in cases where we expect to observe differences.[40] Additionally, I have established that NYTD participants are positively selected. Failure to correct for non-response may lead to biased estimates depending on how the response rate is correlated with the treatment. After employing methods to mitigate non-response bias, I still conclude that extended foster care benefits foster youth as they transition to adulthood.

Extended foster care reduces homelessness, incarceration, and disconnectedness in the short run. Compared to *access* to Homebase Centers, extended foster care is twice as effective in reducing homelessness (Goodman et al., 2016), but relative to *receiving* emergency rent and Homebase services, extended foster care is only about half as effective (Rolston et al., 2013; Evans et al., 2016). Extended foster care is more effective in reducing incarceration among foster

[39] The seven states recently approved include Colorado, Florida, Georgia, New Mexico, North Carolina, Ohio, and Rhode Island. Louisiana and Nevada are pending approval.
[40] For example, results are sensitive to excluding state fixed effects, so assuming states have similar foster care environments and other time-invariant characteristics to make comparisons across states is problematic.

youth than each of the top five policy reforms in states across the country (Schrantz et al., 2018). Finally, reductions in disconnectedness mean that youth are more likely to be working and/or attending school. This result is reassuring as many states require school and/or work requirements for extended foster care participation. Although, this study does not measure participation, it would be concerning if an outcome related to eligibility was not improved by the existence of extended foster care.

Extended foster care also increases educational attainment through increased high school/GED completion and college enrollment. It is well known that the pecuniary and nonpecuniary returns to education are large for both individuals and society, even without degree completion.[41] Interestingly, youth appear to be making a tradeoff between college enrollment and employment. Extended foster care availability at age 18 initially increases employment, however over time, youth are less likely to work. This finding taken together with college enrollment and disconnectedness, indicates that extended foster care may provide youth with enough resources so that they can attend school without the additional burden of working.

Importantly, extended foster care appears to mitigate the consequences of common hardships that foster youth experience as minors, such as substance abuse and homelessness. Mitigating these hardships might have beneficial long-run effects that should be considered as states design and enact programs in the future.

All of these beneficial effects are primarily driven by the federal program. This finding suggests that the federal program is more effective than the state programs, which may result from greater reach and increased quality and quantity of resources.[42] Implementing federally-

[41] See Angrist & Krueger (1991), Ashenfelter & Krueger (1994), Oreopoulos & Salvanes (2011) and Oreopoulos & Petronijevic (2013) and Shapiro et al., (2014) for more information about the returns to education.
[42] Pinning down how much each of these mechanisms drive the results is left for future research.

funded extended foster care is a tangible way for states to assist foster youth through their transition to adulthood.

There are two limitations of this study and recommendations for future research. First, the specific mechanism (i.e. housing, social, or financial support) is ignored. Extended foster care programs vary by state, and this analysis estimates the effect of the bundle of services and supports. Exploratory research reveals that most of the effect may be driven by the housing and social support. Interviewed foster youth often acknowledge that the program has helped them by providing housing and mentors to develop life skills.[43] The availability of these supports is also consistent with the finding that extended foster care mitigates childhood hardships such as homelessness and substance abuse. Future research will focus on specific programs in states proving to be successful to better understand the most beneficial and cost-effective services.[44] Second, due to data limitations, this analysis is unable to estimate take-up rates and the treatment-on-treated effect. Without administrative records and correspondence with individual state agencies, extended foster care participation is not accurately identified. Future research will focus on overcoming this challenge, so that we can learn more about extended foster care participants. This analysis provides emerging causal evidence of the beneficial impacts of extended foster care nationwide and provides many directions for future research.

[43] See this AJC article for an example.

[44] Some extended foster care programs to investigate include California's AB12 and Nebraska's Bridge to Independence. Existing research shows both of these programs are effective (Courtney et al., 2018; Sepulveda et al., 2019). Nebraska's program offers medical care, housing assistance, and case management.

Chapter 2: Child Maltreatment Referrals and Mandatory Reporting Laws

2.1. Introduction

In 2017, an estimated 7.5 million children in the United States were referred to Child Protective Services for maltreatment, an increase of 1.7 million children (29%) from 2007 (ACF, 2008; ACF, 2019). Child Protective Services (CPS) is the government agency in each state accountable for children's well-being with the main responsibility of responding to child maltreatment referrals. The referral is the first step in a lengthy process to substantiation. A referral can be made by anyone, but is mandated for certain professions. These "mandatory reporter" classifications vary by state and over time. In 2004, the most common reporters were teachers (19%), law enforcement (18%), and social services staff (11%). In 2017, the most common reporters were education (20%), legal (20%), social services (12%), and medical (11%) personnel. After a referral is made, it is screened in or out based on state criteria. Consistent with the child welfare literature, I refer to referrals that are screened-out as "referrals screened-out" and referrals that are screened-in as "reports." There is no additional follow-up for referrals that are screened-out, but reports are then investigated for child maltreatment. A caseworker visits the family and observes the living environment to determine a disposition. A report is substantiated if there is enough evidence to prove maltreatment occurred, and unsubstantiated otherwise. The purpose of this paper is to understand how child maltreatment reporting responds to changes in mandatory reporter legislation.

Observed child maltreatment is a function of the interaction between maltreatment and reporting. The true nature of maltreatment is unknown and widely regarded as underreported (MacMillan et al., 2003; Fallon et al., 2010; GAO, 2011; National Research Council, 2014). In response, there has been considerable effort trying to identify behaviors and circumstances that are predictive of maltreatment.

Poor economic conditions such as unemployment, low-income, and inadequate housing are correlated with increases in child abuse and neglect (Weinberg, 2001; Paxson & Waldfogel, 2002; Slack et al., 2003; Seiglie, 2004; Lindo et al., 2013; Warren & Font, 2015; Berger et al., 2017; Lindo et al., 2018; Brown & De Cao, 2020). A randomized control trial in Delaware and an evaluation of the Welfare Reform Act of 1996 found that less generous welfare programs increase child maltreatment (Fein & Lee, 2003; Paxson & Waldfogel, 2003). Alternatively, safety net programs can reduce the nature of child maltreatment. Berger and colleagues (2017) show that an increase in income from the earned income tax credit reduces CPS's involvement, Raissian & Bullinger (2017) find that increasing the minimum wage reduces child maltreatment, and Brown & De Cao (2020) show that states with longer durations of unemployment insurance had fewer maltreatment reports during the Great Recession. Additionally, more generous child support payments reduce the number of screened-in reports of maltreatment (Cancian et al., 2013).

Environmental factors, such as availability of services, substance abuse, and temperature, might also impact child maltreatment. Access to abortions have been found to reduce child maltreatment (Bitler & Zavodny, 2004; Seiglie, 2004). Substance abuse, regardless of the drug, impacts parental functioning and places a child at risk of maltreatment (Wells, 2009). While there is no evidence relating temperature to child maltreatment,[45] there is emerging evidence that hotter temperatures increase violence (Anderson et al., 2000; Anderson, 2001; Almas et al., 2019; Heilmann & Kahn, 2019). Lastly, family structure contributes to maltreatment.

[45] One notable exception is Jessica Pac's third dissertation chapter (Pac, 2019).

Specifically, changes in family structure to single parenting or blended families (step parents and siblings) is associated with abuse and neglect (National Research Council, 2014; Schneider, 2016).

Since the underlying causes of maltreatment have been investigated in great detail, this paper focuses on the second factor that determines the number of maltreatment investigations, reporting. Reporting is a function of social norms around reporting maltreatment and the number of mandatory reporters. Recently, there has been increased interest in the role mandatory reporters play detecting child maltreatment, especially school teachers.[46] However, there is limited conclusive research around mandatory reporting laws and referrals. On one hand, universal reporting laws[47] are associated with higher report rates for abuse and neglect (Krase & DeLong-Hamilton, 2015; Palusci et al., 2016). Alternatively, Ho et al. (2017) conclude that child maltreatment reporting did not differ across states with and without universal mandatory reporting laws in 2013. These papers make comparisons across counties and states, so their findings are not necessarily causal. Instead these differences may be confounded by demographic, economic, and cultural differences across places.

I contribute to this body of research by investigating the effect of changing mandatory reporter laws over a 14-year period in the United States on child maltreatment referrals and reports. Specifically, I answer the following three research questions:

1. How has mandatory reporter legislation changed over time?

[46] Recent working papers that discuss the role of teachers in detecting child maltreatment early include Fitzpatrick et al. (2020) and Cabrera-Hernandez & Padilla-Romo (2020).
[47] Universal reporting laws require all persons to report suspected maltreatment regardless of their profession.

2. What is the relationship between changes in mandatory reporter legislation and child maltreatment referrals and reports?
3. Do different professions have different impacts?

Mandatory reporting laws designate professions that are required to report child abuse or neglect and establish the reporting process, such as who to call and what details to provide. Some legislation adds new job classifications to the list of mandatory reporters, whereas other legislation provides clarification about making a report and privileged communication or adds training requirements. I use changes in the list of mandatory reporters from 2004 to 2017 to estimate how increasing the number of professions designated as mandatory reporters impacts child maltreatment reporting.[48]

The predicted change in maltreatment referrals and reports is theoretically ambiguous, but empirically testable and important. Behavioral responses from both reporters and perpetrators are possible explanations for long-run changes in reporting. As more people understand the dangers of child maltreatment, recognize it as a public concern, and better understand their and CPS's role, then the quantity of referrals and reports may increase, holding the true amount of child maltreatment constant. Better identification of child maltreatment would be indicated by increased substantiation rates and is important in detecting the true nature of maltreatment. Alternatively, the bystander effect may explain a decrease in reporting, holding child maltreatment constant.[49] Lastly, if increasing reporting and awareness of child maltreatment

[48] While there are differences across states in their standards for making a report and privileged communication, these differences are mostly invariant over time from 2004 to 2017. Alternatively, training requirements have become more popular in recent years and considerable effort is needed to track down the changes over time for all 50 states and DC. For these reasons, I do not focus on these changes and leave them for future research.

[49] The bystander effect refers to the phenomenon where increasing the number of people that witness a crime reduces the probability of reporting. This phenomenon was first discovered by psychologists Darley & Latané after a woman was murdered in New York in front of dozens of neighbors (ESRC, nd). Factors

deters perpetrators, then we would expect to see a decline in reporting, holding the nature of reporting constant. Deterring perpetrators would be a beneficial impact of mandatory reporter legislation.

Using a two-way fixed effects model with a state-level linear time trend, I do not find any evidence that the referral rate responds to changes in mandatory reporters, and I can rule out responses larger than 10 percent. Alternatively, I find that increasing the number of jobs listed as mandatory reporters increases the report rate by 4 percent. While there are more reports, most of the increase is driven by unsubstantiated reports. Moreover, a legislation change that increases the number of mandatory reporters leads to a smaller increase in reports in states with more mandatory reporters, relative to states with fewer mandatory reporters.

I also investigate whether any changes in reporting can be detected in the short-run. If the legislation change was highly publicized, then we might expect a larger temporary shock in the short-run compared to the long-run. However, the direction of this shock is ambiguous. On the one hand, as mandatory reporters learn about their new responsibility, they may either report more or less, depending on the size of the bystander effect. Alternatively, perpetrators may change their behavior temporarily to prevent getting caught. Using an event-study design and difference-in-differences approach, I compare child maltreatment reporting five months before and after a legislation change within a state. I find suggestive evidence that reporting is overall unresponsive to mandatory reporter legislation changes in the short-run, and I can rule out increases and decreases greater than 8 percent. This is perhaps unsurprising, since in most states mandatory reporter legislation seems to be modified with little publicity.

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contributing to the bystander effect have since been researched in detail (e.g. Fischer et al., 2011); however, no examples, to my knowledge, of the bystander effect have been reported in the child maltreatment literature.

In addition, I advance the existing research by quantifying the effect of specific professions added to the list of mandatory reporters. Coaches, college staff, and camp staff were the most commonly added professions between 2004 and 2017. Different professions are expected to have different impacts on reporting due to the degree of interaction with children and their reason for being included. Moreover, reports made by professionals have higher substantiation rates than reports made by nonprofessionals (Wolfe, 2012; King et al., 2013). I find that the majority of added professions do not significantly impact reporting. As states contemplate adjusting mandatory reporter laws, they may question whether more professions need to be listed, and if so, which ones.

Child maltreatment reports reflect the true nature of maltreatment and impact the demand for child protective services. In addition, mandatory reporter laws are policy levers that states can enact and modify rather cheaply. For these reasons, it is important to understand how changing who is required to report may or may not effectively detect child maltreatment.

2.2. Background on Mandatory Reporter Laws and Their Impact on Reporting

Under the Child Abuse Prevention and Treatment Act, states are required to designate certain professionals as mandatory reporters. A mandatory reporter is someone required by law to report when they know or suspect child maltreatment. Some states require universal reporting, where all adults are considered mandatory reporters, whereas other states designate a list of professions as mandatory reporters. Typically, these mandatory reporters work in professions that interact with children regularly, such as teachers, pediatricians, and childcare providers. In some states with specific industries, professions such as film and photograph processors, computer technicians, and camp counselors are required to report.

Over the years, more professions have been added to the list of mandatory reporters. In 2004, the most common professions listed as mandatory reporters across all states included social workers, teachers, health care workers, mental health professionals, childcare providers, and law enforcement officers (CWIG, 2003). On average, eight broad job categories were indicated as mandatory reporters, with some states indicating zero professions[50] and others indicating as many as 15. By 2017, an average of nine job categories were indicated, with as few as zero and as many as 19. Common professions added during this time period include coaches, university staff, and camp staff.

Currently, some states are discussing scaling back their list of mandatory reporters because they fear the additional unsubstantiated referrals are overburdening CPS and support is not being allocated effectively (i.e. to those most in need).[51] While these concerns are frequently brought up (Melton, 2005; Mathews & Kenny, 2008; Cecka, 2015; Raz, 2017), the evidence is less clear. Cross country analyses find that mandatory reporting laws increase the number of reports (Mathews & Kenny, 2008; Donald, 2012). In Australia, mandatory reporting legislation increased the detection of sexual abuse (Mathews et al., 2016), and in Canada, mandatory reporting laws increased contact with CPS for severe and frequent maltreatment, but it is unclear whether this increased contact with CPS improved child wellbeing (Tonmyr et al., 2018).

In the United States, the evidence is mixed. A presentation at the American Economic Association in 2020 found preliminary evidence that the first laws of mandatory reporters in the 1960s and 70s[52] led to increased awareness of child abuse and reductions in child mortality for children under one years old (Arteaga & Barone, 2020). These reductions become apparent 4 to

[50] States that do not indicate certain professions have a universal reporting law, in which all people are required to report regardless of their profession.
[51] For example, Idaho passed a bill in February 2020 to eliminate some mandatory reporting requirements.
[52] These first mandatory reporter laws listed doctors to prevent "Battered Child Syndrome."

5 years after the policy was enacted. In a county-level study, universal reporting laws are associated with higher report rates for abuse, and clergy reporting requirements are associated with increased total reports, although not all are substantiated (Palusci et al., 2016). In a state-level study, universal reporting laws are associated with higher report rates for neglect (Krase & DeLong-Hamilton, 2015). Alternatively, Ho et al. (2017) conclude that child maltreatment reporting did not differ across states with and without universal mandatory reporting laws. Overall, the research around mandatory reporting laws and referrals is scant, and few explanations for differences are given.

In this paper, I discuss potential mechanisms and explain their implications. Behavioral responses from both reporters and perpetrators are possible explanations for long-run changes in reporting. Specifically, there is a knowledge effect, bystander effect, and deterrent effect. Child maltreatment is underreported for a variety of reasons including a lack of understanding what constitutes maltreatment and fear that steps following the report will make the situation worse (National Research Council, 2014). As more people understand the dangers of child maltreatment, recognize it as a public concern, and better understand their and CPS's role, then the quantity of reports may increase, holding the true amount of child maltreatment constant. This situation describes the knowledge effect and explains how reporting might increase in response to a mandatory reporter legislation change.

The bystander effect refers to the phenomenon where increasing the number of people that witness a crime reduces the probability of reporting. Two primary factors that contribute to this phenomenon are the diffusion of responsibility (Darley & Latané, 1968) and not understanding the environment (Darley & Latané, 1970). Moreover, the bystander effect is reduced in emergency situations, when perpetrators are present, and when the costs of intervening are physical, and not financial or opportunity costs (Fisher et al., 2011; Panchanathan et al., 2013). Child maltreatment reporting may be especially prone to the bystander effect for three reasons. First, in states with relatively more mandatory reporters, requiring more people to report allows mandatory reporters to diffuse their responsibility of reporting more easily. In fact, some mandatory reporters already report passing on the responsibility to their supervisors (McTavish et al., 2017). Second, there is evidence that mandatory reporters do not understand the reporting environment. For example, seasoned mandatory reporters indicate needing better training to identify and report maltreatment (McTavish et al., 2017). Lastly, child maltreatment incidences have the opposite characteristics of the situations in which the bystander effect is mitigated. They are generally not emergencies and learned about after the fact when the child is away from the perpetrator, and mandatory reporters face non-physical costs to intervene. The bystander effect might be one explanation for a decline in reporting.

The deterrent effect might be another explanation for a decline in reporting. An increase in mandatory reporters and awareness of child maltreatment may deter perpetrators. In this situation, we would expect to see a decline in reporting, holding the nature of reporting constant. Disentangling the bystander effect from the deterrent effect is difficult; however, the decline in reports from these two mechanisms have very different policy implications. The bystander effect implies children may be going unreported and the deterrent effect implies fewer children are being maltreated. One way to disentangle the deterrent effect from the bystander effect is by examining how substantiated and unsubstantiated reports respond. For example, a decrease in substantiated reports, or victims, relative to unsubstantiated reports, holding constant the investigation process, would indicate less maltreatment. Deterring perpetrators would be a beneficial impact of the mandatory reporter legislation, whereas the bystander effect would be an unintended consequence.

In the short-run there might be a publicity effect, or a temporary shock in reporting, if the legislation change was highly publicized. However, the direction of this shock is ambiguous. On the one hand, as mandatory reporters learn about their new responsibility, there may either be more or less reports, depending on the size of the mechanical effect, the bystander effect, and the deterrent effect. The mechanical effect is the effect of increasing the number of people required to report maltreatment, holding constant reporter and perpetrator behavior. For example, as more people are required to report, the quantity of reports may simply increase as a result of children interacting with more mandatory reporters, assuming these people know of their new responsibilities. Alternatively, mandatory reporters may diffuse their responsibility to report. The bystander effect relies on the assumption that mandatory reporters know there are other people required to report maltreatment. Lastly, perpetrators may change their behavior temporarily to prevent getting caught. If legislation changes are not well publicized, then we would not expect to see evidence of the publicity effect.

2.3. Data

I create a state panel of mandatory reporter legislation, child maltreatment referrals and reports,[53] case dispositions, demographic characteristics, and economic conditions from 2004 to 2017. I start in 2004 because this is the first year in which all but two states report child

[53] See Appendix Figure D1 and D2 for the average report rate by state and year, respectively.

maltreatment data to the National Data Archive on Child Abuse and Neglect (NDACAN).[54] I end in 2017 because this is the most recent year of updated data available.[55]

The mandatory reporter job classifications for each state come from changes in mandatory reporting laws.[56] I construct the first state-level panel of mandatory reporter legislation changes from 2004 to 2017.[57] Table 7 summarizes these changes over time.[58] From 2004 to 2017, 27 states have updated their list of mandatory reporters at least once. One-third, 9 states, have only made one change, whereas the remaining 18 states have made two to six changes. Each change adds anywhere from one to four more broad categories of professions to the list of mandatory reporters. During this time period, only one state, Virginia, removed a profession, clergy, from the list in 2007, so overall a change in legislation represents an increase in mandatory reporters.

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[54] Maryland and Michigan are missing one year of child maltreatment data in 2006 and 2007, respectively. Oregon and North Dakota are missing multiple years of child maltreatment data. North Dakota started reporting child maltreatment data in 2010 and Oregon started reporting in 2011.

[55] As states resubmit their records, NDACAN provides updated versions of data.

[56] I am very grateful to the Child Welfare Information Gateway librarians, especially John Vogel and Sara-Jane Ziaya, for sending me the archived documents on mandatory reporting laws. Dates are verified with the NCSL and Westlaw databases.

[57] Mathematica publicly released the SCAN Policies Database April 2021, which provides differences in state policies for one year, 2019. Per email correspondence with the database team, state policies in 2021 will be released in 2022. While this will be a valuable resource in tracking policy changes over time, it is limited to changes starting after 2019, whereas the panel I constructed starts in 2004.

[58] A more detailed table with effective and enacted dates and added professions is available upon request.

Table 7: Mandatory Reporter Law Changes in Each State from 2004 to 2017

State	Number of Mandatory Reporter Changes	Number of Professions in 2004	Number of Professions in 2017	Number of Added Professions
Arizona	0	6	6	0
Connecticut	0	14	14	0
Florida	0	8	8	0
Hawaii	0	9	9	0
Idaho	0	7	7	0
Indiana	0	2	2	0
Iowa	0	10	10	0
Kentucky	0	8	8	0
Maryland	0	4	4	0
Massachusetts	0	13	13	0
Michigan	0	9	9	0
Minnesota	0	8	8	0
Mississippi	0	8	8	0
Missouri	0	11	11	0
Montana	0	9	9	0
Nebraska	0	3	3	0
New Hampshire	0	8	8	0
New Jersey	0	0	0	0
New Mexico	0	6	6	0
North Carolina	0	0	0	0
Rhode Island	0	1	1	0
Texas	0	5	5	0
Utah	0	1	1	0
Wyoming	0	0	0	0
Alabama	1	8	9	1
Alaska	1	10	11	1
Delaware	1	4	5	1
North Dakota	1	9	10	1
Oklahoma	1	3	4	1
Oregon	1	12	15	3
Pennsylvania	1	9	13	4
South Carolina	1	14	15	1
Tennessee	1	8	9	1
Arkansas	2	12	14	2
California	2	15	19	4
Georgia	2	7	11	4
Kansas*	2	9	9	0
Maine	2	14	15	1
South Dakota	2	10	12	2

State	Number of Mandatory Reporter Changes	Number of Professions in 2004	Number of Professions in 2017	Number of Added Professions
Washington	2	8	10	2
West Virginia	2	9	12	3
Illinois	3	14	19	5
Louisiana	3	11	15	4
New York	3	11	13	2
Ohio	3	10	11	1
Wisconsin	3	10	12	2
Colorado	4	9	14	5
DC	4	7	10	3
Nevada*	4	13	13	0
Vermont	4	10	11	1
Virginia	6	9	13	4

Notes: The table is organized by number of changes and then alphabetical order. The number of changes is determined from reading state legislation provided by the Westlaw database. Number of professions in 2004 and 2017 is the number of broad job categories listed in the state statutes provided by the CWIG in 2003 and 2015, respectively. A value of zero indicates that no broad job categories are listed, i.e. the state has a universal mandatory reporting law and requires all persons to report. The number of added professions is the difference between the 2017 and 2004. In states with no change, there are no added professions. A detailed state panel of effective and enacted dates and professions added from 2002 to 2020 is available upon request.
* Both Kansas and Nevada have multiple mandatory reporter legislation changes, but report an unchanged number of professions because the professions added to the list do not fall under the general categories considered in this analysis.

The child maltreatment data come from National Child Abuse and Neglect Data System (NCANDS) housed by NDACAN. I use both the agency and child files. The NCANDS agency file contains the number of referrals screened-out in a state during a specific year. One drawback of using just the agency file is that it only includes referrals, which are volatile from year to year as a result of changes that are not always related to mandatory reporting legislation.[59] Alternatively, the NCANDS child file contains detailed information about a report of maltreatment and the child characteristics. Reports are recorded during the fiscal year in which

[59] For example, in 2010 Alabama's screened-out referrals dropped by almost 100 percent as a result of correcting an error from previous years (ACF, 2011). In Arkansas, the number of referrals screened-out increased as a result of changing child maltreatment statutes and staff training (ACF, 2014).

the disposition was decided, but I restructure this data to obtain the number of reports, unique children, and victims in a given month, year, and state. The average time to disposition is 56 days, so this structure does not drastically change the results.

Finally, state characteristics and economic conditions, which are included as controls in robustness specifications, come from the Annie E. Casey foundation KidsCount data book and the University of Kentucky Center for Poverty Research welfare dataset.

2.4. Empirical Strategy

I first analyze trends in reporting using annual state-level data. I use a linear time trends model with fixed effects to estimate how changes in child maltreatment referrals and reports correspond to changes in mandatory reporters from 2004 to 2017. The main regression equation is:

$$Y_{sy} = \beta_0 + \beta_1 MR_{sy} + \gamma_s + \gamma_y + \gamma_s * year + \varepsilon_{sy} \qquad (2.1)$$

Where Y_{sy} is the child maltreatment rate, per 100,000 children for state s in year y. The four specific rates are total referrals, screened-out referrals, reports (i.e. screened-in referrals), and children investigated. Effects on screened-out referrals and reports are included to determine the composition of total referrals. MR_{sy} is a discrete variable that tracks changes in mandatory reporters. State fixed effects, γ_s, allow for within state comparisons to avoid confounding the impact of changes in mandatory reporters with state differences in culture and other policies. Year fixed effects, γ_y, are included to capture any time shocks that may impact the entire country similarly, such as the Great Recession. Lastly, state-specific linear time trends, $\gamma_s * year$, are imposed to capture the trends in child maltreatment over time within a state.[60] The error term is ε_{sy}.

β_1 estimates the effect of changing mandatory reporter legislation on child maltreatment referrals and reports within a state. The validity of this approach relies on the assumption that states did not change their mandatory reporter legislation concurrently with other child welfare legislation that affects referrals and reports. For example, changing the intake process or what constitutes abuse and neglect might bias the results. Delaware and Pennsylvania are two examples of states that enacted a comprehensive package of child welfare reforms to better protect children in response to high profile incidences (ACF, 2012; ACF, 2018), but the majority of states did not change mandatory reporting and other child welfare legislation concurrently. Such incidences are uncommon but should be kept in mind when interpreting results.

In addition to an annual analysis, I construct a state-level panel of monthly observations. One advantage of this approach is that more observations can lead to more statistical power. In addition, the timing of treatment is more precise. On the other hand, monthly report rates are more volatile than annual report rates. Figure 2 shows how child maltreatment reports fluctuate across months.[61] For this reason, the monthly analysis includes a year-by-month fixed effect, in place of the year fixed effect, in equation 2.1.

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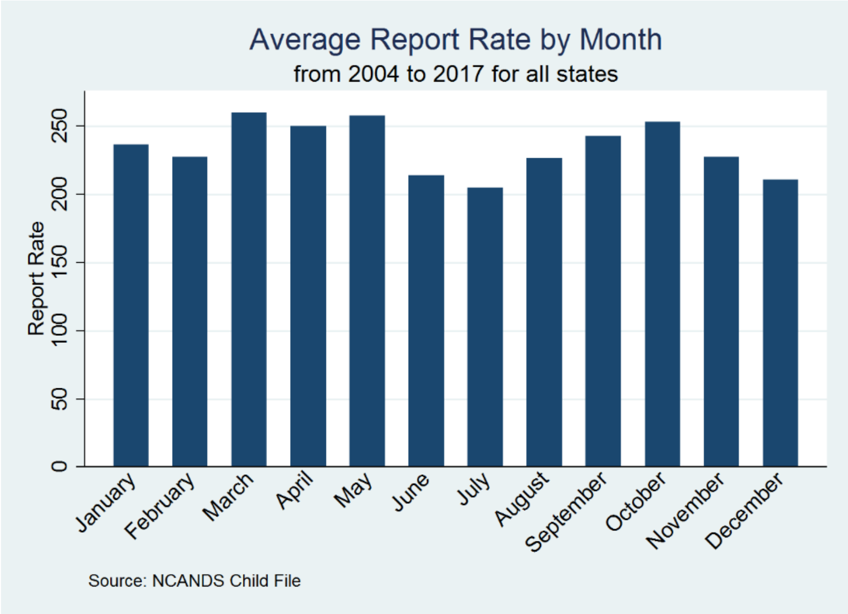
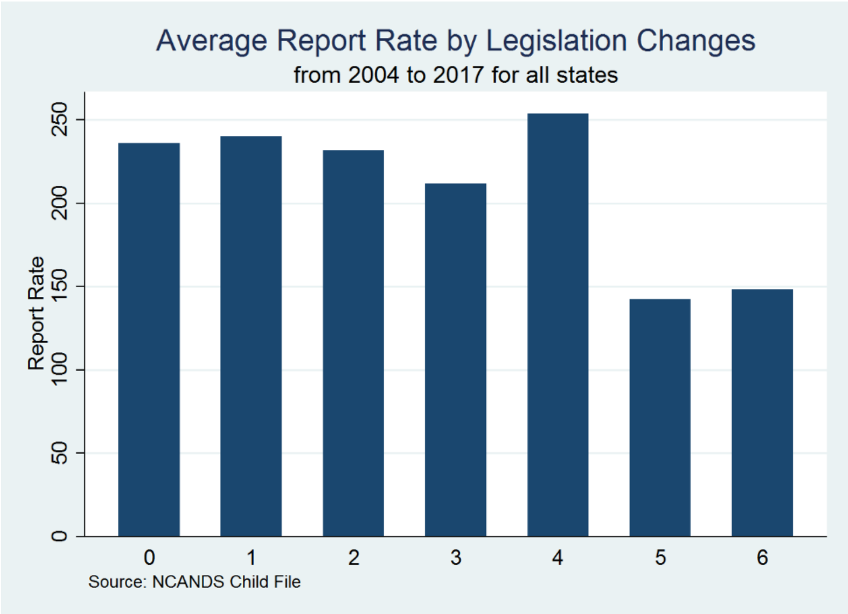
[60] As a robustness check, I include state characteristics, such as racial, age, educational, and family structure composition, and economic conditions, such as the poverty and unemployment rate. Results are similar. Some of these variables will impact the underlying behavior of child maltreatment. Controlling for these variables assumes that the underlying nature of child maltreatment is constant, whereas excluding these allows the nature of child maltreatment to adjust to the mandatory reporter legislation.
[61] See Appendix Figure D2 for the average report rate by year.

Figure 2: Average Report Rate by Month



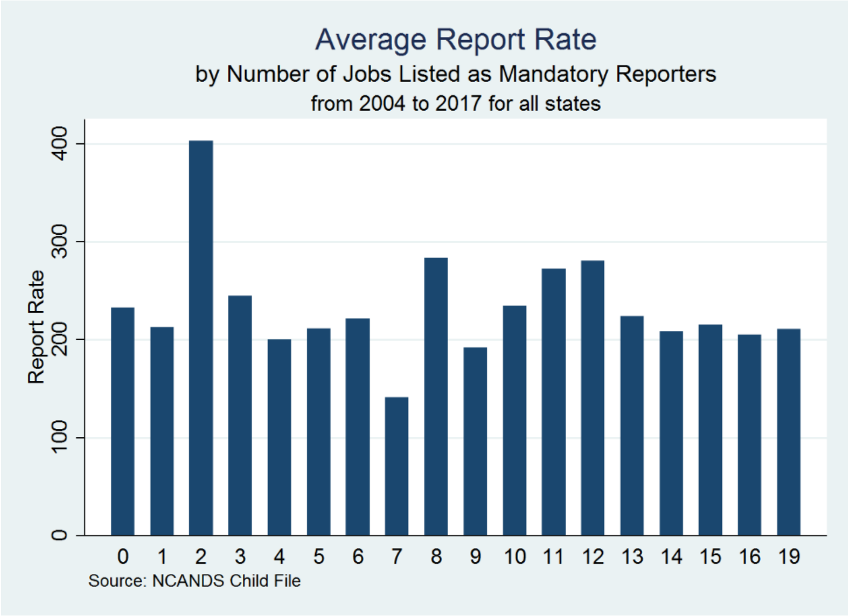
Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all states and years and is weighted by the state population. This figure masks within state and across year variation.

Figure 3: Average Report Rate by Number of Changes in Mandatory Reporter Legislation



Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all states and years and is weighted by the state population. This figure masks within state and across year variation.

Figure 4: Average Report Rate by Number of Jobs Listed as Mandatory Reporters



Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all states and years and is weighted by the state population. This figure masks within state and across year variation.

There may also be differential effects by the number of changes and amount of mandatory reporters. Figure 3 plots the average number of reports by change in mandatory reporter legislation, and Figure 4 plots the average number of reports by number of jobs listed as mandatory reporters.[62] Both of these figures suggest there are non-linear effects, and the interaction between legislation changes and number of mandatory reports is complex. For example, a legislation change in states with a relatively low number of reporters may have a different impact than a legislation change in states with a relatively high number of reporters. I estimate the following equation to quantify this interaction effect:

[62] These averages are weighted by the population size.

$$Y_{smy} = \delta_0 + \delta_1 changeMR_{smy} + \delta_2 totalMR_{smy} + \delta_3(changeMR_{smy} X\ totalMR_{smy}) + \gamma_s + \gamma_{my} + \gamma_s * year + \varepsilon_{smy} \quad (2.2)$$

Where Y_{smy} is the report rate in state *s* for month *m* of year *y*, *changeMR* indicates the number of legislation changes, *totalMR* indicates the number of broad job categories classified as mandatory reporters, and γ_s, γ_{my}, $\gamma_s * year$ are the same as equation 2.1. δ_1 estimates the effect when a state changes it mandatory reporter legislation, δ_2 estimates the effect when a state adds another job to the list of mandatory reporters, and δ_3 estimates the effect of a policy change for states with more mandatory reporters, relative to states with fewer mandatory reporters.

Next, to investigate the sensitivity of reporting in the short-run, I use an event study design, similar to Leslie & Wilson (2020). I investigate the effect five months before and after the legislation change, centering around the month in which the change occurred. Since monthly report rates are volatile, I include a control period to make comparisons in the same month before and after the legislation change. The regression equation is:

$$Y_{smy} = \sum_{\tau=-5}^{5} \beta_\tau [1(Month_{smy} = \tau) * Treat_{my}] + \sum_{\tau=-5}^{5} \Theta_\tau 1(Month_{smy} = \tau) + \alpha Treat_{my} + \gamma_s + \gamma_m + \gamma_y + \varepsilon_{smy} \quad (2.3)$$

The outcomes are the report rate, substantiation rate, and victim rate (per 100,000 children) in state *s* for month *m* in year *y*. The indicator function $1(Month = \tau)$, for all $\tau \in \{-5,5\}$, takes a value of one if the month is within the five months before or after the effective date of the mandatory reporter change. $Treat$ is a binary variable that equals one in the five-month period both before and after the legislation change in the period the change occurred, and zero in the five-month period both before and after the legislation change one year earlier, i.e. the control period. I also control for state differences, monthly trends, and annual changes. As a result, β_τ estimates the effect for each of the five months before and after the legislation change within a state relative to the previous period. A valid design will not have pre-trends, that is β_{-5} through β_{-1} will not differ from zero.

To quantify the five-month average effects, I also estimate a difference-in-differences model. This approach estimates the short-run impact of changing mandatory reporters, relative to the same time period in the earlier year. This approach is validated by the lack of pre-trends from the event study.

2.5. Results

2.5.1. Annual Results

Table 8 reports the results from equation 2.1 for the annual analysis. There are 589 state-by- year observations.[63] The first panel reports the effect of an additional change in mandatory reporter legislation and the second panel reports the effect of an additional job category added to the list of mandatory reporters. Column 1 reports the results for the total referral rate, per 100,000 children. The coefficient on the change in mandatory reporter legislation is -102.2 and the coefficient on the change in mandatory reporters is -107.1. Neither effect size is statistically different from zero; however, I can rule out large effects and conclude that total referrals do not increase or decrease by more than 10 percent in response to changes in mandatory reporter legislation. One potential explanation of this result is that changes in referrals screened-out and referrals screened-in (reports) might move in opposite directions. In other words, mandatory reporter laws might change the composition of total referrals.

[63] New York, North Carolina, and Pennsylvania are excluded because they do not have data for these outcomes.

Table 8: Regression Results from Annual Analysis

	(1) Total referrals (per 100,000 children)	(2) Referrals screened out (per 100,000 children)	(3) Reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)
Panel 1				
Number of mandatory reporter policy changes	-102.2 (147.6)	-238.3 (244.6)	136.1 (178.9)	228.7 (300.5)
Mean of dep. var.	4,419	1,821	2,598	4,364
Observations	589	589	589	589
Adjusted R^2	0.769	0.684	0.547	0.547
Panel 2				
Number of jobs listed as mandatory reporters	-107.1 (182.4)	-236.4 (173.4)	129.3 (116.3)	217.3 (195.3)
Mean of dep. var.	4,419	1,821	2,598	4,364
Observations	589	589	589	589
Adjusted R^2	0.770	0.689	0.553	0.553

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. In the first panel, the independent variable is the number of changes in the mandatory reporter legislation, and in the second panel, the independent variable is the number of jobs listed as mandatory reporters. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year fixed effects, and a state-specific linear time trend. New York, North Carolina, and Pennsylvania are excluded because they do not have data for these outcomes.

Column 2 reports the results for screened-out referrals and column 3 reports the results for screened-in referrals (reports). Regardless of the independent variable, change in legislation or number of jobs, the coefficients on referrals screened-out are negative, and the coefficients on reports are positive. However, none of these coefficients are statistically significant. Lastly, I investigate whether the number of children investigated for maltreatment responds to mandatory reporter legislation (column 4). While the coefficients are positive, they are indistinguishable from zero.

The imprecise estimates from the annual analysis persist even when I separately add a quadratic time trend and state controls for demographic composition and economic conditions. They also persist when I construct a balanced sample of the 26 states that appear in the data all years from 2004 to 2017.[64] Two reasons for these imprecise estimates may be lack of power or vague timing of legislation changes. For example, a legislation change in January probably has a different impact on reporting the following year compared to a legislation change in November. To address these concerns, I use the NCANDS child file and estimate the monthly effect.

2.5.2. Monthly Results

Table 9 reports the results of the monthly analysis. Although there are more than 12 times the number of observations and more precisely defined treatment periods compared to the annual analysis, the effect of changes in mandatory reporter legislation on reporting is similar. The effect size on the number of reports (column 1) is marginally significant. All else equal, a change in mandatory reporting legislation is associated with an increase of 12.9 reports per 100,000 children (or 5.5 percent). Similarly, adding a job to the list of mandatory reporters increases the report rate by 9.34 reports per 100,000 children (or 4 percent). This impact is driven by an increase in unsubstantiated reports, and not substantiated reports. All else equal, a change in mandatory reporting legislation leads to a 6 percent increase in the unsubstantiated report rate (column 3). The remaining results, substantiated rate (column 2), children investigated (column 4), and victim rate (column 5) are statistically insignificant. However, I can rule out effects sizes larger than 16 percent. In other words, changes in mandatory reporting legislation will not impact

[64] These results are available upon request.

substantiation rates by more than 16 percent. These results persist when state-level controls are added and different combinations of fixed effects are included.[65]

Following the recent difference-in-differences discussion, treatment effects likely differ over time and by intensity (de Chaisemartin & D'Haultfoeuille, 2019; Goodman-Bacon, 2019; Callaway & Sant'Anna, 2020). One way to understand how the impact may differ by time and intensity is to estimate the group-time average treatment effect (Callaway & Sant'Anna, 2020). Similar to Callaway and Sant'Anna (2020), I estimate differential effects by treatment group. In this setup, states are grouped by the number of mandatory legislation changes they experienced from 2004 to 2017. There are nine states with one legislation change, eight with two changes, five with three changes, four with four changes, and one with six changes.[66] The overall average effect should be the weighted average of the group effects. The results from this exercise are provided in Appendix Table D1. The key takeaway from this exercise is that the states with one, four, and six changes are driving the results, depending on the outcome. In other words, there are differential, non-linear effects by the number of policy changes. This result, in combination with Figures 3 and 4, motivates estimating the interaction effect between the policy change and number of mandatory reporters. The differences in the number of mandatory reporters across states may be able to explain some of the differential effects by policy change.

[65] Alternatively, the results are statistically significant with at least 95 percent confidence when standard errors are clustered at the month-by-state level and year-by-state level. It is good practice to be conservative and avoid bias by using bigger, more aggregated clusters (Cameron & Miller, 2015), so I only report results clustered at the state-level. Results are available upon request.

[66] Refer to Table 7 for the list of states in each group.

Table 9: Regression Results from Monthly Analysis

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
Panel 1					
Number of mandatory reporter policy changes	12.90* (7.41)	1.86 (2.36)	11.03* (5.71)	28.76 (19.06)	2.76 (4.26)
Mean of dep. var.	234.3	55.0	179.3	427.7	86.1
Observations	8,422	8,422	8,422	8,422	8,422
Adjusted R^2	0.598	0.431	0.660	0.529	0.416
Panel 2					
Number of jobs listed as mandatory reporters	9.34* (5.23)	1.35 (1.88)	7.99* (4.27)	18.67 (13.72)	1.13 (3.13)
Mean of dep. var.	234.3	55.0	179.3	427.7	86.1
Observations	8,422	8,422	8,422	8,422	8,422
Adjusted R^2	0.598	0.431	0.660	0.528	0.415

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. In the first panel, the independent variable is the number of changes in the mandatory reporter legislation, and in the second panel, the independent variable is the number of jobs listed as mandatory reporters. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend. Results are similar to including year and month fixed effects, instead of the year-by-month fixed effect. They are also invariant to including the following set of state-level time-varying controls: racial composition, age composition, education composition, living situation, poverty variables (teen birth rate, households without a vehicle, poverty rate, food insecurity), unemployment rate, safety net generosity (TANF & SNAP), governor political affiliation, income per capita, and minimum wage. These two sets of results are available upon request.

2.5.2.1. Interaction effects

Table 10 reports the results from estimating equation 2.2. All else equal, both a change in mandatory reporter legislation and an increase in the number of jobs classified as mandatory reporters leads to an increase in the report rate. States with relatively more mandatory reporters experienced a smaller increase in reporting. This effect comes from fewer unsubstantiated reports and not substantiated ones. To further investigate differential effects, I estimate the short-run effects up to five months after the first policy change.[67]

Table 10: Interaction Effect between Policy Changes and Number of Jobs Listed as Mandatory Reporters

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
Policy changes	46.47***	2.182	44.29***	109.7**	13.89
	(16.44)	(8.598)	(13.21)	(42.59)	(11.78)
Jobs	10.99**	0.946	10.04***	21.30*	1.276
	(4.333)	(2.022)	(3.509)	(11.86)	(3.281)
Policy changes x Jobs	-3.534***	-0.0933	-3.441***	-8.147***	-1.004
	(1.175)	(0.630)	(0.982)	(3.033)	(0.853)
Mean of dep. var.	234.3	55.0	179.3	427.7	86.1
Observations	8,422	8,422	8,422	8,422	8,422
Adjusted R^2	0.604	0.431	0.667	0.536	0.417

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. The coefficient for "Policy changes" is the effect when a state changes it mandatory reporter legislation, the coefficient for "Jobs" is the effect when a state adds another job to the list of mandatory reporters, and the coefficient on the interaction term, "Policy changes x Jobs," is the effect of a policy change for states with more mandatory reporters, relative to states with fewer mandatory reporters. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend.

[67] Callaway & Sant'Anna (2020), de Chaisemartin & D'Haultfoeuille (2019) and Goodman-Bacon (2019) provide additional ways to disentangle timing and heterogeneous treatment effects. Future work hopes to continue to incorporate their techniques.

2.5.2.2. Short-run effects

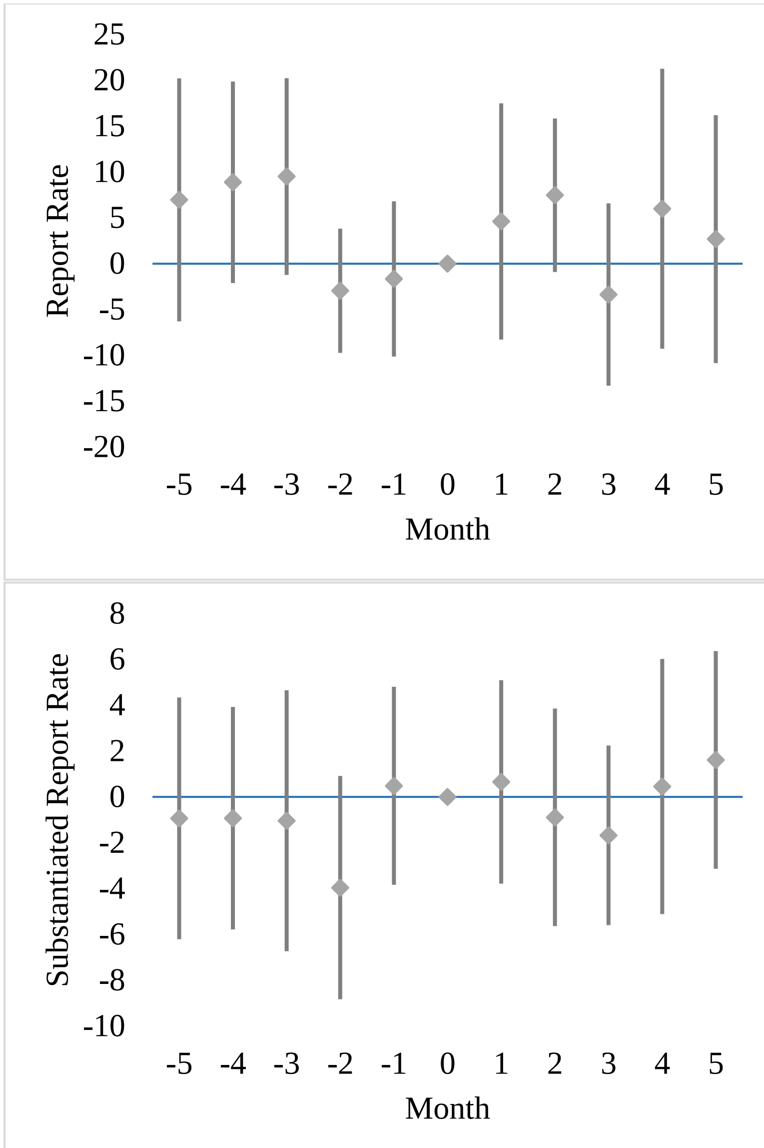
The results for the short-run analysis, estimating equation 2.3, are given by the event study figures and the difference-in-differences table. The event study analysis, Figure 5, confirms that there were no pre-trends five months prior to the change in legislation. After the change, there does not seem to be a statistically significant impact either.

The difference-in-differences results are reported in Table 11. On average, over the five months after the mandatory reporter legislation change, there are no statistically significant differences in the report rates relative to the same time period in the prior year.[68] Although the effect sizes are imprecisely estimated, I can rule out effect sizes greater than 8 percent. In other words, changes in mandatory reporter legislation will not increase or decrease child maltreatment reporting by more than 8 percent in the short-run.

To check the sensitivity of this short-run analysis, I estimate this effect for policy changes that occurred between 2012 and 2014. Most of these policy changes added university staff, camp staff, and athletic coaches. The results of this sensitivity analysis are provided in Appendix Table D2, and are similar to the results from Table 11.

[68] See Appendix Figure D3 to see these trends graphically in a handful of states after the first legislation change. Appendix Figure D3 further supports this finding.

Figure 5: Event-Study Analysis for First Legislation Change



Notes: This figure plots the coefficients from equation 2.2 where the outcome is the report rate per 100,000 children (top) or substantiated report rate per 100,000 children (bottom) at the state-by-month level. Each coefficient five months before and after the mandatory reporter legislation change is plotted with its 95 percent confidence interval. These values represent the change in the treatment year, relative to the prior year (control year). State, year, and month fixed effects are included.

Table 11: Short-Run Effects of the First Legislation Change

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
Post	-14.73*	-4.185**	-10.55*	-31.29	-6.619**
	(7.490)	(1.810)	(6.162)	(18.61)	(3.135)
Treatment	-22.18**	-3.662	-18.52**	-45.50**	-4.994
	(8.786)	(2.364)	(8.008)	(19.20)	(3.882)
Post x Treatment	0.184	1.101	-0.916	3.689	2.644
	(5.065)	(0.982)	(5.111)	(12.53)	(1.983)
Mean of dep. var.	230.0	54.0	176.0	425.6	84.1
Observations	568	568	568	568	568
Adjusted R^2	0.378	0.224	0.384	0.286	0.202

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. "Post" is equal to one five months after the legislation change and zero for the five months prior. "Treatment" is equal to one in the year of the legislation change and zero in year prior. Post x Treatment estimates the effect in the months after the legislation change, relative to the same time period the year before. The mean of the dependent variable in the post months of the control year is provided. Regressions include state, year, and month fixed effects. The 26 states included in this analysis are Alabama, Alaska, Arkansas, California, Colorado, DC, Delaware, Georgia, Illinois, Kansas, Louisiana, Maine, Nevada, New York, North Dakota, Ohio, Oklahoma, Pennsylvania, South Carolina, South Dakota, Tennessee, Vermont, Virginia, Washington, West Virginia, and Wisconsin.

2.6. Who's Reporting?

In addition, I investigate who is making these reports because they potentially impact the probability of substantiation (Wolfe, 2012; King et al., 2013). Education personnel and law enforcement report the most cases of maltreatment across all states. Table 12 reports the changes in report source as additional jobs are added to the list of mandatory reporters for all states (panel 1), the 9 states with only one change in mandatory reporter legislation (panel 2), and for the years post 2012 (panel 3). Across all analysis samples, as more professions are added to the list of mandatory reporters, the report rates by education personnel, law enforcement, medical personnel, and social services personnel are unchanged. However, the report rate from other sources increases by 3 to 10 percent, depending on the sample. The increase in reports from other sources and the increase in unsubstantiated reports from earlier supports the idea that reports by nonprofessionals have lower rates of substantiation and complements related work (e.g. Wolfe, 2012; King et al., 2013).

A supplemental analysis, Table 13, investigates differences in report rates by profession, recognizing that some people have more interaction with children than others. These differences might contribute to differences in reporting. For example, adding camp staff would likely have a smaller impact than adding coaches because camp staff interact with children less often than coaches. Including the job classifications that had at least two changes over 2004 to 2017 demonstrates that camp staff do have a smaller impact than coaches, although neither impact is statistically significant. In fact, only computer technicians contribute to a statistically significant change in reports. Interestingly, computer technicians contribute to a decline in reports. Although not statistically significant, adding clergy and coaches has the biggest impact of a 10 to 16 percent increase in reports, whereas adding camp staff and university staff has the smallest impact of a 2.5 to 3.2 percent decline. For comparison, Baron et al. (2020) and Cabrera-Hernandez and Padilla-Romo (2020) find that reporting declined by 21 to 30 percent during the COVID-19 pandemic as a result of reduced interactions between children and teachers. In addition, Palusci et al. (2016) find that counties in states that added clergy to the list of mandatory reporters between 2000 and 2010 had significantly more reports, but fewer substantiated reports; however, they do not provide percent changes.

Table 12: Source of Reports

	(1) Education personnel and day care providers	(2) Legal, law enforcement, or criminal justice	(3) Medical personnel	(4) Social services personnel	(5) Other source
Panel 1: All States					
Number of jobs listed as mandatory reporters	0.215 (1.524)	1.519 (1.141)	-0.0295 (0.512)	0.956 (1.278)	6.674** (2.927)
Mean of dep. var.	44.3	42.5	21.7	27.7	98.4
Observations	8,377	8,377	8,377	8,377	8,377
Adjusted R^2	0.715	0.630	0.561	0.431	0.536
Panel 2: States with no or one change					
Number of jobs listed as mandatory reporters	-1.040 (1.573)	0.282 (1.702)	-0.605 (1.642)	0.286 (0.639)	10.97*** (3.571)
Mean of dep. var. before change in control states	42.6	48.5	23.7	27.3	108.6
Observations	5,353	5,353	5,353	5,353	5,353
Adjusted R^2	0.725	0.631	0.564	0.494	0.581
Panel 3: Post 2012					
Number of jobs listed as mandatory reporters	-0.603 (0.681)	-0.125 (0.354)	-0.430 (0.360)	-0.981 (1.017)	3.720* (2.099)
Mean of dep. var. before change in control states	42.7	47.4	23.6	25.6	111.2
Observations	3,672	3,672	3,672	3,672	3,672
Adjusted R^2	0.701	0.523	0.499	0.355	0.584

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a different report source rate (per 100,000 children). The first panel includes all states. In the second panel, the analysis sample is limited to the 9 states with just one mandatory reporter change between 2004 to 2017 and the 27 states with no changes. The third panel is for the years between 2012 and 2017. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend.

Table 13: Effect of Specific Jobs

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
computer technicians	-22.81*	-6.927	-15.88*	-65.67**	-16.31
	(12.55)	(5.669)	(8.172)	(31.80)	(9.758)
probation and parole officers	19.12	15.41	3.708	39.70	19.07
	(23.48)	(10.38)	(14.13)	(51.25)	(17.36)
camp staff	7.578	8.633	-1.055	-1.534	8.215
	(15.28)	(5.842)	(12.73)	(30.34)	(9.161)
animal control and humane officers	15.84	-6.629	22.47	47.33	-4.505
	(28.17)	(11.77)	(17.21)	(68.40)	(20.58)
CASAs and child advocates	-10.21	-4.437	-5.777	-23.29	-6.594
	(10.21)	(2.778)	(7.947)	(24.24)	(5.415)
clergy members	37.47	11.70	25.77	82.08	0.522
	(27.91)	(8.783)	(30.75)	(65.31)	(6.701)
college staff	-6.040	-8.708	2.668	-23.00	-12.81
	(18.79)	(7.370)	(13.47)	(41.60)	(11.88)
emergency medical	18.28	-0.289	18.57	22.02	-0.672
	(11.43)	(2.668)	(11.68)	(21.53)	(4.643)
coaches or employees of rec sports	23.83	8.522	15.31	73.87	15.70
	(17.65)	(8.433)	(11.75)	(44.60)	(14.83)
Mean of dep. var.	234.3	55.0	179.3	427.7	86.1
Observations	8,422	8,422	8,422	8,422	8,422
Adjusted R^2	0.601	0.443	0.662	0.535	0.601

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend.

This analysis provides a starting point in understanding how reports vary by profession. Future research should continue to investigate why certain professions were added to the list to mandatory reporters and try to better understand the role these professions play in detecting child maltreatment.[69] Understanding which professions can correctly detect maltreatment could inform states' decisions on who to add to the list of mandatory reporters and how to prepare and regulate training.

2.7. Conclusion

The Penn State scandal, USA Gymnastics scandal, and COVID-19 pandemic have highlighted the vital role mandatory reporters play in protecting the wellbeing of children. Mandatory reporters have a legal obligation to report suspected maltreatment. Since child maltreatment is believed to be underreported, laws that increase the number of professions required to report may be an easy way for policymakers to approach the true amount of child maltreatment. Alternatively, competing forces, unintended consequences, or weak salience could render these policies ineffective.

In response to a mandatory reporter legislation change, there are numerous ways reporters, perpetrators, and CPS may respond. Reporters may be more or less likely to report child maltreatment depending on the strength of the knowledge and bystander effect, perpetrators may be deterred, and CPS may modify their intake and investigation processes and allocate resources differently. An important limitation of this work is the inability to disentangle each of

[69] There has been considerable effort in understanding the role teachers, pediatricians, clergy, and police play in detecting child maltreatment (e.g. Baron et al., 2020; Cabrera-Hernandez & Padilla-Romo, 2020; Fitzpatrick et al., 2020; Warner & Hansen, 1994; Palusci et al., 2016; Edwards, 2019), but there is no information, to my knowledge, about the role of coaches, camp staff, computer technicians, university staff, humane officers, and other professions that have recently been added to the list of mandatory reporters.

these mechanisms. Future research may want to concentrate on disentangling these mechanisms as their competing forces might attenuate the overall effectiveness of mandatory reporter laws, and each mechanism implies different policy responses.

One potential unintended consequence of a mandatory reporter legislation change might be failing to identify and support the children with the greatest need for intervention (Raz, 2017). Determining whether changes in referrals are beneficial or burdensome depends on the composition of referrals screened-out and in. In the annual-level analysis, I find that after a mandatory reporter legislation change fewer referrals are screened-out and more referrals are screened-in and investigated. This finding suggests that mandatory reporter legislation may help identify cases of maltreatment. However, none of these coefficients are statistically significant, so this is speculative at best.

In the monthly-level analysis, I find that increasing the number of mandatory reporters leads to a 4 percent increase in reporting. This effect size is smaller than the impact of a one-dollar increase in the minimum wage and a one percentage point change in the unemployment rate.[70] When there is an increase in reporting, CPS may have to modify their operations (e.g., changing the amount of time spent on each investigation, employing additional staff, differential investigation process, etc.). It is unclear which modification dominates or is more beneficial, but a 4 percent increase in reports is modest and may not require substantial changes to CPS' operations.

Of the reports screened-in for investigation, it is important to understand how the composition of substantiated and unsubstantiated reports changes. Increased reporting may result

[70] A one-dollar increase in the minimum wage reduced neglect by 9.6 percent (Raissian & Bullinger, 2017), and a one percentage point increase in the unemployment rate increased overall abuse by 10 percent (Brown & De Cao, 2020).

in better identification of child maltreatment or increased workload for the investigation staff. Better identification would be indicated by relatively more substantiated reports, whereas increased workload would be indicated by relatively more unsubstantiated reports. Regardless of whether the investigation process remains the same or responds to an increase in reports, more unsubstantiated reports are indicative of an increased workload for the child welfare staff. For example, if staff have more reports to investigate and remain as diligent as before, then the increase in unsubstantiated reports is an indicator of increased workload. The staff have to investigate more, potentially less-severe, reports of maltreatment. Alternatively, if the investigation process remains the same and staff have more reports to investigate, then they may do so less diligently, thus not substantiating as many reports. In this scenario, the increase in unsubstantiated reports is a consequence of the increased workload. I find that the increase in reporting is entirely driven by unsubstantiated reports. While an increase in unsubstantiated reports is indicative of increased workload for the child welfare staff, it is unclear whether an unsubstantiated finding is helpful or harmful to the children and families involved. In 2019, almost one-third of the children involved in unsubstantiated reports received follow-up support and services (ACF, 2021). Alternatively, involvement in the child welfare system can have detrimental effects, especially for low-income and minority families (Fong, 2020; Merritt, 2020).[71]

Recognizing the potential for differential effects by treatment and timing, I interact legislation changes with the number of mandatory reporters already listed and conduct a short-run analysis. I find that an additional legislation change leads to a smaller increase in reporting

[71] See Wilson et al. (2020) for a detailed qualitative review of children's experiences with CPS. Experiences vary from being traumatized by the investigation process to appreciating the material support provided by CPS.

for states with more mandatory reporters, relative to states with fewer mandatory reporters. This finding suggests that mandatory reporter legislation may have diminishing marginal returns. In the short-run, I find that child maltreatment reporting is somewhat unresponsive to mandatory reporter legislation changes. I can rule out increases and decreases greater than 8 percent. In other words, I do not find any evidence of a publicity effect. This finding is unsurprising as it appears that many of the mandatory reporter legislation changes occurred with little publicity. A policy implication of these results relating to saliency could be to add more training that addresses both how to detect maltreatment and explains the responsibilities of reporters. Future work should investigate how changes in mandatory reporters interacts with training requirements and punishments for failing to report.

In summary, I use the NCANDS agency and child files to examine how screened-out referrals, reports, and substantiated and unsubstantiated reports responded to changes in mandatory reporter laws from 2004 to 2017. It is important to note that the results of this paper are strictly related to reporting and cannot make inferences about welfare effects. Without tracking children's long-run outcomes, it is unclear whether more reporting guarantees improved child wellbeing. Another limitation of this study is that the NCANDS data only reports intrafamily child maltreatment. However, mandatory reporters are responsible for reporting child maltreatment that occurs both within and outside the family. While I do not find evidence of coaches, college staff, and other recently added professions impacting child maltreatment within a family, that does not mean that these professions have not impacted child maltreatment from perpetrators outside the family. Nonetheless, understanding the nature and reporting of intrafamily child maltreatment is important for the entire child welfare system because maltreatment makes up the demand for child protective services, including family preservation and foster care placements. Presently, simply adding professions to an arbitrary list of mandatory reporters has done little to improve child maltreatment detection and reporting. In a system already overburdened with cases and limited funds (Giammarise, 2017; Raz, 2017; CBS News, 2019), thoughtful consideration pertaining to who should be included on the mandatory reporter list coupled with training and incentives may be a relatively cheap way to more effectively detect child maltreatment and manage child welfare.

Chapter 3: Underreporting Child Maltreatment during the Pandemic: Evidence from Colorado

3.1. Introduction

At the beginning of the COVID-19 pandemic, headlines across the United States read "Child abuse hotline calls are down during COVID-19, but abuse fears are up" and "More than 60% drop in calls to child abuse hotline spark safety concerns" (Callahan & Mink, 2020; Quander, 2020). State agencies across the country were reporting that child abuse and neglect reports dropped drastically, but they cautioned that the decline was not necessarily a function of reduced maltreatment, and instead a function of reduced reporting.[72] Another headline read, "Advocates express concerns about children falling through the cracks" (WCTV, 2020). In Colorado, I find that reporting decreased by 15 percent in 2020 relative to 2019. The biggest drop in reporting, of 31 percent, occurred between April and June, but reporting remained 14 to 18 percent below 2019 levels for the remainder of the year.

Over the past year, child maltreatment research has shown that overall fewer allegations of maltreatment were reported than expected in March and April (Baron et al., 2020; Rapoport et al., 2020; Weiner et al., 2020), school closures drastically reduced the number of cases detected (Baron et al., 2020, Cabrera-Hernández & Padilla-Romo, 2020), and stay-at-home orders increased the incidence of neglect (Bullinger et al., 2020). All of these studies use different methods and data, yet come to the same conclusion in line with the concerns expressed by news articles: potential victims of child abuse and neglect are going unnoticed. Baron and colleagues (2020) use real-time data from Florida to estimate that a total of 212,500 allegations across the

[72] This concern is not unique to the United States. Headlines in Canada read "Child protection reports on P.E.I. climb despite fewer eyes amid COVID."

US, 40,000 of which would have been substantiated, went unreported in March and April of 2020 as a result of school closures. In this paper, I use real-time data from Colorado to provide an updated national estimate on the number of unreported allegations and victims for the entire year. I find that millions of allegations may have gone unreported, potentially impacting over 100,000 victims during the year. In addition, I estimate how child maltreatment incidences and reporting changed as a result of the COVID-19 pandemic, school closures, and the stay-at-home order. Not surprising, all three events concurrently resulted in the largest decline in reporting, followed by the pandemic-induced school closures.

There are three main contributions of this paper. First, this paper uses real-time child maltreatment data and a model, similar to Baron et al. (2020), to predict the number of calls that would have been made to the Child Protective Services' (CPS) hotline in 2020 had the pandemic not occurred. The counterfactual number of child maltreatment referrals is calculated two ways. The simplest way is by assuming that referrals would have followed a similar pattern in 2020 as previous years. Alternatively, the pandemic limited interactions between children and mandatory reporters through school closings and stay-at-home orders, and increased child maltreatment risk factors, such as unemployment, parental burnout, and adverse coping mechanisms, like alcohol abuse.[73] For this reason, a second counterfactual is estimated taking into account the rise in unemployment and alcohol consumption. Comparing the two counterfactuals and the observed number of referrals sheds insight onto the two mechanisms in which the pandemic impacted child maltreatment. The first counterfactual underscores the importance of mandatory reporters.

[73] Brown & De Cao (2020) find that unemployment is positively correlated with child maltreatment, Griffith (2020) explains how limited availability of social supports and child care can lead to parental burnout, which in turn can result in neglect and abuse (Mikolajczak et al., 2019), and the WHO published a brief explaining the links between alcohol abuse and neglect (WHO, nd).

The second counterfactual demonstrates how economic hardships and coping mechanisms brought about by the pandemic contribute to child maltreatment.

As a second contribution, this paper uses the timing differences between the COVID-19 national emergency, school closures, and stay-at-home order to determine the impact that each of these events had on the decline in child maltreatment referrals for the full year. Prior research has looked at either school closures or stay-at-home orders in isolation and only examined the effect from March to May (e.g. Baron et al, 2020; Bullinger et al., 2020). This is the first study to provide the impacts of all three events for the full year. To differentiate these three impacts, separate regression equations are estimated with an independent variable equal to the proportion of the quarter in which the event happened. I find evidence that the largest decline in reporting came when the three events were happening concurrently. Alternatively, the pandemic, without school closures and stay-at-home orders, had the smallest impact on underreporting. Understanding the difference in reported maltreatment as a result of the pandemic and policy responses to curb the spread of the virus helps quantify the effectiveness of these policy responses. In addition, understanding the difference in reported maltreatment as a result of the pandemic and school closures contributes to the emerging body of literature emphasizing teachers' roles in detecting child maltreatment (Fitzpatrick et al., 2020; Cabrera-Hernandez & Padilla-Romo, 2020).

The last contribution of this paper is to identify the impact of the pandemic and pandemic-induced policies on the *type* of child maltreatment reported and substantiated. Child welfare experts observed an increase in serious abuse (Hofmann, 2021), and doctors claimed the severity of the abuse they saw in the ER at the start of the pandemic was much worse (Schmidt & Natanson, 2020). In Colorado, I do not find evidence of these claims. Overall, the proportion of neglect, physical abuse, and sexual abuse allegations is unchanged from 2019 to 2020. In addition, I use economic and seasonal trends to predict the type of maltreatment that might be going unreported. Based on this approach, victims of neglect are most likely being missed. In order to better prepare and target interventions, it is important to understand the type of maltreatment occurring and being underreported as a result of the COVID-19 pandemic.

3.2. The COVID-19 Pandemic and Child Maltreatment in Colorado

The COVID-19 pandemic national emergency was announced on March 13, 2020 and continued through the end of the year. To curb the spread of the virus in the early months, schools halted in-person learning, stay-at-home orders were issued, and non-essential employees worked from home. The unemployment rate rose to an all-time high of 14.8 percent in April 2020 and remained above 6 percent for the remainder of the year (Trading Economics, nd). Frequency of alcohol consumption increased by 14 percent (Pollard et al., 2020), domestic violence calls increased by 7.5 percent (Leslie & Wilson, 2020), people's mental health deteriorated (Brodeur et al., 2020), and parental burnout probably increased (Griffith, 2020). Despite these hardships and risk factors of child maltreatment, hotline calls to state agencies plummeted (Schmidt & Natanson, 2020), raising concerns that abuse and neglect are going unreported (MacFarlane et al., 2020).[74] Research suggests that these drops in reporting came from the pandemic-induced school closures which limited interactions with mandatory reports (Baron et al., 2020, Cabrera-Hernández & Padilla-Romo, 2020).

Colorado is no exception to the situation described above. School closures began at the end of March and continued until September. Compared to non-pandemic years, children were

[74] Alternatively, Ortiz et al. (2021) find that the volume of text messages to Childhelp, the only national hotline providing counseling services with a focus on child abuse and neglect, increased in 2020 compared to 2019.

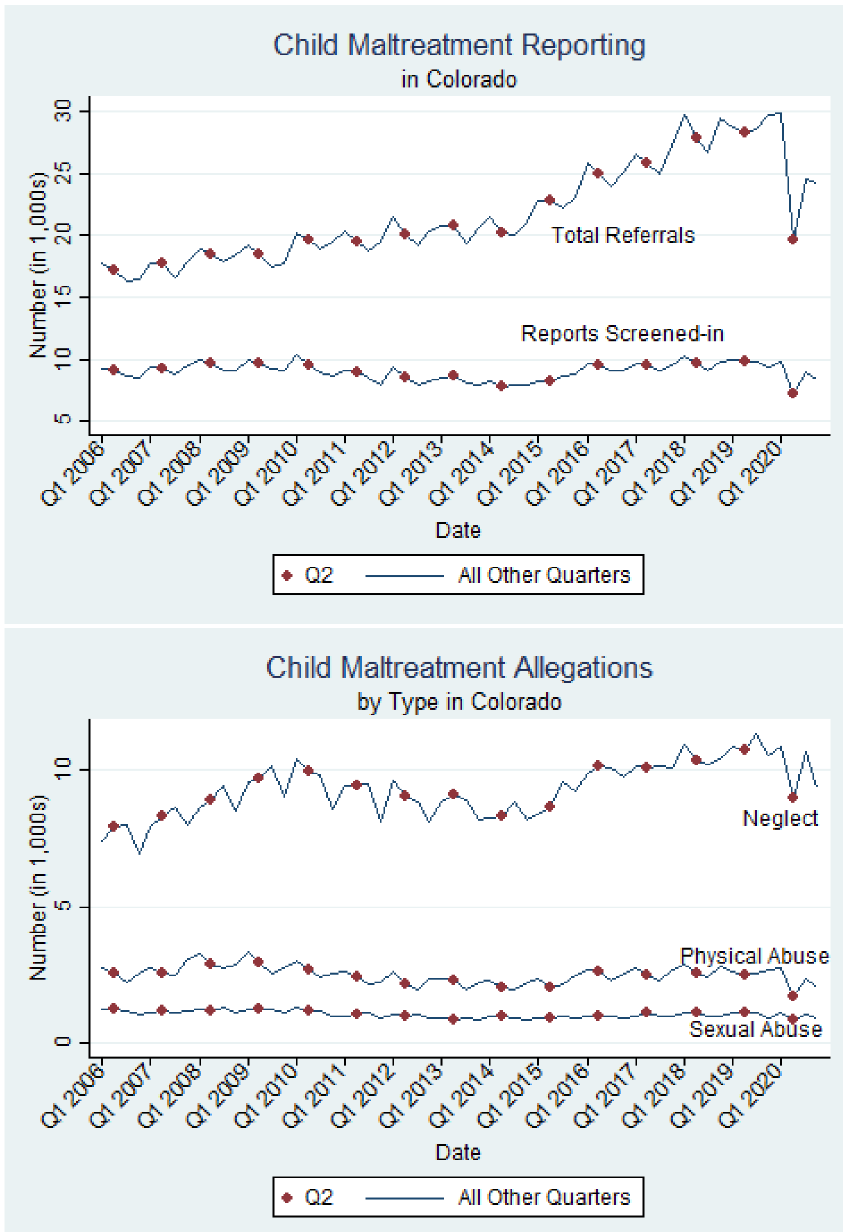
out of school for three additional months, but they continued to have access to school meals (Grewe, 2020). In addition, Colorado issued a stay-at-home order from March 26 to April 26. Colorado permitted going outside during the stay-at-home order as long as social distancing was followed. In fact, the public health order specifically listed walking, hiking, skiing, snowshoeing, biking, and running as acceptable activities (Grewe, 2020). People could also go to the grocery store, liquor store, convenience store, cannabis store, banks, and pharmacies (Grewe, 2020). The unemployment rate fluctuated from 6.8 to 11 between April to December and alcohol sales increased by 8 percent.[75]

Figure 6 shows the number of child maltreatment referrals received and screened-in for investigation from 2006 through 2020 in the state. From 2006 to 2020, there was a steady incline, with a steeper incline following the introduction of the statewide hotline in 2015. Between April and June of 2020, the number of total referrals and reports screened-in[76] dropped by 31 and 26 percent, respectively, relative to the same time period in 2019. In the remaining months of the year, child maltreatment reporting rebounded somewhat, but referrals and reports still remained below pre-pandemic levels. The bottom graph of Figure 6 shows the number of allegations reported by maltreatment type, indicating the biggest drop and rebound in neglect allegations, and a small uptick in sexual abuse allegations. According to the Colorado Department of Human Resources, calls from education and medical personnel decreased by 30

[75] Author's calculations based on unemployment data from the BLS and alcohol sales data from NIAAA. These data sources are described in the next section. Additionally, the Liquor Excise Tax Reports show a similar increase and be accessed through Colorado's Department of Revenue.
[76] Total referrals include both the calls to the hotline that are screened out and in. Referrals that are screened-in are also referred to as reports. There is no additional follow-up for referrals that are screened-out, but reports are investigated for child maltreatment.

and 11 percent, respectively; however, calls from friends and family increased by 5 percent (CDHS, 2021).

Figure 6: Child Maltreatment Referrals in Colorado from 2006 to 2020



Notes: This figure shows the trend in child maltreatment reporting from 2006 to 2020. The top graph shows the total number of child maltreatment referrals (in thousands) reported to child welfare agencies in the state as well as the number of reports screened-in (in thousands). The bottom graph shows the number of allegations by maltreatment type (in thousands). The total number of maltreatment allegations in the bottom graph is less than the total referrals because not all referrals are screened-in and investigated. In addition, the total number of maltreatment allegations is greater than the screened-in reports because a report can be assigned multiple maltreatment types.

Referrals of child maltreatment are a function of actual incidences and reporting. The current pandemic, which increased economic hardship while reducing contact with mandatory reporters, poses a particularly unique challenge for child welfare agencies to detect child maltreatment. These two opposing forces will attenuate the impact of the pandemic on child maltreatment reporting towards zero. Alternatively, school closures and the stay-at-home order limited interactions with mandatory reporters. These two events should be driving the decline in reporting, and might explain some of the rebound in reporting after they ended.

Since the stay-at-home order limited all potential interactions with mandatory reporters and occurred concurrently with the first month of school closures, we should see this event driving the decline experienced between April and June. While the stay-at-home order limited interactions, it also may have increased household stress and parental burnout, potentially more so for those who complied. Parental burnout can manifest into neglect (Mikolajczak et al., 2019). As a result, we expect to see increases in child maltreatment for counties with higher compliance relative to counties with worse compliance. This behavior might also be able to explain the uptick in neglect and sexual abuse.

3.3. Data

The data for this study come from multiple public sources. The child maltreatment data come from Colorado's Department of Human Services (CDHS), which provides real-time quarterly counts of calls made to the child abuse and neglect hotline for each county in Colorado starting in 2006. For this time-sensitive project, the CDHS data are preferred over the National Child Abuse and Neglect Data System (NCANDS) because the national level data have a two-year time lag and only provide screened-in reports in counties with more than 1,000 records, whereas the real-time CDHS data provide the total number of hotline calls at the county level. Another advantage of the CDHS data, relative to other states' real-time data, is that they provide the type of alleged maltreatment and the finding of the allegation. These data are used to test the hypothesis that child welfare experts have posited; more severe cases of abuse will result from the pandemic. Prior research in Florida and New York did not estimate the real-time composition of child maltreatment reports (Baron et al., 2020; Rapaport et al., 2020), and research in Indiana found an increase in neglect, not physical abuse (Bullinger et al., 2020).

One drawback of these data, is that the analysis is limited to a single state. The extent to which these results can be generalized to the entire country is questionable. Colorado had one of the highest child maltreatment referral rates of 85.2 referrals per 1,000 children in 2019 (ACF, 2021). The average referral rate for states across the country was 59.5 (ACF, 2021). In addition, Colorado screened out more referrals than the average state. Colorado screened out 66.4 percent of their referrals in 2019, whereas the average screen-out rate was 40.7 percent (ACF, 2021). Of the calls that were screened-in, about 34 percent were substantiated in Colorado, compared to an average of 29 percent across the country (ACF, 2021). Finally, the most common types of maltreatment in both the US and Colorado are neglect, abuse, and sexual abuse (ACF, 2021); however, neglect is relatively higher and physical abuse is relatively lower in Colorado compared to the typical state. While Colorado may not be representative of the typical state in the US, these results are essential to provide more evidence of the impacts of the pandemic and pandemic-induced policies.

The remaining data come from multiple sources and are used to supplement the main analyses. First, I use employment and population data from the Bureau of Labor Statistics (BLS) and US Census to control for changes in economic conditions. The BLS provides county and state-level unemployment rates and employment counts, quarterly from 2006 through 2020, and the Census provides the county population size, annually from 2008 to 2019. To estimate the 2020 population numbers, I use the 3-year average percent change in each county.[77] The population size and employment counts are used to determine the employment to population ratio. In addition, I use the population to determine county-level child maltreatment rates. Next, I use alcohol sales data from the "Surveillance Report #115" and "Alcohol Sales during the COVID-19 Pandemic" files, maintained by National Institute on Alcohol Abuse and Alcoholism,[78] to proxy for alcohol consumption at the state-level. This estimate, in combination with the unemployment rate, is used to create a second counterfactual maltreatment number that accounts for an economic hardship and potential coping strategy. Finally, to proxy for stay-at-home order compliance, I obtain county-level data on COVID-19 cases and deaths from Colorado's Outbreak Data, maintained by Colorado's Department of Public Health and Environment (CDPHE).[79] These data are updated weekly and available online for transparency and evidenced-based decision-making, but may not be comparable across counties over time and should not be used to associate exposure risk with certain settings (CDPHE, nd). I use these data from the beginning of the pandemic (March 14, 2020 to May 10, 2020) to observe how caseloads changed *within* a county prior to and during the stay-at-home order to get an idea of stay-at-home order compliance. While these four additional sources of data do not control for all potential confounders, they enrich analyses that solely rely on seasonal and longitudinal trends.

[77] More specifically, I first calculated the percent changes in population size from 2016 to 2017, 2017 to 2018, and 2018 to 2019. Then, I calculated the 3-year average and used this average to estimate the 2020 population size.
[78] These data can be found here.
[79] These data were downloaded March 24, 2021 from https://covid19.colorado.gov/covid19-outbreak-data.

Table 14 provides statewide differences in child maltreatment reporting, economic conditions, and alcohol sales for each quarter between 2019 and 2020 in Colorado. In addition, percent changes are provided. Overall, child maltreatment reporting declined by 15 percent, with the biggest decline of 31 percent occurring between April and June. The proportion of screened-in and substantiated reports remained similar between 2019 and 2020. Table 15 provides summary statistics of child maltreatment reporting and economic conditions for all 64 counties over the 4 quarters and 13 years. The average number of referrals received in a county during a given quarter between the years 2008 and 2020 is 18, per 1,000 children. I also provide the 2019 and 2020 averages and a p-value indicating if they are statistically different from each other.

Table 14: State-Level Differences in Child Maltreatment Reporting and Economic Conditions between 2019 and 2020

Year	2019					2020				
Quarter	Jan - Mar	Apr - Jun	Jul - Sep	Oct - Dec	TOTAL	Jan - Mar	Apr - Jun	Jul - Sep	Oct - Dec	TOTAL
Child Maltreatment Variables										
Total Referrals Received	28626	28281	28549	29722	115178	29819	19577	24482	24280	98158
Screened-in	9896	9782	9850	9422	38950	9727	7190	8856	8348	34121
Screened-out	18730	18499	18699	20300	76228	20092	12387	15626	15932	64037
Total Allegations of Maltreatment	15333	15195	15688	14875	61091	15457	12020	14757	13019	55253
Substantiated	3487	3397	3649	3197	13730	3426	3014	3322	2922	12684
Unsubstantiated	11846	11798	12039	11678	47361	12031	9006	11435	10097	42569
Neglect Allegations	10865	10785	11349	10577	43576	10865	8955	10660	9386	39866
Physical Abuse Allegations	2623	2528	2548	2694	10393	2765	1693	2363	2045	8866
Sexual Abuse Allegations	1106	1122	1106	938	4272	1105	842	1091	935	3973
Substantiated Neglect	2786	2661	2940	2541	10928	2718	2462	2662	2335	10177
Substantiated Physical Abuse	289	336	310	295	1230	336	249	306	254	1145
Substantiated Sexual Abuse	311	299	310	270	1190	268	221	269	238	996
Economic Conditions										
Unemployment rate	3.10	2.80	2.63	2.50		3.40	11.00	6.83	7.07	
Employment-population ratio	66.93	67.13	67.57	67.70		66.67	59.87	62.37	63.50	
Alcohol Purchased (gallons per capita)					2.78					2.99
Percent Change between 2020 and 2019										
Child Maltreatment Variables										
Total Referrals Received	0.04	-0.31	-0.14	-0.18	-0.15					
Screened-in	-0.02	-0.26	-0.10	-0.11	-0.12					
Screened-out	0.07	-0.33	-0.16	-0.22	-0.16					
Total Allegations of Maltreatment	0.01	-0.21	-0.06	-0.12	-0.10					
Substantiated	-0.02	-0.11	-0.09	-0.09	-0.08					

Unsubstantiated	0.02	-0.24	-0.05	-0.14	-0.10
Neglect Allegations	0.00	-0.17	-0.06	-0.11	-0.09
Physical Abuse Allegations	0.05	-0.33	-0.07	-0.24	-0.15
Sexual Abuse Allegations	0.00	-0.25	-0.01	0.00	-0.07
Substantiated Neglect	-0.02	-0.07	-0.09	-0.08	-0.07
Substantiated Physical Abuse	0.16	-0.26	-0.01	-0.14	-0.07
Substantiated Sexual Abuse	-0.14	-0.26	-0.13	-0.12	-0.16
Economic Conditions					
Unemployment rate	0.10	2.93	1.59	1.83	
Employment-population ratio	0.00	-0.11	-0.08	-0.06	
Alcohol Purchased (gallons per capita)					0.08

Notes: This table reports the number of referrals, screened-in and screened-out reports, allegations (including disposition), and allegations by maltreatment type for the state of Colorado for each quarter and the full year in 2019 and 2020. In addition, some economic conditions (unemployment rate and employment-population ratio) and a measure of a coping technique (alcohol sales) is included. The bottom panel reports the percent change between 2020 and 2019 for quarter and the year for each variable.

Table 15: Summary Statistics of Child Maltreatment Reporting and Economic Conditions in Colorado

	Mean (N=3,328)	Std. Dev.	2019 Average (N=256)	2020 Average (N=256)	p-value
Child Maltreatment Variables (per 1,000 children)					
Total Referrals	18.03	9.66	24.66	20.16	0.00
Screened-in	7.97	5.00	7.83	7.03	0.05
Screened-out	10.06	7.21	16.83	13.14	0.00
Total Allegations of Maltreatment	12.81	9.23	12.53	11.70	0.27
Substantiated	3.12	3.46	2.61	2.57	0.86
Unsubstantiated	9.69	7.34	9.92	9.14	0.21
Neglect Allegations	8.45	6.73	8.72	7.80	0.09
Physical Abuse Allegations	2.37	2.04	2.20	2.08	0.48
Sexual Abuse Allegations	0.90	1.12	0.79	0.87	0.33
Substantiated Neglect	2.26	2.73	1.97	1.83	0.51
Substantiated Physical Abuse	0.36	0.67	0.25	0.25	0.85
Substantiated Sexual Abuse	0.23	0.58	0.19	0.26	0.19
Economic Conditions					
Unemployment Rate	5.46	2.82	2.86	6.41	0.00
Employment to Population Ratio	50.55	9.67	53.68	48.95	0.00
Child Population (0 to 17)	19471	39899	19680	19665	1.00

Notes: This table provides summary statistics for child maltreatment reporting and economic conditions across all counties in Colorado from 2008 to 2020. The mean and standard deviation are given for all 3,328 observations (13 years x 4 quarters x 64 counties) in columns 1 and 2. The 2019 and 2020 averages and corresponding p-value from a t-test are provided in columns 3-5. All averages for the child maltreatment variables are provided as rates, per 1,000 children, so the average of 18.03 means that in a typical quarter a county received 18.03 referrals, per 1,000 children.

3.4. Empirical Strategy

Similar to Baron et al. (2020), I first predict the counterfactual number of child maltreatment referrals, screened-in reports, and substantiated reports for the state of Colorado by estimating the following equation:

$$Y_{qy} = \beta_0 + \varphi_q + f_g(qy) + \varepsilon_{qy} \qquad (3.1)$$

Where Y is the outcome of interest (i.e. number of referrals made to the hotline, number of reports screened-in, number of substantiated reports, etc.) in Colorado during quarter q of year

y, φ_q is the quarter fixed effect included to capture seasonal trends,[80] $f_g(qy)$ is a polynomial in time of order g, and ε_{qy} is the error term. In the main specification the polynomial takes a cubic form; however, the counterfactual results are similar across alternative specifications.[81] This equation is estimated for each of the four quarters from the years 2006 to 2019. These estimates are then used to predict the outcomes for each quarter in year 2020. This approach assumes that the number of maltreatment referrals and reports would have been similar in 2020 as 2019, had the pandemic not occurred. Alternatively, the hardships and stresses brought about by the pandemic might increase child abuse and neglect. In attempt to capture the increase in maltreatment due to hardships, I estimate equation 3.1 again controlling for the unemployment rate and alcohol purchases. This approach assumes the relationships between unemployment and child maltreatment and alcohol purchases and child maltreatment are similar in 2020 and 2019. Estimating two counterfactuals based on seasonal and longitudinal trends is useful as there is no feasible control group since the announcement of the national emergency and subsequently policy responses occurred at the same time for the entire country.

After understanding the difference between the counterfactual and actual scenarios, the next step is to understand how much of these differences are driven by the pandemic, the pandemic-induced school closures, and the pandemic-induced stay-at-home order. I estimate the following equations to differentiate these three effects:

$$Y_{cqy} = \beta_0 + \beta_1 covid_{\,qy} + \varphi_q + \gamma_y + \rho_c + \varepsilon_{cqy} \qquad (3.2)$$

$$Y_{cqy} = \alpha_0 + \alpha_1 schclo_{\,qy} + \varphi_q + \gamma_y + \rho_c + \varepsilon_{cqy} \qquad (3.3)$$

[80] Quarters one and four experience the highest call volume, whereas the quarters spanning the summer experience the lowest call volumes. This can be seen in Figure 6.
[81] See Appendix Figure E1 for a comparison of the different approaches that use a linear and quadratic polynomial.

$$Y_{cqy} = \delta_0 + \delta_1 sah_{\ qy} + \varphi_q + \gamma_y + \rho_c + \varepsilon_{cqy} \qquad (3.4)$$

Where Y is the outcome of interest in county *c* during quarter q of year y, φ_q, γ_y, and ρ_c are the quarter, year, and county fixed effects, respectively, and ε_{cqy} is the error term. The independent variables of interest, *covid, schclo,* and *sah*, identify the proportion of the quarter in which the condition exists in quarter q of year y. For example, the COVID-19 pandemic national emergency was announced March 13, 2020 and continued through 2020,[82] so *covid* is assigned a value of one-sixth in quarter one in 2020, and a value of one for the remaining quarters in 2020. The national emergency forced schools to close in March and delayed openings, so *schclo* equals one-sixth in quarter one, two-thirds in quarter two, and one-sixth in quarter three during 2020. Lastly, in attempt to slow the spread of the virus, Colorado issued a stay-at-home order from March 26th to April 26th, so *sah* in equation 3.4 is assigned one-third in quarter two of year 2020 and zero otherwise.[83] Table 16 provides the dates and values defined and Figure 7 provides a graphic representation of the timing of the events.

These three effects cannot be estimated together because they are correlated with each other due to the timing of the events. For example, when the stay-at-home order is in effect, schools are closed and the pandemic exists. After the stay-at-home order is lifted, when schools are closed, the pandemic exists. The timing overlap of these events implies that $\delta_1$captures the impact of the pandemic, school closures, and stay-at-home order concurrently on child maltreatment reporting. Similarly, α_1 captures the impact of the pandemic-induced school closures, and β_1 estimates the impact of the pandemic without stay-at-home orders or school closings, like the end of 2020. This setup implies δ_1>α_1>β_1.

[82] See The White House notice on the continuation of the National Emergency found here.
[83] In all equations, standard errors are clustered at the county by quarter level. Results are similar when standard errors are clustered at the county level and available upon request.

Table 16: Timeline of Events and Independent Variable Values

Event	Dates	Independent Variable Values
COVID-19 National Emergency	March 13, 2020 – March 2021[84]	$covid_{qy} = \begin{cases} 0 \; if \; y < 2020 \\ \frac{0.5}{3} if \; y = 2020 \cap q = 1 \\ 1 \; if \; y = 2020 \; \cap q > 1 \end{cases}$
School Closures in Colorado	March 16, 2020 – August 24, 2020[85]	$schclo_{qy} = \begin{cases} \frac{0.5}{3} if \; y = 2020 \cap q = 1 \\ \frac{2}{3} if \; y = 2020 \cap q = 2 \\ \frac{0.5}{3} if \; y = 2020 \; \cap q = 3 \\ 0 \; otherwise \end{cases}$
Stay-at-home Order in Colorado	March 26, 2020 – April 26, 2020[86]	$sah_{qy} = \begin{cases} \frac{1}{3} if \; y = 2020 \cap q = 2 \\ 0 \; otherwise \end{cases}$

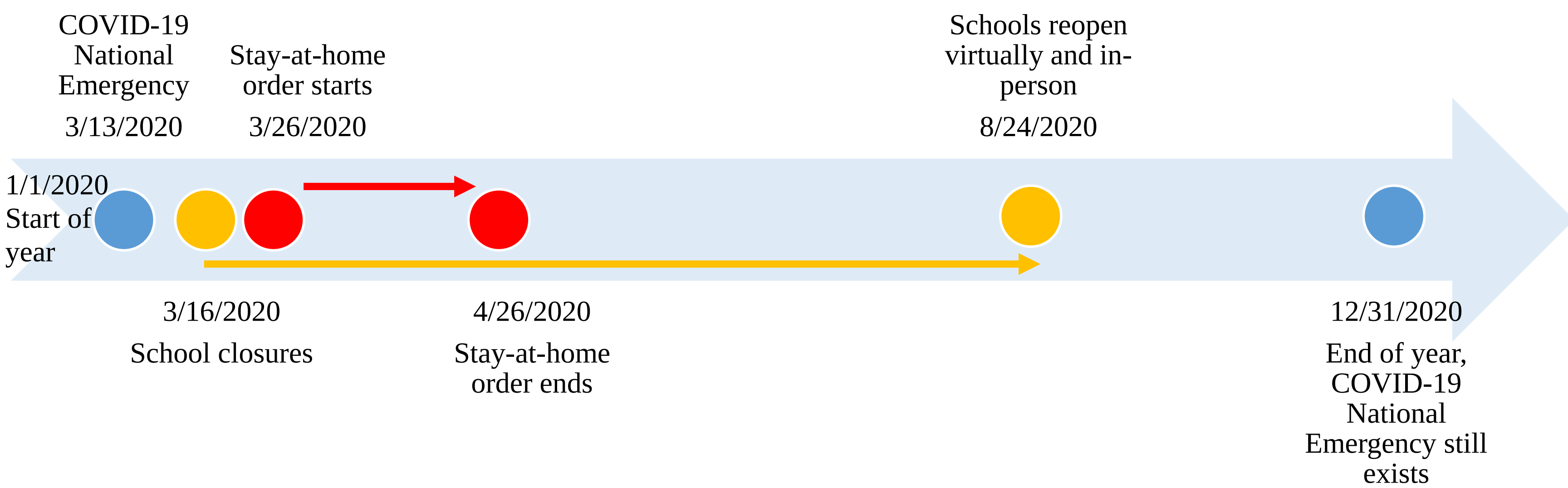
Notes: This table lists the dates of the COVID-19 national emergency, stay-at-home order, and school closures in Colorado. Using these dates, the independent variables are defined. The variable y indicates the year, and the variable q indicates the quarter. The independent variable is rounded to the nearest half month, out of 3 months. For example, the COVID-19 pandemic was announced as a national emergency March 13th, so about 0.5 months out of 3 were impacted by the pandemic. The national emergency existed for the rest of 2020, so for the remaining three quarters, three out of three months were impacted, which equals one. The stay-at-home order primarily took place in April, so in 2020 for quarter 2, *sah* equals one-third and zero otherwise. Lastly, school closure is defined based on the month impacted by the pandemic. For example, quarter two consists of April, May, and June, and in June, schools would have been closed regardless of the pandemic, so *schclo* is two-thirds, and not three-thirds (i.e. one).

[84] See The White House notice on the continuation of the National Emergency found here.

[85] Between the following two sources, https://co.chalkbeat.org/2020/3/12/21178764/the-complete-list-of-coronavirus-related-colorado-school-closures and https://www.denverpost.com/2020/07/01/colorado-schools-reopening-coronavirus-covid/, most school districts in Colorado closed on March 16, 2020 and most districts delayed opening in the fall by a few weeks, resulting in opening dates between August 24 and September 1. In-person and virtual learning varied by district, but since Colorado counties and school districts do not align, the school closure variable is the same for all counties, regardless of instructional mode and based on the date that impacts most of the state. As a robustness check, I allow the opening dates to vary for a few counties that are clearly defined, such as those in the Denver metro-area, but the results are similar.

[86] See https://www.westword.com/news/covid-19-colorado-stay-at-home-order-shorter-than-most-in-america-11682795 for a list of state closing and opening dates. Some jurisdictions, like Denver, extended their stay-at-home order, and Colorado issued a “safer-at-home” order following the stay-at-home order. See https://www.kktv.com/content/news/Gov-Polis-issues-Executive-Order-on-Safer-at-Home-569966341.html for more details. These variations are not accounted for in this analysis.

Figure 7: Timeline of Events in 2020



Notes: This figure plots the timeline of events during 2020 in Colorado. The COVID-19 National Emergency was announced on March 13, 2020 and continued through the year. Schools closed on March 16 and the stay-at-home order began March 26. The stay-at-home order ended a month later, and schools reopened for in-person and virtual learning at the end of August. The red arrow shows when the stay-at-home order happened, the yellow arrow shows when schools were closed for in-person learning, and the blue arrow shows when the pandemic existed. All three events happened concurrently from March 26 to April 26, and two of the events happened concurrently from April 26 to August 24. After August 24, only the COVID-19 national emergency was happening.

Finally, to determine differential effects of the stay-at-home order by compliance, I estimate the following equation:

$$Y_{cqy} = \delta_0 + \delta_1 sah_{\ qy} + \delta_2 cc_c + \delta_3 sah_{\ qy}\ x\ cc_c + \varphi_q + \gamma_y + \rho_c + \varepsilon_{cqy} \quad (3.5)$$

Where the majority of the terms are defined above. *cc* measures the pre-stay-at-home order COVID-19 cases per 100,000 residents for county c, so δ_2 estimates the relation between COVID-19 cases and child maltreatment reporting, and $\delta_3$estimates the interaction effect between the stay-at-home order and COVID-19 cases on child maltreatment. A positive coefficient on δ_3 means the stay-at-home order increased child maltreatment reporting for counties with higher COVID-19 cases, relative to counties with no COVID-19 cases as of March 26th.

The validity of this approach to yield causal estimates relies on two assumptions. First, counties with COVID-19 cases prior to March 26th had similar child maltreatment reporting trends as counties with no COVID-19 cases prior to March 26th. Second, counties with COVID-19 cases prior to March 26th were more compliant to the stay-at-home order. One crude proxy for compliance is the number of COVID-19 cases during the stay-at-home order. Counties with fewer cases per 100,000 residents are considered more compliant, especially if they had cases prior to the stay-at-home order. Appendix Table E1 provides summary statistics of child maltreatment reporting by COVID-19 cases, and Appendix Figure E2 plots the relationship between COVID-19 cases prior to the stay-at-home order versus during the stay-at-home order with the regression adjusted correlation coefficient. Neither of these set of results provide convincing evidence that the two assumptions are satisfied, so this analysis is exploratory.

To understand the direction of the potential bias, I estimate the relationship between compliance on pre-pandemic child maltreatment. For this analysis, compliance is measured as the ratio between COVID-19 cases prior to and during the stay-at-home order.[87] In this setup, counties with ratios greater than or equal to one are considered more compliant than counties with ratios less than one. I do not find strong evidence that compliance is correlated with child maltreatment prior to the pandemic, thus I cannot sign the potential bias.

3.5. Results

3.5.1. Counterfactual Number of Referrals and Reports

Figure 8 shows the predicted versus actual number of referrals from 2006 to 2020. In 2019, there were a total of 115,178 referrals made, 38,950 reports were screened-in, and 13,730 reports were substantiated. In contrast, in 2020, there were a total of 98,158 referrals made, 34,121 reports were screened-in, and 12,684 reports were substantiated. Counterfactual 1 shows the predicted number of referrals assuming the pandemic had not occurred, and counterfactual 2 shows the predicted number of referrals after accounting for increases in unemployment and alcohol sales, de facto consumption.

Comparing the actual number of referrals to counterfactual 1, an estimated 30,276 referrals went unreported in 2020. Alternatively, recognizing that the pandemic has brought on significant hardships, we might expect the number of children suffering from maltreatment to be even greater in 2020 relative to 2019. Comparing the actual number of referrals to counterfactual 2, an estimated 38,794 referrals went unreported in 2020.

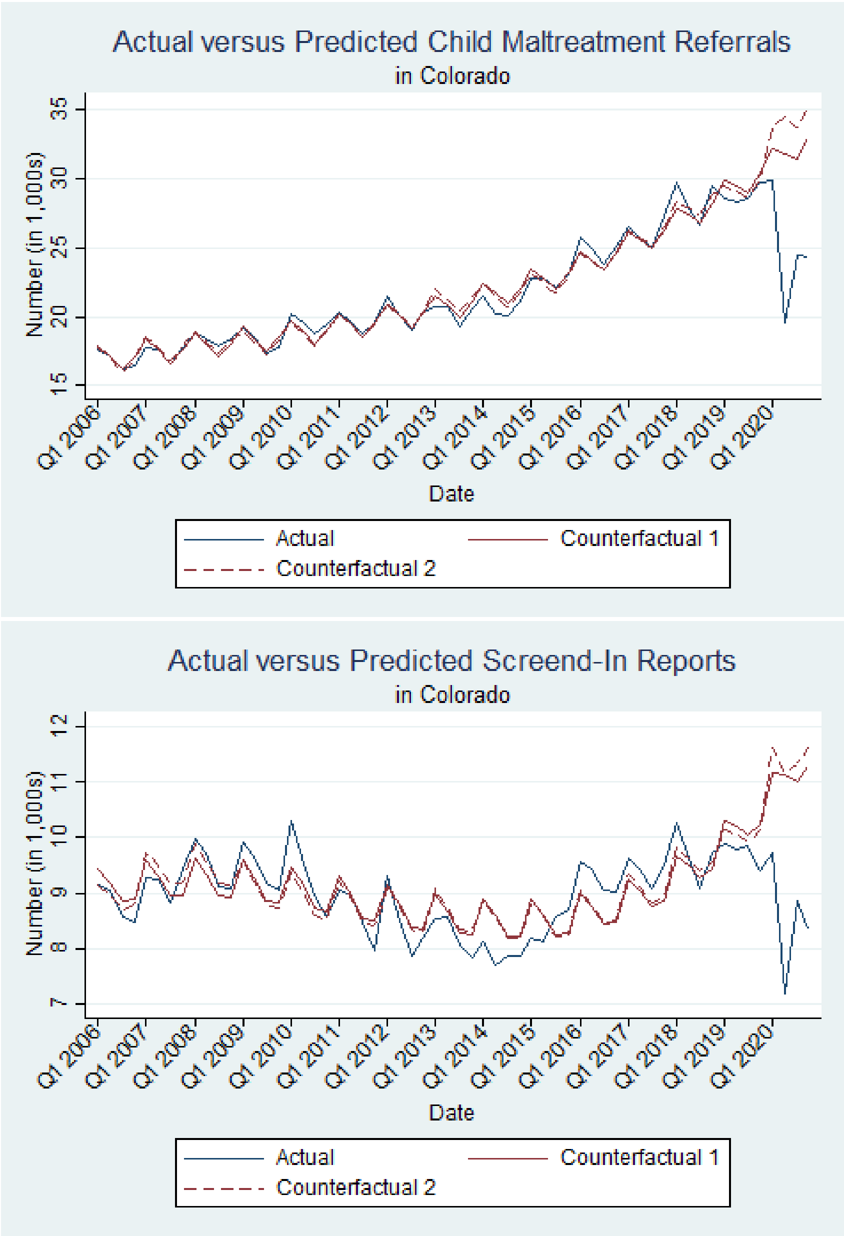
Colorado has a high referral rate, but a high proportion of referrals are screened-out and unsubstantiated. Next, I compare the actual screened-in reports to the predicted numbers of

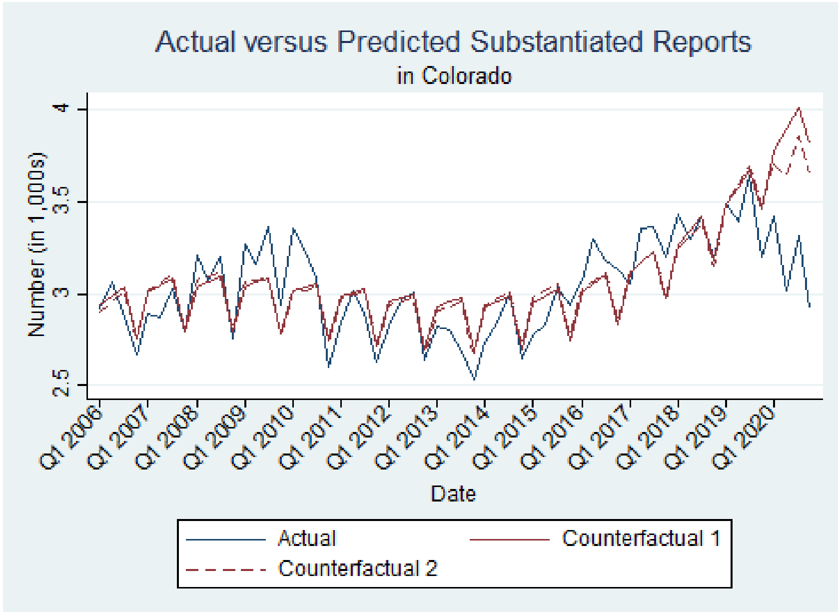
[87] The majority of counties (45) reported zero COVID-19 cases prior to and during the stay-at-home order. For these counties the ratio is set equal to one. In the remaining 19 counties, there were COVID-19 cases before and/or during the stay-at-home order. In one county, they reported zero COVID-19 cases during the stay-at-home order, resulting in an invalid ratio (i.e. zero in the denominator). For this county, I set the ratio equal to the number of cases prior to the stay-at-home order.

screened-in reports to investigate whether the screening process changed during the pandemic. If the screening process remained the same, then the proportion of the predicted screened-in reports would be the same as the proportion of the actual screened-in reports. Figure 8 also shows the predicted versus actual number of screened-in reports from 2006 to 2020. Between 10,500 to 11,500 fewer reports were screened-in in 2020 compared to counterfactual estimates. The proportion of reports that were screened-in in 2020 and the proportion of reports that should have been screened-in based on the counterfactual estimates was 33 to 35 percent, indicating the screening process remained the same during the pandemic.

Lastly, I compare the actual number of substantiated reports to the predicted number of substantiated reports to determine whether the nature of child maltreatment changed during the pandemic. Figure 8 shows the predicted versus actual number of substantiated reports from 2006 to 2020; 2,200 to 2,800 substantiated reports were missed. The different trends for each line and the uptick in actual reports in quarter 3 of year 2020 make it difficult to interpret how substantiated maltreatment has changed over the year.

Figure 8: Actual versus Predicted Child Maltreatment Referrals and Reports in Colorado from 2006 to 2020

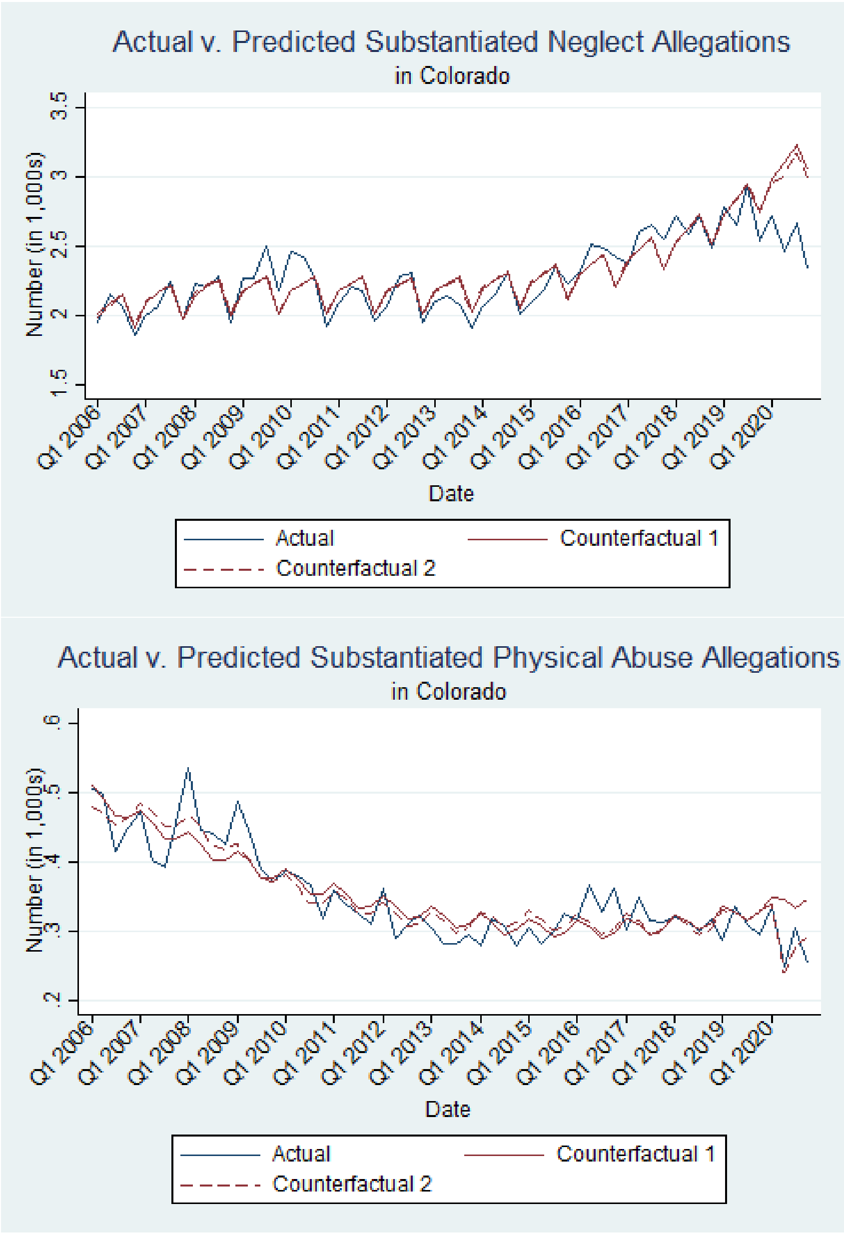


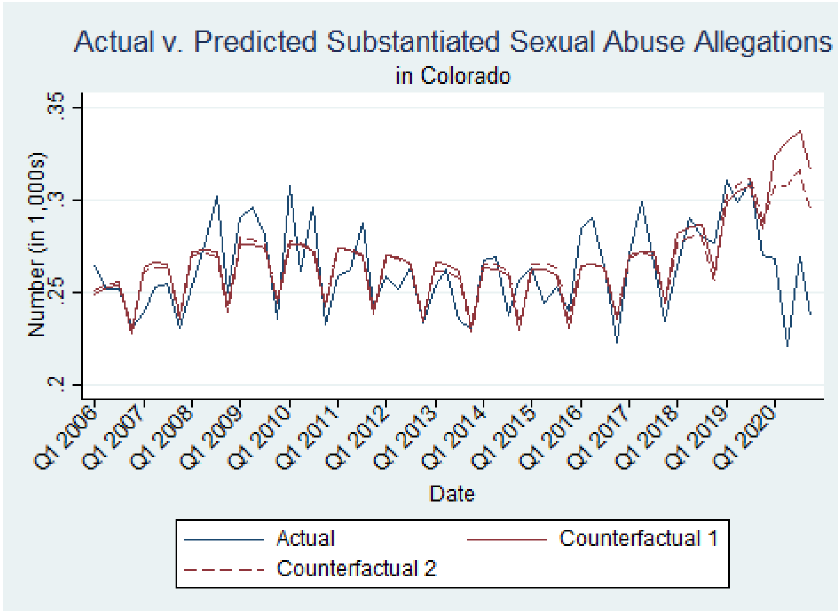


Notes: These figures plot the actual versus predicted counts of child maltreatment referrals (top graph), screened-in reports (middle graph), and substantiated reports (bottom graph). Two counterfactuals are estimated: counterfactual one assumes child maltreatment would have been the same in 2020 as 2019 had the pandemic not occurred, and counterfactual two accounts for changes in maltreatment as a result of changes in the unemployment rate and alcohol purchases.

In addition to estimating counterfactuals for the screened-in and substantiated reports, the CDHS data also allow me to estimate counterfactuals for the composition of substantiated reports by maltreatment type. Figure 9 plots the predicted versus actual number of substantiated neglect, physical abuse, and sexual abuse allegations. The majority of unreported victims seem to be suffering from neglect.

Figure 9: Actual versus Predicted Substantiated Allegations by Child Maltreatment Type in Colorado from 2006 to 2020





Notes: These figures plot the actual versus predicted counts of substantiated neglect (top graph), physical abuse (middle graph), and sexual abuse (bottom graph) allegations. Two counterfactuals are estimated: counterfactual one assumes child maltreatment would have been the same in 2020 as 2019 had the pandemic not occurred, and counterfactual two accounts for changes in maltreatment as a result of changes in the unemployment rate and alcohol purchases.

3.5.2. Impact of the COVID-19 Pandemic, School Closures, and the Stay-at-Home Order on Child Maltreatment Reporting

Table 17 provides the main results of the paper. All else equal, an additional quarter with the COVID-19 pandemic reduced the number of referrals made to the hotline by 2.5 per 1,000 children (or 10% relative to the average 2019 referral rate in a county). The screened-in report rate and substantiation rate are not statistically different as a result of the COVID-19 pandemic. All else equal, an additional quarter with pandemic-induced school closures reduced the number of referrals by 7.9 per 1,000 children (or 32% relative to the average 2019 referral rate in a county) and reports screened-in by 1.8 per 1,000 children (or 24% relative to the average 2019 report rate in a county). Finally, all else equal, an additional quarter with a stay-at-home order reduced the number of referrals by 14.8 per 1,000 children (or 60% relative to the average 2019 referral rate in a county) and reports screened-in by 3.3 per 1,000 children (or 42% relative to the average 2019 report rate in a county). Similar to the COVID-19 pandemic, neither school closings nor the stay-at home order had a statistically significant impact the substantiation rate.

Table 17: Estimated Impacts of COVID-19 Pandemic, School Closures, and Stay-at-Home Order on Child Maltreatment Reporting in Colorado

	Main Outcomes			Type of Maltreatment		
	Total Referrals (per 1,000 children)	Reports Screened-in (per 1,000 children)	Substantiated Reports (per 1,000 children)	Neglect Allegations (per 1,000 children)	Physical Abuse Allegations (per 1,000 children)	Sexual Abuse Allegations (per 1,000 children)
Ind. Var.: COVID-19	-2.541*	-0.079	-0.876	-0.445	-0.084	0.251
	(1.415)	(0.710)	(0.715)	(1.078)	(0.279)	(0.178)
Ind. Var.: School Closure	-7.874***	-1.838**	0.615	-0.961	-0.229	0.115
	(1.829)	(0.846)	(0.966)	(1.239)	(0.512)	(0.350)
Ind. Var.: Stay-at-home Order	-14.787***	-3.273**	0.628	-2.430	-0.638	0.265
	(3.070)	(1.479)	(1.764)	(2.260)	(0.909)	(0.625)
2019 Average	24.66	7.83	2.61	8.72	2.2	0.79
Observations	3,328	3,328	3,328	3,328	3,328	3,328

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the county-by-quarter level, in parentheses. Each column indicates an outcome of interest, provided as a rate per 1,000 children. Each row represents a separate regression analysis, so row 1 reports the coefficient from equation 3.2 where *covid* is the independent variable of interest. Row 2 reports the coefficient from equation 3.3 where *schclo* is the independent variable of interest, and row 3 reports the coefficient from equation 3.4 where *sah* is the independent variable of interest. Each regression includes year, county, and quarter fixed effects.

Rescaling the quarterly effect to a monthly effect, implies that an additional month of the COVID-19 pandemic, school closures, and stay-at home order reduced child maltreatment reporting by 0.85, 2.6, and 4.9 referrals per 1,000 children, respectively. The effect of the stay-at-home order is almost six times as large as the effect of the pandemic and almost twice as large as the effect of the school closings. Finally, rescaling the monthly impact to an annual impact based on the number of months for each of the events implies the COVID-19 pandemic, school closures, and stay-at-home order reduced maltreatment reporting by 8, 7.9, and 4.9 referrals per 1,000 children, respectively. With a child population of 1.26 million, approximately 10,000, 9,900, and 6,200 referrals went unreported as a result of the COVID-19 pandemic, school closures, and stay-at-home order, respectively, in Colorado.

3.5.3. Changes in Type of Maltreatment

So far this paper has demonstrated that the COVID-19 pandemic, and subsequent school closures and stay-at-home order drastically decreased the child maltreatment referral and report rate, but had no statistically significant impact on the substantiation rate. Next, I explore whether the type of maltreatment reported changed during the pandemic. The right side of Table 17 provides the results from estimating equations 3.2-3.4 for the neglect, physical abuse, and sexual abuse referral rates. Overall, there is no evidence that the pandemic altered the type of maltreatment reported. The direction of the coefficients implies fewer neglect and physical abuse allegations, and more sexual abuse allegations were reported as a result of the pandemic and subsequent policy responses; however, none of these estimates are statistically significant. As a result of statistically insignificant changes in the type of maltreatment referrals made, there are no statistically significant changes in the type of maltreatment substantiated.[88]

[88] Results available upon request.

3.5.4. Impact of Stay-at-Home Order Compliance on Child Maltreatment Reporting

Finally, Table 18 provides the estimates from the interaction between the stay-at-home order and COVID-19 cases. All else equal, an additional quarter under the stay-at-home order in counties with COVID-19 cases is associated with a smaller decline in the number of total referrals and screened-in referrals by 0.09 and 0.05 per 1,000 children, relative to counties without COVID-19 cases. Moreover, this interaction analysis indicates that counties with COVID-19 cases experienced a smaller decline in the neglect abuse allegation rate and an increase in the sexual abuse allegation rate, as a result of the stay-at-home order, relative to counties without COVID-19 cases.

Table 18: Estimated Impact of Stay-at-Home Order Interacted with COVID-19 Cases on Child Maltreatment Reporting

	Main Outcomes			Type of Maltreatment		
	Total Referrals (per 1,000 children)	Reports Screened-in (per 1,000 children)	Substantiated Reports (per 1,000 children)	Neglect Allegations (per 1,000 children)	Physical Abuse Allegations (per 1,000 children)	Sexual Abuse Allegations (per 1,000 children)
Stay-at-home	-15.413***	-3.607**	0.664	-2.879	-0.619	0.220
	(3.104)	(1.491)	(1.812)	(2.293)	(0.935)	(0.642)
COVID-19 Cases	0.024	-0.123***	0.045***	-0.030	-0.037	-0.014
	(0.077)	(0.030)	(0.015)	(0.051)	(0.023)	(0.012)
Stay-at-home x COVID-19 Cases	0.094***	0.050***	-0.005	0.067***	-0.003	0.007**
	(0.015)	(0.006)	(0.008)	(0.011)	(0.005)	(0.003)
Observations	3,328	3,328	3,328	3,328	3,328	3,328

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the county-by-quarter level, in parentheses. Each column indicates an outcome of interest, provided as a rate per 1,000 children. Row 1 reports the coefficient on the stay-at-home order, row 2 reports the coefficient on the number of pre-stay-at-home order COVID-19 cases, and row 3 reports the coefficient on the interaction term between the stay-at-home order and pre-stay-at-home COVID-19 cases. Each regression includes year, county, and quarter fixed effects.

3.5.5. Alternative Analyses and Permutation Tests

To determine the sensitivity of the main results, I estimate alternative analyses varying the sample size and control variables. Table 19 reports the results for different measures of the child maltreatment referral rate across different analyses. Column 1 provides the main results again. Columns 2 and 3 provide results from varying the sample size, and column 4 provides results that include county-level controls for economic conditions. The first panel of results shows the change in the rate, per 1,000 children, the second panel uses logged values, and the third panel uses the level values.

Overall, the impact of the pandemic, school closures, and stay-at-home order on the referral rate (panel 1) and logged number of referrals (panel 2) is similar across varying sample sizes. In column 2, the time period is restricted to the years 2010 to 2020, to exclude any impacts of the Great Recession. In column 3, the Denver metro-area is excluded to test whether these results are generalizable to all counties in Colorado or unique to the most populous areas. When observing total referral levels (panel 3), the coefficients are not sensitive to excluding the Great Recession years, but they are drastically reduced when excluding the Denver metro-area. This analysis indicates that relatively more referrals are going unreported in the Denver metro-area relative to other parts of the state, which makes sense since there are more people and children in the Denver metro-area. This sensitivity also underscores the importance of using rates, and not levels. In all cases, including the economic conditions reduces the magnitude of the effect size, relative to the main specification. However, the change in magnitude is not statistically different from the main estimates. The same conclusions apply across analyses variations for the screened-in and substantiation rates.[89]

[89] See Appendix Table E2 and E3 for the results.

Table 19: Robustness Analyses for Child Maltreatment Referrals

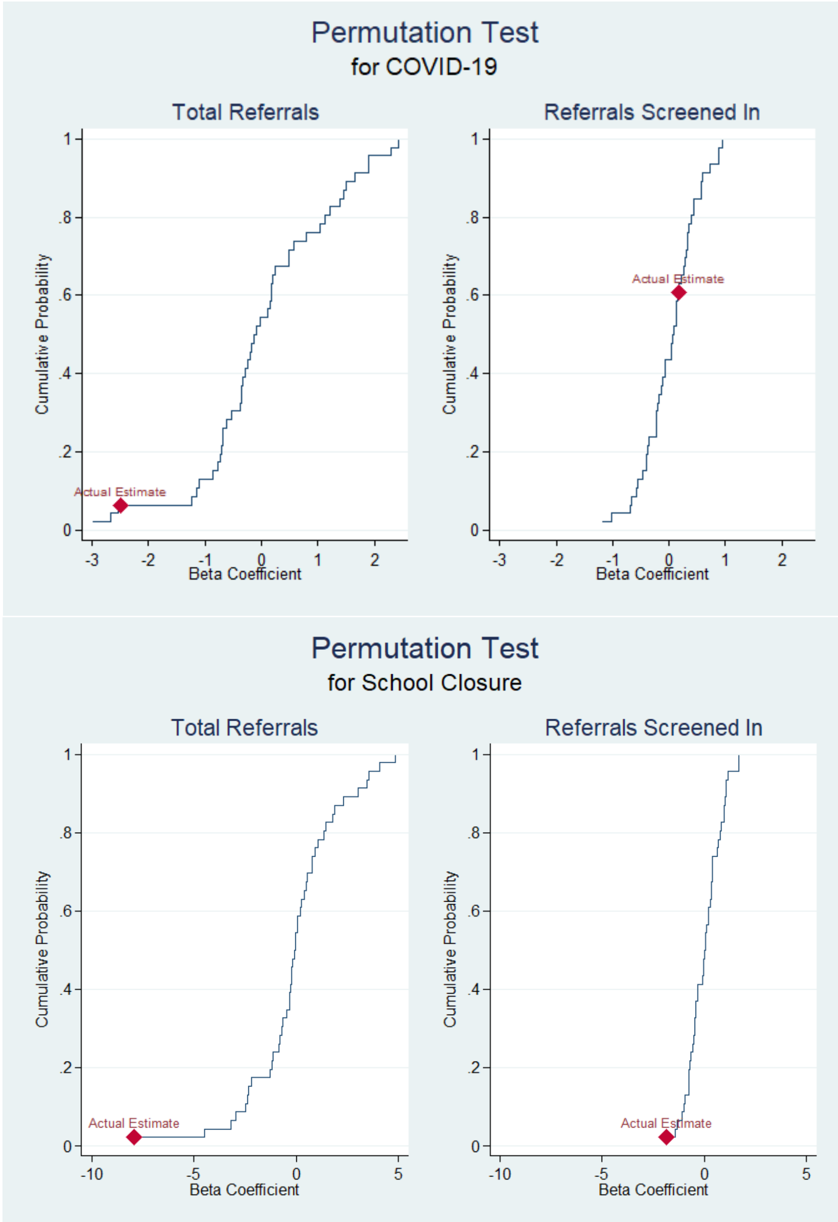
		(1) Main Results	(2) Post 2010	(3) Exclude Denver Metro-area	(4) Include Economic Controls
Panel A: Rate (per 1,000 children)	COVID-19	-2.541*	-2.514*	-1.868	-1.053
		(1.415)	(1.418)	(1.632)	(1.547)
	School Closure	-7.874***	-7.358***	-7.684***	-6.402***
		(1.829)	(1.799)	(2.107)	(1.917)
	Stay-at-home	-14.787***	-14.019***	-14.396***	-12.415***
		(3.070)	(3.056)	(3.538)	(3.274)
Panel B: Log	COVID-19	-0.133*	-0.131*	-0.096	-0.056
		(0.074)	(0.072)	(0.085)	(0.080)
	School Closure	-0.420***	-0.392***	-0.397***	-0.353***
		(0.114)	(0.111)	(0.131)	(0.120)
	Stay-at-home	-0.780***	-0.744***	-0.733***	-0.681***
		(0.192)	(0.188)	(0.221)	(0.206)
Panel C: Levels	COVID-19	-110.255***	-110.765***	-42.456*	-73.236**
		(34.192)	(29.319)	(24.229)	(32.786)
	School Closure	-146.414***	-141.809***	-72.856***	-89.796***
		(32.469)	(36.791)	(25.657)	(28.643)
	Stay-at-home	-301.492***	-295.858***	-148.273***	-201.033***
		(61.579)	(67.786)	(48.256)	(53.836)
	Observations	3,328	2,560	2,860	3,328

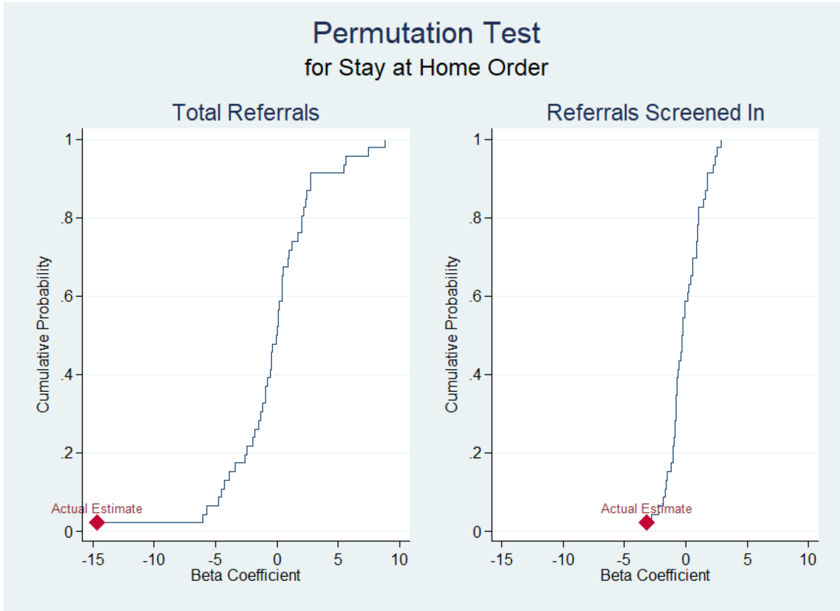
Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the county-by-quarter level, in parentheses. Each column indicates a separate regression analysis. The first column provides the main results. The second column is post-2010. The third column excludes the Denver metro-area, and the fourth column includes controls for economic conditions (i.e. the unemployment rate and employment-to-population ratio). Each row represents a separate regression analysis for the three independent variables of interest, so row 1 reports the coefficient from equation 3.2 where *covid* is the independent variable of interest. Row 2 reports the coefficient from equation 3.3 where *schclo* is the independent variable of interest, and row 3 reports the coefficient from equation 3.4 where *sah* is the independent variable of interest. There are 3 separate panels as well. Panel one reports the effect on the total referral rate, per 1,000 children. The second panel reports the effect on the log total referrals, and the third panel reports the effect on the total referrals (level). Each regression includes year, county, and quarter fixed effects.

Since the pandemic started in 2020, there are only a few treated observations. Few treated observations can lead to improper inference (Cameron et al., 2008; MacKinnon & Webb, 2017, 2018; Ferman & Pinto, 2019). One way to correct for this is to perform a set of permutation tests (Chetty et al., 2009; Buchmueller et al., 2011; Baron et al., 2020). I estimate equations 3.2-3.4 with a placebo independent variable. This is done for every quarter-year combination from 2008 to 2019. This approach results in 48 placebo estimates (12 years x 4 quarters). The distribution of the 48 placebo estimates and one actual estimate from equations 3.2-3.4 represents the sampling distribution of $\hat{\beta}_1$, $\hat{\alpha}_1, \hat{\delta}_1$.

Figure 10 shows the cumulative distribution function of the placebo and actual estimates on *covid, schclo,* and *sah,* respectively for the referral rate and screened-in report rate. The actual estimate on *covid* is not statistically different from all of the placebo estimates, whereas the actual effect of the school closures and stay-at-home order are statistically different from the placebo estimates. These permutation tests indicate that the estimated impacts from school closures and the stay-at-home order are unlikely to be a result of chance.

Figure 10: Cumulative Distribution Function of Estimates from Permutation Tests





Notes: These figures plot the cumulative distribution function of the beta coefficient for the 48 placebo and one actual estimate. The top graph plots the coefficients on *covid* from equation 3.2, the middle graph plots the coefficients on *schclo* from equation 3.3, and the bottom graph plots the coefficient on *sah* from equation 3.4. The left-hand graphs plot the coefficient for the total referral rate (per 1,000 children) and the right-hand graphs plot the coefficient for the screened-in report rate.

3.6. Conclusion

There are three major findings of this paper. First, in Colorado, the COVID-19 pandemic and subsequent policy responses resulted in a 15 percent decline in reporting in 2020, compared to 2019. The biggest decline occurred between April and June as a result of the stay-at-home order and initial shift to virtual schooling; however, delayed school openings and continued spreading of the Coronavirus kept reporting below pre-pandemic averages for the remainder of 2020. Using a model that accounts for economic hardships and harmful coping strategies brought about by the pandemic, an estimated 38,800 referrals went unreported and 2,200 victims went unnoticed. Applying these numbers to the whole country while taking into account Colorado's unusually high reporting rate, an estimated 1.38 million referrals and 112,000 victims may have gone unreported across the country during 2020.[90] These estimates can (and should) be verified in two to three years when NCANDS releases the national-level data.[91]

There are three reasons to verify these numbers after the pandemic ends. First, for documentation purposes, it is important to correctly quantify the detrimental impacts of the pandemic. Second, these numbers can be used to predict the extent of underreporting in the event of another national emergency that alters maltreatment and reporting, simultaneously. Finally, child abuse and neglect has lasting consequences on educational attainment, employment and earnings, and health (Slade & Wissow, 2007; Irigaray et al., 2013; Kalmakis & Chandler, 2015; Doyle & Aizer, 2018), so states should be making efforts to follow-up with the children who may have been missed. Having an accurate count for how many children may have experienced abuse or neglect during the pandemic allows states to know when their follow-up efforts have reached all potential candidates.

The second key finding is that the stay-at-home order, school closings, and COVID-19 national emergency all substantially reduced child maltreatment referrals and screened-in reports. Many child welfare experts and governors understand they are missing the opportunity to protect children (NGA, 2020); however, some experts disagree and claim that the pandemic is filtering out the flood of unsubstantiated reports.[92] In Colorado, I do not find evidence that the composition of substantiated and unsubstantiated reports changes. Moreover, even if the

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[90] First the 38,800 unreported referrals are multiplied by 51. Next, 1,978,800 is divided by 1.43 because Colorado's report rate is 1.43 times higher than the typical state. The number of victims is not rescaled because Colorado's victim rate is similar to the average state (9.7 victims per 1,000 children versus 8.9 victims per 1,000 children).

[91] If one were to scale early predictions from Baron et al., 2020 and Rapaport et al., 2020 to the full year, they would estimate that 833,000 to 1.06 million referrals went unreported.

[92] For an example, see the opinion piece, "National Opinion: COVID-19 is not leading to more child abuse, it's cleaning the 'pollution' of false reports," published in the Arizona Daily Star on September 4, 2020.

pandemic filtered out unsubstantiated reports, it is unclear whether this is a good thing for child welfare. Ultimately, the answer depends on the state and what supports are provided to children and families of unsubstantiated reports. For example, in Colorado, children and families of unsubstantiated reports can be referred to other services, so a call to the hotline may connect families with needed resources (CDHS, nd b). In this case, fewer calls, regardless of the disposition, is concerning.

While child welfare experts predicted that more severe child maltreatment would arise from the pandemic (Hofmann, 2021), and some research has found that the proportion of ER visits from child abuse and neglect almost doubled, relative to 2019 proportions (Swedo et al., 2020), in Colorado, I do not find evidence of this hypothesis yet. Whether this is a limitation of the data or a glimmer of hope is unclear. Identifying victims from hotline calls is especially difficult in a state that screens out the majority of referrals and substantiates so few reports. Stephens-Davidowitz (2013) proposed two clever ways to try to identify victims of maltreatment. One method relies on using fatality counts (i.e. extreme cases of child maltreatment) and the other relies on using Google searches including terms like "child abuse and neglect." Unfortunately, this study is not able to employ either method. First, CDHS does not provide real-time data on fatalities. Second, Google searches related to child abuse and neglect in 2020 saw a substantial uptick the first week of March, prior to the pandemic. This uptick follows the release of the Netflix true crime miniseries documentary, "The Trials of Gabriel Fernandez," which was released February 26th. This limitation is an area that future research should continue to address as different types of maltreatment victims require different interventions.[93]

[93] Fatality data during 2020 will be available through NCANDS for all states in two to three years.

Finally, I find that the referral rate for neglect decreased by less in counties that were more likely to comply with the stay-at-home order relative to counties that were less likely to comply.[94] This result provides suggestive evidence of parental burnout and is supported by early research in the medical literature. For example, childhood injuries occurring in the home or on bicycles increased relative to sport and playground injuries during the pandemic (Bram et al., 2020). In addition to targeting support and services to communities with historical records of substantiated cases, agencies need to target support and services to communities hit hard by the pandemic.

Eventually the pandemic will be a thing of the past; however, the findings of this study have implications beyond the pandemic. These findings quantify another hardship brought about by the pandemic: underreporting child maltreatment. The prevalence of underreporting highlights the role mandatory reporters play in detecting child maltreatment. These results can be used to inform policy decisions related to underreporting, mandatory reporting, and training. For example, states might want to consider additional ways to detect child maltreatment that do not rely on mandatory reporters to prepare for future events that may limit interactions between children and mandatory reporters. These results also speak to resource allocation for intervention after a pandemic. Based on findings from this paper, states should target resources to assist neglected children. For example, states may want to allocate additional funding to address the consequences of neglect. This paper can also inform policy decisions related to future pandemic responses. While school closures and stay-at-home orders reduced the spread of Coronavirus (Auger et al., 2020; Castillo et al., 2020), policymakers also have to consider the impact such policies had on child maltreatment reporting to design even better responses in the future. For

[94] Bullinger et al. (2020) come to a similar conclusion in Indiana.

example, the Department of Education in Maine provided an updated guide for teachers and others who care for children to detect maltreatment virtually (Maine DOE, 2020). Finally, this paper can be used as a reference to understand how events that alter maltreatment and reporting simultaneously impact child maltreatment referrals and substantiation rates in the data. Ultimately, fluctuations in the data seem to be more reflective of reporting than actual incidences.

Appendix A – Extended Foster Care Effective Dates and Policy Details

The source of identification comes from state and federal policy changes to extended foster care. Prior to the Fostering Connections Act of 2008 (FCA) only a handful of states allowed foster youth to remain in care beyond their 18th birthday. In response to the FCA, many states extended their age-out age to 21 years old via state funding and/or federal reimbursement. States that are federally reimbursed for extended foster care support and services face more reporting and accountability requirements compared to states that solely rely on state funds to implement extended foster care. In addition, states with federally-funded extended foster care can support more youth by using both federal and state dollars.

In 2010, 25 states and the District of Columbia had extended foster care, and in 2017, 48 states and the District of Columbia, had extended foster care. Oklahoma is the only state that does not offer extended foster care. Louisiana and South Dakota have an exception that youth still in high school can remain in foster care until 21 years old, but otherwise youth age-out at 18 years old. Wisconsin only offers extended foster care to youth with Individual Education Plans (IEPs). There is considerable variation in timing, age-out age, requirements to be in extended foster care, and transitional services available. Appendix Table A1 provides more specific details about extended foster care in each state.

Although there is variation across many dimensions, I primarily exploit the timing variation for a few reasons. First, federal funding for independent living programs (ILPs) have existed since the 1980s, well before the FCA; therefore, all states offer some sort of independent living support to their youth aging out of foster care. Second, the marginal costs of pinning down all of the intricacies in every single state outweigh the marginal benefits at this time. Lastly, there is not enough data to effectively estimate a model that exploits the variation within each of these alternative dimensions.

Information about extended foster care in each state comes from a host of sources ranging from government reports and documents to state statutes and house bills. First, I used reports and documents from 2014 to 2019 created by Child Trends, Child Welfare Information Gateway, Congressional Research Service, National Conference of State Legislatures (NCSL), Pew Charitable Trusts, and the U.S. Government Accountability Office to get a time frame as to when a state implemented extended foster care. Each of these reports lists either “HHS, Children’s Bureau,” or “responses from state agencies” as their source. These reports include a map or table identifying states with state or federal extended foster care at a single point in time. Some of these resources also include current state statutes, administrative codes, and agency policies providing additional details and context. In combination, these sources allow me to observe changes over time and infer a time frame in which a state implemented extended foster care. For example, the 2014 Pew Charitable Trusts report shows that North Carolina does not have extended foster care, but the 2017 NCSL webpage shows that North Carolina does have extended foster care, so I can infer that North Carolina implemented extended foster care sometime between 2014 and 2017. Although the time frame provides a good starting point, for my analysis I need specific dates in which extended foster care was implemented.

Next, I used legal databases to verify details and record effective dates of statutes and policies. The Juvenile Law Center (JLC) developed a tool that provides state-level information about implementation of extended foster care, such as availability, eligibility, and funding. Additionally, this tool provides the statute or policy from which the information comes. Using Westlaw Campus Research, a legal database provided by Georgia State University, I then looked up the referenced statutes and recorded the appropriate effective date. This database tracks the history of the statues, so I can read older versions and determine the first year a state implemented the extended foster care program. I use the earliest effective date, as long as there have not been revisions.

I used the NCSL's child welfare database to differentiate between state and federal extended foster care and to double check statue codes against JLC and effective dates against the Westlaw database. The NCSL database contains child welfare legislation related to foster care, services for older youth, and funding for child welfare services, among other topics, that have been enacted between 2012 and 2018 for all 50 states and D.C. For some states, the legal documentation can be viewed and tracked, and for others the state legal database was accessible to further look up the statute. Another way I determined if a state has federally-funded extended foster care was by noting the definition of a child and language related to juvenile court jurisdiction. States eventually seeking federal reimbursement, at a minimum, must change the statutory definition of "child" for Title IV-E programs[95]. The NCSL resource provides rich detail about more recent legislation, but I needed to use Westlaw for policies that predated 2012. Together these resources were used to verify and adjust effective dates of the state or federally-funded extended foster care.

Finally, for states where dates were still missing or resources yielded conflicting dates, I google searched "<<state>> extended foster care." Often this search resulted in state specific journal articles discussing the policy climate at the time of publication, and sometimes referenced specific house bills.

[95] JCYOI. 2014. A Guide to Support the Implementation of Foster Care beyond 18. Pg. 6.

Appendix Table A1: Effective Dates and Details of Policy Changes

State	Date effective	Age-Out Age	Federal Reimbursement	Treatment	Eligibility Requirements	Process to Stay	Re-entry Allowed	Direct Payment to Youth	Law/Bill/Act and extra notes
AL	10/1/2010	21	yes	Always federal	least restrictive	Automatic with VPA	yes		Ala. Admin. Code § 660-5-22-.06(11)(a).; state policy prior to FCA
AK	1/1/2011	21	no	Always state	unknown	Court approved with VPA	unknown	yes	HB126; HB27 adds eligibility requirements and reentry in 2016
AZ	11/30/2012	21	no	Nothing to state	least restrictive	VPA	yes, until 20	yes	AZ ADC R21-5-205; Navajo Nation and Pascua Yaqui federally reimbursed starting in 2014 and 2016
AR	6/1/2011	21	yes	Always federal	least restrictive	VPA	yes		
CA	1/1/2012	19; 21 in 2014	yes	Nothing to federal	least restrictive	Automatic with VPA	yes	yes	AB12; age-out age increased incrementally until 2014
CO	1/1/2012	21	no	Nothing to state	least restrictive	Court ordered	no		CO ST § 19-3-205
CT	6/30/2007	21	no	State to federal	enrolled in school		unknown		CT ST § 46b-129; Youth can stay until 23 in some cases.
	10/1/2013		yes		least restrictive	Voluntary opt-in	yes		
DC	10/1/2010	21	yes	Always federal	least restrictive	Automatic	yes		DC CODE § 16-2303. State policy prior to FCA
DE	7/5/2012	21	no	Nothing to state	unknown	Automatic with VPA or court ordered	yes	yes	HJR18 (146th GA), SB113
FL	1/1/2014	21	no	Nothing to state	least restrictive	Automatic with VPA or court ordered	yes		FL ST § 39.6251; 22 if disability.
GA	2/6/2012	21.5	no	Nothing to state	enrolled in high school	VPA	yes, until 20		GA ST § 15-11-2 in 2014
HI	7/1/2014	21	yes	Nothing to federal	least restrictive	Court approved with VPA	yes		Senate Bill 1340 (Act252).Program name: Imua Kakou.
ID	7/1/2010	21	no	Always state	unknown	Court approved with VPA	no		ID ST § 39-1202. Referred to as "continued care".
IL	10/1/2010	21	yes	Always federal	least restrictive	Automatic with VPA or court ordered	yes		State policy prior to FCA
IN	3/14/2012	20	no	State to federal	least restrictive	Court approved with VPA	yes	yes	IN ST 31-28-5.8-5
	7/1/2012		yes						
IA	1/1/2009	19	no	Always state	enrolled in high school	VPA	yes		Iowa Code § 234.1(2)
KS	5/31/2012	21	no	Nothing to state	enrolled in high school	Court approved with VPA	no		Kan. Stat. § 38-2203

State	**Date effective**	**Age-Out Age**	**Federal Reimbursement**	**Treatment**	**Eligibility Requirements**	**Process to Stay**	**Re-entry Allowed**	**Direct Payment to Youth**	**Law/Bill/Act and extra notes**
KY	4/11/2012	21	no	Nothing to state	none specified	VPA	yes, until 19		KY S 213
LA	6/1/2018	21	no	Nothing	enrolled in high school	VPA	no		La. Stat. § 46:286.24(A). 21 if still in HS. Young Adult Program (YAP) prior to 2013, ended due to budget cuts.
ME	9/28/2011	20	no	State to federal	least restrictive	VPA	yes		Me. Rev. Stat. tit 22, § 4037-A(1)(a). V9 Program/Agreement
	1/1/2012		yes						
MD	10/1/2010	21	yes	Always federal	least restrictive	Court approved with VPA	yes, until 20.5		State policy prior to FCA
MA	10/1/2010	21	yes	Always federal	least restrictive	VPA	yes	yes	MA ST 119 § 21. State policy prior to FCA
MI	11/22/2011	21	no	State to federal	least restrictive	VPA	yes		MI ST 400.645
	7/1/2012		yes						
MN	10/1/2010	21	yes	Always federal	least restrictive	VPA	yes	yes	MN ST § 260C.451; State policy prior to FCA
MS	7/1/2013	21	no	Nothing to state	enrolled in high school	Automatic with VPA	no		MS ST § 43-15-13
MO	8/28/2013	21	no	Nothing to state	none specified	Court approved with VPA	yes, until 20		MO ST 211.036
MT	11/29/2017	21	no	Nothing	enrolled in high school	Court approved with VPA	no		MT ADC 37.51.102. No age limit if in secondary school starting in 2018. Transitional living program.
NE	12/1/2008	19	no	State to federal	unknown		unknown		
	9/1/2014	21	yes		least restrictive	VPA	yes	yes	2013 Young Adult Voluntary Services and Supports Act. Program name: Bridge to Independence (b2i).
NV	10/1/2015	19	no	Nothing to state	NA	VPA	no	yes	
NH	1/1/2009	18	no	Nothing	unknown	VPA	yes		NH ST § 169-C:34 (V-a). Voluntary services until 21
NJ	7/1/2006	21	no	Always state	enrolled in school, working at least part time, or unable due to medical or disability	Court approved with VPA	yes	yes	NJ ST 30:4C-2.3. Direct payments used for independent living
NM	9/29/2015	18	no	Nothing	NA	Court approved with VPA	no	yes	N.M. Stat. § 32A-4-25.3. Navajo Nation federally reimbursed starting in 2014.
NY	10/1/2010	21	yes	Always federal	least restrictive		yes		NY FAM CT § 1055

State	Date effective	Age-Out Age	Federal Reimbursement	Treatment	Eligibility Requirements	Process to Stay	Re-entry Allowed	Direct Payment to Youth	Law/Bill/Act and extra notes
NC	1/1/2017	21	yes	Nothing	least restrictive	Court approved with VPA	yes, until 20	yes	N.C. Gen. Stat. §108A-48(c). Eastern Band federally reimbursed starting in 2015.
ND	1/1/2012	21	yes	Nothing to federal	least restrictive	Court approved with VPA	yes		ND ST 27-20-30.1
OH	9/13/2016	21	no	Nothing	least restrictive	VPA	no		HB 50 of the 131 GA
	10/1/2018		yes				yes	yes	
OK	11/1/2015	18	no	Nothing	unknown	Court ordered	yes		OK ST T. 10A § 1-9-107. Successful Adulthood Act.
OR	4/1/2011	21	yes	Always federal	least restrictive	Automatic	no	yes	OR ADC 413-030-0220; OR ST § 418.330. Direct payments used for tuition and waiver fees.
PA	1/1/2010	21	no	State to federal	enrolled in school or unable due to medical or disability	Court approved with VPA	no		
	7/1/2012		yes		least restrictive		yes		PA H 1261
RI	6/28/2018	21	no	Nothing	least restrictive	VPA	yes	yes	RI ST § 14-1-6 (c). Had extended foster care prior to 2007, but then scaled back.
	1/1/2019		yes						
SC	4/26/1996	21	no	Always state	enrolled in school or working at least part time	VPA	yes		SC ADC 114-595. Referred to as Aftercare Placement.
SD	1/1/1991	21	no	Always state	enrolled in high school	VPA	no		SD ST § 26-6-6.1
TN	10/1/2010	21	yes	Always federal	enrolled in school or unable due to medical or disability	VPA	yes	yes	Tennessee's Transitioning Youth Empowerment Act of 2010
TX	10/1/2010	21	yes	Always federal	least restrictive	VPA	yes		40 TX ADC § 700.346. 22 if still in HS. State policy prior to FCA.
UT	4/1/2015	21	no	Nothing to state	unknown	VPA	yes		Transition to Adult Living Program. Navajo Nation federally reimbursed starting in 2014.
VT	6/6/2007	22	no	Always state	least restrictive	VPA	yes		VT ST T. 33 § 4904
VA	7/1/2015	21	no	Nothing to state	unknown	VPA	yes		VA ST § 63.2-905.1
	7/1/2016		yes		least restrictive	Automatic with VPA		yes	Fostering Futures Program

State	Date effective	Age-Out Age	Federal Reimbursement	Treatment	Eligibility Requirements	Process to Stay	Re-entry Allowed	Direct Payment to Youth	Law/Bill/Act and extra notes
WA	7/22/2011	21	yes	Always federal	Restrictions loosened overtime. Most restrictive in 2011 and least restrictive in 2016.	VPA	yes	yes	WA ST 74.13.020. Pilot program prior to FCA.
WV	1/1/2011	21	yes	Always federal	enrolled in school	VPA	yes, until 20		WV ST § 49-2B-2
WI	8/1/2014	21	no	State to federal	enrolled in high school	Court approved with VPA. Needs IEP	unknown		Wisconsin Act 334
	7/14/2015		yes				yes		Wis. Stat. Ann. § 48.975(3m);
WY	3/4/2016	21	no	Nothing	unknown	Court approved with VPA	no		WY ST § 14-3-431

Notes: This table provides an overview of the dates and details about each states' extended foster care policy. The effective date is used to determine whether a youth has EFC available at the time they turned 18 years old. Most states with EFC extend the age-out age to 21; however, some states have younger ages. Federal reimbursement indicates that the state has an approved Title IV-E plan and receives federal reimbursement for EFC services. States that receive federal reimbursement are said to have "federally-funded EFC." The treatment column specifies how each state is represented in my sample. "Nothing" means that there was no policy prior to 2016. "Nothing to state" means that a state adopted a policy between 2012 and 2016. "Nothing to federal" means that a state adopted a policy and is receiving federal reimbursements between 2012 and 2016. "State to federal" identifies the seven states that have both a state and federal policy between the years 2012 and 2016. "Always state" means that the state had a policy prior to 2012, and "always federal" means that the state had a policy and is receiving federal reimbursement prior to 2012. Eligibility requirements are referred to as "least restrictive" in states that allow youth to participate in extended foster care if any of the following requirements are met: enrolled in secondary school, enrolled in post-secondary school, working part-time, participating in training programs to reduce barriers to work or school, or unable to do the above due to a medical condition or disability. More restrictive eligibility requirements are specified. Most states require youth to sign a voluntary placement agreement (VPA) in order to remain in care, and some have the additional step of court approval. The majority of states allow for re-entry and some states pay their foster care maintenance payments directly to the youth. The final column references laws, bills, and acts when appropriate and provides additional details about a state's specific program. All of the information in this table comes from the collection of sources discussed above. A more detailed excel spreadsheet is available upon request.

Appendix B – What Factors Predict Extended Foster Care Implementation?

A common concern using a difference-in-differences approach is that treated subjects differ from untreated subjects (i.e. the parallel trends assumption is not satisfied). In my analysis, I use cross-sectional data to compare outcomes for youth before and after the implementation of extended foster care in a specific state. Since I use cross-sectional data, I cannot verify the parallel trends assumption, but in this appendix, I demonstrate that treated states do not differ from the untreated states in ways that would bias the results.

First, I provide statistics by treatment status. Appendix Table B1 provides NYTD participant characteristics aggregated at the state level by treatment. The average high school enrollment rate ranges from 86 to 91 percent, and the youth employment rate ranges from 13 to 17 percent with no notable monotonic trend. The foster care environment, as indicated by age of entry, removal reasons, and placements, is similar across treatment status. One monotonic trend worth noting is survey participation. Average survey participation rates range from 53 to 76 percent, decline with age, and are higher among states with extended foster care. This pattern indicates differences in attrition between the treatment and control groups and is addressed in the main paper.

Next, Appendix Table B2 summarizes the economic conditions and safety net generosity as NYTD participants transition to adulthood by treatment. There are some differences across cohorts, but no notable differences across treatment status. For example, the unemployment rate ranges from 6.5 to 8 percent for the older cohort and 4.3 to 5.5 for the younger cohort. Income per capita (in 2016 USD) ranges from $42,000 to $51,000 for the older cohort and $44,000 to $53,000 for the younger cohort. Finally, the number of Medicaid beneficiaries ranges from 156 to 201 per 1,000 people for the older cohort and 189 to 250 per 1,000 people for the younger cohort. In the younger cohort, states that implemented extended foster care between 2012 and 2016 have overall fewer Medicaid beneficiaries.

Finally, I create a state panel of economic conditions, safety net generosity, and foster care environment for the years 2008 to 2017 to further demonstrate that these factors are uncorrelated with implementing federally-funded extended foster care and have little explanatory power. I estimate the following fixed effects linear in probability model:

$$Prob(FedEFC_{st} = 1) = \beta_0 + \boldsymbol{X_{st}\beta} + \gamma_s + \gamma_t + \gamma_s * Year \text{ (B1)}$$

Where FedEFC is a binary indicator that equals one if state *s* has federally-funded extended foster care in year *t*, **X** is a vector of predictive factors for state *s* in year *t*, such as the unemployment rate, and γ_s and γ_t are state and year fixed effects, respectively. The final term $\gamma_s * Year$ captures the state-specific linear trends. The results from this analysis are provided in Appendix Table B3.

The first three models reveal correlations between implementation and economic conditions and the foster care environment. There are only a few notable correlations. First, states with higher monthly SNAP benefits and fewer Medicaid beneficiaries are more likely to implement federally-funded extended foster care. Although statistically significant, this finding is economically insignificant. For example, increasing the monthly SNAP payment by $23 (one standard deviation) is correlated with a 0.23 percent increase in extended foster care implementation. Second, having a Democratic Governor is correlated with a 14 percent increase in the likelihood of implementing extended foster care. Finally, states with more disconnected youth between the ages of 16 to 24 are marginally less likely to have extended foster care.

The final model uses lagged independent variables to try to determine whether the conditions of the previous year have any explanatory power for future implementation. In this model, the earlier correlations go away and only the proportion of foster youth ages 16 to 21 funded with Title IV-E dollars has explanatory power. States that experienced a 1 percent increase in the proportion of youth ages 16 to 21 funded with Title IV-E dollars were 0.88 percent less likely to implement extended foster care. In other words, states with more Title IV-E eligible youth are less likely to implement extended foster care.

Overall, there are few notable correlations implying implementation of federally-funded extended foster care is unpredictable, at least based on a variety of observable characteristics. After controlling for state and cohort effects, implementation of extended foster care should be as good as random.

Appendix Table B1: State Characteristics by Treatment

	Federal policy prior to 2012		State policy prior to 2012		Nothing to federal policy between 2012 and 2016		Nothing to state policy between 2012 and 2016		State to federal policy between 2012 and 2016		No policy as of 2016	
Number of States	13		7		3		12		7		9	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Number of NYTD Participants	745.9	572.4	305.1	293.6	1,500	2,407	600	335.3	767.6	527.9	349.7	265.6
Percent of youth that participated in survey at 19	76.02	11.8	75.54	10.11	75.76	9.028	72.99	10.26	66.85	12.06	68.08	9.765
Percent of youth that participated in survey at 21	74.37	9.523	68.28	10.07	70.57	4.23	64.30	15.47	52.76	18.5	67.16	7.773
Percent female	49.13	4.808	51.09	5.268	55.54	3.645	49.03	6.380	45.9	5.039	45.54	5.479
Percent Non-Hispanic White	44.77	21.55	53.06	25.46	32.85	26.66	49.91	14.47	49.63	15.62	52.75	17.59
Percent Non-Hispanic Black	33.6	25.21	18.99	21.96	10.83	11.17	30.58	18.34	29.03	14.82	20.70	21.60
Percent Hispanic	12.87	10.84	8.811	6.1	17.64	25.3	13.70	11.44	12.6	6.55	14.25	14.90
Percent Other Race	8.757	7.55	19.14	23.64	38.68	35.24	5.802	3.578	8.74	1.914	12.29	8.904
Percent of youth ever diagnosed with disability	58.54	24.24	37.51	20.04	63.22	25.95	54.86	27.06	56.77	17.45	51.98	21.99
Total removals as a child	1.531	0.215	1.466	0.116	1.563	0.118	1.440	0.161	1.453	0.2	1.571	0.236
Total number of placements as a child	7.222	2.093	6.595	1.379	6.234	1.104	7.718	2.084	6.337	1.304	7.102	2.633
Cumulative length of stay in foster care as a child	4.446	1.36	3.721	0.597	4.173	1.136	3.615	0.374	3.987	1.004	3.918	0.682
Age of first removal	11.66	1.318	12.75	0.782	11.25	1.515	12.53	0.574	12.2	0.879	11.86	1.000

	Federal policy prior to 2012		State policy prior to 2012		Nothing to federal policy between 2012 and 2016		Nothing to state policy between 2012 and 2016		State to federal policy between 2012 and 2016		No policy as of 2016	
Number of States	13		7		3		12		7		9	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Percent placed in a foster home	60.73	13.88	58.75	14.27	66.81	15.99	59.73	11.39	61.18	10.34	60.59	15.92
Percent placed in a group home	32.69	15.1	33.2	12.37	25.04	13.35	34.56	12.97	32.47	10.81	34.61	16.96
Percent placed in other setting	6.588	2.6	8.053	6.631	8.144	2.658	5.705	4.106	6.346	5.025	4.802	2.027
Age of last placement	17.43	0.747	16.99	0.337	17.12	0.183	17.20	0.422	17.56	0.578	17.07	0.512
Last placement setting as a child: kinship care	10.47	5.861	11	5.385	17.66	12.11	8.338	4.967	12.96	3.106	12.32	6.420
Last placement setting as a child: foster family	39.37	8.528	40.79	13.2	43.77	3.29	41.57	10.51	39.94	12.14	35.60	10.40
Last placement setting as a child: group home	31.65	13.25	25.69	12.39	26.62	8.278	32.79	16.12	28.29	11.13	32.64	13.84
Last placement setting as a child: supervised independent living	5.901	3.155	4.87	4.861	1.709	2.961	4.538	3.998	6.91	8.194	6.846	8.225
Percent ever removed for abuse	27.14	13.3	26.68	10.8	24.58	15	25.39	7.537	24.45	7.215	23.83	10.07
Percent ever removed for neglect	48.32	20.89	54.2	25.25	48.8	23.9	53.61	20.30	56.78	28.02	59.66	18.07
Percent ever removed for parental incarceration	5.129	3.854	6.912	7.517	4.552	2.758	7.695	3.626	7.387	2.085	6.549	4.314
Percent ever removed for	19.93	13.39	22.63	14.21	13.97	7.226	20.36	7.213	22.61	8.702	19.10	10.78

	Federal policy prior to 2012		State policy prior to 2012		Nothing to federal policy between 2012 and 2016		Nothing to state policy between 2012 and 2016		State to federal policy between 2012 and 2016		No policy as of 2016	
Number of States	13		7		3		12		7		9	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
parental substance abuse												
Percent ever removed for inadequate housing	8.831	6.746	7.628	7.516	11.66	13.64	14.29	8.468	12.93	5.469	7.693	6.488
Percent ever removed for child-related problems	40.23	17.75	38.61	24.51	34.52	33.26	45.49	24.78	42.58	16.11	36.86	23.15
Median monthly foster care payment	$2,233	$1,362	$1,086	$561	$1,858	$1,204	$2,029	$2,738	$2,216	$848	$1,510	$1,214
Percent in foster care at 17	100	0	99.88	0.312	100	0	99.13	2.052	99.54	0.914	99.64	0.882
Percent enrolled in HS at 17	91.39	3.231	91.61	4.189	89.22	3.651	86.25	14.30	92.51	3.253	87.80	4.550
Percent homeless prior to 17	15.58	7.653	19.09	12.76	23.83	7.173	17.70	6.082	17.88	7.506	21.94	11.37
Percent employed at 17	13.35	6.501	16.39	5.129	14.35	7.165	13.12	3.191	15.08	4.734	17.42	5.700
Percent incarcerated prior to 17	31.03	9.921	31.37	17.12	36.09	13.84	38.43	13.97	36.82	11.33	34.79	13.68
Percent referred for substance abuse prior to 17	26.37	7.531	29.99	8.968	34.08	9.601	28.29	11.26	29.34	6.982	29.88	10.88
Percent not enrolled or employed at 17	5.399	2.947	4.787	2.4	5.741	2.262	5.244	3.936	4.561	0.946	7.564	3.445
Percent enrolled in college at 17	2.792	1.276	2.887	1.729	4.803	1.966	7.833	13.98	2.953	1.893	3.847	1.808
Percent in foster care at 19	41.74	22.47	6.663	9.627	31.82	23.69	16.81	17.86	27.4	18.13	6.899	8.078

	Federal policy prior to 2012		State policy prior to 2012		Nothing to federal policy between 2012 and 2016		Nothing to state policy between 2012 and 2016		State to federal policy between 2012 and 2016		No policy as of 2016	
Number of States	13		7		3		12		7		9	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Percent homeless in past 2 years	19.23	7.053	29.64	13.72	31.26	12.99	21.01	6.323	20.81	4.946	27.37	9.192
Percent that have graduated high school by 19	40.41	16.21	43.85	21.52	55.99	4.375	47.33	20.04	40.77	10.38	46.63	11.53
Percent enrolled in college at 19	26.18	8.967	20.19	10.99	30.41	6.535	25.68	11.51	26.77	6.955	21.29	5.871
Percent employed at 19	35.99	4.473	40.54	3.771	38.68	6.222	38.36	8.905	35.86	4.895	37.41	5.028
Percent not enrolled or employed at 19	30.85	5.963	33.22	6.094	31.86	6.069	29.56	7.633	30.56	9.613	33.13	4.665
Percent incarcerated in past 2 years	24.33	8.126	24.62	11.67	21.03	5.741	25.29	9.342	27.59	8.741	25.66	11.29
Percent in foster care at 21	21.11	18.18	2.722	6.734	15.9	14.41	7.159	17.60	8.338	10.11	1.158	2.236
Percent homeless in past 2 years	25.77	8.335	37.63	13.07	27.39	4.009	32.36	7.813	30.33	6.717	35.12	8.765
Percent that have graduated high school by 21	69.37	12.31	74.74	9.011	83.28	5.753	78.25	10.86	68.57	12.8	71.78	11.42
Percent enrolled in college at 21	20.6	6.199	19.41	8.709	29.28	6.964	22.54	8.810	22.01	8.039	15.53	5.144
Percent employed at 21	49.28	5.516	48.71	7.683	56.19	8.779	53.85	11.06	50.99	6.744	52.86	9.480
Percent not enrolled or employed at 21	38.42	6.473	42.03	10.21	31.42	3.413	35.35	10.43	36.83	7.321	36.25	6.818
Percent incarcerated in past 2 years	24.69	7.947	30.12	13.27	23.58	7.109	27.37	10.11	26.14	12.38	27.15	9.787

Appendix Table B2: Average Economic Conditions and Safety Net Generosity by Treatment

	Federal policy prior to 2012		State policy prior to 2012		Nothing to federal policy between 2012 and 2016		Nothing to state policy between 2012 and 2016		State to federal policy between 2012 and 2016		No policy as of 2016	
Number of States	13		7		3		12		7		9	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
					Cohort 1 3-Year Average (2011, 2012, 2013)							
Unemployment Rate	7.82	1.02	6.69	1.96	6.47	3.62	7.92	1.64	7.48	1.69	7.10	1.77
Poverty Rate	14.92	3.24	12.36	2.57	12.88	2.79	15.12	3.27	12.55	1.45	15.14	4.22
Income per Capita (in 2016 USD)	$48,097	$10,259	$46,412	$8,143	$50,522	$4,124	$41,816	$5,060	$47,228	$9,323	$44,516	$5,937
Gross State Product (in millions of 2016 USD)	$470,440	$460,338	$154,951	$180,817	$793,747	$1,264,000	$279,790	$212,161	$303,536	$206,575	$194,921	$195,266
TANF Recipients (per 1,000 people)	13.35	7.30	9.12	4.06	20.87	15.60	8.85	4.11	10.75	3.46	9.04	6.72
Child-only TANF Recipients (per 1,000 children)	9.69	4.84	7.10	3.02	12.83	13.70	7.62	4.62	7.95	1.38	7.95	4.67
Monthly TANF Benefit for 3-person family	$449	$191	$524	$246	$617	$143	$350	$98	$482	$122	$461	$162
SNAP Recipients (per 1,000 people)	164.80	35.93	135.60	28.39	104.20	21.10	149.00	41.85	143.40	34.70	148.20	48.21
Monthly SNAP Benefit for 1-person household	$210	$0	$215	$15	$250	$70	$210	$0	$210	$0	$210	$0
Medicaid Beneficiaries (per 1,000 people)	201.60	49.10	171.90	45.37	181.50	80.99	156.00	42.93	180.60	35.72	179.50	60.37

	Federal policy prior to 2012		State policy prior to 2012		Nothing to federal policy between 2012 and 2016		Nothing to state policy between 2012 and 2016		State to federal policy between 2012 and 2016		No policy as of 2016	
Number of States	13		7		3		12		7		9	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
					Cohort 2 3-Year Average (2014, 2015, 2016)							
Unemployment Rate	5.54	0.83	4.76	1.38	4.31	1.85	5.29	1.03	4.95	0.95	5.23	1.05
Poverty Rate	13.66	3.16	11.79	1.91	11.79	2.38	13.99	3.75	11.73	1.56	13.92	4.32
Income per Capita (in 2016 USD)	$51,050	$11,166	$48,744	$8,032	$53,187	$3,232	$44,094	$5,252	$49,483	$9,052	$46,402	$6,191
Gross State Product (in millions of 2016 USD)	$507,666	$498,001	$164,549	$191,815	$898,508	$1,437,000	$302,953	$235,459	$323,665	$221,817	$205,675	$212,882
TANF Recipients (per 1,000 people)	12.77	9.96	7.85	4.05	19.97	19.00	6.95	3.60	12.31	13.37	7.30	4.85
Child-only TANF Recipients (per 1,000 children)	8.70	4.54	6.89	3.41	9.67	9.32	6.89	4.54	6.91	1.73	6.88	4.70
Monthly TANF Benefit for 3-person family	$442	$189	$519	$228	$628	$148	$342	$95	$469	$115	$460	$166
SNAP Recipients (per 1,000 people)	157.90	33.86	125.20	21.60	104.70	30.31	139.90	41.13	133.30	23.02	142.80	50.68
Monthly SNAP Benefit for 1-person household	$194	$0	$199	$14	$242	$83	$194	$0	$194	$0	$194	$0
Medicaid Beneficiaries (per 1,000 people)	250.90	56.41	200.60	59.25	195.00	71.81	189.20	55.89	193.30	34.53	218.00	84.66

Appendix Table B3: Predictors of Implementing Federally-Funded Extended Foster Care

	Outcome: Federally-funded extended foster care			
Independent Variables	(1)	(2)	(3)	(4)
Unemployment rate	-0.003	-0.002	-0.001	-0.004
	(0.025)	(0.025)	(0.026)	(0.031)
Gross state product (in millions of 2016 USD)	0.334	0.471	0.382	-1.046
	(0.703)	(0.760)	(0.751)	(1.197)
Poverty Rate	0.006	0.006	0.007	-0.001
	(0.008)	(0.008)	(0.008)	(0.010)
Income per capita (in 2016 USD)	0.011	0.015	0.012	0.024
	(0.018)	(0.017)	(0.016)	(0.019)
TANF recipients (per 1000 people)	-0.006	-0.006	-0.006	-0.005
	(0.007)	(0.007)	(0.007)	(0.007)
Monthly TANF benefit for 3-person family (in 2016 USD)	0.000	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
SNAP recipients (per 1000 people)	-0.001	-0.001	-0.001	0.002
	(0.003)	(0.002)	(0.002)	(0.002)
Monthly SNAP benefit for 1-person household (in 2016 USD)	0.010***	0.010***	0.009***	0.000
	(0.003)	(0.003)	(0.003)	(0.002)
Child-only TANF recipients (per 1000 children)	0.011	0.012	0.010	0.011
	(0.017)	(0.018)	(0.018)	(0.022)
Medicaid beneficiaries (per 1000 people)	-0.001	-0.002*	-0.002*	-0.001*
	(0.001)	(0.001)	(0.001)	(0.001)
Governor is Democrat	0.145**	0.141**	0.139**	0.106
	(0.061)	(0.061)	(0.061)	(0.077)
Federal medical assistance percentage	0.829	0.914	0.823	2.351
	(1.281)	(1.290)	(1.352)	(1.435)
Foster youth (per 1000 people)		0.049	0.060	-0.017
		(0.091)	(0.094)	(0.117)
Proportion of Foster Youth aged 16 to 21		0.158	0.021	0.970
		(1.234)	(1.242)	(0.969)
Proportion of Foster Youth that are Funded under Title IV-E		0.284	0.310	0.672
		(0.498)	(0.512)	(0.451)
Proportion of Foster Youth that are Funded under Title IV-E, age 16 to 21		0.001	-0.010	-0.888**
		(0.346)	(0.358)	(0.367)
Proportion of Foster Youth in Supervised Independent Living, age 16 to 21		-0.733	-0.758	-1.059
		(0.618)	(0.622)	(0.635)
Median Monthly Payment for Foster Youth, age 16 to 21		-0.000	-0.000	-0.000*
		(0.000)	(0.000)	(0.000)
Median Monthly Payment for Foster Youth		0.000	0.000	0.000**

		(0.000)	(0.000)	(0.000)
Homeless (per 1000 people)			0.015	0.021
			(0.050)	(0.041)
Percent of disconnected youth, age 16 to 24			-2.140*	0.579
			(1.135)	(1.154)
Percent of youth enrolled in college, age 18 to 24			-0.528	-0.844
			(0.755)	(0.881)
Observations	510	510	510	459
Number of States	51	51	51	51
Adjusted R-squared	0.629	0.632	0.632	0.565

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions include year fixed effects, state fixed effects, and a state linear time trend. The fourth column uses lagged independent variables, thus has one less year of data.

Appendix C – Appendix Tables and Figures for Chapter 1

Appendix Table C1: Summary Statistics for NYTD Participants (Full Set of Controls)

		For 19 Year Olds (N=11,120)		For 21 Year Olds (N=8,416)	
	Variable	Mean	Std. Dev.	Mean	Std. Dev.
Extended Foster Care Policy	Federal EFC at 18	0.51	0.50	0.51	0.50
	State EFC at 18	0.24	0.43	0.23	0.42
	Average Number of Years with Federal EFC			1.92	1.74
	Average Number of Years with State EFC			0.86	1.35
NYTD Cohort	Cohort 1 (17 in FY2011)	0.47		0.46	
	Cohort 2 (17 in FY2014)	0.53	0.50	0.54	0.50
Demographic Characteristics	Female	0.54	0.50	0.57	0.50
	Non-Hispanic White	0.42	0.49	0.41	0.49
	Non-Hispanic Black	0.30	0.46	0.30	0.46
	Non-Hispanic Other	0.08	0.27	0.08	0.28
	Hispanic	0.20	0.40	0.21	0.40
	Ever diagnosed with a disability	0.58	0.49	0.58	0.49
Experiences at 17	Ever been homeless	0.17	0.38	0.17	0.38
	Employed at 17	0.15	0.36	0.15	0.36
	Ever been incarcerated	0.27	0.44	0.26	0.44
	Ever been referred for substance abuse	0.23	0.42	0.22	0.41
Foster Care History	Total removals from home as a child	1.39	0.66	1.39	0.67
	Total placements as a child	7.16	7.15	7.13	6.98
	Cumulative length of stay in foster care as a child (in years)	4.43	3.65	4.44	3.64
	Age at first removal	11.72	4.76	11.72	4.72
	Age at last removal	17.28	1.98	17.27	1.99
First Placement	Kinship Care	0.16	0.37	0.16	0.37
	Foster home	0.49	0.50	0.50	0.50
	Group home	0.29	0.45	0.28	0.45
	Other	0.06	0.23	0.06	0.24
Ever removed for… These do not add up to 100% because a child may be removed for multiple reasons.	Abuse	0.27	0.45	0.27	0.45
	Neglect	0.56	0.50	0.56	0.50
	Parental Incarceration	0.06	0.24	0.06	0.24
	Parental Substance Abuse	0.19	0.39	0.19	0.39
	Inadequate Housing	0.10	0.30	0.10	0.30
	Child-related issue	0.32	0.47	0.31	0.46
	Kinship Care	0.12	0.32	0.12	0.33

	Variable	For 19 Year Olds (N=11,120) Mean	Std. Dev.	For 21 Year Olds (N=8,416) Mean	Std. Dev.
Last Placement Setting under 18	Foster home	0.44	0.50	0.45	0.50
	Group home	0.29	0.45	0.28	0.45
	Other	0.16		0.15	
State Controls (3-Year Average)	Unemployment Rate	6.69	1.86	5.22	1.31
	Poverty Rate	13.98	2.65	12.99	2.69
	Income per Capita (in 2016 USD)	$48,115	$7,509	$50,146	$8,065
	Gross State Product (in millions of 2016 USD)	$855,344	$904,501	$939,415	$986,356
	TANF Recipients (per 1,000 people)	15.50	13.55	13.61	12.49
	Child-only TANF Recipients (per 1,000 children)	10.82	7.56	9.53	6.35
	Monthly TANF Benefit for 3-person family	$500	$197	$500	$200
	SNAP Recipients (per 1,000 people)	137.04	33.17	130.63	31.88
	Monthly SNAP Benefit for 1-person household	$202	$11	$194	$9
	Medicaid Beneficiaries (per 1,000 people)	202.28	54.09	218.15	64.18
Outcomes	Supervised Independent Living	0.23	0.42	0.24	0.42
	Foster Care	0.40	0.49	0.21	0.41
	Uses ILP Services	0.86	0.35	0.78	0.42
	Homelessness	0.20	0.40	0.37	0.48
	Enrolled in high school	0.29	0.45	0.06	0.24
	Finished high school/GED	0.56	0.50	0.81	0.40
	Enrolled in college/post-secondary education	0.28	0.45	0.27	0.44
	Employed	0.38	0.49	0.56	0.50
	Disconnected	0.25	0.43	0.30	0.46
	Incarceration	0.19	0.39	0.28	0.45

Notes: The sample is restricted to foster youth who completed the NYTD survey at 19 and/or 21 years old and are not missing demographic information, foster care history, nor outcomes. Less than one percent of the observations are missing the indicator for high school graduation at age 19. The summary statistics do not vary much when restricting the sample to the youth that are not missing this variable and so I report the results of the larger sample. The similarity in demographic characteristics and foster care history across ages 19 and 21 indicates similar youth responded to the survey in both years. This table includes the three-year average state-level controls, in addition to those already presented in Table 1.

Appendix Table C2: Summary Statistics for NYTD Participants by Treatment

		No EFC (N=2,804)		State EFC (N=2,670)		Federal EFC (N=5,646)	
		Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
NYTD Cohort	Cohort 1 (17 in FY2011)	0.73		0.42		0.36	
	Cohort 2 (17 in FY2014)	0.27	0.44	0.58	0.49	0.64	0.48
Demographic Characteristics	Female	0.53	0.50	0.54	0.50	0.55	0.50
	Non-Hispanic White	0.52	0.50	0.50	0.50	0.34	0.47
	Non-Hispanic Black	0.27	0.44	0.33	0.47	0.30	0.46
	Non-Hispanic Other	0.08	0.28	0.08	0.27	0.08	0.27
	Hispanic	0.13	0.33	0.10	0.29	0.28	0.45
	Ever diagnosed with a disability	0.52	0.50	0.45	0.50	0.66	0.47
Experiences at 17	Ever been homeless	0.19	0.39	0.17	0.38	0.16	0.37
	Employed at 17	0.15	0.36	0.17	0.37	0.14	0.35
	Ever been incarcerated	0.31	0.46	0.30	0.46	0.23	0.42
	Ever been referred for substance abuse	0.24	0.43	0.23	0.42	0.22	0.41
Foster Care History	Total removals from home as a child	1.35	0.63	1.40	0.67	1.40	0.67
	Total placements as a child	8.08	8.52	7.18	7.24	6.70	6.26
	Cumulative length of stay in foster care as a child (in years)	4.24	3.44	3.87	3.09	4.79	3.95
	Age at first removal	12.16	4.46	12.33	4.40	11.22	5.00
	Age at last removal	17.08	1.77	17.07	1.69	17.47	2.18
First Placement	Kinship Care	0.15	0.36	0.12	0.33	0.19	0.39
	Foster home	0.51	0.50	0.48	0.50	0.49	0.50
	Group home	0.29	0.45	0.34	0.48	0.26	0.44
	Other	0.05	0.21	0.06	0.23	0.06	0.24
Ever removed for… These do not add up to 100% because a child may be removed for multiple reasons.	Abuse	0.26	0.44	0.29	0.45	0.27	0.44
	Neglect	0.53	0.50	0.51	0.50	0.59	0.49
	Parental Incarceration	0.08	0.27	0.07	0.26	0.04	0.20
	Parental Substance Abuse	0.20	0.40	0.22	0.41	0.17	0.37
	Inadequate Housing	0.12	0.32	0.11	0.32	0.08	0.28

		No EFC (N=2,804)		State EFC (N=2,670)		Federal EFC (N=5,646)	
		Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	Child-related issue	0.35	0.48	0.41	0.49	0.27	0.45
Last Placement Setting under 18	Kinship Care	0.10	0.30	0.09	0.29	0.14	0.35
	Foster home	0.45	0.50	0.43	0.50	0.43	0.50
	Group home	0.28	0.45	0.31	0.46	0.28	0.45
	Other	0.17		0.17		0.15	
State Controls (3-Year Average)	Unemployment Rate	7.03	1.86	6.26	1.82	6.73	1.83
	Poverty Rate	14.36	2.89	13.61	2.94	13.96	2.34
	Income per Capita (in 2016 USD)	$43,924	$4,619	$45,768	$7,659	$51,306	$7,156
	Gross State Product (in millions of 2016 USD)	$394,817	$453,910	$309,506	$224,578	$1,342,184	$1,000,992
	TANF Recipients (per 1,000 people)	9.51	7.17	7.78	3.51	22.13	15.51
	Child-only TANF Recipients (per 1,000 children)	8.42	5.86	7.03	3.55	13.80	8.42
	Monthly TANF Benefit for 3-person family	$404	$136	$404	$152	$594	$197
	SNAP Recipients (per 1,000 people)	143.85	34.57	141.24	35.84	131.68	30.11
	Monthly SNAP Benefit for 1-person household	$206	$11	$202	$10	$200	$11
	Medicaid Beneficiaries (per 1,000 people)	174.19	53.16	178.89	39.78	227.28	48.32
Outcomes	Supervised Independent Living	0.13	0.34	0.13	0.33	0.32	0.47
	Foster Care	0.19	0.39	0.15	0.36	0.61	0.49
	Uses ILP Services	0.85	0.36	0.86	0.35	0.86	0.35
	Homelessness	0.22	0.41	0.21	0.41	0.18	0.38
	Enrolled in high school	0.27	0.45	0.29	0.46	0.29	0.46
	Finished high school/GED	0.57	0.50	0.51	0.50	0.57	0.49
	Enrolled in college	0.28	0.45	0.24	0.43	0.30	0.46
	Employed	0.37	0.48	0.41	0.49	0.38	0.48

	No EFC (N=2,804)		State EFC (N=2,670)		Federal EFC (N=5,646)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Disconnected	0.26	0.44	0.27	0.44	0.24	0.43
Incarceration	0.20	0.40	0.20	0.40	0.17	0.38

Notes: This table reports the summary statistics by treatment status for youth in the 19-year-old analytical sample.

Appendix Table C3: Differences in Controlling for and Omitting the State Policy

	Outcomes at 19 Years Old		
	(1)	(2)	(3)
	Main Results	Omit State Policy	Combine State and Federal Policy
	Outcome: Homelessness		
Fed EFC at 18	-0.048*	-0.037	
	(0.025)	(0.026)	
State EFC at 18	-0.015		
	(0.021)		
Any EFC at 18			-0.021
			(0.022)
Observations	11,120	11,120	11,120
Adjusted R-squared	0.088	0.088	0.088
	Outcome: Incarceration		
Fed EFC at 18	-0.053*	-0.039*	
	(0.029)	(0.023)	
State EFC at 18	-0.021		
	(0.016)		
Any EFC at 18			-0.027*
			(0.013)
Observations	11,120	11,120	11,120
Adjusted R-squared	0.197	0.196	0.196
	Outcome: Disconnected		
Fed EFC at 18	-0.043**	-0.055***	
	(0.020)	(0.014)	
State EFC at 18	0.017		
	(0.015)		
Any EFC at 18			0.006
			(0.020)
Observations	11,120	11,120	11,120
Adjusted R-squared	0.049	0.049	0.048
	Outcome: High School Enrollment		
Fed EFC at 18	0.052*	0.056**	
	(0.029)	(0.021)	
State EFC at 18	-0.005		
	(0.022)		
Any EFC at 18			0.004
			(0.028)
Observations	11,097	11,097	11,097
Adjusted R-squared	0.041	0.041	0.040
	Outcome: College Enrollment		
Fed EFC at 18	0.010	0.035	
	(0.038)	(0.039)	
State EFC at 18	-0.041		
	(0.025)		

Any EFC at 18			-0.029
			(0.026)
Observations	6,155	6,155	6,155
Adjusted R-squared	0.083	0.082	0.083
	Outcome: Employment		
Fed EFC at 18	0.083***	0.047**	
	(0.028)	(0.019)	
State EFC at 18	0.051***		
	(0.018)		
Any EFC at 18			0.056***
			(0.016)
Observations	11,120	11,120	11,120
Adjusted R-squared	0.045	0.045	0.045
	Outcomes at 21 Years Old		
	(1)	(2)	(3)
	Main Results	Omit State Policy	Combine State and Federal Policy
	Outcome: Homelessness		
Years with Fed EFC	-0.026*	-0.021*	
	(0.014)	(0.012)	
Years with State EFC	-0.030***		
	(0.010)		
Years with Any EFC			-0.030***
			(0.010)
Observations	8,416	8,416	8,416
Adjusted R-squared	0.139	0.138	0.139
	Outcome: Incarceration		
Years with Fed EFC	-0.035***	-0.031***	
	(0.007)	(0.006)	
Years with State EFC	-0.019**		
	(0.008)		
Years with Any EFC			-0.029***
			(0.006)
Observations	8,416	8,416	8,416
Adjusted R-squared	0.234	0.234	0.234
	Outcome: Disconnected		
Years with Fed EFC	-0.031**	-0.028*	
	(0.013)	(0.015)	
Years with State EFC	-0.019		
	(0.012)		
Years with Any EFC			-0.024**
			(0.009)
Observations	8,416	8,416	8,416
Adjusted R-squared	0.062	0.062	0.062
	Outcome: High School Graduation		
Years with Fed EFC	0.014**	0.016***	

	(0.006)	(0.006)	
Years with State EFC	-0.012		
	(0.009)		
Years with Any EFC			0.003
			(0.007)
Observations	8,416	8,416	8,416
Adjusted R-squared	0.069	0.069	0.068
Outcome: College Enrollment			
Years with Fed EFC	0.045**	0.043**	
	(0.019)	(0.019)	
Years with State EFC	0.014		
	(0.013)		
Years with Any EFC			0.027*
			(0.016)
Observations	8,416	8,416	8,416
Adjusted R-squared	0.143	0.143	0.142
Outcome: Employment			
Years with Fed EFC	-0.028***	-0.029***	
	(0.010)	(0.010)	
Years with State EFC	0.009		
	(0.012)		
Years with Any EFC			-0.007
			(0.011)
Observations	8,416	8,416	8,416
Adjusted R-squared	0.066	0.066	0.066

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects. The abbreviation EFC is shorthand for extended foster care. "Fed" and "State" indicate how the program is funded. The first column reports the main results again for easy reference, the second column reports the results when the state EFC variable is omitted, and the third column reports the results when the federal and state policy are combined, effectively a state either has EFC or not.

Appendix Table C4: Measuring the Full Policy Potential

Outcomes at 21 Years Old					
(1)		(2)		(3)	
Effect of having EFC at Age 18		Marginal Effect of an Additional Year Exposed to EFC		Policy Effect of being Exposed to EFC	
Outcome: Homelessness					
Fed EFC at 18	-0.061**	Years exposed to Fed EFC	-0.026*	1 Year exposed to Fed EFC	-0.003
	(0.029)		(0.014)		(0.054)
State EFC at 18	-0.038	Years exposed to State EFC	-0.030***	2 Years exposed to Fed EFC	-0.087*
	(0.023)		(0.010)		(0.044)
				3 Years exposed to Fed EFC	-0.133***
					(0.048)
				4 Years exposed to Fed EFC	-0.144***
					(0.046)
				1 Year exposed to State EFC	0.017
					(0.027)
				2 Years exposed to State EFC	-0.056
					(0.036)
				3 Years exposed to State EFC	-0.074**
					(0.033)
				4 Years exposed to State EFC	-0.138***
					(0.039)
Mean of Control Group (No EFC at 18)	0.418	Mean of Control Group (No Policy Ever)	0.444	Mean of Control Group (No Policy Ever)	0.444
Adjusted R-Squared	0.138	Adjusted R-Squared	0.139	Adjusted R-Squared	0.139
Outcome: Incarceration					

Fed EFC at 18	-0.086***	Years exposed to Fed EFC	-0.035***	1 Year exposed to Fed EFC	-0.023
	(0.019)		(0.007)		(0.049)
State EFC at 18	-0.039	Years exposed to State EFC	-0.019**	2 Years exposed to Fed EFC	-0.086
	(0.026)		(0.008)		(0.071)
				3 Years exposed to Fed EFC	-0.137**
					(0.067)
				4 Years exposed to Fed EFC	-0.160**
					(0.065)
				1 Year exposed to State EFC	0.016
					(0.028)
				2 Years exposed to State EFC	-0.006
					(0.041)
				3 Years exposed to State EFC	-0.032
					(0.037)
				4 Years exposed to State EFC	-0.082**
					(0.035)
Mean of Control Group (No EFC at 18)	0.308	Mean of Control Group (No Policy Ever)	0.295	Mean of Control Group (No Policy Ever)	0.295
Adjusted R-Squared	0.233	Adjusted R-Squared	0.234	Adjusted R-Squared	0.234
Outcome: Disconnected					
Fed EFC at 18	-0.053*	Years exposed to Fed EFC	-0.031**	1 Year exposed to Fed EFC	-0.092**
	(0.031)		(0.013)		(0.039)
State EFC at 18	-0.046*	Years exposed to State EFC	-0.019	2 Years exposed to Fed EFC	0.033

	(0.025)		(0.012)		(0.050)
				3 Years exposed to Fed EFC	-0.024
					(0.054)
				4 Years exposed to Fed EFC	-0.064
					(0.055)
				1 Year exposed to State EFC	0.022
					(0.032)
				2 Years exposed to State EFC	0.012
					(0.035)
				3 Years exposed to State EFC	-0.048
					(0.040)
				4 Years exposed to State EFC	-0.052
					(0.042)
Mean of Control Group (No EFC at 18)	0.321	Mean of Control Group (No Policy Ever)	0.321	Mean of Control Group (No Policy Ever)	0.321
Adjusted R-Squared	0.061	Adjusted R-Squared	0.062	Adjusted R-Squared	0.063
Outcome: High School Graduation					
Fed EFC at 18	0.000	Years exposed to Fed EFC	0.014**	1 Year exposed to Fed EFC	0.007
	(0.022)		(0.006)		(0.042)
State EFC at 18	-0.035	Years exposed to State EFC	-0.012	2 Years exposed to Fed EFC	0.044
	(0.021)		(0.009)		(0.042)
				3 Years exposed to Fed EFC	0.036
					(0.037)

				4 Years exposed to Fed EFC	0.053
					(0.038)
				1 Year exposed to State EFC	-0.020
					(0.025)
				2 Years exposed to State EFC	-0.016
					(0.035)
				3 Years exposed to State EFC	-0.043
					(0.037)
				4 Years exposed to State EFC	-0.050
					(0.038)
Mean of Control Group (No EFC at 18)	0.821	Mean of Control Group (No Policy Ever)	0.798	Mean of Control Group (No Policy Ever)	0.798
Adjusted R-Squared	0.069	Adjusted R-Squared	0.069	Adjusted R-Squared	0.068
Outcome: College Enrollment					
Fed EFC at 18	-0.010	Years exposed to Fed EFC	0.045**	1 Year exposed to Fed EFC	0.063**
	(0.043)		(0.019)		(0.029)
State EFC at 18	-0.012	Years exposed to State EFC	0.014	2 Years exposed to Fed EFC	-0.053
	(0.026)		(0.013)		(0.038)
				3 Years exposed to Fed EFC	-0.051
					(0.044)
				4 Years exposed to Fed EFC	0.033
					(0.045)
				1 Year exposed to State EFC	0.010

					(0.020)
				2 Years exposed to State EFC	0.002
					(0.020)
				3 Years exposed to State EFC	-0.051
					(0.034)
				4 Years exposed to State EFC	0.003
					(0.036)
Mean of Control Group (No EFC at 18)	0.249	Mean of Control Group (No Policy Ever)	0.214	Mean of Control Group (No Policy Ever)	0.214
Adjusted R-Squared	0.141	Adjusted R-Squared	0.143	Adjusted R-Squared	0.146
Outcome: Employment					
Fed EFC at 18	0.001	Years exposed to Fed EFC	-0.028***	1 Year exposed to Fed EFC	-0.014
	(0.039)		(0.010)		(0.045)
State EFC at 18	0.035	Years exposed to State EFC	0.009	2 Years exposed to Fed EFC	-0.060
	(0.034)		(0.012)		(0.050)
				3 Years exposed to Fed EFC	-0.082
					(0.050)
				4 Years exposed to Fed EFC	-0.114**
					(0.050)
				1 Year exposed to State EFC	-0.033
					(0.039)
				2 Years exposed to State EFC	-0.036
					(0.055)

				3 Years exposed to State EFC	0.031
					(0.056)
				4 Years exposed to State EFC	0.032
					(0.051)
Mean of Control Group (No EFC at 18)	0.550	Mean of Control Group (No Policy Ever)	0.561	Mean of Control Group (No Policy Ever)	0.561
Adjusted R-Squared	0.066	Adjusted R-Squared	0.066	Adjusted R-Squared	0.066

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects. The abbreviation EFC is shorthand for extended foster care. "Fed" and "State" indicate how the program is funded. The first column reports the results using a binary indicator for whether EFC was available when the youth turned 18. The second column reports the marginal effect of an additional year exposed to EFC. The final column reports the fixed effect for the number of years exposed. If the effect were identical over time, then results in column 1 would be similar to results at 3 and 4 years in column 3. If the effect were perfectly linear, then the results in column 2 multiplied by 3 would be the same as the results at year 3 in column 3.

Appendix Table C5: Regression Results Testing the Impact of Extended Foster Care at Age 17

	Outcomes at 19 Years Old (N=11,120)					
	Homelessness	Incarceration	Disconnectedness	High School Enrollment	College Enrollment	Employment
Fed EFC at 18	-0.048*	-0.053*	-0.043**	0.052*	0.010	0.083***
	(0.025)	(0.029)	(0.020)	(0.029)	(0.038)	(0.028)
State EFC at 18	-0.015	-0.021	0.017	-0.005	-0.041	0.051***
	(0.021)	(0.016)	(0.015)	(0.022)	(0.025)	(0.018)
Fed EFC at 17	0.011	0.006	-0.071***	0.013	0.041	0.059**
	(0.023)	(0.029)	(0.014)	(0.032)	(0.052)	(0.025)
State EFC at 17	-0.022	-0.026	-0.004	0.007	-0.073**	0.014
	(0.025)	(0.023)	(0.027)	(0.024)	(0.036)	(0.030)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects. The abbreviation EFC is shorthand for extended foster care. "Fed" and "State" indicate how the program is funded. The main results are presented in the first panel for ease of comparison. The second panel presents results when the independent variable is an indicator for EFC at 17 years old, as opposed to 18. The adjusted R-squared is similar across both models. See the main results for the adjusted R-squared.

Appendix Table C6: Regression Results from Alternative Specifications

	Outcomes at 19 Years Old			
	(1)	(2)	(3)	(4)
	Main Results	DDD Results	Probit - Marginal Effects	Logit - Odds Ratio
	Outcome: Homelessness			
Fed EFC at 18	-0.048*	-0.049	-0.047*	0.715*
	(0.025)	(0.029)	(0.027)	(0.140)
State EFC at 18	-0.015	0.022	-0.020	0.868
	(0.021)	(0.024)	(0.021)	(0.133)
Observations	11,120	11,120	11,120	11,120
Adjusted R-squared	0.088	0.089		
	Outcome: Incarceration			
Fed EFC at 18	-0.053*	-0.039	-0.053**	0.650**
	(0.029)	(0.024)	(0.025)	(0.136)
State EFC at 18	-0.021	-0.030	-0.024	0.831
	(0.016)	(0.021)	(0.015)	(0.101)
Observations	11,120	11,120	11,120	11,120
Adjusted R-squared	0.197	0.198		
	Outcome: Disconnected			
Fed EFC at 18	-0.043**	-0.022	-0.042**	0.783**
	(0.020)	(0.028)	(0.019)	(0.081)
State EFC at 18	0.017	0.038**	0.018	1.100
	(0.015)	(0.016)	(0.015)	(0.092)
Observations	11,120	11,120	11,120	11,120
Adjusted R-squared	0.049	0.049		
	Outcome: High School Enrollment			
Fed EFC at 18	0.052*	0.081***	0.057*	1.325
	(0.029)	(0.020)	(0.032)	(0.227)
State EFC at 18	-0.005	-0.003	0.002	1.000
	(0.022)	(0.016)	(0.026)	(0.132)
Observations	11,097	11,097	11,097	11,097
Adjusted R-squared	0.040	0.043		
	Outcome: College Enrollment			
Fed EFC at 18	0.010	-0.010	0.008	1.036
	(0.038)	(0.045)	(0.039)	(0.178)
State EFC at 18	-0.041	-0.052	-0.044*	0.824
	(0.025)	(0.038)	(0.026)	(0.098)
Observations	6,155	6,155	6,150	6,150
Adjusted R-squared	0.083	0.085		
	Outcome: Employment			
Fed EFC at 18	0.083***	0.052***	0.087***	1.485***
	(0.028)	(0.014)	(0.029)	(0.196)
State EFC at 18	0.051***	0.034**	0.054***	1.286***
	(0.018)	(0.014)	(0.019)	(0.113)

Observations	11,120	11,120	11,120	11,120
Adjusted R-squared	0.045	0.047		
Outcomes at 21 Years Old				
	(1)	(2)	(3)	(4)
	Main Results	DDD Results	Probit - Marginal Effects	Logit - Odds Ratio
Outcome: Homelessness				
Years with Fed EFC	-0.026*	-0.026	-0.026*	0.875*
	(0.014)	(0.018)	(0.014)	(0.061)
Years with State EFC	-0.030***	-0.062***	-0.029***	0.860***
	(0.010)	(0.014)	(0.010)	(0.044)
Observations	8,416	8,416	8,416	8,416
Adjusted R-squared	0.139	0.142		
Outcome: Incarceration				
Years with Fed EFC	-0.035***	-0.043***	-0.035***	0.796***
	(0.007)	(0.009)	(0.008)	(0.042)
Years with State EFC	-0.019**	-0.048***	-0.018**	0.885**
	(0.008)	(0.015)	(0.008)	(0.047)
Observations	8,416	8,416	8,416	8,416
Adjusted R-squared	0.234	0.237		
Outcome: Disconnected				
Years with Fed EFC	-0.031**	-0.046***	-0.032**	0.845**
	(0.013)	(0.010)	(0.014)	(0.060)
Years with State EFC	-0.019	-0.008	-0.019	0.908
	(0.012)	(0.019)	(0.012)	(0.057)
Observations	8,416	8,416	8,416	8,416
Adjusted R-squared	0.062	0.065		
Outcome: High School Graduation				
Years with Fed EFC	0.014**	0.011	0.014**	1.107***
	(0.006)	(0.007)	(0.006)	(0.043)
Years with State EFC	-0.012	-0.000	-0.012	0.919
	(0.009)	(0.013)	(0.010)	(0.069)
Observations	8,416	8,416	8,411	8,411
Adjusted R-squared	0.069	0.071		
Outcome: College Enrollment				
Years with Fed EFC	0.045**	0.067***	0.051***	1.291***
	(0.019)	(0.014)	(0.019)	(0.124)
Years with State EFC	0.014	0.028**	0.017	1.092
	(0.013)	(0.011)	(0.014)	(0.081)
Observations	8,416	8,416	6,870	6,870
Adjusted R-squared	0.143	0.146		
Outcome: Employment				
Years with Fed EFC	-0.028***	-0.029***	-0.028***	0.884***
	(0.010)	(0.011)	(0.010)	(0.038)
Years with State EFC	0.009	-0.006	0.010	1.047

	(0.012)	(0.016)	(0.011)	(0.052)
Observations	8,416	8,416	8,416	8,416
Adjusted R-squared	0.066	0.068		

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, and experiences at 17 years old, state controls, and include cohort and state fixed effects, unless otherwise noted. The abbreviation EFC is shorthand for extended foster care. "Fed" and "State" indicate how the program is funded. The first column reports the main results again for easy reference. The second column reports the results from a triple differences specification, so it includes a cohort by state fixed effect and does not include state controls. The third column reports marginal effects from a probit model, and the fourth column reports the odds ratio from the logit model.

Appendix Table C7: Regression Results Changing the Set of Control Variables

	Outcomes at 19 Years Old						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Main Results	Excludes all Controls	Excludes Foster Care History Controls	Excludes Controls for Experiences at 17 Years Old	Excludes State Controls	Excludes State Fixed Effects	Excludes Cohort Fixed Effects
Outcome: Homelessness							
Fed EFC at 18	-0.048*	-0.023	-0.044*	-0.042	-0.035	-0.011	-0.049*
	(0.025)	(0.023)	(0.024)	(0.026)	(0.026)	(0.021)	(0.025)
State EFC at 18	-0.015	-0.018	-0.020	-0.012	-0.014	0.002	-0.016
	(0.021)	(0.023)	(0.021)	(0.022)	(0.022)	(0.016)	(0.021)
Observations	11,120	11,120	11,120	11,120	11,120	11,120	11,120
Adjusted R-squared	0.088	0.023	0.070	0.061	0.086	0.075	0.088
Outcome: Incarceration							
Fed EFC at 18	-0.053*	-0.049	-0.051*	-0.053	-0.049*	0.024	-0.054*
	(0.029)	(0.031)	(0.028)	(0.033)	(0.026)	(0.022)	(0.028)
State EFC at 18	-0.021	-0.034*	-0.025	-0.017	-0.024	0.014	-0.021
	(0.016)	(0.017)	(0.016)	(0.018)	(0.016)	(0.018)	(0.016)
Observations	11,120	11,120	11,120	11,120	11,120	11,120	11,120
Adjusted R-squared	0.197	0.030	0.184	0.117	0.196	0.183	0.197
Outcome: Disconnected							
Fed EFC at 18	-0.043**	-0.034	-0.042**	-0.043**	-0.037*	0.012	-0.039*
	(0.020)	(0.020)	(0.020)	(0.021)	(0.020)	(0.018)	(0.020)
State EFC at 18	0.017	0.016	0.012	0.017	0.020	0.021	0.018
	(0.015)	(0.016)	(0.015)	(0.015)	(0.016)	(0.015)	(0.014)
Observations	11,120	11,120	11,120	11,120	11,120	11,120	11,120
Adjusted R-squared	0.049	0.015	0.036	0.042	0.048	0.043	0.049
Outcome: High School Enrollment							
Fed EFC at 18	0.052*	0.052*	0.053*	0.050*	0.053*	0.031	0.054*

	(0.029)	(0.027)	(0.028)	(0.029)	(0.028)	(0.025)	(0.027)
State EFC at 18	-0.005	-0.007	-0.003	-0.007	-0.008	0.017	-0.004
	(0.022)	(0.025)	(0.021)	(0.022)	(0.026)	(0.025)	(0.021)
Observations	11,097	11,097	11,097	11,097	11,097	11,097	11,097
Adjusted R-squared	0.040	0.041	0.040	0.033	0.040	0.038	0.040
Outcome: College Enrollment							
Fed EFC at 18	0.010	-0.009	0.009	0.003	-0.002	-0.007	0.005
	(0.038)	(0.036)	(0.037)	(0.039)	(0.037)	(0.028)	(0.038)
State EFC at 18	-0.041	-0.032	-0.040	-0.040	-0.036	-0.044**	-0.044*
	(0.025)	(0.023)	(0.026)	(0.026)	(0.024)	(0.020)	(0.024)
Observations	6,155	6,155	6,155	6,155	6,155	6,155	6,155
Adjusted R-squared	0.083	0.039	0.074	0.075	0.083	0.070	0.083
Outcome: Employment							
Fed EFC at 18	0.083***	0.070***	0.083***	0.087***	0.074***	0.006	0.076***
	(0.028)	(0.023)	(0.027)	(0.029)	(0.025)	(0.020)	(0.026)
State EFC at 18	0.051***	0.051**	0.055***	0.053***	0.049**	0.020	0.048***
	(0.018)	(0.020)	(0.017)	(0.018)	(0.020)	(0.018)	(0.017)
Observations	11,120	11,120	11,120	11,120	11,120	11,120	11,120
Adjusted R-squared	0.045	0.015	0.039	0.033	0.045	0.040	0.045
Outcomes at 21 Years Old							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Main Results	Excludes all Controls	Excludes Foster Care History Controls	Excludes Controls for Experiences at 17 Years Old	Excludes State Controls	Excludes State Fixed Effects	Excludes Cohort Fixed Effects
Outcome: Homelessness							
Years with Fed EFC	-0.026*	-0.036***	-0.030**	-0.029**	-0.029**	-0.017**	-0.027*
	(0.014)	(0.012)	(0.012)	(0.014)	(0.014)	(0.008)	(0.014)
Years with State EFC	-0.030***	-0.034**	-0.035***	-0.031***	-0.031***	-0.007	-0.030***
	(0.010)	(0.014)	(0.011)	(0.010)	(0.011)	(0.007)	(0.010)

Observations	8,416	8,416	8,416	8,416	8,416	8,416	8,416
Adjusted R-squared	0.139	0.035	0.114	0.117	0.139	0.125	0.139
			Outcome: Incarceration				
Years with Fed EFC	-0.035***	-0.050***	-0.037***	-0.040***	-0.037***	0.004	-0.035***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.009)	(0.007)
Years with State EFC	-0.019**	-0.021	-0.023***	-0.018*	-0.016	0.004	-0.018**
	(0.008)	(0.013)	(0.008)	(0.009)	(0.010)	(0.007)	(0.008)
Observations	8,416	8,416	8,416	8,416	8,416	8,416	8,416
Adjusted R-squared	0.234	0.041	0.218	0.166	0.233	0.217	0.234
			Outcome: Disconnected				
Years with Fed EFC	-0.031**	-0.037**	-0.033**	-0.033**	-0.033**	0.001	-0.033**
	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)	(0.008)	(0.013)
Years with State EFC	-0.019	-0.014	-0.022*	-0.019	-0.013	-0.003	-0.017
	(0.012)	(0.014)	(0.012)	(0.012)	(0.013)	(0.006)	(0.012)
Observations	8,416	8,416	8,416	8,416	8,416	8,416	8,416
Adjusted R-squared	0.062	0.017	0.058	0.051	0.061	0.051	0.061
			Outcome: High School Graduation				
Years with Fed EFC	0.014**	0.018***	0.014***	0.015**	0.017**	-0.004	0.013**
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)
Years with State EFC	-0.012	-0.006	-0.010	-0.012	-0.009	-0.006	-0.012
	(0.009)	(0.012)	(0.009)	(0.009)	(0.012)	(0.008)	(0.009)
Observations	8,416	8,416	8,416	8,416	8,416	8,416	8,416
Adjusted R-squared	0.069	0.031	0.056	0.059	0.068	0.049	0.069
			Outcome: College Enrollment				
Years with Fed EFC	0.045**	0.052***	0.046**	0.047**	0.045**	0.007	0.047**
	(0.019)	(0.019)	(0.019)	(0.018)	(0.018)	(0.010)	(0.018)
Years with State EFC	0.014	0.013	0.015	0.014	0.013	0.011	0.012
	(0.013)	(0.020)	(0.013)	(0.013)	(0.016)	(0.008)	(0.012)
Observations	8,416	8,416	8,416	8,416	8,416	8,416	8,416
Adjusted R-squared	0.143	0.044	0.141	0.138	0.142	0.121	0.143
			Outcome: Employment				

Years with Fed EFC	-0.028***	-0.018*	-0.026***	-0.026**	-0.022**	-0.010**	-0.027**
	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)	(0.005)	(0.010)
Years with State EFC	0.009	0.004	0.012	0.009	0.004	-0.006	0.008
	(0.012)	(0.010)	(0.011)	(0.012)	(0.010)	(0.007)	(0.012)
Observations	8,416	8,416	8,416	8,416	8,416	8,416	8,416
Adjusted R-squared	0.066	0.013	0.062	0.058	0.066	0.060	0.066

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. The abbreviation EFC is shorthand for extended foster care. "Fed" and "State" indicate how the program is funded. The first column reports the main results again for easy reference. The main results regression controls for demographic characteristics, foster care history, and experiences at 17 years old, state controls, and include cohort and state fixed effects. The remaining columns indicate which set of controls are excluded from the regression.

Appendix Table C8: Regression Results Changing the Set of States in the Sample

	Outcomes at 19 Years Old			
	(1)	(2)	(3)	(4)
	Main Results	Sample of treated states only	Sample excludes states that went from state to federal policy	Sample excludes states with state policy
	Outcome: Homelessness			
Fed EFC at 18	-0.048*	-0.053*	-0.082***	-0.078***
	(0.025)	(0.026)	(0.016)	(0.012)
State EFC at 18	-0.015	-0.002	0.019	
	(0.021)	(0.020)	(0.024)	
Observations	11,120	6,851	9,610	7,639
Adjusted R-squared	0.088	0.082	0.090	0.092
	Outcome: Incarceration			
Fed EFC at 18	-0.053*	-0.056*	-0.002	-0.028
	(0.029)	(0.032)	(0.006)	(0.020)
State EFC at 18	-0.021	-0.027	-0.023	
	(0.016)	(0.020)	(0.016)	
Observations	11,120	6,851	9,610	7,639
Adjusted R-squared	0.197	0.208	0.197	0.203
	Outcome: Disconnected			
Fed EFC at 18	-0.043**	-0.015	-0.057***	-0.043
	(0.020)	(0.027)	(0.011)	(0.026)
State EFC at 18	0.017	0.039**	0.025*	
	(0.015)	(0.016)	(0.014)	
Observations	11,120	6,851	9,610	7,639
Adjusted R-squared	0.049	0.049	0.047	0.050
	Outcome: High School Enrollment			
Fed EFC at 18	0.052*	0.061**	0.100***	0.057**
	(0.029)	(0.025)	(0.007)	(0.027)
State EFC at 18	-0.005	0.007	-0.042*	
	(0.022)	(0.016)	(0.025)	
Observations	11,097	6,835	9,594	7,625
Adjusted R-squared	0.040	0.047	0.044	0.026
	Outcome: College Enrollment			
Fed EFC at 18	0.010	-0.010	0.039***	0.043
	(0.038)	(0.043)	(0.011)	(0.034)
State EFC at 18	-0.041	-0.049	-0.089***	
	(0.025)	(0.035)	(0.016)	
Observations	6,155	3,936	5,417	4,315
Adjusted R-squared	0.083	0.090	0.078	0.061
	Outcome: Employment			
Fed EFC at 18	0.083***	0.069**	0.040***	0.061***
	(0.028)	(0.025)	(0.007)	(0.019)

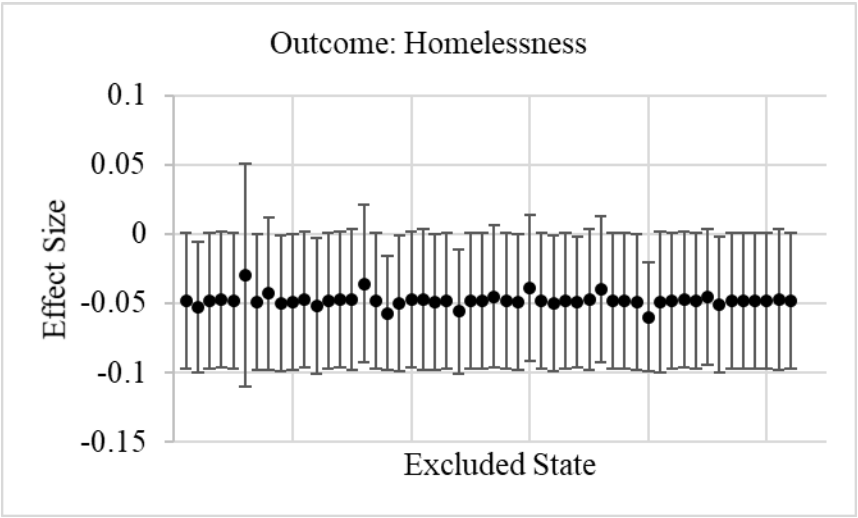
State EFC at 18	0.051***	0.032*	0.074***	
	(0.018)	(0.016)	(0.022)	
Observations	11,120	6,851	9,610	7,639
Adjusted R-squared	0.045	0.049	0.043	0.039
		Outcomes at 21 Years Old		
	(1)	(2)	(3)	(4)
	Main Results	Sample of treated states only	Sample excludes states that went from state to federal policy	Sample excludes states with state policy
		Outcome: Homelessness		
Years with Fed EFC	-0.026*	-0.014	-0.027	-0.024
	(0.014)	(0.012)	(0.019)	(0.014)
Years with State EFC	-0.030***	-0.034**	-0.035***	
	(0.010)	(0.013)	(0.011)	
Observations	8,416	5,037	7,467	5,827
Adjusted R-squared	0.139	0.126	0.145	0.143
		Outcome: Incarceration		
Years with Fed EFC	-0.035***	-0.039***	-0.035***	-0.031***
	(0.007)	(0.007)	(0.007)	(0.007)
Years with State EFC	-0.019**	-0.040***	-0.021**	
	(0.008)	(0.012)	(0.010)	
Observations	8,416	5,037	7,467	5,827
Adjusted R-squared	0.234	0.244	0.228	0.242
		Outcome: High School Graduation		
Years with Fed EFC	0.014**	0.018**	0.007	0.017**
	(0.006)	(0.008)	(0.005)	(0.007)
Years with State EFC	-0.012	-0.022*	-0.008	
	(0.009)	(0.012)	(0.011)	
Observations	8,416	5,037	7,467	5,827
Adjusted R-squared	0.069	0.068	0.063	0.070
		Outcome: Disconnected		
Years with Fed EFC	-0.031**	-0.037**	-0.040***	-0.036***
	(0.013)	(0.016)	(0.011)	(0.011)
Years with State EFC	-0.019	-0.028**	-0.011	
	(0.012)	(0.012)	(0.012)	
Observations	8,416	5,037	7,467	5,827
Adjusted R-squared	0.062	0.062	0.063	0.061
		Outcome: College Enrollment		
Years with Fed EFC	0.045**	0.054**	0.067***	0.049**
	(0.019)	(0.022)	(0.016)	(0.019)
Years with State EFC	0.014	0.027**	-0.003	
	(0.013)	(0.011)	(0.010)	
Observations	8,416	5,037	7,467	5,827
Adjusted R-squared	0.143	0.147	0.145	0.126
		Outcome: Employment		

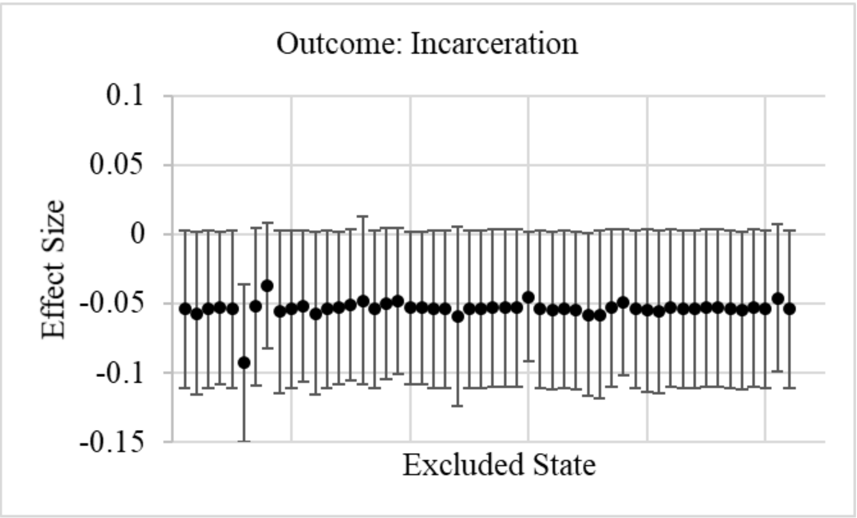
Years with Fed EFC	-0.028***	-0.035***	-0.037***	-0.025**
	(0.010)	(0.010)	(0.010)	(0.010)
Years with State EFC	0.009	0.013	0.016	
	(0.012)	(0.012)	(0.012)	
Observations	8,416	5,037	7,467	5,827
Adjusted R-squared	0.066	0.073	0.065	0.065

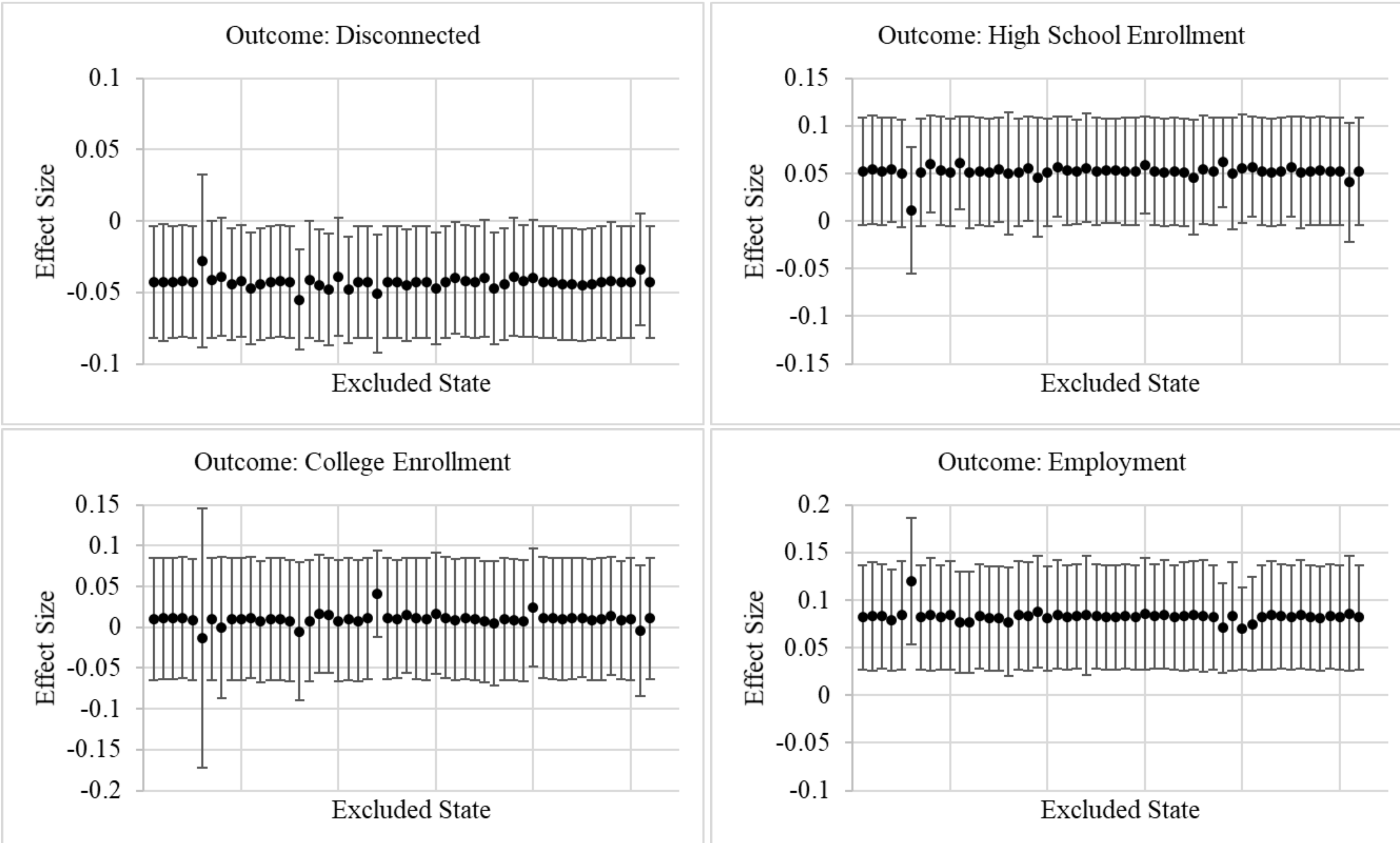
Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects. The abbreviation EFC is shorthand for extended foster care. "Fed" and "State" indicate how the program is funded. The first column reports the main results again for easy reference, the second column limits the sample to the 22 treated states, the third column excludes the 7 states that changed from a state to federal policy, and the fourth column excludes the 19 states with state policies.

Appendix Figure C1: Graphical Display of Effect Size for Outcomes at Age 19 Omitting One State at a Time

Each graph plots the effect size (in percentage points) and the 95 percent confidence interval for the federally-funded extended foster care indicator variable for each outcome at age 19. There are 52 estimates plotted in each graph. The first estimate (left most) is the main result, and the remaining 51 are the results when a single state is omitted from the analysis. States are dropped in alphabetical order, so the sixth estimate is the result when California is excluded.

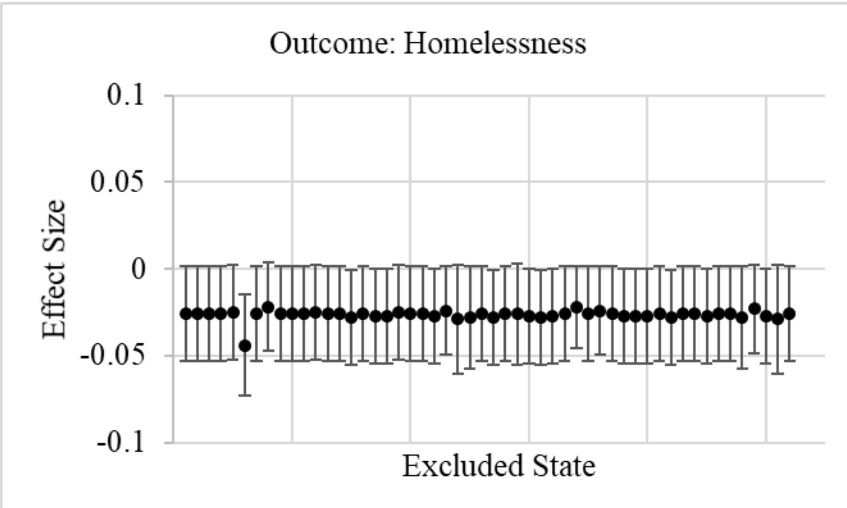


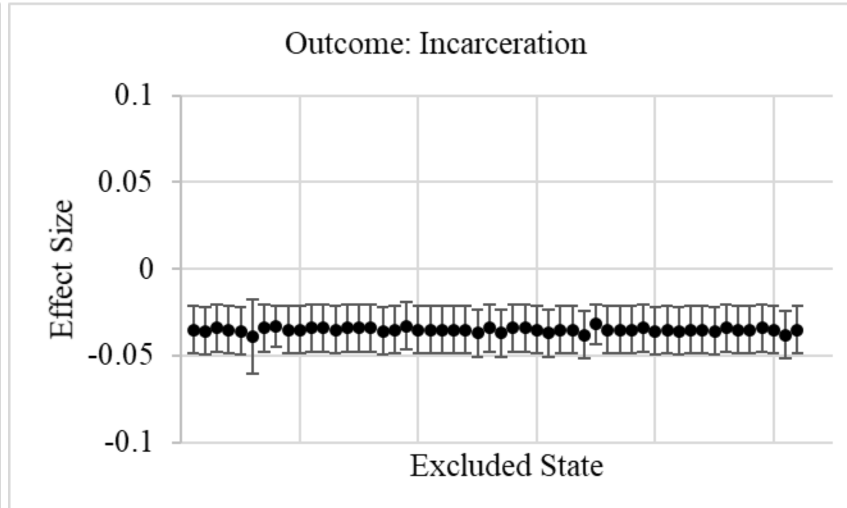


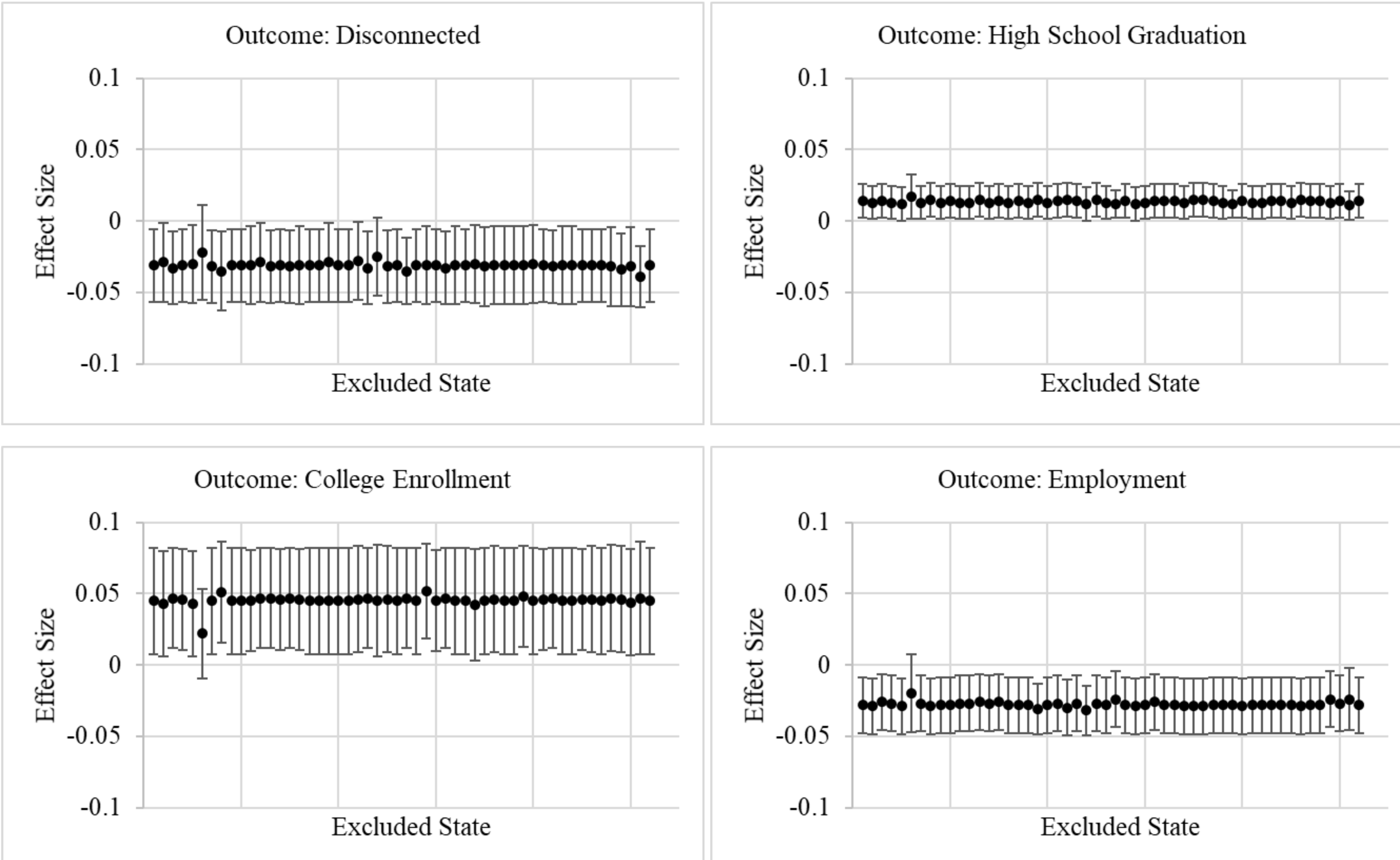

Outcome: Disconnected
0.1
0.05
0
-0.05
-0.1
Effect Size
Excluded State
Outcome: High School Enrollment
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
Effect Size
Excluded State
Outcome: College Enrollment
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
Effect Size
Excluded State
Outcome: Employment
0.2
0.15
0.1
0.05
0
-0.05
-0.1
Effect Size
Excluded State

Appendix Figure C2: Graphical Display of Effect Size for Outcomes at Age 21 Omitting One State at a Time

Each graph plots the marginal effect size (in percentage points) and the 95 percent confidence interval for the federally-funded extended foster care counter variable for each outcome at age 21. There are 52 estimates plotted in each graph. The first estimate (left most) is the main result, and the remaining 51 are the results when a single state is omitted from the analysis. States are dropped in alphabetical order, so the sixth estimate is the result when California is excluded.






Outcome: Disconnected
Effect Size
0.1
0.05
0
-0.05
-0.1
Excluded State
Outcome: High School Graduation
Effect Size
0.1
0.05
0
-0.05
-0.1
Excluded State
Outcome: College Enrollment
Effect Size
0.1
0.05
0
-0.05
-0.1
Excluded State
Outcome: Employment
Effect Size
0.1
0.05
0
-0.05
-0.1
Excluded State

Appendix Table C9: Regression Results Changing the Sample Size

	Outcomes at 19 Years Old		
	(1)	(2)	(3)
	Main Results	Sample varies by outcome measure	Sample limited to those who participated in survey at 21
	Outcome: Homelessness		
Fed EFC at 18	-0.048*	-0.041	-0.054**
	(0.025)	(0.026)	(0.024)
State EFC at 18	-0.015	-0.010	-0.015
	(0.021)	(0.021)	(0.028)
Observations	11,120	11,420	7,994
Adjusted R-squared	0.088	0.087	0.094
	Outcome: Incarceration		
Fed EFC at 18	-0.053*	-0.048*	-0.057***
	(0.029)	(0.026)	(0.018)
State EFC at 18	-0.021	-0.019	-0.014
	(0.016)	(0.015)	(0.016)
Observations	11,120	11,697	7,994
Adjusted R-squared	0.197	0.214	0.184
	Outcome: Disconnected		
Fed EFC at 18	-0.043**	-0.050**	-0.064***
	(0.020)	(0.021)	(0.013)
State EFC at 18	0.017	0.009	0.024*
	(0.015)	(0.014)	(0.013)
Observations	11,120	11,498	7,994
Adjusted R-squared	0.049	0.048	0.050
	Outcome: High School Enrollment		
Fed EFC at 18	0.052*	0.057*	0.065***
	(0.029)	(0.029)	(0.020)
State EFC at 18	-0.005	-0.004	-0.027
	(0.022)	(0.022)	(0.021)
Observations	11,097	11,485	7,980
Adjusted R-squared	0.040	0.040	0.044
	Outcome: College Enrollment		
Fed EFC at 18	0.010	0.012	0.043
	(0.038)	(0.039)	(0.039)
State EFC at 18	-0.041	-0.037	-0.001
	(0.025)	(0.024)	(0.026)
Observations	6,155	6,362	4,780
Adjusted R-squared	0.083	0.083	0.077
	Outcome: Employment		
Fed EFC at 18	0.083***	0.080***	0.070*
	(0.028)	(0.026)	(0.037)

State EFC at 18	0.051***	0.050***	0.032
	(0.018)	(0.016)	(0.021)
Observations	11,120	11,915	7,994
Adjusted R-squared	0.045	0.047	0.048
Outcomes at 21 Years Old			
	(1)	(2)	(3)
	Main Results	Sample varies by outcome measure	Sample limited to those who participated in survey at 19
Outcome: Homelessness			
Years with Fed EFC	-0.026*	-0.058***	-0.020
	(0.014)	(0.012)	(0.015)
Years with State EFC	-0.030***	-0.042***	-0.022**
	(0.010)	(0.010)	(0.011)
Observations	8,416	9,435	7,994
Adjusted R-squared	0.139	0.145	0.124
Outcome: Incarceration			
Years with Fed EFC	-0.035***	-0.064***	-0.030***
	(0.007)	(0.008)	(0.008)
Years with State EFC	-0.019**	-0.037***	-0.004
	(0.008)	(0.010)	(0.008)
Observations	8,416	9,470	7,994
Adjusted R-squared	0.234	0.273	0.217
Outcome: High School Graduation			
Years with Fed EFC	0.014**	0.048**	0.018***
	(0.006)	(0.021)	(0.006)
Years with State EFC	-0.012	-0.029***	-0.016*
	(0.009)	(0.010)	(0.008)
Observations	8,416	14,165	7,994
Adjusted R-squared	0.069	0.104	0.066
Outcome: Disconnected			
Years with Fed EFC	-0.031**	-0.028**	-0.022
	(0.013)	(0.013)	(0.014)
Years with State EFC	-0.019	-0.017	-0.012
	(0.012)	(0.012)	(0.012)
Observations	8,416	10,189	7,994
Adjusted R-squared	0.062	0.059	0.060
Outcome: College Enrollment			
Years with Fed EFC	0.045**	0.037**	0.042**
	(0.019)	(0.015)	(0.019)
Years with State EFC	0.014	0.014	0.013
	(0.013)	(0.010)	(0.014)
Observations	8,416	12,354	7,994
Adjusted R-squared	0.143	0.180	0.142
Outcome: Employment			

Years with Fed EFC	-0.028***	-0.017	-0.036***
	(0.010)	(0.011)	(0.010)
Years with State EFC	0.009	0.013	0.003
	(0.012)	(0.010)	(0.012)
Observations	8,416	10,407	7,994
Adjusted R-squared	0.066	0.072	0.064

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects. The abbreviation EFC is shorthand for extended foster care. "Fed" and "State" indicate how the program is funded. The first column reports the main results again for easy reference. The main analysis sample is restricted to youth that are linked across NYTD and AFCARS and not missing any of the above outcomes. The second column lets the sample size vary by outcome measure, and the third column limits the sample to youth who participated in all three waves of the NYTD survey.

Appendix Table C10: Characteristics of NYTD Survey Participants

	Full Sample (N=15,733)		All Surveys (N=9,349)		Returned (N=1,696)		Drop-out (N=2,705)		No Surveys (N=1,983)	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Any EFC at 18	0.733	0.443	0.748	0.434	0.686	0.464	0.747	0.435	0.680	0.466
Fed EFC at 18	0.484	0.500	0.514	0.500	0.420	0.494	0.471	0.499	0.415	0.493
State EFC at 18	0.249	0.432	0.235	0.424	0.266	0.442	0.276	0.447	0.266	0.442
NYTD Cohort 2014	0.520	0.500	0.534	0.499	0.481	0.500	0.524	0.500	0.482	0.500
Female	0.513	0.500	0.551	0.497	0.514	0.500	0.445	0.497	0.429	0.495
Non-Hispanic White	0.426	0.494	0.414	0.493	0.415	0.493	0.443	0.497	0.466	0.499
Non-Hispanic Black	0.297	0.457	0.305	0.460	0.312	0.463	0.296	0.457	0.249	0.432
Non-Hispanic Other Race	0.0828	0.276	0.0829	0.276	0.0825	0.275	0.0791	0.270	0.0877	0.283
Hispanic	0.194	0.396	0.198	0.399	0.191	0.393	0.182	0.386	0.197	0.398
Homeless at 17	0.177	0.382	0.170	0.376	0.201	0.401	0.173	0.378	0.198	0.399
Employment at 17	0.144	0.351	0.153	0.360	0.141	0.348	0.133	0.340	0.119	0.324
Incarcerated at 17	0.307	0.461	0.264	0.441	0.376	0.485	0.336	0.472	0.407	0.491
Referred for substance abuse at 17	0.251	0.434	0.220	0.414	0.293	0.455	0.275	0.446	0.331	0.471
Total time in foster care under 18 (in years)	4.299	3.604	4.477	3.674	4.029	3.515	4.302	3.605	3.685	3.249

	Full Sample (N=15,733)		All Surveys (N=9,349)		Returned (N=1,696)		Drop-out (N=2,705)		No Surveys (N=1,983)	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Total removals from home under 18	1.398	0.673	1.391	0.662	1.428	0.714	1.399	0.676	1.402	0.684
Total number of placements under 18	7.311	7.499	7.197	7.223	7.837	8.417	7.436	7.395	7.224	8.050
Ever removed for abuse	0.260	0.439	0.279	0.449	0.227	0.419	0.244	0.429	0.221	0.415
Ever removed for neglect	0.538	0.499	0.559	0.497	0.500	0.500	0.531	0.499	0.482	0.500
Ever removed for parental incarceration	0.0587	0.235	0.0600	0.238	0.0560	0.230	0.0591	0.236	0.0545	0.227
Ever removed for parental substance abuse	0.184	0.387	0.191	0.393	0.182	0.386	0.176	0.381	0.160	0.367
Ever removed for inadequate housing	0.0974	0.296	0.101	0.301	0.0861	0.281	0.0972	0.296	0.0918	0.289
Ever removed for child-related issue	0.350	0.477	0.321	0.467	0.390	0.488	0.372	0.484	0.424	0.494
Ever diagnosed with disability	0.571	0.495	0.581	0.493	0.551	0.498	0.569	0.495	0.548	0.498
Age at first removal	11.85	4.736	11.67	4.765	12.20	4.655	11.79	4.808	12.49	4.497
Age at most recent placement	17.27	1.876	17.27	1.991	17.26	1.643	17.30	1.939	17.24	1.337

	Full Sample (N=15,733)		All Surveys (N=9,349)		Returned (N=1,696)		Drop-out (N=2,705)		No Surveys (N=1,983)	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
First Placement: Foster home	0.634	0.482	0.658	0.474	0.598	0.490	0.615	0.487	0.575	0.494
First Placement: Group home or institution	0.305	0.460	0.286	0.452	0.327	0.469	0.326	0.469	0.345	0.476
Monthly foster care maintenance payment as an adult	$1,507	$2,530	$1,461	$2,431	$1,541	$2,633	$1,613	$2,658	$1,555	$2,710

Notes: "Full sample" consists of all NYTD and AFCARS records that were successfully linked. Youth that participated in all surveys answered at least one question at ages 17, 19, and 21. "Returned" means youth completed the survey at ages 17 and 21, but not at age 19. "Drop-out" means youth completed the survey at ages 17 and 19, but not at age 21. "No surveys" means youth with records that were successfully linked across NYTD and AFCARS did not answer any questions in the surveys at ages 17, 19, nor 21.

Appendix Table C11: Results from NYTD Participation Regression

	Participated at 19	Participated at 21
Fed EFC	0.032	0. 175***
	(0.045)	(0.044)
State EFC	0.031	0. 102***
	(0.028)	(0.021)
Average Participation Rate without EFC	0.72	0.66
Observations	15,733	15,733
R-Squared	0.0526	0.1004

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. Regressions include a dummy variable for sample states, and cohort and state fixed effects. Sample states only followed up with the youth randomly selected. Positive coefficients suggest NYTD participation is positively correlated with EFC.

Appendix Table C12: Full Set of Results from Techniques that Address Non-Response

	Outcomes at 19 Years Old				
	(1)	(2)	(3)	(4)	(5)
	Main Results	Inversely Weighted by State Survey Participation Rate	Inversely Weighted by Individual Response Rate	Mean Imputed	Regression Imputed
	Outcome: Homelessness				
Fed EFC at 18	-0.048*	-0.043	-0.046**	-0.034*	-0.030*
	(0.025)	(0.029)	(0.022)	(0.018)	(0.018)
State EFC at 18	-0.015	-0.016	-0.008	-0.005	-0.010
	(0.021)	(0.021)	(0.023)	(0.015)	(0.017)
Observations	11,120	11,120	11,120	15,733	15,733
Adjusted R-squared	0.088	0.088	0.087	0.062	0.124
	Outcome: Incarceration				
Fed EFC at 18	-0.053*	-0.059*	-0.046	-0.038**	-0.038*
	(0.029)	(0.029)	(0.028)	(0.016)	(0.022)
State EFC at 18	-0.021	-0.023	-0.012	-0.008	-0.014
	(0.016)	(0.016)	(0.017)	(0.011)	(0.013)
Observations	11,120	11,120	11,120	15,733	15,733
Adjusted R-squared	0.197	0.197	0.193	0.155	0.281
	Outcome: Disconnected				
Fed EFC at 18	-0.043**	-0.046**	-0.047***	-0.037**	-0.047***
	(0.020)	(0.021)	(0.016)	(0.014)	(0.015)
State EFC at 18	0.017	0.016	0.009	0.007	0.003
	(0.015)	(0.015)	(0.012)	(0.009)	(0.014)
Observations	11,120	11,120	11,120	15,733	15,733
Adjusted R-squared	0.049	0.049	0.046	0.035	0.070
	Outcome: High School Enrollment				
Fed EFC at 18	0.052*	0.049	0.054*	0.038**	0.035
	(0.029)	(0.030)	(0.032)	(0.018)	(0.022)
State EFC at 18	-0.005	-0.005	-0.012	-0.013	-0.022
	(0.022)	(0.022)	(0.022)	(0.017)	(0.022)
Observations	11,097	11,097	11,097	15,733	15,733
Adjusted R-squared	0.040	0.041	0.039	0.029	0.063
	Outcome: College Enrollment				
Fed EFC at 18	0.010	0.014	0.012	0.011	0.016
	(0.038)	(0.039)	(0.036)	(0.037)	(0.037)
State EFC at 18	-0.041	-0.040	-0.038	-0.025	-0.029
	(0.025)	(0.025)	(0.027)	(0.024)	(0.022)
Observations	6,155	6,155	6,155	6,657	6,657
Adjusted R-squared	0.083	0.084	0.081	0.080	0.089
	Outcome: Employment				

Fed EFC at 18	0.083***	0.093***	0.070***	0.050**	0.069**
	(0.028)	(0.033)	(0.023)	(0.021)	(0.027)
State EFC at 18	0.051***	0.051***	0.058***	0.031**	0.043**
	(0.018)	(0.019)	(0.018)	(0.012)	(0.016)
Observations	11,120	11,120	11,120	15,733	15,733
Adjusted R-squared	0.045	0.046	0.047	0.036	0.065

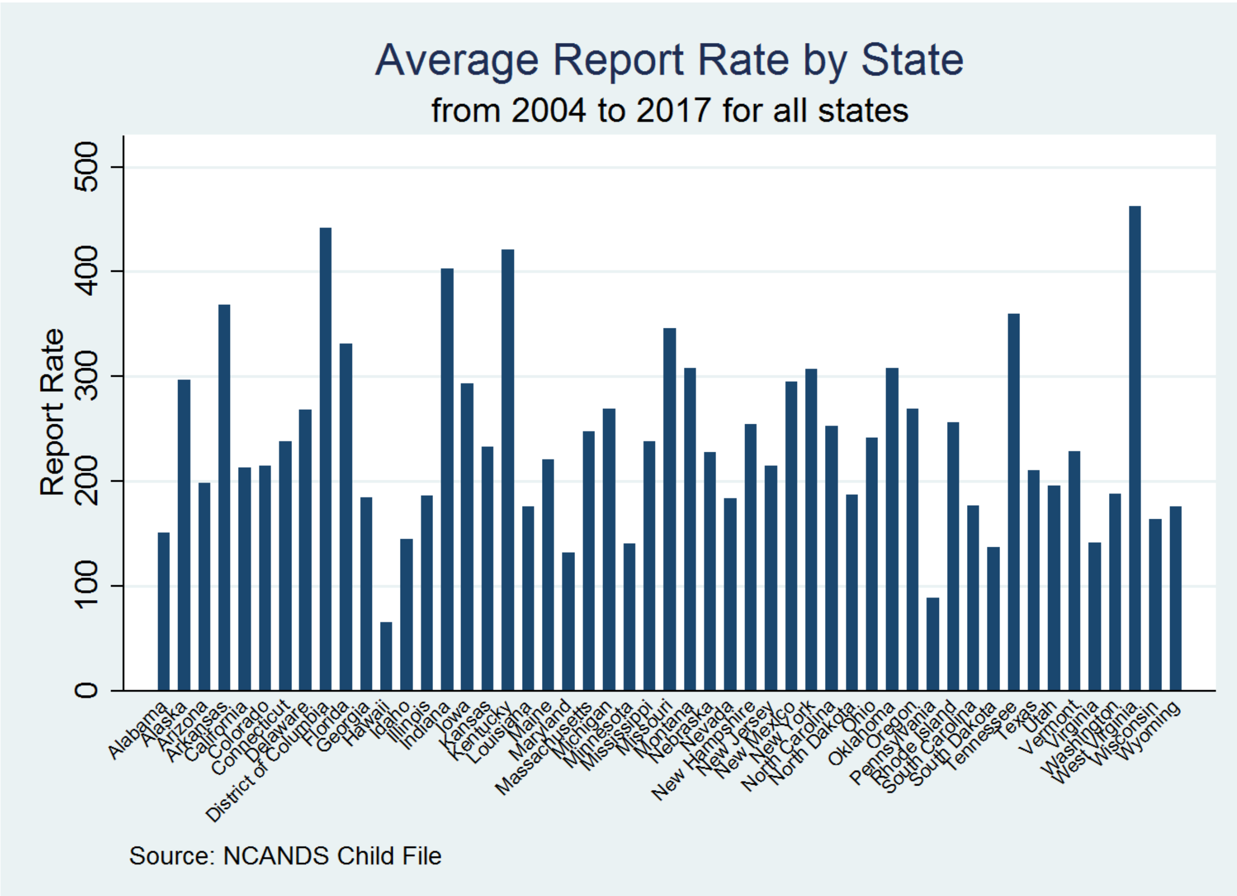
	Outcomes at 21 Years Old				
	(1)	(2)	(3)	(4)	(5)
	Main Results	Inversely Weighted by State Survey Participation Rate	Inversely Weighted by Individual Response Rate	Mean Imputed	Regression Imputed
Outcome: Homelessness					
Years with Fed EFC	-0.026*	-0.029*	-0.023	-0.052***	-0.055***
	(0.014)	(0.015)	(0.014)	(0.007)	(0.009)
Years with State EFC	-0.030***	-0.033***	-0.028***	-0.028***	-0.035***
	(0.010)	(0.011)	(0.010)	(0.008)	(0.009)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.139	0.140	0.132	0.099	0.214
Outcome: Incarceration					
Years with Fed EFC	-0.035***	-0.039***	-0.036***	-0.066***	-0.058***
	(0.007)	(0.008)	(0.006)	(0.017)	(0.007)
Years with State EFC	-0.019**	-0.021**	-0.018**	-0.033***	-0.033***
	(0.008)	(0.008)	(0.008)	(0.010)	(0.009)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.234	0.238	0.231	0.206	0.388
Outcome: Disconnected					
Years with Fed EFC	-0.031**	-0.029**	-0.033**	-0.021*	-0.017
	(0.013)	(0.013)	(0.012)	(0.011)	(0.013)
Years with State EFC	-0.019	-0.020*	-0.018	-0.015**	-0.019*
	(0.012)	(0.011)	(0.012)	(0.007)	(0.010)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.062	0.062	0.059	0.041	0.088
Outcome: High School Graduation					
Years with Fed EFC	0.014**	0.014**	0.013**	0.047***	0.049***
	(0.006)	(0.006)	(0.006)	(0.014)	(0.017)
Years with State EFC	-0.012	-0.016	-0.011	-0.008	-0.013
	(0.009)	(0.010)	(0.009)	(0.008)	(0.009)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.069	0.069	0.067	0.077	0.100
Outcome: College Enrollment					
Years with Fed EFC	0.045**	0.043**	0.045**	0.036*	0.035**
	(0.019)	(0.018)	(0.018)	(0.018)	(0.015)

Years with State EFC	0.014	0.015	0.013	0.013	0.013
	(0.013)	(0.012)	(0.013)	(0.009)	(0.010)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.143	0.142	0.152	0.091	0.148
			Outcome: Employment		
Years with Fed EFC	-0.028***	-0.025**	-0.026**	-0.007	-0.012
	(0.010)	(0.010)	(0.011)	(0.007)	(0.011)
Years with State EFC	0.009	0.007	0.008	0.015*	0.022**
	(0.012)	(0.011)	(0.011)	(0.008)	(0.009)
Observations	8,416	8,416	8,416	15,733	15,733
Adjusted R-squared	0.066	0.067	0.060	0.056	0.100

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the state level are in parentheses. All regressions control for demographic characteristics, foster care history, experiences at 17 years old, state controls, and include cohort and state fixed effects, unless otherwise noted. The abbreviation EFC is shorthand for extended foster care. "Fed" and "State" indicate how the program is funded. The first column reports the main results again for easy reference, the second and third columns report estimates weighted by response rate at the state and individual level, respectively. The fourth and fifth columns report results from imputation methods. These regressions also control for missingness. This table is similar to Table 5, but includes the coefficient on state EFC as well.

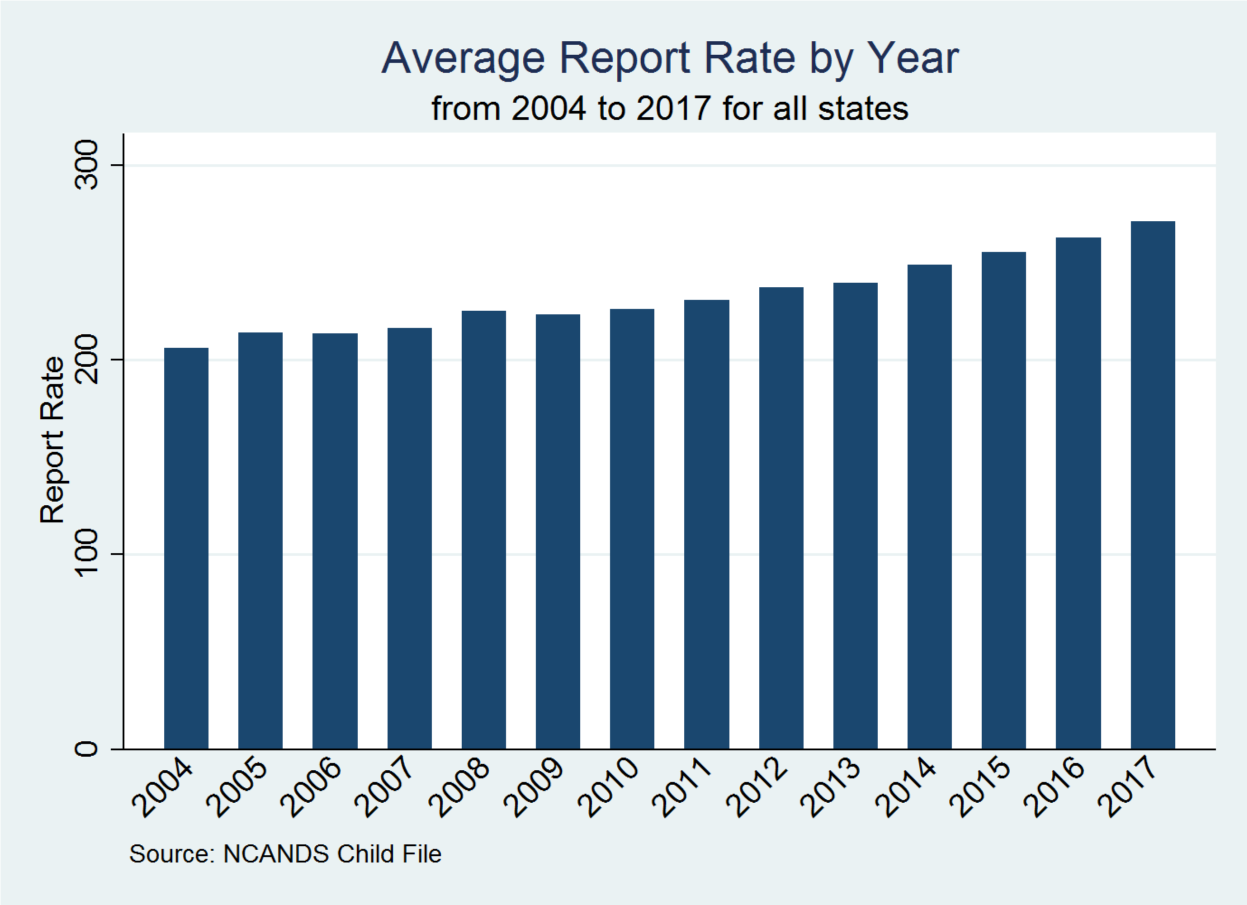
Appendix D – Appendix Tables and Figures for Chapter 2

Appendix Figure D1: Average Report Rate by State



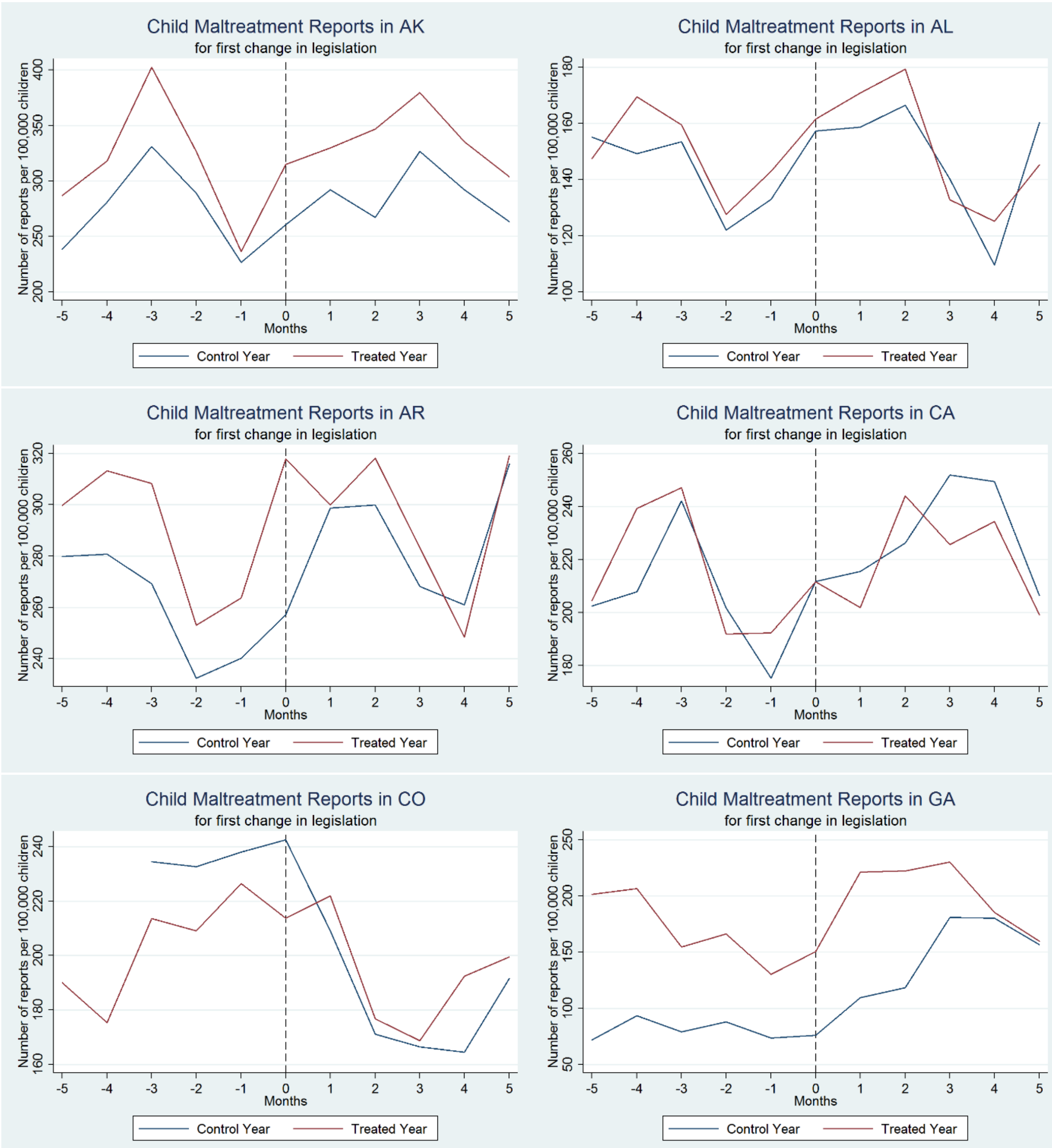
Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all years.

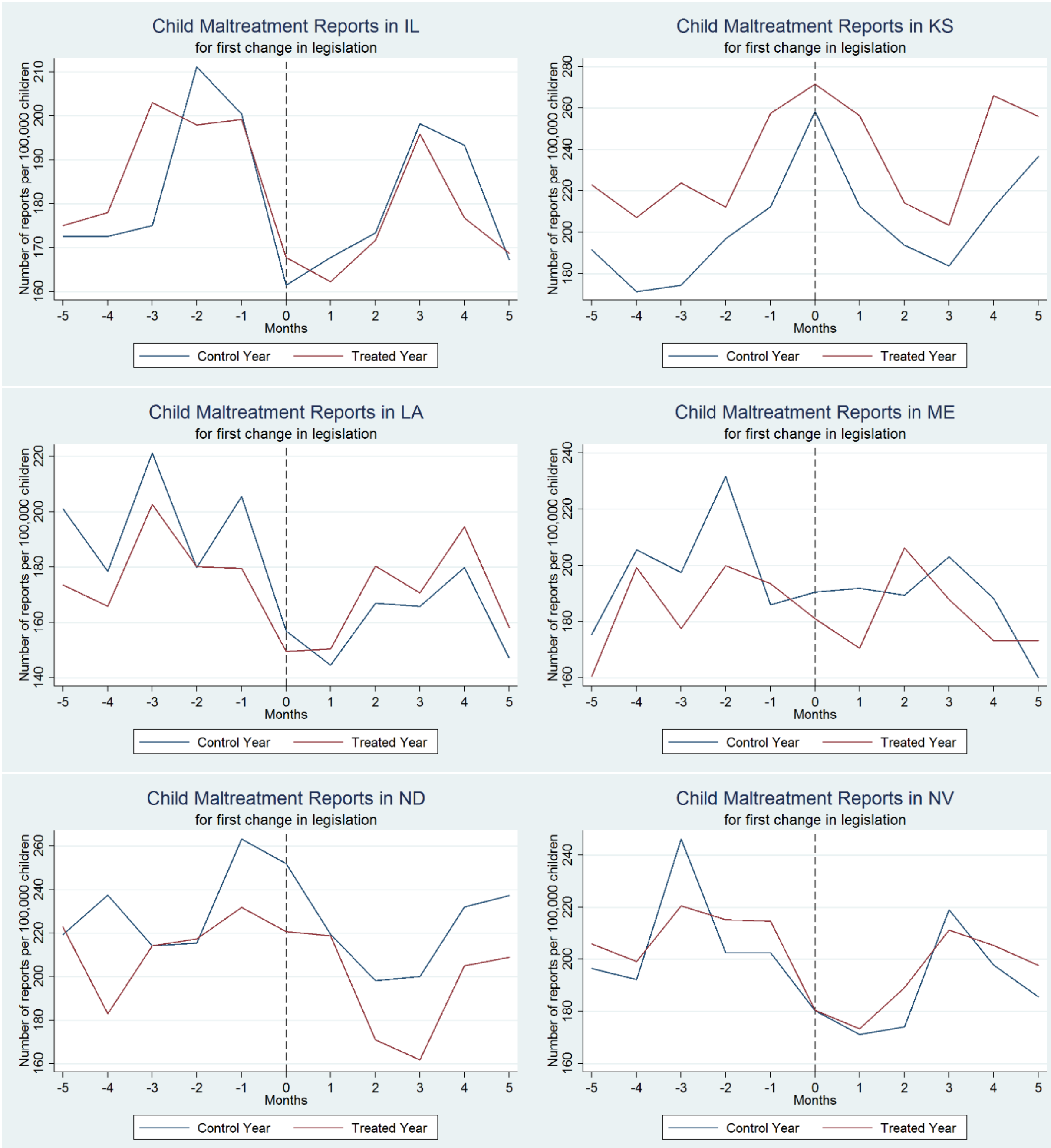
Appendix Figure D2: Average Report Rate by Year

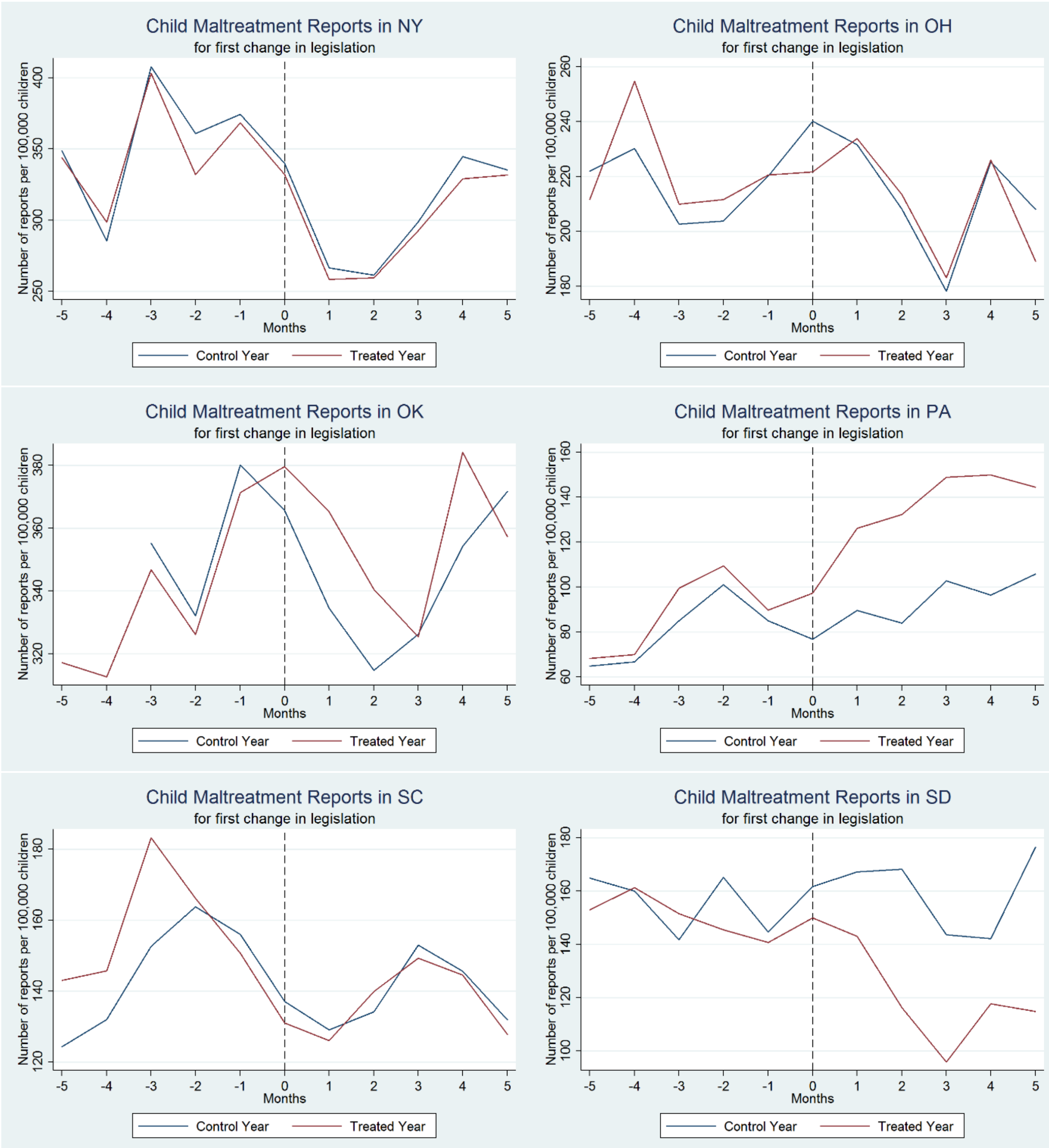


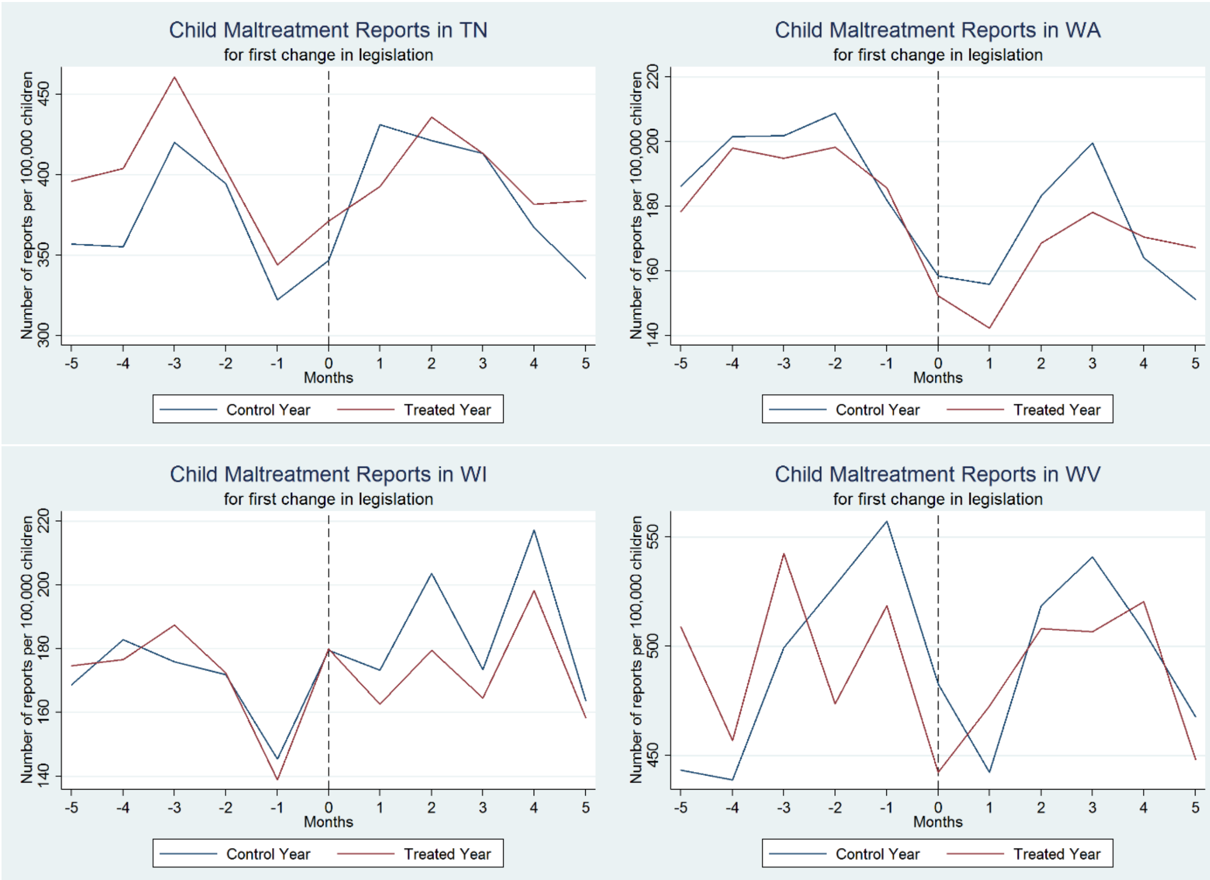
Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all states and is weighted by the state population.

Appendix Figure D3: Selected Charts of the Report Rate Before and After the First Mandatory Reporter Legislation Change, Relative to the Previous Year




Child Maltreatment Reports in IL
for first change in legislation
Number of reports per 100,000 children
160 170 180 190 200 210
-5 -4 -3 -2 -1 0 1 2 3 4 5
Months
Control Year
Treated Year
Child Maltreatment Reports in KS
for first change in legislation
Number of reports per 100,000 children
180 200 220 240 260 280
-5 -4 -3 -2 -1 0 1 2 3 4 5
Months
Control Year
Treated Year
Child Maltreatment Reports in LA
for first change in legislation
Number of reports per 100,000 children
140 160 180 200 220
-5 -4 -3 -2 -1 0 1 2 3 4 5
Months
Control Year
Treated Year
Child Maltreatment Reports in ME
for first change in legislation
Number of reports per 100,000 children
160 180 200 220 240
-5 -4 -3 -2 -1 0 1 2 3 4 5
Months
Control Year
Treated Year
Child Maltreatment Reports in ND
for first change in legislation
Number of reports per 100,000 children
160 180 200 220 240 260
-5 -4 -3 -2 -1 0 1 2 3 4 5
Months
Control Year
Treated Year
Child Maltreatment Reports in NV
for first change in legislation
Number of reports per 100,000 children
160 180 200 220 240
-5 -4 -3 -2 -1 0 1 2 3 4 5
Months
Control Year
Treated Year


Child Maltreatment Reports in NY
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in OH
for first change in legislation
Child Maltreatment Reports in OK
for first change in legislation
Child Maltreatment Reports in PA
for first change in legislation
Child Maltreatment Reports in SC
for first change in legislation
Child Maltreatment Reports in SD
for first change in legislation



Notes: These charts plot the report rate, per 100,000 children, the five months before and after the mandatory reporter legislation change in the year the change occurred (treated year), relative to the previous year (control year) for a handful of states. Pennsylvannia is a good example of what we would expect to see if mandatory reporter legilsation led to an increase in reporting, and South Dakota is a good example of what we would expect if mandatory reporter legislation led to a decrease in reporting.

Appendix Table D1: Group Effects for Mandatory Reporter Policy Changes

Outcome Var.	Overall Effect	Group (1)	(2)	(3)	(4)	(5)
Reports (per 100,000 children)	12.90*	19.91	12.97	-3.913	20.05*	3.958
	(7.408)	(12.00)	(18.44)	(8.426)	(11.18)	(2.825)
Substantiated reports (per 100,000 children)	1.862	-0.648	0.432	-3.723	6.754	2.469***
	(2.363)	(4.014)	(2.852)	(3.317)	(4.287)	(0.883)
Unsubstantiated reports (per 100,000 children)	11.03*	20.55**	12.54	-0.190	13.29*	1.489
	(5.711)	(9.308)	(17.05)	(5.393)	(7.059)	(2.235)
Number of children investigated (per 100,000 children)	28.76	20.09	31.02	-8.825	51.92	11.96**
	(19.06)	(23.36)	(42.52)	(14.97)	(33.15)	(5.461)
Number of victims (per 100,000 children)	2.756	-3.597	0.648	-6.262	11.47	2.011
	(4.257)	(6.242)	(4.587)	(5.381)	(8.434)	(1.471)
Observations	8,422	5,398	5,376	4,872	4,704	4,200
Number of Treated States	27	9	8	5	4	1

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each cell reports the beta coefficient and standard error for the outcome-group regression analysis combination. The first column reports the overall results again, and then the remaining columns report the results for groups 1 through five. In agreement with Callaway and Sant'Anna (2020), the overall effect is similar to the weighted average of the group effects. Group 1 consists of the 9 states that only had one mandatory reporter legislation change, group 2 consists of the 8 states with two changes, etc. In addition to the treated states, the 24 control states are included in all analyses. These are the states that did not change their mandatory reporter legislation between 2004 and 2017. All regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend.

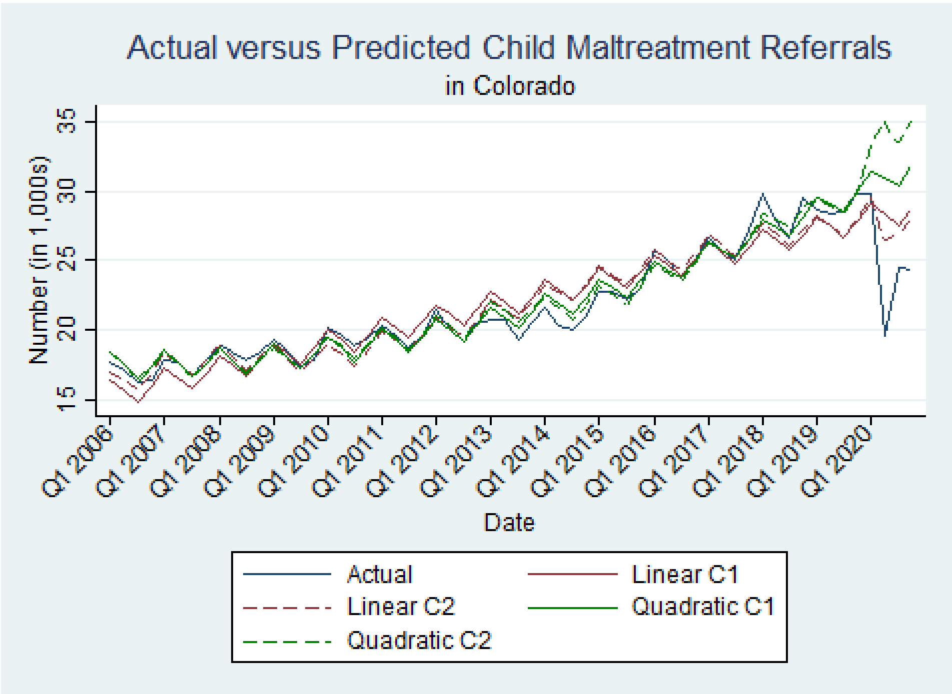
Appendix Table D2: Short-Run Effects of the Legislation Change that Added Athletic Coach, College Staff, or Camp Staff between 2012 and 2014

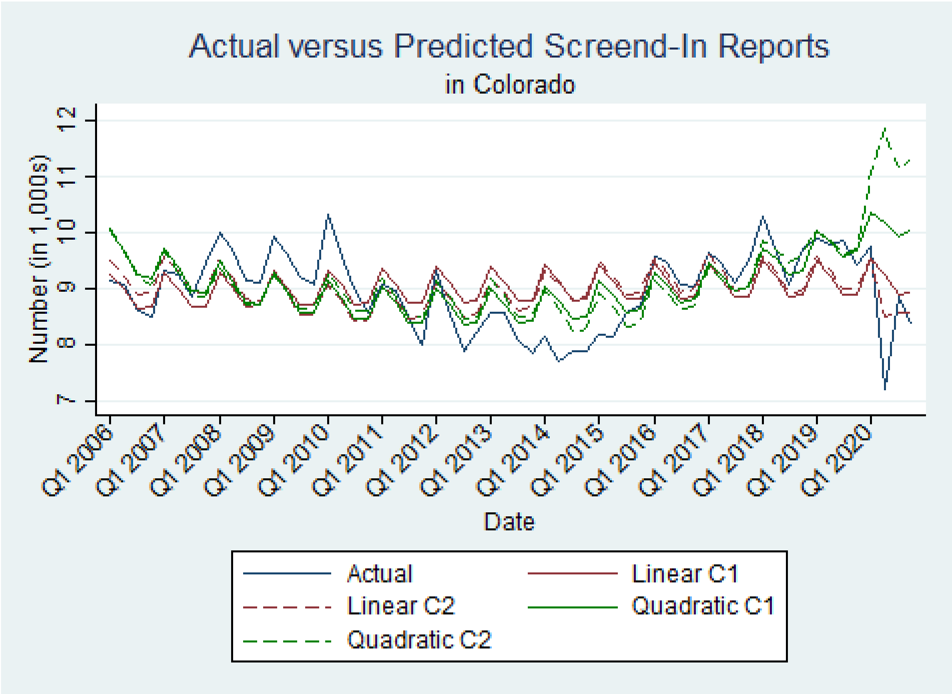
	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
Post	5.145	0.237	4.908	1.930	-0.358
	(10.69)	(3.292)	(8.087)	(17.98)	(5.354)
Treatment	7.127	-0.707	7.835	-1.530	-1.681
	(18.69)	(5.877)	(13.85)	(32.34)	(9.410)
Post x Treatment	-6.168	-1.653	-4.515	-12.40	-2.716
	(4.111)	(1.724)	(4.019)	(7.629)	(3.362)
Mean of dep. var.	237.4	49.9	187.4	438.4	77.4
Observations	308	308	308	308	308
Adjusted R^2	0.384	0.201	0.381	0.296	0.163

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. “Post” is equal to one five months after the legislation change and zero for the five months prior. “Treatment” is equal to one in the year of the legislation change and zero in year prior. Post x Treatment estimates the effect in the months after the legislation change, relative to the same time period the year before. The mean of the dependent variable in the post months of the control year is provided. Regressions include state, year, and month fixed effects. The 14 states included in this analysis are Alabama, Alaska, Arkansas, California, Colorado, Georgia, Illinois, Louisiana, New York, Pennsylvania, Tennessee, Virginia, Washington, West Virginia.

Appendix E – Appendix Tables and Figures for Chapter 3

Appendix Figure E1: Actual versus Predicted Child Maltreatment Reporting using Linear and Quadratic Polynomials





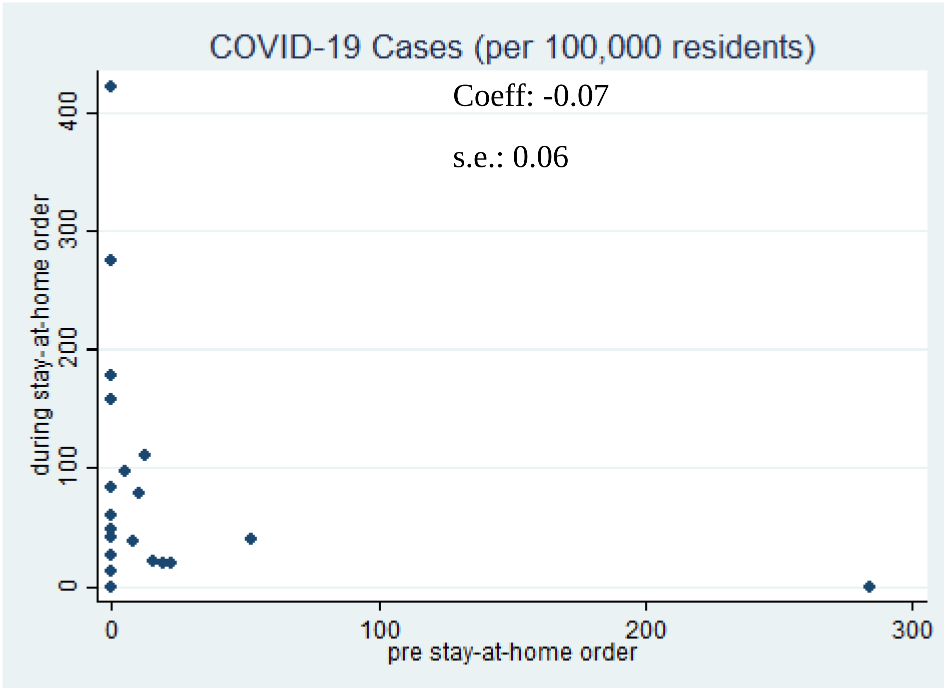
Notes: This figure plots the counterfactual estimates from equation 3.1 using a linear and quadratic polynomial, as opposed to the preferred cubic specification. The linear model is less predictive than the quadratic, and the quadratic model is similar to the cubic model.

Appendix Table E1: Summary Statistics of Child Maltreatment Reporting by COVID-19 Cases

	2019			2020			
	(1) Non-COVID-19 Counties	(2) COVID-19 Counties	p-value	(3) Non-COVID-19 Counties	(4) COVID-19 Counties	p-value	Unconditional DD Estimate (4-2-(3-1))
Child Maltreatment Variables (per 1,000 children)							
Total Referrals	24.83	23.62	0.17	20.23	19.77	0.61	0.76
Screened-in	7.81	7.96	0.71	7.03	6.99	0.90	-0.20
Substantiated Reports	2.66	2.34	0.31	2.62	2.24	0.25	-0.06
Neglect Allegations	8.73	8.62	0.84	7.76	8.05	0.57	0.40
Physical Abuse Allegations	2.22	2.06	0.36	2.13	1.72	0.02	-0.26
Sexual Abuse Allegations	0.78	0.80	0.79	0.89	0.78	0.26	-0.13
Economic Conditions							
Unemployment Rate	2.89	2.64	0.02	6.33	6.93	0.24	0.85
Employment to Population Ratio	53.75	53.24	0.62	48.79	49.97	0.26	1.70
Child Population	6009	103223	0.00	5988	103247	0.00	45.36
COVID-19 Cases (per 100,000 residents)							
pre stay-at-home order				0.00	47.61	0.15	
during stay-at-home order				23.75	47.04	0.17	
post stay-at-home order				59.23	26.84	0.31	
Number of Counties	55	9		55	9		

Notes: This table reports the average child maltreatment referral, screened-in, and substantiated report rate, and economic conditions across all counties and quarters in 2019 and 2020 based on the counties' COVID-19 caseload. Nine counties ("treated") reported COVID-19 cases prior to the stay-at-home order, whereas 55 counties ("control") reported no cases prior to the stay-at-home order. The p-value for t-tests between the COVID-19 and non-COVID-19 counties are reported for each year, and a crude DD is provided.

Appendix Figure E2: Relationship between COVID-19 Cases Before and During the Stay-at-Home Order



Notes: This figure plots the number of COVID-19 cases (per 100,000 residents) before the stay-at-home order against the number of cases during the month of the stay-at-home order. The negative regression adjusted coefficient (-0.07) suggests places with higher pre-stay-at-home order COVID-19 cases might have been more compliant to the stay-at-home order.

Appendix Table E2: Robustness Analyses for Screened-In Reports

		(1) Main Results	(2) Post 2010	(3) Exclude Denver Metro-area	(4) Include Economic Controls
Panel A: Rate (per 1,000 children)	COVID-19	-0.079	-0.079	-0.024	0.118
		(0.710)	(0.708)	(0.801)	(0.795)
	School Closure	-1.838**	-1.814**	-1.711*	-1.883*
		(0.846)	(0.831)	(0.966)	(0.961)
	Stay-at-home	-3.273**	-3.291**	-3.146*	-3.514**
		(1.479)	(1.466)	(1.681)	(1.759)
Panel B: Log	COVID-19	-0.004	0.003	0.012	0.031
		(0.095)	(0.094)	(0.106)	(0.104)
	School Closure	-0.201*	-0.195*	-0.152	-0.176
		(0.121)	(0.117)	(0.137)	(0.134)
	Stay-at-home	-0.382*	-0.375*	-0.309	-0.347
		(0.211)	(0.205)	(0.238)	(0.242)
Panel C: Levels	COVID-19	-19.531*	-21.527*	-8.392	-15.955
		(11.504)	(11.230)	(9.196)	(11.007)
	School Closure	-45.600***	-43.433***	-21.899**	-41.476**
		(16.603)	(15.174)	(10.072)	(16.030)
	Stay-at-home	-89.454***	-86.960***	-44.373**	-84.199***
		(29.659)	(27.306)	(18.952)	(28.668)
	Observations	3,328	2,560	2,860	3,328

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the county-by-quarter level, in parentheses. Each column indicates a separate regression analysis. The first column provides the main results. The second column is post-2010. The third column excludes the Denver metro-area, and the fourth column includes controls for economic conditions (i.e. the unemployment rate and employment-to-population ratio). Each row represents a separate regression analysis for the three independent variables of interest, so row 1 reports the coefficient from equation 3.2 where *covid* is the independent variable of interest. Row 2 reports the coefficient from equation 3.3 where *schclo* is the independent variable of interest, and row 3 reports the coefficient from equation 3.4 where *sah* is the independent variable of interest. There are 3 separate panels as well. Panel one reports the effect on the screened-in report rate, per 1,000 children. The second panel reports the effect on the log screened-in reports, and the third panel reports the effect on the total screened-in reports (level). Each regression includes year, county, and quarter fixed effects.

Appendix Table E3: Robustness Analyses for Substantiated Reports

		(1) Main Results	(2) Post 2010	(3) Exclude Denver Metro-area	(4) Include Economic Controls
Panel A: Rate (per 1,000 children)	COVID-19	-0.876	-0.947	-1.077	-0.855
		(0.715)	(0.701)	(0.817)	(0.749)
	School Closure	0.615	0.412	0.870	0.933
		(0.966)	(0.961)	(1.106)	(1.150)
	Stay-at-home	0.628	0.227	0.939	1.236
		(1.764)	(1.748)	(2.028)	(2.166)
Panel B: Log	COVID-19	-0.046	-0.063	-0.079	-0.016
		(0.154)	(0.146)	(0.171)	(0.161)
	School Closure	0.206	0.179	0.281	0.300
		(0.201)	(0.191)	(0.225)	(0.227)
	Stay-at-home	0.315	0.261	0.428	0.516
		(0.356)	(0.338)	(0.401)	(0.416)
Panel C: Levels	COVID-19	-5.794	-7.053	-5.118	-5.196
		(8.598)	(7.548)	(7.281)	(8.558)
	School Closure	-5.966	-5.429	0.329	-3.918
		(8.473)	(6.771)	(6.232)	(8.431)
	Stay-at-home	-13.249	-13.255	-1.623	-9.381
		(15.645)	(12.636)	(12.047)	(15.559)
	Observations	3,328	2,560	2,860	3,328

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, clustered at the county-by-quarter level, in parentheses. Each column indicates a separate regression analysis. The first column provides the main results. The second column is post-2010. The third column excludes the Denver metro-area, and the fourth column includes controls for economic conditions (i.e. the unemployment rate and employment-to-population ratio). Each row represents a separate regression analysis for the three independent variables of interest, so row 1 reports the coefficient from equation 3.2 where *covid* is the independent variable of interest. Row 2 reports the coefficient from equation 3.3 where *schclo* is the independent variable of interest, and row 3 reports the coefficient from equation 3.4 where *sah* is the independent variable of interest. There are 3 separate panels as well. Panel one reports the effect on the substantiated report rate, per 1,000 children. The second panel reports the effect on the log substantiated reports, and the third panel reports the effect on the total substantiated reports (level). Each regression includes year, county, and quarter fixed effects.

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VITA

Curran Alexandra Prettyman, more commonly known as Alexa Prettyman, grew up in Maryland. She was involved in multiple extracurricular activities including the Academic Team, Chemistry Team, and Friends for Life. Outside of academics, she rode horses competitively. During the summer before her senior year, she interned at The Johns Hopkins University Applied Physics Laboratory and presented her work to colleagues and classmates. In 2011, she graduated high school with top honors and moved to Lexington, Kentucky to further her education at the University of Kentucky.

As an undergraduate, Alexa was on the Equestrian Team and competed at Nationals numerous years; she was a Resident Advisor and actively involved in the community; and she was a Research Assistant for Dr. Jim Ziliak in the Center for Poverty Research and Dr. Chris Bollinger in the Center for Business and Economic Research. She graduated in 2015 from the Honors Program and Summa Cum Laude with a B.S. in Equine Science and Management and a B.S. in Mathematical Economics.

After working a year as an equestrian trainer and coach, in 2016, Alexa moved to Atlanta, Georgia to pursue graduate school at Georgia State University (GSU). As a graduate student, she was the President of the Economics Graduate Student Association; she was a research assistant for the Georgia Policy Labs (GPL); and she taught two upper-level economics courses related to labor economics. Alexa has coauthored numerous policy briefs for GPL, competed in GSU's 3-Minute Thesis Competition and has received numerous teaching and leadership certificates and university fellowships and awards, including the Second Century Initiative Graduate Fellowship, the Jack Blicksilver Scholarship in Economics, and the Outstanding Graduate Assistant Award. She received her M.A. in Economics in 2020 and was awarded her PhD in 2021.

Alexa has accepted a Senior Statistician and Research Supervisor position at the California Center for Population Research, affiliated with UCLA. She begins her new role in July 2021.