

# The Impact of Unemployment on Child Maltreatment in the United States\*

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## Abstract

In this paper, we show that unemployment increases child neglect in the United States during the period from 2004 to 2012. A one percentage point increase in the unemployment rate leads to a 20 percent increase in neglect. We identify this effect by instrumenting for the county-level unemployment rate with a Bartik instrument, which we create as the weighted average of the national-level unemployment rates across each of twenty industries, where the weights are the county-level fraction of the employed working-age population in each industry at the start of the sample period. An important mechanism behind this effect is that parents lack social and private safety nets. The effect on neglect is smaller in states that introduce longer extensions to unemployment benefits, and is greater in counties where an initially larger fraction of children are not covered by health insurance. We find no evidence that the effect is driven by alcohol consumption or divorce.

*JEL:* I10, J12, J13, J65, K42

*Keywords:* child abuse and neglect, unemployment rate, recession, safety net, unemployment insurance.

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# 1 Introduction

Child maltreatment is the abuse and neglect of children under 18 years old. In the United States, child maltreatment is a major problem. The U.S. Department of Health and Human Services estimates that there were approximately 683,000 victims in 2015 alone, equivalent to a rate of 9.2 victims per 1,000 children (US Department of Health and Human Services, 2015). Child maltreatment can have long-term consequences on the victims including mental health problems, substance abuse, lower levels of education, productivity or earnings, and a higher probability of engaging in crime (e.g., Dube et al., 2003; Springer et al., 2007; Currie and Widom, 2010; Currie and Tekin, 2012).

The correlation between economic conditions and child maltreatment has been studied in both the economic and sociological literature (Elder Jr., 1974; Becker, 1993). Low-income parents are more likely to use physical forms of discipline to alter their children's behaviour because they cannot provide other types of incentives (Weinberg, 2001). At the same time they may lack the resources to provide for their children's basic needs. In addition, recent studies find that the Great Recession is associated with more hospital-diagnosed instances of child head trauma and physical abuse (Huang et al., 2011; Wood et al., 2012), high frequency spanking (Brooks-Gunn et al., 2013), and maternal harsh parenting (Lee et al., 2013).

A few papers have considered the effects of aggregate economic conditions on child abuse and neglect, and present mixed findings. Early economic studies have found only a weak relationship between economic conditions and child maltreatment (Paxson and Waldfogel, 2002; Bitler and Zavodny, 2004; Seiglie, 2004). More recently, Lindo et al. (2013) use administrative data from California and find that male layoffs increase child maltreatment whilst the opposite is true for female layoffs. Stephens-Davidowitz (2013) considers reported cases of maltreatment, rates of child mortality and Google searches for child maltreatment, and finds that the Great Recession led to a decrease in reporting of child maltreatment, but an increase in actual incidences. Raissian and Bullinger (2017) use reports of child maltreatment and find that increases in the minimum wage at the state level are associated with decreases in reports, particularly neglect reports. Finally, Schneider et al. (2017) use data from the Fragile Families and Child Wellbeing Study and find that the Great Recession led to a higher risk of child abuse, but a lower risk of child neglect.<sup>1</sup> Nonetheless, these studies assume that area-level unobservable characteristics

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<sup>1</sup>Other similar studies focus on one or few states, see for example, Slack et al. (2003); Millett et al. (2011); Raissian (2015).

related to child maltreatment are not correlated with the economic variable considered.

Causal empirical evidence is scarce (Berger and Waldfogel, 2011).<sup>2</sup> To the best of our knowledge there is only one paper that attempts to use exogenous variation in economic conditions. Berger et al. (2016) use data from the Fragile Families and Child Wellbeing Study and use differences between states and over time in the generosity of the Earned Income Tax Credit for which a family is eligible to identify exogenous variation in income. The authors find that an increase in income is associated with a modest reduction in child neglect and a large reduction in Child Protective Services involvement, especially among low-income single-mother families.

In this paper, we ask whether unemployment causes child maltreatment. We make three main contributions. First, we use a new dataset containing one observation for every reported incident of child abuse and neglect made to the state Child Protective Services for nearly every state in the U.S. from 2004 to 2012. Our dataset comes from the National Child Abuse and Neglect Data System (NCANDS), produced by the National Data Archive on Child Abuse and Neglect (NDACAN). Previous papers have used an earlier and more limited version of this dataset, with information available only at a state level (e.g., Paxson and Waldfogel, 1999, 2002, 2003). To the best of our knowledge, we are the first authors to use a restricted-access version of this dataset with county-level identifiers.<sup>3</sup> This dataset covers the whole of the U.S., which allows us to improve upon recent articles that consider only one state (e.g., Lindo et al., 2013; Frioux et al., 2014; Raissian, 2015). Moreover, our dataset contains referrals to the Child Protective Services and not self-reported measures of child maltreatment which are collected in the Fragile Families and Child Wellbeing Study and used in Berger et al. (2016) and Schneider et al. (2017).

Second, we identify the casual impact of unemployment on child maltreatment using an instrumental variables approach. We instrument for the county-level unemployment

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<sup>2</sup>See Berger and Waldfogel (2011) for an extensive literature review of studies focusing on the economic determinants and consequences of child abuse and neglect. In addition, three studies present suggestive but not conclusive evidence of a causal link between low income and child maltreatment. They are based in the US and focus on a cash assistance program to families at risk of having their child placed in foster care in Illinois (Shook and Testa, 1997), on a welfare reform program in Delaware (Fein and Lee, 2003), and on a randomised child support and welfare reform experiment in Wisconsin (Cancian et al., 2013). See also Lindo and Schaller (2014) for an overview of the challenges faced by researchers attempting to identify the causal effect of economic conditions on child maltreatment.

<sup>3</sup>Palusci et al. (2016), for example, use NCANDS data at the county level, but only have access to counties where 1,000 or more reports are made. They consider only 192 counties, which is about 6% of all counties. This leads to a serious censoring problem.

rate using a predicted county-level unemployment rate, which we create as the weighted average of the national-level unemployment rates across each of twenty industries, where the weights are the county-level fraction of the employed working-age population in each industry the year before the start of the sample period, in 2003. This type of instrument has been widely used in the labour economics literature, and is commonly referred to as the Bartik instrument (Bartik, 1991; Blanchard and Katz, 1992). Our approach improves on previous work (Paxson and Waldfogel, 1999, 2002; Bitler and Zavodny, 2004; Seiglie, 2004; Stephens-Davidowitz, 2013; Lindo et al., 2013; Schneider et al., 2017) in which the authors assume that area-level unobservable characteristics related to child maltreatment are not correlated with the economic variable considered. The Bartik instrument has also been used in a related literature that investigates the effect of economic conditions on intimate partner violence (e.g., Aizer, 2010; Anderberg et al., 2016).

Third, we investigate the lack of safety nets as a potential mechanism for our results. We ask whether there is a role for government policy in mitigating the adverse effects of poor economic conditions on child maltreatment. We investigate the effect of the extension of the duration of unemployment benefits which occurred in response to the recession. We also explore the role of two private safety nets. We look at the effect of health insurance and having two employed parents living in the household. We ask whether unemployment causes a change in expenditure on basic goods, and whether unemployment causes repeated cases of child maltreatment.

We find that a one percentage point increase in the unemployment rate leads to a 10 percent increase in overall abuse. We look at the effect of unemployment on different types of maltreatment, and find that the effect on overall abuse is driven by an increase in neglect. A one percentage point increase in the unemployment rate leads to a 20 percent increase in neglect. We demonstrate that the results are robust, and capture an effect on the actual incidence of neglect and not reporting behaviour.

We find evidence that both social and private safety nets mitigate the effect of unemployment on neglect. The effect is smaller in states that introduced longer extensions to the duration of unemployment benefits. A one percentage point increase in the unemployment rate at the 25th percentile of the 2008-12 distribution of the duration of benefits (55 weeks) leads to a 21 percent increase in neglect, whilst at the 75th percentile (87 weeks) it leads to only a 14 percent increase. In addition, the effect is greater in counties in which an initially larger fraction of children are not covered by health insurance. For a county with the median poverty rate, a one percentage point increase in the unemployment rate at the 25th percentile of the distribution of the fraction of uninsured children

(0.081) leads to a 10 percent increase in neglect, whilst at the 75th percentile (0.156) it leads to a 14 percent increase. We find evidence that the effect is greater amongst Black children, who are less likely to live in a household with two employed parents than White or Hispanic children. We also find that unemployment causes a decrease in real expenditure on basic goods, as we might expect in the absence of safety nets. Finally, we find that unemployment causes repeated neglect. If unemployment persists then income may continue to stay below the level required to meet a child’s basic needs. We rule out two alternative explanations, namely that alcohol consumption or divorce drive the effect.

The paper is structured as follows. In Section 2, we provide definitions of each type of child abuse and neglect, describe the system of Child Protective Services and outline the process of child maltreatment reporting in the United States. We present our empirical strategy and describe the NCANDS dataset in Section 3. In Section 4, we outline the main results. In Section 5, we explore mechanisms for those results. We present robustness checks in Section 6, and conclude in Section 7.

## **2 Context: The Child Protective Services and the Process of Child Maltreatment Reporting in the United States**

At the Federal level, child abuse and neglect are defined by the Child Abuse Prevention and Treatment Act (CAPTA) as: ‘Any recent act or failure to act on the part of a parent or caregiver, which results in death, serious physical or emotional harm, sexual abuse or exploitation, or an act or failure to act which presents an imminent risk of serious harm’ (Child Welfare Information Gateway, 2014). There exist some differences in the way that specific types of child abuse or neglect are defined across states. Neglect is generally defined as the failure of a parent or other caregiver to provide the necessary food, clothing, shelter, medical care or supervision to the point that the child’s health, safety and well-being are threatened with harm. Physical abuse is generally defined as any non-accidental physical injury to the child. The definition of sexual abuse generally includes the encouragement or coercion of a child to engage in any sexually explicit conduct or simulation of such conduct for the production of child pornography, as well as rape, molestation, incest, or prostitution. Emotional abuse is generally defined as injury to the psychological capacity or emotional stability of the child, as evidenced by

an observable or substantial change in behaviour, emotional response or cognition (Child Welfare Information Gateway, 2014).

All fifty states and the District of Columbia have a Child Protective Services (CPS) agency, which is responsible for investigating reports of child abuse and neglect.<sup>4</sup> The process of child maltreatment reporting varies by state, but typically works as follows.<sup>5</sup> All but ten states have a centralised statewide hotline that reporters can call if they suspect child abuse or neglect.<sup>6</sup> Individuals in some professions, such as teachers and doctors, are mandated to report any suspicion of child maltreatment, but reports can come from any member of the public, for example neighbours, family or friends.<sup>7</sup> Trained specialists at either the state or county hotline receive the call, obtain as much information about the case as possible from the reporter, and make a judgement about whether the case warrants an investigation in accordance with state law. This often requires that the specialists call other agencies, such as law enforcement and schools, to gather additional information.

After the initial phone call, the case is allocated to the CPS office in the county in which the child resides. A CPS caseworker makes initial face to face contact with the family, before undertaking an investigation.<sup>8</sup> During the investigation, the caseworker may talk to the child, to the child's family, as well as professionals who are involved in the child's life. The caseworker will decide whether there is sufficient evidence that child abuse or neglect has taken place. In the event that the report is substantiated, a range of actions can be taken. In extreme cases, the child can be removed from his or her family home for protection. More often, the caseworker will recommend a plan to

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<sup>4</sup>The CPS falls under different departments in different states, for example the Department of Health and Welfare in Idaho or the Office of Children and Family Services in New York.

<sup>5</sup>We have contacted every Child Protective Services agency in the United States by phone and email to understand the process of child maltreatment reporting. The information provided in Section 2 is based on those phone calls and emails. A useful review of procedures is available for New York state, at the website of the Office of Children and Family Services: <http://ocfs.ny.gov/ohrd/ccg/>, which is similar to procedures in many other states. Recommended procedures for CPS caseworkers can be found at Children's Bureau (2003).

<sup>6</sup>Alabama, California, Hawaii, Maryland, Minnesota, North Carolina, North Dakota, South Carolina, Wisconsin and Wyoming do not have a statewide hotline, and so reporters must call the specific county office in the county in which the child resides. In states with a statewide hotline, the initial call is often processed at the state-level, but it can be directed to county hotlines.

<sup>7</sup>In some states, such as New Hampshire, everyone is a mandated reporter by law.

<sup>8</sup>Initial face to face contact is made within one or few days of the call depending on the degree of emergency of the report, after which an investigation is undertaken over a longer period, which can be a few months.

the family involving, for example, cognitive-behavioural therapy, school-based training, or counselling and other supportive services (Children’s Bureau, 2003). The CPS cannot directly prosecute the parents, but they can recommend cases to law enforcement agencies and the courts.

In 2012, the CPS agencies received approximately 3.4 million reports of child abuse or neglect involving approximately 6.3 million children. Of these, 62.0% are investigated, leading to a national rate of investigated reports of 28.3 per 1,000 children (US Department of Health and Human Services, 2012). Professionals made 58.7% of reports, with 16.7% made by legal and law enforcement personnel, 16.6% by education personnel and 11.1% by social services personnel (US Department of Health and Human Services, 2012). Of the child-reports that are investigated, 19% of cases are found to be substantiated.<sup>9</sup>

### 3 Identification Strategy and Data

#### 3.1 Identification Strategy: The Bartik Instrument

We wish to understand the effect of unemployment on the incidence of child abuse and neglect. Unobservable worker characteristics within a county might be correlated with both the unemployment rate and the incidence of child abuse or neglect in that county. To deal with this concern, we use an instrumental variables approach. We instrument for the county-level unemployment rate using a predicted county-level unemployment rate, which combines national-level unemployment rates across industries with differences in the initial industrial structure across counties. This instrument isolates a measure of local labour demand that is unrelated to local labour supply. It therefore allows us to separate demand-driven unemployment rate shocks from supply-driven shocks that could be correlated with unobservables that are also related to child maltreatment. This approach is introduced by Bartik (1991). It has been used many times in the labour economics literature (e.g., Blanchard and Katz, 1992; Luttmer, 2005; Wozniak, 2010), and has been used recently in papers on violence against women (e.g., Aizer, 2010; Anderberg et al., 2016).

Our instrument is a weighted average of the national-level unemployment rates across each of twenty industries,<sup>10</sup> where the weights are the fraction of the employed working-age population in each industry the year before the start of the sample period, in 2003, in

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<sup>9</sup>These figures apply to the Federal Fiscal Year 2012.

<sup>10</sup>For the industry classification, we use the North American Industry Classification System (NAICS), see [www.census.gov/eos/www/naics](http://www.census.gov/eos/www/naics).

the county. National-level unemployment rates are plausibly exogenous to county-level worker characteristics in any individual county, since counties are small in size relative to the whole of the United States (the U.S. consists of 3,143 counties). The initial industrial structure in a county is likely correlated with its workers' characteristics, which is a threat to the validity of the instrument. However, the initial industrial structure is by definition time invariant at the county level, and so we can deal with this threat by controlling for county-level fixed effects, which we do in all regressions. We estimate the following, where equation (2) is the first stage, and equation (1) is the second stage of the IV procedure:

$$Y_{cst} = \beta Unemp_{cst} + X'_{cst} \phi + \psi_{st} + \eta_c + \epsilon_{cst} \quad (1)$$

$$Unemp_{cst} = \delta(\sum_j w_{csj} N_{tj}) + X'_{cst} \theta + \pi_{st} + \tau_c + v_{cst} \quad (2)$$

Here,  $Y_{cst}$  is the natural logarithm of the number of abuses per year in county  $c$ , in state  $s$ , in year  $t$ , for the type of abuse of interest.<sup>11</sup> We count only allegations that are found to be substantiated.<sup>12</sup>  $Unemp_{cst}$  is the unemployment rate in county  $c$ , in state  $s$ , in year  $t$ . The Bartik instrument comprises of the weights,  $w_{csj}$ , which are the fraction of employed working-age individuals in each industry,  $j$ , at the start of the sample period (year 2003) in county  $c$ , in state  $s$ ; and the national-level unemployment rate,  $N_{tj}$  in each industry,  $j$ , in each time period,  $t$ .<sup>13</sup> We control for county fixed effects through  $\eta_c$ , to ensure the validity of our instrument as discussed above.

We take the natural logarithm of the number of abuses because its distribution (even when expressed as a rate per child) has a long right-hand tail, and we do not want the estimates to be dominated by the effects in counties with the most extreme maltreatment. We control for the natural logarithm of the child population in  $X'_{cst}$ , rather than consider the abuse rate per child as the dependent variable, because doing so is a more general approach which does not impose that the coefficient on the natural logarithm of the child population is equal to one (see also Aizer, 2010).

The coefficient of interest is  $\beta$ . This coefficient tells us the percentage change in the number of abuses in a county as a result of an increase in the unemployment rate by one percentage point.

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<sup>11</sup>Some counties have zero abuses for particular abuse types in a given year. This varies by the type of abuse, but, for example, 10% of county-years have no incidents of physical abuse and 8% of county-years have no incidents of neglect. To ensure that no county-year has a zero, we add 0.001 abuses to every county-year before taking the natural logarithm. We check the robustness of the results to adding 0.01 and 0.0001 abuses in columns (5)-(8) of Table A.10.

<sup>12</sup>In the robustness checks in Section 6.1, we also consider unsubstantiated cases.

<sup>13</sup>The industry assigned to an unemployed person is the industry of the last job that person held.



A further identification concern comes from the measurement of the left hand side variables. The measurement of abuse and neglect differs across states and may change over time within states. If the unobservable reasons for these differences are correlated with unemployment, our estimate of  $\beta$  will be biased. Differences in measurement arise from several sources. First, the definitions of child abuse and neglect vary across states and may vary over time within states. Whilst the Child Abuse Prevention and Treatment Act (CAPTA) provides federal definitions, state definitions can differ (Child Welfare Information Gateway, 2014). For example, Washington state does not recognise emotional abuse.<sup>14</sup> Second, some states include specific exceptions in their definitions of child abuse and neglect. For example, in thirty-one states and D.C., an exception is made for parents who choose not to seek medical care for their children due to religious beliefs. Third, states differ in who is mandated to report child abuse. Fourth, states have different systems to determine whether a referral should be classified as substantiated. Since these differences only occur at the state-year level, we can deal with this concern by controlling for state-year fixed effects through  $\psi_{st}$ .

In  $X'_{cst}$ , we also control for the fractions of the population that are Black, Hispanic, and Other Race (which includes individuals who are Asian, Alaskan Native or American Indian, Native Hawaiian or Other Pacific Islander and Two or More Races). We weight observations by the child population in the county-year, because the child population varies considerably across counties and we wish to estimate an effect that is representative for children across the U.S.<sup>15</sup>

We cluster standard errors at the state level. Our main concern here is that the CPS is organised at the state level, and so the number of substantiated incidents may be correlated across counties within the same state. This is true even though we control for state-year fixed effects. For example, if CPS workers are re-allocated within the state from one county office to another, changes in the quality of investigation (and so perhaps the number of incidents found to be substantiated) may be correlated across those two counties. We later demonstrate the robustness of results to clustering at a county level.

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<sup>14</sup>There are several other examples, which are summarised in Child Welfare Information Gateway (2014). For example, seven states explicitly include human trafficking in their definition of child sexual abuse. Twenty-five states and D.C. include a failure to educate a child as required by law in the definition of neglect.

<sup>15</sup>For example, the year before the start of the sample period in 2003, 1% of counties had 222 or fewer children. By contrast, 25% of counties had a child population of greater than or equal to 15,297, whilst the largest county, Los Angeles, had 2,678,788 children. In Table A.12, we check that the results are robust to dropping counties with a population of more than one million children at the start of the sample period, given that those counties receive a very large weight in the regressions.

## 3.2 Data

### 3.2.1 Outcome Variables: Child Abuse and Neglect

We use a dataset which contains every reported incident of child abuse and neglect made to state Child Protective Services in nearly every state in the U.S. for the years 2004-12. To our knowledge, we have a unique restricted-access version of this dataset with information on the county of the report. This dataset comes from the National Child Abuse and Neglect Data System (NCANDS), produced by the National Data Archive on Child Abuse and Neglect (NDACAN). We focus on reports of neglect, physical, sexual and emotional abuse. For each child maltreatment report, we observe the gender, age and ethnic group of the perpetrator and victim, the report date, the type of maltreatment alleged, the county of the report and the outcome of the investigation.

For each county and year, we create a count of the total number of incidents of each type of maltreatment.<sup>16</sup> If multiple children are maltreated within a single report, we count an incident for each of the children. We count only incidents where that specific type of maltreatment is found to be substantiated by the Child Protective Services.<sup>17</sup> We take the natural logarithm of those counts, after first adding 0.001 to all county-years, since some county-years have zero reports of some types of maltreatment. We later demonstrate the robustness of the results to alternative methods of dealing with zeros. In each year, a small number of states do not submit information to NCANDS, which is a voluntary reporting system. The median number of states reporting in each year is 49 (including D.C.), and the lowest is 45 in 2004. Our analysis focuses on a final sample of 2,803 counties from forty-six states.<sup>18</sup>

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<sup>16</sup>Emotional abuse is not recorded in Washington state in any year, or in: D.C. in 2004, Idaho in 2006, Indiana in 2004-7, Rhode Island 2007, or Vermont in 2012.

<sup>17</sup>Specifically, we count incidents that are coded as ‘substantiated’, ‘indicated or reason to suspect’, and ‘alternative response disposition - victim’ for the given type of maltreatment, since states differ slightly in the way that they classify the outcome of investigations.

<sup>18</sup>We exclude Alaska, South Dakota, Illinois, North Dakota and Oregon from the analysis. The Child Protective Services in Alaska is organised by boroughs, which have different boundaries to the FIPS counties used in all federal reporting systems including NCANDS. The county of report that Alaska submits to NCANDS is created after the fact, based on computer codes that have changed over time and has a tenuous link to the borough boundaries. Only 25 out of 66 counties in South Dakota are included in the NCANDS data, whilst in Illinois the fraction of counties reporting decreases from all to less than one third between 2010 and 2011. Finally, North Dakota and Oregon do not report any information before 2009, and so are not observed before the start of the recession, the time at which the main unemployment shock occurs in our sample period.

### 3.2.2 Unemployment Rate

We focus on the annual unemployment rate at a county level, using data from the Local Area Unemployment Statistics (LAUS) produced by the Bureau of Labour Statistics (BLS). The BLS calculates unemployment rates using information collected in the Current Population Survey, Current Employment Statistics survey, and state Unemployment Insurance systems.

### 3.2.3 Control Variables

We measure the child population using the Population and Housing Unit Estimates (PHUE), produced by the Census Bureau. The Census Bureau uses data on births, deaths and migration to update decennial census data to produce these estimates. We define a child as any individual between the ages of 0 and 17. We also use the PHUE to measure the fraction of the population that are of each ethnic group (Black, Hispanic and Other Race).

### 3.2.4 Summary Statistics

In Table 1, we present unweighted means and standard deviations for the main variables we use in the regression analysis. Neglect is considerably more common than the other three types of maltreatment, with the mean number of incidents of neglect more than four times greater than the mean number of incidents of physical abuse, the next most common type of maltreatment. The variance in the number of incidents of all types of maltreatment per year is large, but particularly so for emotional abuse.

The main results are driven by the sharp increase in the unemployment rate between 2007 and 2009, following the onset of the recession. We show this increase in Figure 1, in which we plot the weighted average unemployment rate across counties over the sample period. We weight county-years by the child population so that the unemployment rate plotted is representative of where children reside in the U.S. The median county in our sample experienced an increase in the unemployment rate of 4.5 percentage points between 2007 and 2009.

This unemployment shock disproportionately affected the long-term unemployed. In Figure 2, we plot the fraction of all unemployed individuals in each of four categories by the duration of unemployment. The long-term unemployed, defined as individuals who have been unemployed for twenty-seven weeks or more,<sup>19</sup> comprised more than forty

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<sup>19</sup>This is the BLS definition of long-term unemployment. For more information, see:

percent of total unemployment from 2010 onwards, having comprised less than one fifth of total unemployment in 2008.

We show the main sources of variation underlying the regression results in Figures 3 and 4. In Figure 3, we plot the change in the unemployment rate between 2004-6 and 2010-12 after controlling for state-year fixed effects. This map demonstrates that the size of the unemployment shock varied considerably across counties within states. Similarly, in Figure 4, we plot the change in the overall abuse rate per 100,000 children between 2004-6 and 2010-12 after controlling for state-year fixed effects. This map highlights the heterogeneity in the change in the overall abuse rate across counties within states, which we seek to explain in the regression analysis.

Finally, in the Appendix in Figures A.1 to A.3, we present some descriptive statistics by the demographic characteristics of the perpetrator and victim. These patterns are interesting as it is rare for datasets on domestic violence to contain detailed demographic information about the victim and perpetrator. Figure A.1 shows the abuse rates by the victim's age. The rate of neglect amongst children aged 0-4 is nearly twice the rate amongst children aged 5-17. The rate of sexual abuse amongst children aged 5-17 is more than three times greater than the rate amongst 0-4 year olds. Figure A.2 shows the overall abuse rate by the perpetrator and victim's gender. Women are relatively more likely to abuse or neglect their children. However, women usually spend more time with children, both within two-parent households and because the incidence of single motherhood is greater than single fatherhood. Whilst these figures are not restricted to incidents perpetrated by parents, for 86.8% of substantiated incidents, at least one of the perpetrators is a parent.<sup>20</sup> Figure A.3 shows the overall abuse rate trends for the least poor and poorest 10% of counties, categorising counties by their overall poverty rate. We caution that differences in abuse rates between the poorest and least poor counties may be partly driven by differences in definitions of abuse at the state level, by changes in definitions of abuse over time, or by differences in which counties are included in the calculation of the weighted mean in each year.<sup>21</sup> However, with those cautions in mind, this Figure demonstrates that overall abuse is more common in the poorest 10%

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[https://www.bls.gov/bls/cps\\_fact\\_sheets/ltu\\_mock.htm](https://www.bls.gov/bls/cps_fact_sheets/ltu_mock.htm).

<sup>20</sup>To define a parent, we include the following categories from the NCANDS dataset: parent, relative foster parent, non-relative foster parent, legal guardian, foster parent. We exclude: other relative, group home or residential facility staff, child daycare provider, unmarried partner of parent, other professionals, friends or neighbours, and other. The relationship of the perpetrator to the child is recorded in most, but not all, state-years.

<sup>21</sup>Not all counties are included in the calculation in every year because not all states submit information to NCANDS in every year.

of counties than the least poor 10% of counties.

## 4 Results

### 4.1 Unemployment Causes Child Neglect

In Table 2, we present the main results. In columns (1) and (2), we estimate the effect of unemployment on overall abuse using OLS and IV respectively. The measure of overall abuse combines any incident of neglect, physical, sexual or emotional abuse. We find that the coefficients on the unemployment rate are positive, and for the IV regression the effect is statistically significant at the 5% level. A one percentage point increase in the unemployment rate leads to a 10 percent increase in overall abuse.

The effect on overall abuse is driven by an effect of unemployment on neglect. In columns (3) to (10), we separate out the four different types of maltreatment. The IV results in column (6) demonstrate that a one percentage point increase in the unemployment rate leads to a 20 percent increase in neglect. This effect is statistically significant at the 1% level. In the county with the median prevalence of neglect, a one percentage point increase in the unemployment rate leads to an increase in neglect by 110 cases per year.<sup>22</sup>

Our results, which are for the U.S. as a whole, contrast with the estimates presented in Lindo et al. (2013) for California. In that single state, they do not find a statistically significant effect of the predicted unemployment rate on neglect.

We find no statistically significant effect on sexual or emotional abuse in either the OLS or IV regressions, and the point estimates are small. For physical abuse, we do find a small statistically significant effect in the OLS, which is no longer significant in the IV regression. Though the point estimate is larger in the IV regression, it is still less than one third the size of the effect on neglect. We therefore focus on understanding the effect on neglect in the rest of the paper.

The Bartik instrument is highly relevant, the Kleibergen-Paap F-statistic on the first stage is 34.9. We present the first stage results in the Appendix in Table A.1. The coefficient on the Bartik instrument suggests that a one percentage point increase in the predicted unemployment rate leads to a 0.63 percentage point increase in the actual unemployment rate.

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<sup>22</sup>The weighted median county-year in the final sample (weighting by the child population) has 536 cases of neglect. Using the estimates in column (6) of Table 2, a 20.44 percent increase in neglect translates into an increase by  $(0.2044 \times 536 = 110)$  cases.

For both overall abuse and neglect, the two main IV results, the OLS estimates are smaller and not statistically significant. There are two explanations for these differences. The first is that the OLS estimates suffer from attenuation bias due to classical errors-in-variables measurement error in the county-level unemployment rate. The county-level unemployment rate from the LAUS program is partially model-based. The LAUS program uses county-level data on the number of unemployment insurance claimants. However, new entrants and re-entrants to the labour force who are unemployed cannot claim unemployment insurance as their current period of unemployment has not been preceded by a period of employment. The number of unemployed new entrants and re-entrants is instead estimated by combining state-level data on labour force entry with county-level demographic information, and this introduces measurement error.<sup>23</sup>

The second is that the OLS estimates suffer from omitted variables bias. One possible omitted variable is the county-level government budget. Following a negative shock to the county government budget,<sup>24</sup> public sector workers are made unemployed. In addition, funding for the CPS decreases.<sup>25</sup> A decrease in funding for the CPS leads to a cut in CPS staff (Eckholm, 2009), which can lead to a decrease in substantiated reports of abuse and neglect for two reasons. First, county hotlines become understaffed, and so reporters who are left on hold for long periods of time may hang up the call and fail to make the report (Stephens-Davidowitz, 2013). Second, the remaining CPS caseworkers are given higher caseloads, which can lead to shortcuts in investigations or in extreme cases reports may not be investigated at all (Buckley and Secret, 2011; Preston, 2013; Santos, 2013). Therefore, the county government budget is negatively correlated with the unemployment rate and positively correlated with maltreatment, which creates a downward bias in the OLS estimates. Shocks to a county government budget will not affect national-level unemployment rates and so the Bartik instrument removes this source of omitted variables bias. To the best of our knowledge, there does not exist a county-level dataset of local government budgets covering our sample period, and so this variable is unobservable. This may be because the structure of county and sub-county governments

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<sup>23</sup>See <http://www.bls.gov/lau/laumthd.htm> for details.

<sup>24</sup>A shock to the county government budget may arise from a change in its ability to raise local revenues, or simply from the mismanagement of revenues from any source (including state and federal transfers). Local governments rely heavily on revenues raised themselves. For example, in 2012, 63% of local government revenue came from its own sources (this figure comes from the Tax Policy Center, at: <http://www.taxpolicycenter.org/statistics/local-general-revenue>).

<sup>25</sup>Resources for the investigation of child maltreatment can vary widely across counties within states. This can be seen, for example, in Pennsylvania, where the Department of Human Services records a review of expenditure per case at the county level in Pennsylvania Department of Human Services (2016).

is complicated and varies by state.<sup>26</sup>

Given the difference between the OLS and IV results, we later demonstrate the robustness of the IV results to four alternative choices of instrument, in Section 6.2. Whilst this does not allow us to distinguish between these two explanations for the difference, it does ensure that the results are not a quirk of our preferred choice of instrument.

## 4.2 Heterogeneity

In this subsection, in Tables A.2 and A.3, we ask whether the effect of unemployment differs by the victim's age and the perpetrator's gender. In Table A.2, we see that the point estimate for the effect on neglect of young children (0-4 years) is more than double that for older children (5-17 years). A one percentage point increase in the unemployment rate leads to a 25 percent increase in neglect of children aged 0-4 years old. In Table A.3, we see that the point estimate for the effect of unemployment on female perpetrated neglect is more than double the size of that for male perpetrated neglect. A one percentage point increase in the unemployment rate increases female perpetrated neglect by 28 percent.

## 5 Mechanisms

We hypothesise that one important mechanism behind the effect of unemployment on neglect is that parents lack safety nets. We define a safety net as any source of cash or in-kind benefit that helps to prevent an individual from falling into poverty in response to a transitory negative income shock, such as a job loss. In the absence of safety nets, unemployment will force parents to decrease expenditure, perhaps to a level that they can no longer provide for their children's basic needs. We test this hypothesis in several ways. We ask whether the effect of unemployment on neglect is greater where safety nets are lacking. We distinguish between social safety nets, which are safety nets provided by the state, and private safety nets. We directly ask whether unemployment causes a decrease in expenditure on basic goods, which could in turn lead to child neglect. Finally, we ask whether unemployment leads to repeated neglect. We also provide evidence on two alternative mechanisms, which are substance abuse and divorce.

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<sup>26</sup>County governments exist in every state other than Connecticut, Rhode Island, D.C.. There exist four levels of sub-county government: municipal and township governments (general purpose), and school and special districts (special purpose). Which levels of government exist, and which levels have responsibility for the many functions of government varies considerably across states and can even vary across regions within states (U.S. Census Bureau, 2013).

## 5.1 Social Safety Nets: Unemployment Insurance

One of the most important social safety nets is Unemployment Insurance (UI). UI provides cash benefits to involuntarily unemployed persons who have sufficiently high earnings histories to qualify.

During the sample period, unemployed individuals who had exhausted benefits from the regular UI program could claim additional benefits through two programs. The first is the Extended Benefits (EB) program. The EB program allowed individuals to claim an additional 13 to 20 weeks of unemployment benefits in states with high unemployment rates. This program existed before the recession. It was initially partly funded by the state and federal governments, and so many states chose to opt out of the program at the time of onset of the recession. However, the program became entirely funded by the federal government under the American Recovery and Reinvestment Act in February 2009. After that date, many states joined and chose low state level unemployment rate ‘triggers’, above which unemployed individuals became eligible for additional benefits (Hagedorn et al., 2013).

The second is the Emergency Unemployment Compensation (EUC) program. The EUC program was introduced in June 2008 in response to the recession, and ended in January 2014. It was entirely federally funded, and again allowed unemployed individuals to claim additional weeks of unemployment benefits if their state level unemployment rate hit certain thresholds. The program had four tiers. All states are eligible for Tier 1, which allowed an extra fourteen weeks of benefits. To qualify for Tiers 2, 3 and 4, a state’s seasonally adjusted total unemployment rate had to reach 6%, 7% and 9% respectively.<sup>27</sup> Under EUC, unemployed individuals could potentially receive up to fifty-three additional weeks of unemployment benefits (Hagedorn et al., 2013).

We have a dataset on the duration of unemployment benefits, which comes from state-specific trigger reports provided by the Department of Labor,<sup>28</sup> and is compiled in Hagedorn et al. (2013).<sup>29</sup> The dataset is provided at the quarterly level, and so we take the mean across quarters to make the dataset annual. In Figure 5, we plot the distribution of the duration of unemployment benefits across the forty-five states that we include in the regression analysis.<sup>30</sup> From 2004-7, almost every state provided twenty-six weeks of

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<sup>27</sup>See <https://workforcesecurity.doleta.gov/unemploy/pdf/euc08.pdf>.

<sup>28</sup>See <http://ows.doleta.gov/unemploy/trigger/> and <http://ows.doleta.gov/unemploy/euc-trigger/>.

<sup>29</sup>We are very grateful to Fatih Karahan at the Federal Reserve Bank of New York for providing us with this dataset.

<sup>30</sup>Data on unemployment benefits is not available for Hawaii.



unemployment benefits. In 2008, almost every state provided between thirty-three and thirty-five weeks. After 2008, there is a large increase in the variation in the duration of benefits across states. We explore this variation in our analysis below. The combination of the EB and EUC programs meant that unemployment benefits were extended from the standard twenty-six weeks to as many as ninety-nine weeks in some states.

We can now see that the EB and EUC programs directly targeted the long-term unemployed, since the long-term unemployed are by definition the group who had exceeded the standard twenty-six weeks of unemployment benefits. As we saw in Figure 2, long-term unemployment increased sharply during the sample period, and so the EB and EUC programs extended a core social safety net to some of a rapidly growing group of previously uninsured individuals.<sup>31</sup>

We interact the duration of unemployment benefits with the unemployment rate to ask whether unemployment has a smaller effect on neglect in the states that introduced longer extensions to the duration of benefits. We do that firstly by using a continuous measure of the number of weeks of unemployment benefits provided in the state-year, and secondly, by using a set of thresholds that we create by discretising the duration of unemployment benefits.

We use the distribution of unemployment benefits in Figure 5 to inform our choice of the thresholds. From 2004-8, unemployment benefits are more limited, but between 2009 and 2012, there is a considerable increase in the variation across states, and all states offer a more generous duration of benefits from 48 to 99 weeks. For that reason, we choose the first band of the threshold to be [0-48] weeks. We use four bands in total. To do this, we use the 33rd and 66th percentile of the 2009-12 distribution of benefits as thresholds. These are 73 and 86 weeks. We therefore use the bands: [0-48], (48-73], (73-86] and (86-99] weeks.<sup>32</sup>

For the continuous measure of the duration of unemployment benefits, we interact the duration of unemployment benefits with the Bartik instrument to obtain an extra instrument. For the discrete measures, we interact the dummy variables for each band with the Bartik instrument, to create a set of additional instruments. We already control for the level of the duration of unemployment benefits through the state-year fixed effects. Table 3 reports the second stage results, while first stage results are reported in the Appendix in Table A.4.

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<sup>31</sup>We cannot directly look at the effect of long-term unemployment on neglect, or at how these two programs may have mitigated any such effect, as no measure of the unemployment rate by duration exists at the county level.

<sup>32</sup>Results are robust to the selection of different sets of thresholds for the period 2009-2012.

The coefficient on *Unemployment Rate*  $\times$  *Benefit Duration* in column (1) is negative and statistically significant at the 10% level, which demonstrates that extending the duration of benefits is indeed associated with a smaller effect of unemployment on neglect. The size of the effect is large. A one percentage point increase in the unemployment rate at the 25th percentile of the 2008-12 distribution of the duration of benefits (55 weeks) leads to a 21 percent increase in neglect, whilst at the 75th percentile (87 weeks) it leads to only a 14 percent increase. In the county with the median prevalence of neglect, this difference equates to 37 fewer cases of neglect per year in response to a one percentage point increase in the unemployment rate. At the maximum duration of benefits of ninety-nine weeks, a one percentage point increase in the unemployment rate still leads to an 11 percent increase in neglect. Part of this residual effect may exist because two groups of parents remain ineligible for unemployment insurance even when benefits are extended to the ninety-nine week maximum. The first is the group of parents who have been unemployed for more than ninety-nine weeks. The second is the group of parents who earned less than a state-determined threshold level of earnings in their previous period of employment (Isaacs and Healy, 2014).

This result is stronger once we consider discrete measures of the duration of unemployment benefits, as shown in column (2). Comparing the coefficient on *Unemployment Rate*  $\times$  *Thres* (87 – 99] to the coefficient on *Unemployment Rate*  $\times$  *Thres* (49 – 73] in column (2), we see that there is a greater mitigating effect of unemployment benefits in the more generous compared to the less generous states. This difference is statistically significant at the 5% level, with a p-value of 0.03 for the associated Wald test for the difference between these two coefficients.

Unemployment benefits are not extended randomly, and so it is possible that the coefficients on the interaction terms (*Unemployment Rate*  $\times$  *Benefit Duration* in column (1), and *Unemployment Rate*  $\times$  *Thresholds* in columns (2) and (3)) of Table 3 might not capture a causal effect. Unobservable worker characteristics at a county level may be correlated with the state level unemployment rate, and so whether triggers for benefit extensions are reached, as well as child neglect. To deal with this, we adopt a contiguous border counties methodology following Dube et al. (2010).<sup>33</sup> See Section B.1.1 in the Appendix for further details and Table A.5 for the results. The effect is no longer statistically significant, but this may be due to low statistical power since the border counties constitute a little over one third of the number of counties in the main sample.

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<sup>33</sup>This approach has been widely used in the minimum wage literature (for a review, see Allegretto et al. (2017)).

We look at two other social safety net programs for which we have a measure of state-level policy differences. These are the Supplemental Nutrition Assistance Program (SNAP), and the Earned Income Tax Credit (EITC).<sup>34</sup> The first stages are not identified, see Table A.6 in the Appendix and Section B.1.2 for further details. Whilst this is unfortunate, the literature suggests that SNAP and EITC are less counter-cyclically responsive than UI (Bitler and Hoynes, 2016), which is the part of the social safety net that we have assessed.

## 5.2 Private Safety Nets

Parents can rely on private safety nets as well as those provided by the state. We observe two county-level measures of private safety net coverage at the start of the sample period: the fraction of children who are not covered by health insurance, and the fraction of children who had two employed parents living in the household with them. Private safety nets can take several other forms, for example parents may receive income transfers from family and friends, or dissave. However, we do not observe county-level measures of these other private safety nets.

### 5.2.1 Health Insurance

In the absence of health insurance, an unemployed parent may be unable to cover the financial costs of adequate medical care for their children. Our measure of neglect includes medical neglect. We have a county-level dataset of the fraction of children who are not covered by health insurance in 2000, which comes from the Small Area Health Insurance Estimates (SAHIE). We interact the fraction of children initially not covered by health insurance, *Fraction Uninsured Children*, with the unemployment rate, as well as with the Bartik instrument to create a second instrument. We already control for the level of *Fraction Uninsured Children* through the inclusion of the county fixed effects.

We are concerned that the existence of private safety nets could just proxy for the level of poverty. We wish to understand the mitigating effect of private safety nets conditional

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<sup>34</sup>UI, SNAP, EITC and the Temporary Assistance for Needy Families program (TANF) programs constitute the core cash or near-cash elements of the social safety net for the non-elderly (Bitler and Hoynes, 2016). We do not have a measure of state-level policy differences for TANF. States have considerable flexibility in how TANF funds are spent. They can use TANF funds for a range of programs other than direct cash transfers, for example family planning and marriage counselling (Schott et al., 2012). Since so many programs exist under the umbrella of TANF, it is difficult to measure state-level policy differences in one or even few dimensions. Measures of total caseloads or total expenditure are likely to capture differences in demand for the program by state, more than differences in the generosity of the program.

on the interaction between poverty and unemployment. This matters because the effect of unemployment on neglect is not concentrated in the poorest households. To see this, we use a variable in the NCANDS dataset that indicates whether the house in which the child resides is substandard, overcrowded, unsafe or otherwise inadequate, including homelessness. We then study the effect of unemployment on neglect for children living in inadequate and adequate housing, and we find that it occurs for children living in adequate housing. The lack of effect for children in the poorest households might be explained if their parents were not affected by the unemployment shock because they were already unemployed. For further details about this analysis, please see Section B.2 and Table A.7 in the Appendix.

In column (1) of Table A.8, we then demonstrate that the poverty rate, which we measure using the Small Area Income and Poverty Estimates (SAIPE), is positively correlated with the fraction of children who are not covered by health insurance. This tells us that a failure to control for the interaction between the poverty rate and the unemployment rate will create a downward bias in the estimate of the coefficient on *Fraction Uninsured Children*  $\times$  *Unemployment Rate*. We therefore control for the interaction between the poverty rate, also measured in 2000,<sup>35</sup> and the unemployment rate, and we create a third instrument as the interaction between the poverty rate and the Bartik instrument. We present results both including and not including this control in Table 4.

We find that unemployment has a greater effect on neglect in counties where a larger fraction of children are initially not covered by health insurance, in column (2) of Table 4. This effect is significant at the 5% level. For a county with the median poverty rate, a one percentage point increase in the unemployment rate at the 25th percentile of the distribution of the fraction of uninsured children (0.081) leads to a 10 percent increase in neglect, whilst at the 75th percentile (0.156) it leads to a 14 percent increase in neglect. Controlling for the interaction between the poverty rate and the unemployment rate removes a downward bias in the estimate of the coefficient on *Fraction Uninsured Children*  $\times$  *Unemployment Rate* as predicted, as can be seen by comparing the results in column (2) to those in column (1).

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<sup>35</sup>We are concerned that the estimates of health insurance coverage and the poverty rate may be related and not truly capture different concepts. However, the models used for the SAIPE and SAHIE are very different. Further, the model used to estimate the poverty rate does not include any measure of health insurance coverage. Details on the methodology can be found at: <https://www.census.gov/did/www/saipe/methods/statecounty/2000county.html> and <https://www.census.gov/did/www/sahie/methods/2000/model.html>.

### 5.2.2 Having Two Employed Parents

The labour supply of other family members can act as a private safety net for an unemployed person (Cullen and Gruber, 2000). If one parent becomes unemployed in a household without a second employed parent, it is more likely that expenditure will decrease below the level required to provide for the children’s basic needs. Paxson and Waldfogel (2002) find that increases in the fraction of children with absent fathers and working mothers in a state, and in the share of families with non-working parents, is related to increases in several measures of child maltreatment.

We create a variable that measures the fraction of children in the county with two employed parents living in the household with them, using data from the Census in 2000. We use the same identification strategy as described in Section 5.2.1. We interact *Fraction Children Two Employed Parents* with the unemployment rate, and control for the interaction between the poverty rate and the unemployment rate. We create two additional instruments by interacting each of *Fraction Children Two Employed Parents* and the poverty rate with the Bartik instrument.<sup>36</sup>

Whilst the coefficient on the interaction term is negative, it is not statistically significant, as demonstrated in column (4) of Table 4. Controlling for the interaction between the poverty rate and the unemployment rate removes an upward bias in the estimate of the coefficient on *Fraction Children Two Employed Parents*  $\times$  *Unemployment Rate* as predicted, as can be seen by comparing the results in column (4) to those in column (3).

We investigate this safety net using a second approach, which is to look at the effect of unemployment on neglect by the ethnic group of the victim. Across the United States, Black and Hispanic children are significantly less likely to live in a household with two employed parents than White children (see column (1) of Table A.9 in the Appendix). Further, Black children are less likely to live with two employed parents than Hispanic children (see column (2) of Table A.9). We separately count incidents of neglect for White, Black and Hispanic children as the dependent variables for the regressions in columns (5) to (7) of Table 4. Both the unemployment rate and the Bartik instrument are not ethnic-group specific. We control for the child population of the relevant ethnic group. We weight observations by the child population of the relevant ethnic group so

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<sup>36</sup>The poverty rate is negatively correlated with the fraction of children with two employed parents, as demonstrated in column (2) of Table A.8. Therefore, a failure to control for the interaction between the poverty rate and the unemployment rate will create an upward bias in the estimate of the coefficient on *Fraction Children Two Employed Parents*  $\times$  *Unemployment Rate*.

that the estimates are representative of the effect where the children of that ethnic group reside in the U.S.

We find that unemployment has a statistically significant effect on the neglect of Black children in column (6), at the 5% significance level. The effects on White and Hispanic children are positive but not statistically significant. A one percentage point increase in the unemployment rate leads to a 31 percent increase in the neglect of Black children. A one standard deviation increase in the within-county variation in the unemployment rate leads to an increase in the neglect of Black children equivalent to 0.28 standard deviations of its within-county variation (the equivalent effect sizes for Hispanic and White children are 0.19 and 0.08 standard deviations respectively, and not statistically significant).<sup>37</sup> We therefore conclude that the evidence on this private safety net is mixed. Whilst the interaction between the unemployment rate and *Fraction Children Two Employed Parents* is not significant in column (4), we do find evidence that the effect of unemployment is largest for the group of children who are least likely to have two employed parents living in the household with them.

### 5.3 Unemployment Reduces Expenditure on Basic Goods

In the absence of safety nets to cover the loss of income, unemployment may force parents to decrease expenditure on basic goods, which may in turn lead to child neglect. Correlations have been found between child maltreatment and food shortages, difficulty with paying for clothing, housing, utilities, or other important bills (e.g. Courtney et al., 2005; Yang, 2015).

We have a state-level dataset from the Bureau of Economic Analysis (BEA) which measures expenditure per capita. We focus on two groups of expenditures on basic goods that could be related to neglect. The first is expenditure per capita on food and beverages purchased for off-premises consumption, and the second is expenditure per capita on clothing and footwear. We convert both variables to be in real terms.<sup>38</sup> We ask whether unemployment causes a decrease in real expenditure per capita on basic goods, using the following IV regression:

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<sup>37</sup>To calculate these standardised effect sizes, we use the within-county variation in the variables. Since we control for county fixed effects, this is the source of identifying variation in the regressions. We take this approach throughout the paper whenever we present standardised effect sizes.

<sup>38</sup>To do so, we use the All Items All Urban Consumers CPI, produced by the BLS.

$$Expenditure_{st} = \beta Unemp_{st} + X'_{st}\rho + \lambda_s + \zeta_t + \epsilon_{st} \quad (3)$$

$$Unemp_{st} = \delta(\Sigma_j w_{sj} N_{tj}) + X'_{st}\varrho + \sigma_s + \varphi_t + \nu_{st} \quad (4)$$

We run separate regressions for expenditure on food and beverages, and clothing and footwear. This IV approach follows the baseline regressions as closely as possible, except that the unit of observation is the state-year. The dependent variable  $Expenditure_{st}$  is the natural logarithm of real expenditure per capita on the goods of interest. We create a state-level Bartik instrument,  $\Sigma_j w_{sj} N_{tj}$ , which is identical to the original Bartik instrument except that the weights,  $w_{sj}$ , are at the state level. The validity of the Bartik instrument in state-level regressions relies on the assumption that no individual state dominates national-level production in an industry, since otherwise state-level unobservable worker characteristics may be related to the national-level unemployment rate in that industry. We control for the fractions of the population that are Black, Hispanic, and Other Race in  $X'_{st}$ , we control for state and year fixed effects, we weight observations by the child population in the state-year, and we cluster standard errors at the state level. We restrict the sample to the four hundred state-years that are included in the baseline regressions, but the results are very similar if we include all state-years.

We present the results in Table 5. We find that unemployment is associated with a decrease in real expenditure per capita on both food and beverages, and clothing and footwear, in the OLS regressions in columns (1) and (3). These associations are significant at the 1% level. In column (2), we find that unemployment causes a decrease in real expenditure per capita on food and beverages, using the IV approach. This effect is also significant at a 1% level. A one percentage point increase in the unemployment rate causes a 1.1 percent decrease in real expenditure on food and beverages per capita. A one standard deviation increase in the unemployment rate leads to a 0.82 standard deviation decrease in real expenditure on food and beverages per capita, using the within-state variation. The effect on clothing and footwear is not statistically significant in the IV regression in column (4), and the point estimate is small.

We therefore conclude that unemployment leads to a decrease in real expenditure on basic goods, which could lead to child neglect. This decrease in expenditure could come from a decrease in the quantity of basic goods purchased, or from the purchase of cheaper goods in the same quantity.

## 5.4 Unemployment Causes Repeated Child Neglect

In the absence of safety nets, if unemployment persists then income may stay persistently below the level required to meet a child's basic needs. In that case, unemployment may cause repeated neglect, which we test in Table 6. Unemployment indeed persisted during the sample period after the initial shock, as demonstrated in Figure 1. Between 2009 and 2012, the unemployment rate fell just 1.06 percentage points.

Thirty-eight states record a unique child ID that takes the same value across the panel.<sup>39</sup> In these states we can track each child over time and so understand whether each substantiated report is the first time the child has been the victim of neglect, or whether they have been a victim before. We rerun the baseline regressions but using four new dependent variables. We separately count first time, second time, third time and fourth or more time cases of neglect.<sup>40</sup> We present the results in columns (2) to (5) of Table 6, after first demonstrating that the main effect still exists in this sub-sample of states in column (1).

Unemployment has a statistically significant effect on first time, second time and third time cases of neglect at least at the 5% level. The point estimates of the effect of unemployment on second time and third time cases of neglect are, respectively, more than double and more than three times the size of the effect on first time cases. The largest effects of unemployment are on repeat cases of child neglect.

We have interpreted the results in columns (3) and (4) of Table 6 as demonstrating that the unemployment shock following the onset of the recession caused repeated child neglect. An alternative explanation is that this unemployment shock only caused one-

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<sup>39</sup>Alabama, Minnesota and South Carolina do not record the child ID consistently over time. Arkansas, Indiana, New Mexico, Texas and Wisconsin do not provide mapping files for the Child ID variable and so we cannot verify whether they record the child ID consistently over time. To be cautious we drop these five states too. We cannot track children over time if they move across states, because the child ID variable is only unique within states.

<sup>40</sup>Our ability to observe whether or not a child has previously been the victim of neglect is limited by the start date of the sample period. For example, if a child is neglected for the second time in 2004, having been neglected in 2003 before the start of the sample period, we will count this second report as a first time incident. The more recent the year, the more years of preceding data we have, and so the less likely it is that we miss a previous case of neglect for a given child due to the truncation of the data. For that reason, it is comforting that the distribution of first to fourth or more time cases is quite similar throughout the panel. For example, in the full sample period, 78.2% of cases of neglect are first time, 15.0% second time, 4.3% third time and 2.6% fourth or more times. In 2012, 73.7% are first time, 16.8% second time, 5.6% third time and 4.0% fourth or more times. Nonetheless, we should treat the results in this subsection with a little caution.



time child neglect, but that children who are neglected before the recession are neglected again. However, for cases of neglect from 2008 onwards, the median lag to the previous case of neglect is just 411 days. This relatively short lag between reports suggests that, indeed, children are often the victim of multiple cases of neglect after the unemployment shock.

## 5.5 Alternative Mechanisms

### 5.5.1 Substance Abuse

Individuals may increase alcohol consumption or drug use to cope with stress after being made unemployed (e.g., Boardman et al. (2001); Eliason and Storrie (2009)). Substance abuse can in turn increase child maltreatment. Previous evidence has demonstrated that substance abuse can lead to violent behaviour within the household (e.g., Lee Luca et al. (2015)). Markowitz and Grossman (1998) find that alcohol regulations reduce violence against children. Further, alcohol and drug use can limit a parent’s ability to care for their children, or drain resources that could otherwise be used to pay for the children’s basic needs, which can lead to neglect.

To explore this mechanism, we use data on the estimated county-level prevalence of heavy and binge drinking over the period from 2004 to 2012.<sup>41</sup> These measures are estimated in a paper by Dwyer-Lindgren et al. (2015), using the Behavioural Risk Factor Surveillance System (BRFSS) dataset. The BRFSS is a health-related telephone survey of U.S. households, which samples approximately 400,000 adults each year and is the largest continuously conducted health survey system in the world.<sup>42</sup>

In Table 7, we test whether unemployment increases overall abuse or neglect by increasing alcohol consumption. In columns (1) to (6), we ask whether unemployment increases heavy or binge drinking. We look at the prevalence rate of heavy and binge drinking at the county level, overall and for men and women separately. We use the same Bartik IV strategy as in the baseline regressions. We weight the observations by the child population in the county-year as in the baseline regressions because we want to estimate an effect of unemployment on alcohol consumption that is representative of the counties where children reside in the U.S.

We in fact find that unemployment causes a decrease in female heavy and binge drink-

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<sup>41</sup>Heavy drinking is classified as more than one drink per day for women, or more than two drinks per day for men. Binge drinking is classified as having more than four drinks for women or five drinks for men on a single day at least once in the previous thirty days.

<sup>42</sup>For details see: <http://www.cdc.gov/brfss/>.

ing. These effects are significant at the 5% and 1% levels respectively. An one standard deviation increase in the unemployment rate leads to a 0.39 standard deviation decrease in the prevalence of female heavy drinking and a 0.59 standard deviation decrease in the prevalence of female binge drinking, using the within-county variation. This result is in line with previous studies where economic downturns have been found to be positively correlated with health, as individuals can no longer afford costly unhealthy behaviours (Ruhm, 2000, 2003, 2005; Ruhm and Black, 2002). The coefficients on the unemployment rate for overall heavy and binge drinking are also negative, though not statistically significant. When we control for the prevalence of overall binge and heavy drinking in the baseline regressions in columns (7) and (8), there is virtually no change in the size or statistical significance of the effect of unemployment on overall abuse or neglect.

We can therefore rule out that unemployment causes an increase in overall abuse or neglect through an increase in alcohol consumption. Combined with the finding that the point estimate for the effect of unemployment on neglect is smaller for White children than Black or Hispanic children in Section 5.2.2, this also suggests that the results in this paper are not related to the rising substance abuse and morbidity of middle-aged White Americans documented in Case and Deaton (2015).

### 5.5.2 Divorce

Unemployment can lead to divorce (e.g., Charles and Stephens (2004); Doiron and Mendolia (2012); Eliason (2012)). Divorce can in turn increase child maltreatment, for at least three reasons (Lindo et al., 2013). Divorce might increase the time that children spend with unrelated adults (the new partners of their parents), who may be particularly prone to abusive behaviour (Sedlak et al., 2010). The children of divorced parents may grow up in single parent households, which may have fewer resources to provide for children's basic needs, leading to an increase in neglect. Finally, divorce may lead to stress or substance abuse, which may lead to abuse or neglect.

In Table 8, we test whether divorce can explain the effect of unemployment on overall abuse or neglect. We use the American Community Survey, available for the period from 2005 to 2012, to measure the prevalence of divorce at a county-level. As in the regressions for alcohol consumption, we weight the observations by the child population in the county-year.

In column (1), we find that there is a large, positive and statistically significant effect of unemployment on divorce. A one percentage point increase in the unemployment rate increases the divorce rate by 0.6 percentage points. A one standard deviation increase

in the unemployment rate leads to a 0.99 standard deviation increase in the rate of divorce, using the within-county variation. Unemployment might trigger divorce with a lag because divorce procedures can be time consuming. We therefore allow for an effect at a one year lag in column (2). As we might expect, the effect is greater at a one year lag than the contemporaneous effect.

In columns (3) and (4), we control for the divorce rate in the baseline regression. If divorce is driving the effect of unemployment on overall abuse or neglect, we would expect the coefficient of interest on unemployment to fall to zero and become statistically insignificant, whilst the coefficient on divorce would be positive and significant. However, controlling for divorce has virtually no effect on the size or statistical significance of the effect of unemployment on overall abuse or neglect. Further, the coefficients on the divorce rate are not statistically significant and are even negative for both types of maltreatment. This suggests that although unemployment does cause an increase in divorce, the effect of unemployment on overall abuse and neglect is not driven by divorce.

## 6 Robustness

### 6.1 An Effect on Actual Incidence or Reporting?

The main concern with the results is whether they capture an effect of unemployment on the reporting of maltreatment, rather than the actual incidence. We test this in three ways in Table 9. First, an unemployment shock may result in a reallocation of labour to high-reporting sectors, such as schools, social services, health care, police, clergy and childcare, which could increase reporting rates for a given level of actual maltreatment. To deal with this, we follow Lindo et al. (2013). In columns (1) and (2), we control for the fraction of the working-age population employed in these six high-reporting sectors. We find that the coefficient on the unemployment rate barely changes in either the regression for overall abuse or neglect, and the fraction employed in high-reporting sectors variables are statistically insignificant in all but one case.

Second, unemployed individuals spend more time at home and may therefore have more opportunity to observe the maltreatment of a neighbour, friend or extended family member's children, for a given level of actual maltreatment. The NCANDS dataset contains a variable that records the identity of the reporter for each report. We therefore create a new dependent variable which does not count incidents where the reporter is a friend, neighbour or other relative.<sup>43</sup> We only count reports made by groups of reporters

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<sup>43</sup>These groups of reporters account for 10.7% of all reports with a non-missing reporter in the state-

who are not affected by this concern. These are: professionals (social services, medical, mental health, legal, law enforcement, criminal justice and education personnel, child day care providers and substitute care providers), parents and the alleged victim or perpetrator. By definition, professionals only interact with the maltreated children through their job, and parents are sufficiently close to their own children that they have an opportunity to observe maltreatment whether they are employed or unemployed. In columns (3) and (4), we rerun the baseline regressions using this dependent variable. We restrict the sample to state-years where the reporter’s identity is non-missing for more than 80% of substantiated reports. Again, we find that the coefficient on the unemployment rate barely changes in size or statistical significance in either regression.

Third, we have so far only considered substantiated reports, which comprise 19 percent of all reports. If unemployment affects reporting, then we should estimate a very similar effect on unsubstantiated reports. If unemployment affects the actual incidence of maltreatment, then we should only see an effect on substantiated reports. In columns (5) and (6), we run the baseline regressions but now count only unsubstantiated reports in the dependent variable. There is no significant effect of unemployment on unsubstantiated reports of either overall abuse or neglect, and the point estimates are approximately one third the size of the estimates for substantiated reports. In summary, all three tests demonstrate that unemployment affects the actual incidence of overall abuse and neglect and not reporting behaviour.

## 6.2 Alternative Choices of the Instrument

The OLS estimates are smaller than the IV estimates and not statistically significant for both overall abuse and neglect. Whilst there are two very plausible explanations for these differences, as described in Section 4.1, we still wish to check that the IV estimates are robust to alternative choices of the instrument. We consider four alternative instruments: change in real exchange rate, the Bartik instrument based on the national-level real value added by industry, the Bartik instrument based on national-level employed by industry, and the Bartik instrument computed with weights based on the fraction of the employed working-age population in each industry in the county in 1995.

Table 10 reports the results. In columns (1) and (2), we instrument using the change in the real exchange rate (RER) from the previous year, calculated as  $(RER_t - RER_{t-1})/RER_{t-1}$ , multiplied by the county-level fraction of the employed working-age years included in these regressions. We also do not count reports made by ‘other’ or ‘anonymous reporter’, as we cannot know whether they are also affected by this concern.

population in manufacturing at the start of the sample period, in 2003. An appreciation of the real exchange rate harms the competitiveness of U.S. manufacturers who compete with foreign producers, thereby increasing unemployment (see Lin, 2008). To measure the real exchange rate, we use the broad real trade-weighted US dollar index, produced by the Federal Reserve Bank of St. Louis.

In columns (3) to (8), we use three alternative Bartik instruments. For the first two, we use alternative proxies of the economic performance of each industry at the national level. We replace the national-level unemployment rate by industry from the original Bartik instrument with the national-level real value added by industry (measured in billions of 2009 dollars) in columns (3) and (4), and with the national-level total employed by industry in columns (5) and (6). Both measures are produced by the BEA.<sup>44</sup> Finally, in columns (7) and (8), we replace the weights in the original Bartik instrument with the fraction of the employed working-age population in each industry in the county in 1995.

The effect of unemployment on neglect is robust across these alternative choices of the instrument. In every case, the effect is still statistically significant at least at the 10% level, and the point estimate ranges from 0.15 to 0.24. The instruments are highly relevant, with Kleibergen-Paap F-statistics ranging from 12.9 to 41.1, and the coefficients on the instrument in the first stage always have the expected sign (positive for the regressions in columns (1), (2), (7) and (8), and negative in columns (3) to (6)). The effect on overall abuse is always positive, but is smaller in size and not always statistically significant.

In Tables A.10-A.12 of Section C in the Appendix, we conduct other robustness tests. We use alternative choices of the dependent variable and make several changes to the specification and the sample.

## 7 Conclusion

Child maltreatment is a severe public health problem with long-term consequences for the victims. Despite this, little causal empirical evidence on its determinants currently exists. In this paper, we address that gap by studying the causal effect of unemployment on child maltreatment. We use a new dataset containing every reported incident of child abuse and neglect made to the state Child Protective Services for nearly every state in the U.S. from 2004 to 2012. To identify the effect of unemployment, we use a Bartik instrument. We instrument for the county-level unemployment rate with the weighted average of the

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<sup>44</sup>We use the total employed rather than the employment rate by industry, because the CPS (used to create the unemployment rate in the original Bartik instrument) does not ask about the industry of individuals who are out of the labour force. For more details see the Table D.1 in the Appendix.

national-level unemployment rates across each of twenty industries, where the weights are the county-level fraction of the employed working-age population in each industry the year before the start of the sample period, in 2003. We find that a one percentage point increase in the unemployment rate leads to a 20 percent increase in neglect. We show that our results are robust and capture an effect on the actual incidence of neglect and not reporting behaviour.

One important mechanism behind the effect of unemployment on neglect is that parents lack safety nets. We find evidence that both social and private safety nets mitigated the effect of unemployment on neglect following the onset of the Great Recession. The effect of unemployment is smaller in states that introduce longer extensions to the duration of unemployment benefits. The effect is greater in counties where an initially larger fraction of children are not covered by health insurance, and is greater amongst the group of children who are least likely to live in a household with two employed parents. We also find evidence that unemployment causes a decrease in real expenditure on basic goods, as we might expect in the absence of safety nets.

These results suggest that a potentially important benefit of the social safety net has been ignored in policy evaluations in the academic literature. The existing literature weighs up a trade-off between the successful poverty-reducing impact of the social safety net (e.g., Ben-Shalom et al. (2012); Moffitt (2013); Bitler et al. (2017)), and the possibility that safety nets have harmful effects on incentives, for example creating a disincentive to engage in job search (e.g., Card and Levine (2000); Barro (2010); Rothstein (2011)). In terms of the former, our paper quantifies an impact of the social safety net that has not previously been considered. Unemployment benefits can prevent children from being neglected when a parent is made unemployed, which may in turn have positive long-term consequences for those children.

These results also suggest that we need to understand whether parents face barriers to creating private safety nets. Private safety nets can prevent children from being neglected when a parent is made unemployed, and yet many households lack even very low-levels of private safety nets. For example, in 2017, more than one quarter of U.S. households are asset poor, meaning that even if they sell their non-liquid assets such as a home or a car, they will still not have enough to replace income at the poverty level for three months (Wiedrich et al., 2017).

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## 8 Tables and Figures

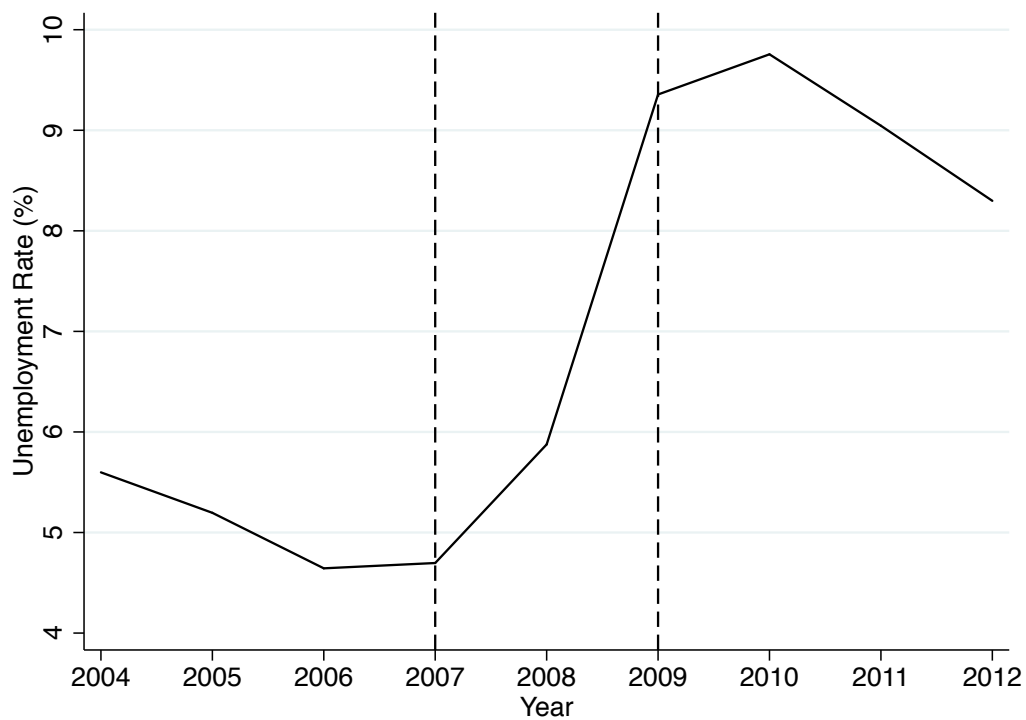


Figure 1: *Notes.* **Trend in the Unemployment Rate.** In this Figure, we present the trend in the weighted average unemployment rate across the 2,803 counties in our final sample. We weight observations by the child population, so that the unemployment rate is representative of where children reside in the U.S. Unemployment rates jumped during the period from 2007 to 2009, with the onset of the recession.

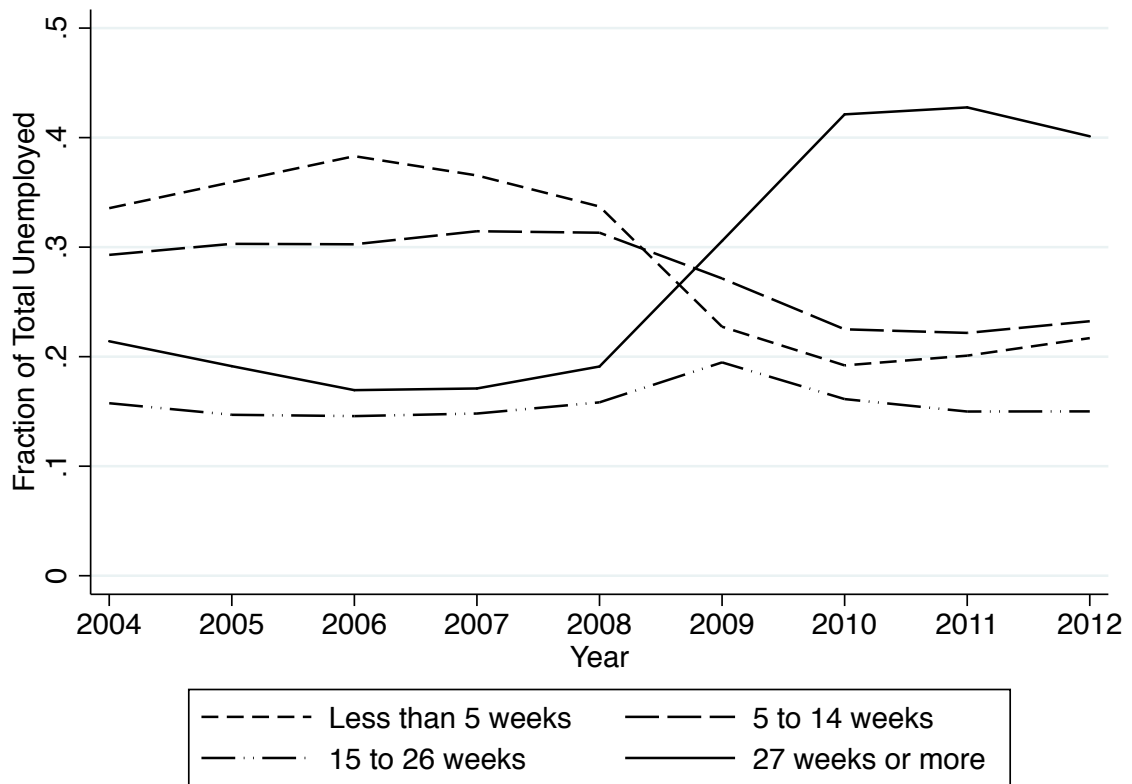


Figure 2: *Notes.* **The Increase in Long-Term Unemployment as a Proportion of Total Unemployment.** In this Figure, we plot the fraction of the total unemployed in each of four duration categories: less than five weeks, five to fourteen weeks, fifteen to twenty-six weeks, and twenty-seven or more weeks. Long-term unemployment is defined as twenty-seven or more weeks. We take a weighted average of the fraction in each category across state-years, where the weights are the number of children in the state-year. We include only the 400 state-years in our final sample.

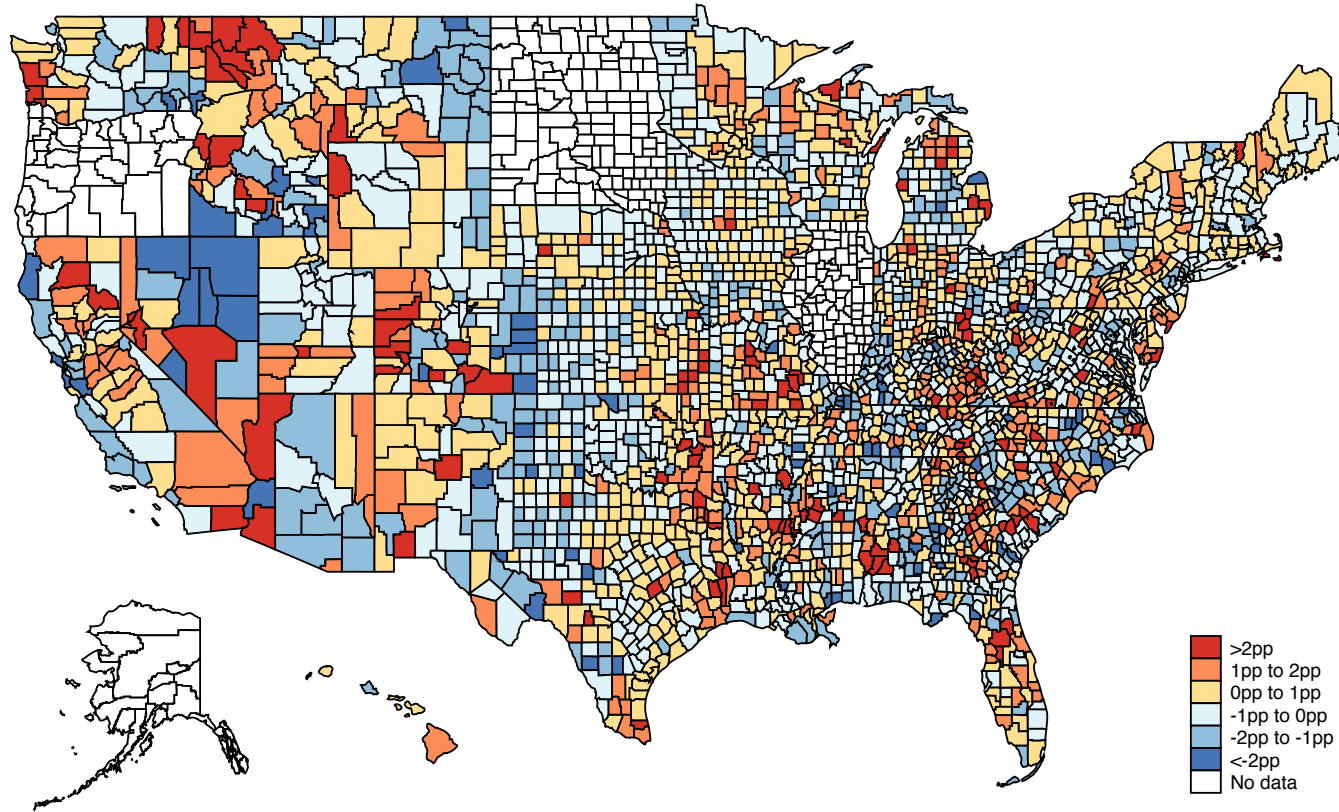


Figure 3: *Notes.* **Change in the Unemployment Rate Before and After the Recession.** This map presents the change in the unemployment rate before and after the recession. We regress the unemployment rate on state-year fixed effects. We take the residuals from this regression, and calculate the average for the pre-recession period 2004-2006 and for the post-recession period 2010-2012. We then subtract the pre-recession average from the post-recession average. Alaska and Hawaii are in the bottom left of the figure.



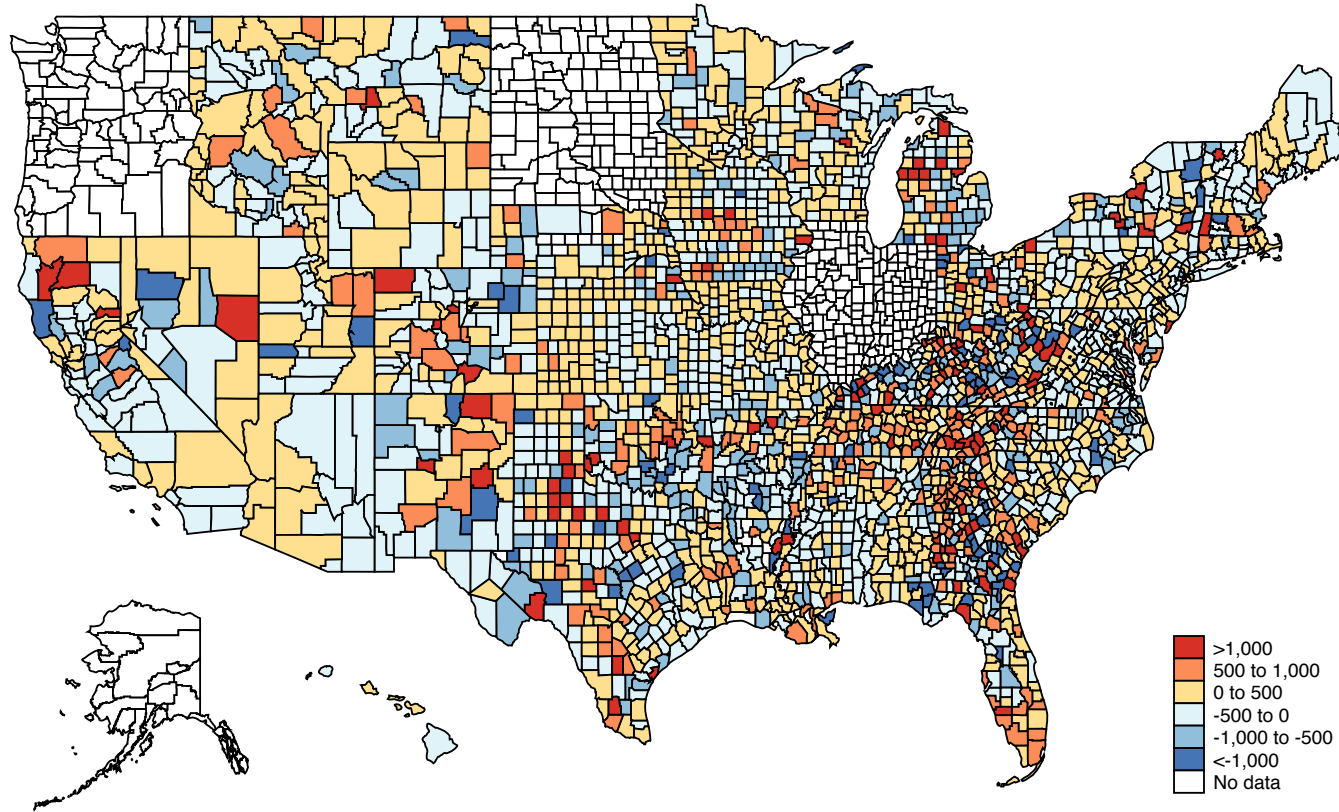


Figure 4: *Notes.* **Change in the Overall Abuse Rate Before and After the Recession.** This map presents the change in the overall abuse rate before and after the recession. We regress the overall abuse rate per 100,000 children on state-year fixed effects. We take the residuals from this regression, and calculate the average for the pre-recession period 2004-2006 and for the post-recession period 2010-2012. We then subtract the pre-recession average from the post-recession average. Alaska and Hawaii are in the bottom left of the figure.

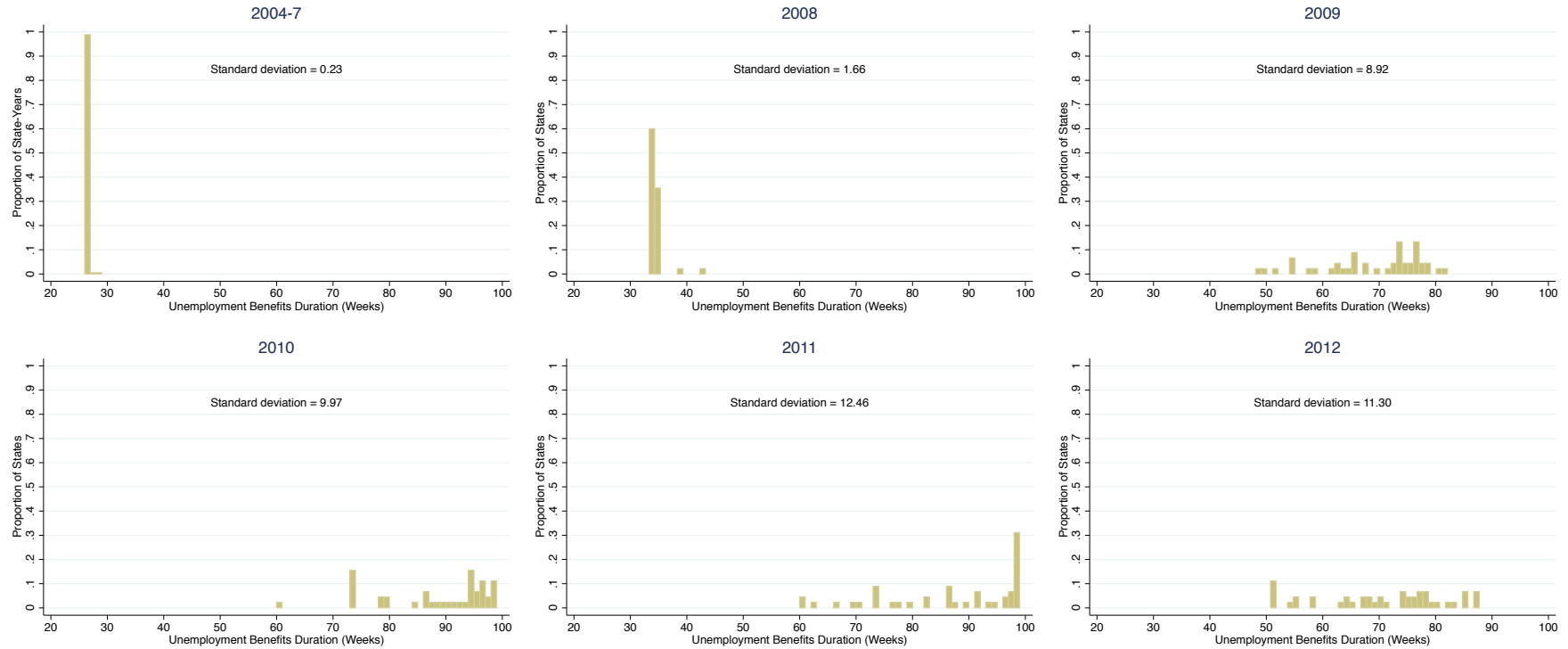


Figure 5: *Notes. The Duration of Unemployment Benefits Over Time.* In this Figure, we plot the duration of unemployment benefits over time. We choose bins with a width of one week (our annual dataset is an average across quarterly data and so we have non-integer values). We plot the proportion of states whose duration of unemployment benefits falls into each bin. The top left panel presents the distribution before the onset of the recession, over the period 2004-7. Nearly every state had 26 weeks of unemployment benefits. The remaining five panels show how that distribution changed in each year from 2008 onwards. States increased the duration of unemployment benefits, and no state provided fewer than 33 weeks over the period 2008-12. In each year, we include all 45 states that are included in the regressions which investigate the effect of unemployment benefits, in Table 3.

Table 1: Summary Statistics

	Mean
Number of Incidents of Physical Abuse	42.22 (135.39)
Number of Incidents of Sexual Abuse	22.60 (67.80)
Number of Incidents of Emotional Abuse	19.68 (176.13)
Number of Incidents of Neglect	182.51 (616.47)
Unemployment Rate (%)	6.85 (2.99)
Fraction Black (%)	9.47 (14.75)
Fraction Hispanic (%)	8.42 (13.59)
Fraction Other Race (%)	3.63 (5.90)
Child Population	24,386 (79,772)
Observations	24,181
Counties	2,803
States	46

*Notes.* In this Table, we present summary statistics for the full sample of 24,181 county-years included in the baseline regressions. We present unweighted means, with unweighted standard deviations in parentheses.

Table 2: Main Results

	Overall		Physical		Neglect		Sexual		Emotional	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Unemployment Rate	0.013 (0.010)	<b>0.096**</b> <b>(0.049)</b>	<b>0.026**</b> <b>(0.0097)</b>	0.060 (0.060)	0.0020 (0.012)	<b>0.20***</b> <b>(0.068)</b>	0.010 (0.014)	0.0037 (0.062)	-0.045 (0.044)	0.032 (0.24)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,468	23,468	24,181	24,181	24,181	24,181	24,181	24,181	23,468	23,468
Counties	2,771	2,771	2,803	2,803	2,803	2,803	2,803	2,803	2,771	2,771
States	45	45	46	46	46	46	46	46	45	45
Mean of outcome	3.77	3.77	1.65	1.65	3.05	3.05	0.94	0.94	-2.19	-2.19
Mean of Unemployment Rate	6.86	6.86	6.85	6.85	6.85	6.85	6.85	6.85	6.86	6.86
Kleibergen-Paap F-stat		34.5		34.9		34.9		34.9		34.5

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. This Table contains the results of the OLS and IV regressions which look at the effect of unemployment on the incidence of overall, physical, sexual, emotional abuse and neglect. In each case, the dependent variable is the natural logarithm of the number of incidents of that abuse type per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of abuse across states. We include state-year fixed effects to control for any changes in the definitions of abuse over time within states. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race (which includes individuals who are Asian, Alaskan Native, American Indian, Native Hawaiian or Other Pacific Islander and Two or More Races). We weight observations by the child population in the county-year. Emotional abuse is not observed in some state-years, which is why there are fewer observations in the regressions in which overall abuse and emotional abuse are the dependent variables.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 3: Unemployment Benefits Mitigate the Effect of Unemployment on Neglect - Second stage

	Continuous Benefit Duration	Set of thresholds
	(1)	(2)
	IV	IV
Unemployment Rate	<b>0.32***</b> (0.12)	<b>0.29***</b> (0.09)
Unemployment Rate x Benefit Duration	<b>-0.0021*</b> (0.0012)	
Unemployment Rate x Thres (48-73]		<b>-0.11**</b> (0.047)
Unemployment Rate x Thres (73-86]		<b>-0.13***</b> (0.049)
Unemployment Rate x Thres (86-99]		<b>-0.14***</b> (0.045)
State-Year FE	Yes	Yes
County FE	Yes	Yes
Controls for Ethnic Group	Yes	Yes
ln(Child Population)	Yes	Yes
Observations	24,145	24,145
Counties	2,799	2,799
States	45	45
Mean of outcome	3.049	3.049
Mean of Unemployment Rate	6.850	6.850
SW F-stat for Unemployment Rate	32.86	89.12
SW F-stat for Unemployment Rate x Benefit Duration	6.43	
SW F-stat for Unemployment Rate x Thres (48-73]		142.3
SW F-stat for Unemployment Rate x Thres (73-86]		84.89
SW F-stat for Unemployment Rate x Thres (86-99]		77.10
P-value Wald test Unemployment Rate x Thres (48-73] = Unemployment Rate x Thres (73-86]		0.057
P-value Wald test Unemployment Rate x Thres (73-86] = Unemployment Rate x Thres (86-99]		0.78
P-value Wald test Unemployment Rate x Thres (48-73] = Unemployment Rate x Thres (86-99]		0.031

*Notes.* Standard errors (in parentheses) are clustered at the state level. This Table contains the second stage results of IV regressions in which we ask whether the effect of unemployment on neglect is mitigated by unemployment benefits. We present the results from a regression in which we interact the unemployment rate with the duration of unemployment benefits that an individual can claim in the state-year. In column (1), we consider a continuous measure of unemployment benefit. In column (2), we consider a set of thresholds [0-48], (48-73], (73-86], (86-99] where each threshold is interacted with the unemployment rate. The reference threshold is [0-48] weeks. Data on the duration of benefits is not available for Hawaii. In each case, the dependent variable is the natural logarithm of the number of incidents of neglect per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of neglect across states. We include state-year fixed effects to control for any changes in the definitions of neglect over time within states. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 4: Private Safety Nets Mitigate the Effect of Unemployment on Neglect

	Heterogeneity by Private Safety Net Coverage				Heterogeneity by Ethnic Group		
	Health Insurance Coverage		Two Employed Parents		White	Black	Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	IV	IV	IV	IV	IV	IV	IV
Unemployment Rate	<b>0.20***</b>	<b>0.14**</b>	0.072	<b>0.19**</b>	0.065	<b>0.31**</b>	0.22
	<b>(0.066)</b>	<b>(0.058)</b>	(0.060)	<b>(0.091)</b>	(0.056)	<b>(0.16)</b>	(0.15)
Fraction Uninsured Children x Unemployment Rate	-0.17	<b>0.52**</b>					
	(0.15)	<b>(0.21)</b>					
Poverty Rate x Unemployment Rate		<b>-0.64***</b>		<b>-0.38**</b>			
		<b>(0.19)</b>		<b>(0.16)</b>			
Fraction Children Two Employed Parents x Unemployment Rate			0.16*	-0.036			
			(0.094)	(0.13)			
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes	No	No	No
ln(Ethnic-Group Specific Child Population)	No	No	No	No	Yes	Yes	Yes
Observations	24,181	24,172	24,181	24,172	23,578	23,461	23,565
Counties	2,803	2,802	2,803	2,802	2,736	2,735	2,736
States	46	46	46	46	45	45	45
Mean of outcome	3.05	3.05	3.05	3.05	2.29	-1.18	-1.58
Mean of Unemployment Rate	6.85	6.85	6.85	6.85	6.85	6.86	6.85
Kleibergen-Paap F-stat					52.2	13.7	16.2
SW F-stat for Unemployment Rate	50.5	85.2	56.7	103.6			
SW F-stat for Unemployment Rate x Private Safety Net	101.5	189.9	221.0	269.7			
SW F-stat for Unemployment Rate x Poverty Rate		235.1		296.6			

*Notes.* Standard errors (in parentheses) are clustered at the state level. In this Table, we ask whether private safety nets mitigate the effect of unemployment on neglect. In columns (1) and (2), we consider the fraction of children who are not covered by health insurance in 2000. In columns (3) and (4), we consider the fraction of children who have two employed parents present in the household with them in 2000. In columns (1) and (3), we interact the unemployment rate with the measure of the private safety net, and interact the Bartik instrument with the measure of the private safety net to create a second instrument. In columns (2) and (4), we additionally control for the interaction between the unemployment rate and the poverty rate in 2000, to ensure the private safety net does not proxy for the interaction with poverty. To do so, we add a third instrument, which is the interaction between the Bartik instrument and the poverty rate in 2000. The poverty rate is measured as a fraction of the population. In columns (5)-(7), we separately count incidents of neglect for White-not-Hispanic, Black-not-Hispanic and Hispanic children for the dependent variables. In each case, the dependent variable is the natural logarithm of the number of incidents of neglect per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of neglect across states. We include state-year fixed effects to control for any changes in the definitions of neglect over time within states. In columns (1)-(4), we control for the natural logarithm of the child population. In columns (5)-(7) we control for the natural logarithm of the ethnic-group specific child population. Finally, we control for the fraction of the overall population that is Black, Hispanic or Other Race. In columns (1)-(4), we weight observations by the child population in the county-year. In columns (5)-(7) we weight observations by the child population of the relevant ethnic group in the county-year. Both the county level unemployment rate and the instrument are not ethnic group specific in any regression. There are fewer observations in the regressions for Black and Hispanic children than White, as some county-years had a total population of zero Black or Hispanic children. The ethnic group of the victim is not observed for Pennsylvania.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 5: Unemployment Decreases Real Expenditure on Basic Items

	Food and Beverages		Clothing and Footwear	
	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
Unemployment Rate	<b>-0.0095***</b> (0.0014)	<b>-0.011***</b> (0.0031)	<b>-0.014***</b> (0.0045)	-0.00040 (0.0090)
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes
Observations	400	400	400	400
States	46	46	46	46
Mean of outcome	7.92	7.92	6.97	6.97
Mean of Unemployment Rate	6.35	6.35	6.35	6.35
Kleibergen-Paap F-stat		18.5		18.5

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. In this Table, we ask whether unemployment decreases real expenditure per capita on basic items that could in turn lead to child neglect. The unit of observation is the state-year. In columns (1) and (2), the dependent variable is the natural logarithm of real expenditure per capita on food and beverages purchased for off-premises consumption (measured in 2012 dollars). In columns (3) and (4), the dependent variable is the natural logarithm of real expenditure on clothing and footwear (measured in 2012 dollars). For the IV regressions, we create a state-level Bartik IV. The specification follows the baseline as closely as possible. We control for state fixed effects, year fixed effects and the fraction of the population that is Black, Hispanic or Other Race. We weight observations by the child population in the state-year. We restrict the sample to the 400 state-years that are included in the baseline regressions.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 6: Unemployment Causes Repeated Neglect

		First Time	Second Time	Third Time	Fourth or More Time
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
Unemployment Rate	<b>0.26***</b> (0.094)	<b>0.23**</b> (0.094)	<b>0.52***</b> (0.18)	<b>0.77***</b> (0.26)	0.30 (0.23)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes	Yes
Observations	17,748	17,748	17,748	17,748	17,748
Counties	2,078	2,078	2,078	2,078	2,078
States	38	38	38	38	38
Mean of outcome	2.93	2.70	-0.29	-2.96	-4.69
Mean of Unemployment Rate	6.86	6.86	6.86	6.86	6.86
Kleibergen-Paap F-stat	26.7	26.7	26.7	26.7	26.7

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. This Table contains the results of IV regressions which ask whether unemployment causes repeated neglect. We separately count first time cases, second time cases, third time cases and fourth or more time cases of neglect, using the Child ID variable in the NCANDS dataset. The dependent variable in column (2) is the natural logarithm of the number of incidents of neglect per year, counting only first time cases, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. In column (3), the dependent variable counts only second time cases, in column (4), third time cases, and in column (5), fourth or more time cases. We can only include states that we can verify consistently record the Child ID variable over time, which means we must drop observations from Alabama, Arkansas, Indiana, Minnesota, New Mexico, South Carolina, Texas and Wisconsin. In column (1), we therefore run the baseline regressions on the sub-sample of 38 states who record the Child ID consistently over time, to check if the main result still exists. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of neglect across states. We include state-year fixed effects to control for any changes in the definitions of neglect over time within states. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



Table 7: Alcohol Consumption Does Not Drive the Effects

	Alcohol Consumption						Abuse and Neglect	
	Both		Male		Female		Overall	Neglect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Heavy	Binge	Heavy	Binge	Heavy	Binge		
	IV	IV	IV	IV	IV	IV	IV	IV
Unemployment Rate	-0.063 (0.051)	-0.10 (0.068)	-0.0091 (0.068)	0.042 (0.12)	<b>-0.12**</b> <b>(0.051)</b>	<b>-0.24***</b> <b>(0.061)</b>	<b>0.093*</b> <b>(0.048)</b>	<b>0.19***</b> <b>(0.065)</b>
Heavy Drinking Prevalence							0.016 (0.040)	0.061 (0.047)
Binge Drinking Prevalence							-0.044 (0.038)	-0.032 (0.042)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	No	No	No	No	No	No	Yes	Yes
Observations	21,370	24,172	21,370	24,172	21,370	24,172	20,782	21,370
Counties	2,802	2,802	2,802	2,802	2,802	2,802	2,770	2,802
States	45	45	45	45	45	45	44	45
Mean of outcome	7.20	16.6	9.71	23.5	4.77	9.99	3.75	3.04
Mean of Unemployment Rate	7.00	6.85	7.00	6.85	7.00	6.85	7.02	7.00
Kleibergen-Paap F-stat	29.8	33.1	29.8	33.1	29.8	33.1	27.0	27.7

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. In this Table, we test the alcohol mechanism. In columns (1) to (6), we use the same Bartik IV strategy to look at whether unemployment causes an increase in alcohol consumption. In columns (1), (3) and (5) we look at the prevalence of heavy drinking, which is defined as consuming more than one drink per day for women and two drinks per day for men for the last thirty days. In columns (2), (4) and (6), we look at the prevalence of binge drinking, which is defined as consuming more than four drinks in a single day for women or five drinks for men, at least once during the past thirty days. In columns (1) and (2) we look at the prevalence rate across genders, in columns (3) and (4) we look at the male prevalence and in columns (5) and (6) female prevalence. In columns (7) and (8), we then control for the prevalence of heavy and binge drinking across genders in the baseline regression. There are no measures of alcohol for D.C. (and so we lose one state, with one county), and heavy drinking is only measured from 2005 onwards. In columns (7) and (8), the dependent variable is the natural logarithm of the number of incidents of that abuse type per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects and state-year fixed effects in all regressions. Finally, we control for the natural logarithm of the child population in columns (7) and (8) and, in all regressions, the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 8: Divorce Does Not Drive the Effects

	Divorce		Maltreatment	
	(1)	(2)	Overall	Neglect
	IV	IV	IV	IV
Unemployment Rate	<b>0.0060***</b> <b>(0.0017)</b>	<b>0.0018**</b> <b>(0.00084)</b>	<b>0.097**</b> <b>(0.049)</b>	<b>0.19***</b> <b>(0.066)</b>
Lag Unemployment Rate		<b>0.0062***</b> <b>(0.0020)</b>		
Divorce Rate			-0.34 (0.37)	-0.38 (0.57)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes
ln(Child Population)	No	No	Yes	Yes
Observations	21,354	21,352	20,766	21,354
Counties	2,799	2,799	2,767	2,799
States	46	46	45	46
Mean of outcome	0.12	0.12	3.75	3.04
Mean of Unemployment Rate	7.00	7.00	7.02	7.00
Kleibergen-Paap F-stat	30.0		30.7	31.2
SW F-stat Unemployment Rate		43.2		
SW F-stat Lag Unemployment Rate		40.3		

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. In this Table, we test the divorce mechanism. In column (1), we use the baseline Bartik IV strategy to look at whether unemployment causes a change in the divorce rate. We consider the divorce rate among individuals aged 18 and over. In column (2), we allow for an lagged effect of unemployment on divorce. To do so we add a second instrument, which is simply the lag of the original instrument. The divorce rate is only measured from 2005 and onwards, which is why we do not lose one year of data when we add the lagged unemployment rate. In columns (3) and (4), we then control for the divorce rate in the baseline regression. In columns (3) and (4), the dependent variable is the natural logarithm of the number of incidents of that abuse type per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects and state-year fixed effects in all regressions. Finally, we control for the natural logarithm of the child population in columns (3) and (4), and, in all regressions, the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 9: The Main Results Capture an Effect on the Actual Incidence of Maltreatment, Not Reporting

	Control Reporting		Professional/Parent Reporters		Unsubstantiated Reports	
	Overall	Neglect	Overall	Neglect	Overall	Neglect
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
Unemployment Rate	<b>0.093*</b> <b>(0.048)</b>	<b>0.19***</b> <b>(0.064)</b>	<b>0.098*</b> <b>(0.054)</b>	<b>0.23***</b> <b>(0.087)</b>	0.035 (0.070)	0.079 (0.085)
Fraction Employed in Schools	0.55 (0.57)	-0.27 (1.15)				
Fraction Employed in Social Services	0.88 (2.46)	1.06 (2.88)				
Fraction Employed in Health Care	0.27 (0.36)	1.03 (0.96)				
Fraction Employed in Police	-2.89 (4.19)	-0.24 (3.65)				
Fraction Employed in Clergy	3.70** (1.62)	0.34 (2.86)				
Fraction Employed in Child Care	-0.091 (1.36)	-0.80 (1.65)				
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,766	21,354	22,686	23,399	23,468	24,181
Counties	2,767	2,799	2,736	2,768	2,771	2,803
States	45	46	43	44	45	46
Mean of outcome	3.75	3.04	3.35	2.53	4.71	4.13
Mean of Unemployment Rate	7.02	7.00	6.85	6.83	6.86	6.85
Kleibergen-Paap F-stat	32.5	33.0	31.2	31.3	34.5	34.9

*Notes.* Standard errors (in parentheses) are clustered at the state level. In this Table, we ask whether the main results capture an effect on the actual incidence of child maltreatment or reporting behaviour. In columns (1) and (2), we control for the fraction of the working-age population employed in high-reporting sectors. In columns (3) and (4), we only count incidents that are reported by a professional or the victim's parent. We restrict the regressions in columns (3) and (4) to state-years for which more than 80% of reports had a non-missing reporter. In columns (5) and (6), we count unsubstantiated reports. In each case, the dependent variable is the natural logarithm of the relevant number of incidents of that abuse type per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of abuse across states. We include state-year fixed effects to control for any changes in the definitions of abuse over time within states. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 10: Robustness of Main Results: Alternative Instruments

	Non-Bartik		Bartik					
	Real Exchange Rate		Value Added		Employment		1995 Weight	
	Overall	Neglect	Overall	Neglect	Overall	Neglect	Overall	Neglect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IV	IV	IV	IV	IV	IV	IV	IV
Unemployment Rate	0.028 (0.065)	<b>0.15*</b> <b>(0.087)</b>	0.086 (0.078)	<b>0.24**</b> <b>(0.12)</b>	0.038 (0.085)	<b>0.21**</b> <b>(0.10)</b>	<b>0.073*</b> <b>(0.038)</b>	<b>0.17***</b> <b>(0.053)</b>
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,468	24,181	23,468	24,181	23,468	24,181	23,459	24,172
Counties	2,771	2,803	2,771	2,803	2,771	2,803	2,770	2,802
States	45	46	45	46	45	46	45	46
Mean of outcome	3.77	3.05	3.77	3.05	3.77	3.05	3.77	3.05
Mean of Unemployment Rate	6.86	6.85	6.86	6.85	6.86	6.85	6.86	6.85
Kleibergen-Paap F-stat	39.0	35.8	20.3	22.2	10.2	12.9	41.2	41.1

*Notes.* Standard errors (in parentheses) are clustered at the state level. In this Table, we check the robustness of the main results to using alternative instruments. In columns (1) and (2), we use the change in the real exchange rate from the previous year, multiplied by the share of the employed working-age population in manufacturing at the county level in 2003. In columns (3)-(8), we use different versions of the Bartik IV. In columns (3) and (4), we use the national-level value added by industry, weighted by the share of the employed working-age population at the county level in 2003. In columns (5) and (6), we use the national-level total number employed by industry, weighted by the share of the employed working-age population at the county level in 2003. In columns (7) and (8), we use the national-level unemployment rate by industry, weighted by the share of the employed working-age population at the county level in 1995. In each case, we present the results from the second stage. The dependent variable is the natural logarithm of the relevant number of incidents of that abuse type per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of abuse across states. We include state-year fixed effects to control for any changes in the definitions of abuse over time within states. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## A Appendix: Tables and Figures

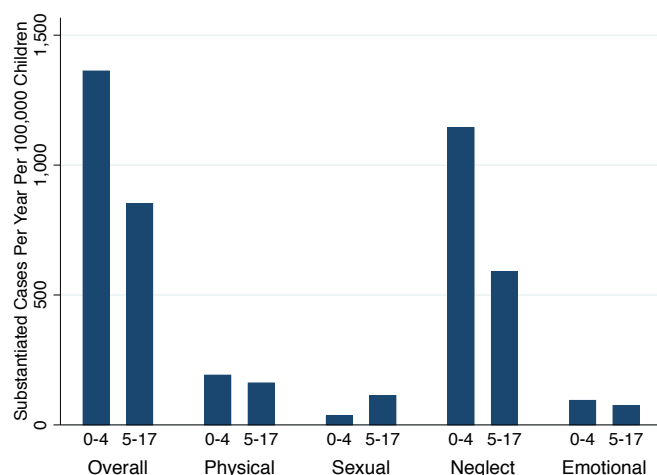


Figure A.1: *Notes.* **Abuse Rates by Child Age.** In this Figure, we plot the weighted mean abuse rate per year per 100,000 children for children aged 0-4 years old and 5-17 years old, for each abuse type. In each county-year, we calculate the abuse rate per 100,000 children by dividing the number of incidents for each age group by the number of children of that age group, using population estimates from the Population and Housing Unit Estimates, before multiplying by 100,000. We then take a weighted mean of the abuse rates across all county-years, where the weights are the child population of the relevant age group in each county-year. We use all available county-years for the entire sample period 2004-12. If a child is abused in multiple ways, we count the case only once in the overall abuse measure, which is why the sum of abuse rates across all individual abuse types exceeds the overall abuse rate.

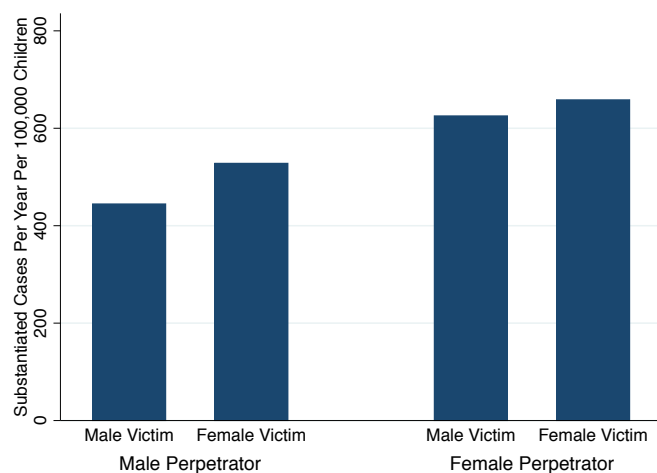


Figure A.2: *Notes.* **Overall Abuse Rate by Perpetrator and Victim Gender.** In this Figure, we plot the weighted mean overall abuse rate per year per 100,000 children for each perpetrator and victim gender combination. In each county-year, we calculate the abuse rate per 100,000 children by dividing the number of incidents of overall abuse in each perpetrator-victim gender group by the number of children of the victim's gender aged 0-17, using population estimates from the Population and Housing Unit Estimates, before multiplying by 100,000. We then take a weighted mean of the abuse rates across all county-years, where the weights are the population of children of the victim's gender aged 0-17 in each county-year. We use all available county-years for the entire sample period 2004-12.

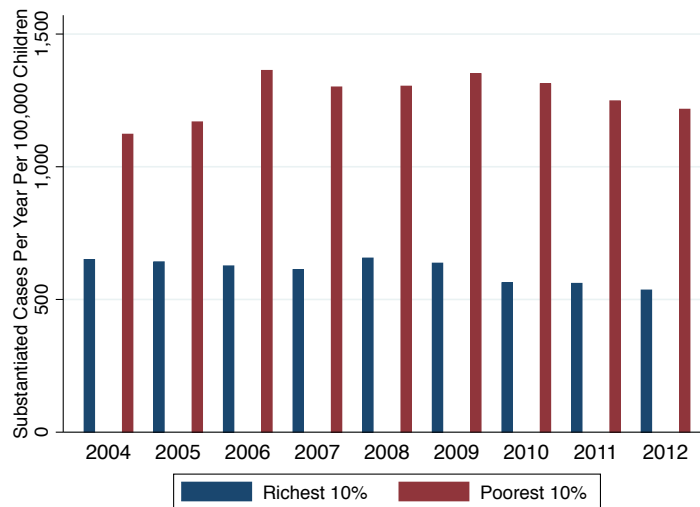


Figure A.3: *Notes.* **Overall Abuse Rate Trends for the Least and Most Poor Counties.** In this Figure, we present the weighted mean overall abuse rate in each year for the initially poorest and least poor 10% of counties in the U.S. We take the 2,803 counties that are included in the final regression analysis, and classify the poorest 10% and least poor 10% in 2003 using the poverty rate taken from the Small Area Income and Poverty Estimates. The poorest 10% of counties are the 273 counties that had a 2003 poverty rate of greater than 20.1%, and the least poor 10% of counties are the 269 counties that had a 2003 poverty rate of less than 7.8%. In each county-year, we calculate the abuse rate per 100,000 children by dividing the number of incidents of overall abuse by the number of children aged 0-17, using population estimates from the Population and Housing Unit Estimates, before multiplying by 100,000. We then take the weighted mean of the abuse rates across all county-years among the least poor or poorest 10% of counties respectively, where the weights are the child population in each county-year. We use all available county-years in any given year to calculate this weighted mean.

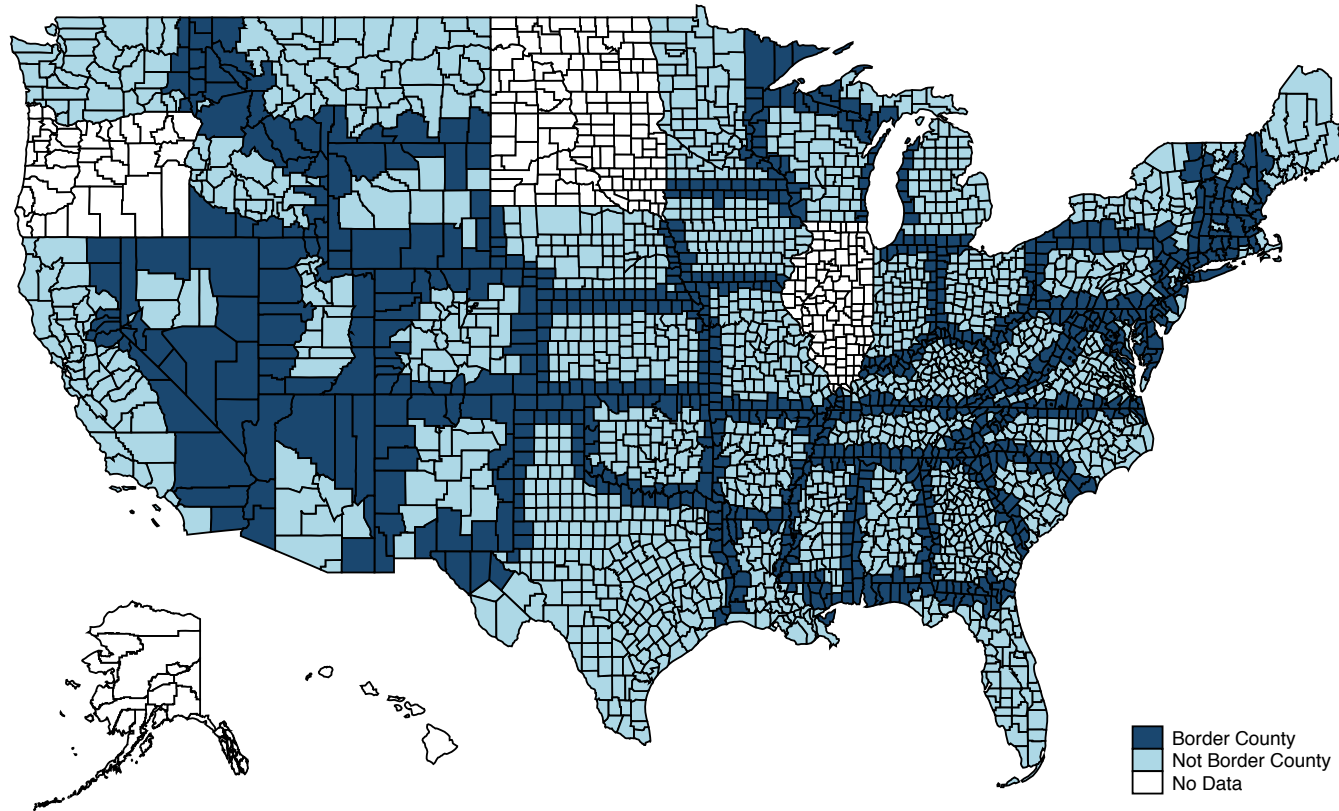


Figure A.4: *Notes. Border Counties.* This map highlights the counties that are included in the sample for the regressions that use the contiguous border counties methodology in Table A.5. These are counties that are part of a contiguous pair which straddles a state border, where both counties are observed in the NCANDS dataset. There are 987 such counties, and they are depicted in dark blue. Alaska and Hawaii are in the bottom left of the figure.

Table A.1: First Stages for the Baseline IV Regressions

	Overall/Emotional	Neglect/Physical/Sexual
	(1)	(2)
	IV	IV
Bartik IV	<b>0.60***</b> <b>(0.10)</b>	<b>0.63***</b> <b>(0.11)</b>
State-Year Fixed Effects	Yes	Yes
County Fixed Effects	Yes	Yes
Controls for Ethnic Group	Yes	Yes
ln(Child Population)	Yes	Yes
Observations	23,468	24,181
Counties	2,771	2,803
States	45	46
Kleibergen-Paap F-stat	34.5	34.9

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. This Table contains the first stage results for the baseline IV estimates in columns (2), (4), (6), (8), and (10) of Table 2. In column (1), we present results from the first stage for overall abuse and emotional abuse. There are fewer observations for those two types of abuse because some state-years are missing observations for emotional abuse. In column (2), we present results from the first stage for neglect, physical abuse and sexual abuse. In each case, we regress the unemployment rate on the Bartik IV. We control for county fixed effects, state-year fixed effects, the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



Table A.2: By Victim Age

	Overall		Neglect	
	Age 0-4	Age 5-17	Age 0-4	Age 5-17
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Unemployment Rate	<b>0.13**</b> <b>(0.062)</b>	<b>0.10**</b> <b>(0.046)</b>	<b>0.25***</b> <b>(0.084)</b>	0.12 (0.11)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes
ln(Age-Group Specific Child Population)	Yes	Yes	Yes	Yes
Observations	23,468	23,468	24,181	24,181
Counties	2,771	2,771	2,803	2,803
States	45	45	46	46
Mean of outcome	2.57	3.24	2.12	2.27
Mean of Unemployment Rate	6.86	6.86	6.85	6.85
Kleibergen-Paap F-stat	32.4	35.0	32.6	35.5

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. This Table contains the results of the IV regressions which look at the effect of unemployment on the incidence of overall abuse and neglect by victim age. In each case, the dependent variable is the natural logarithm of the number of incidents of abuse of that type per year for a victim of the given age group, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We look at two age groups: children aged 0-4 years and children aged 5-17 years. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of abuse across states. We include state-year fixed effects to control for any changes in the definitions of abuse over time within states. Finally, we control for the natural logarithm of the child population of the given age group and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population of the given age-group in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.3: By Perpetrator Gender

	Overall		Neglect	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Unemployment Rate	0.031 (0.060)	<b>0.18***</b> <b>(0.070)</b>	0.13 (0.092)	<b>0.28***</b> <b>(0.083)</b>
State-Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes
Observations	20,893	20,893	21,606	21,606
Counties	2,509	2,509	2,541	2,541
States	42	42	43	43
Mean of outcome	2.89	2.97	1.59	2.58
Mean of Unemployment Rate	6.87	6.87	6.85	6.85
Kleibergen-Paap F-stat	29.3	29.3	30.0	30.0

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. This Table contains the results of the IV regressions which look at the effect of unemployment on the incidence of overall abuse and neglect by perpetrator gender. In each case, the dependent variable is the natural logarithm of the number of incidents of abuse of that type per year perpetrated by someone of the given gender, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of abuse across states. We include state-year fixed effects to control for any changes in the definitions of abuse over time within states. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.4: Unemployment Benefits Mitigate the Effect of Unemployment on Neglect - First Stage

	Continuous Benefit Duration		Set of thresholds			
	Unemployment Rate	Unemployment Rate x Benefit Duration	Unemployment Rate	Unemployment Rate x Thres (48-73]	Unemployment Rate x Thres (73-86]	Unemployment Rate x Thres (86-99]
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
Bartik IV	<b>1.35***</b> (0.26)	<b>153.21***</b> (37.08)	<b>-0.26***</b> (0.07)	<b>-0.41***</b> (0.09)	<b>-0.65***</b> (0.11)	<b>-0.53***</b> (0.075)
Bartik IV x Benefit Duration	<b>-0.007***</b> (0.002)	<b>-1.18***</b> (0.40)				
Bartik IV x Thres (48-73]			<b>0.052***</b> (0.0027)	<b>0.11***</b> (0.0037)	<b>0.0055***</b> (0.0018)	<b>0.0051***</b> (0.0012)
Bartik IV x Thres (73-86]			<b>0.052***</b> (0.0039)	<b>0.0028***</b> (0.0009)	<b>0.11***</b> (0.0040)	<b>0.0034***</b> (0.0009)
Bartik IV x Thres (86-99]			<b>0.049***</b> (0.0029)	<b>0.0015***</b> (0.0005)	0.0006 (0.0008)	<b>0.11***</b> (0.0027)
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes
ln(Child Population	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,145	24,145	24,145	24,145	24,145	24,145
Counties	2,799	2,799	2,799	2,799	2,799	2,799
States	45	45	45	45	45	45

*Notes.* Standard errors (in parentheses) are clustered at the state level. This Table contains the first stage results of the IV regressions in which we ask whether the effect of unemployment on neglect is mitigated by unemployment benefits. The second stage results are reported in Table 3 and refer to a regression in which we interact the unemployment rate with the duration of unemployment benefits that an individual can claim in the state-year. In columns (1)-(2), we consider a continuous measure of unemployment benefit. In columns (3)-(6), we consider a set of thresholds [0-48], (48-73], (73-86], (86-99] where each threshold is interacted with the unemployment rate. The reference threshold is [0-48] weeks. Data on the duration of benefits is not available for Hawaii. In each case, the dependent variable is the natural logarithm of the number of incidents of neglect per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of neglect across states. We include state-year fixed effects to control for any changes in the definitions of neglect over time within states(73-86]. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.5: Unemployment Benefits Mitigate the Effect of Unemployment on Neglect - Contiguous Border Pairs Sample - Second stage

	Continuous Benefit Duration			Set of thresholds	
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
Unemployment Rate	<b>0.18*</b> (0.10)	<b>0.26*</b> (0.14)	0.18 (0.18)	<b>0.24*</b> (0.13)	0.083 (0.17)
Unemployment Rate x Benefit Duration		-0.0025 (0.0016)	-0.003 (0.0025)		
Unemployment Rate x Thres (48-73]				-0.098 (0.066)	-0.047 (0.14)
Unemployment Rate x Thres (73-86]				-0.13 (0.083)	-0.095 (0.13)
Unemployment Rate x Thres (86-99]				<b>-0.071*</b> (0.041)	0.0016 (0.094)
State-Year FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Border-Pair-Year Fixed Effects	No	No	Yes	No	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes	Yes
Observations	17,915	17,915	17,915	17,915	17,915
Mean of outcome	3.056	3.056	3.056	3.056	3.056
Mean of Unemployment Rate	6.798	6.798	6.798	6.798	6.798
Kleibergen-Paap F-stat	11.36				
SW F-stat for Unemployment Rate		47.52	50.01	33.02	28.41
SW F-stat for Unemployment Rate x Benefit Duration		9.759	14.55		
SW F-stat for Unemployment Rate x Thres (48-73]				53.36	35.17
SW F-stat for Unemployment Rate x Thres (73-86]				34.9	38.89
SW F-stat for Unemployment Rate x Thres (86-99]				26.99	44.07
Wald test Unemployment Rate x Thres (48-73] = Unemployment Rate x Thres (73-86]				0.41	0.15
Wald test Unemployment Rate x Thres (73-86] = Unemployment Rate x Thres (86-99]				0.28	0.18
Wald test Unemployment Rate x Thres (48-73] = Unemployment Rate x Thres (86-99]				0.48	0.55

*Notes.* We use two-way clustering of the standard errors at the level of the state and border segment. This Table contains the second stage results of IV regressions in which we ask whether the effect of unemployment on neglect is mitigated by unemployment benefits in a sample restricted to contiguous county-pairs that straddle a state boundary. We present the results from a regression in which we interact the unemployment rate with the duration of unemployment benefits that an individual can claim in the state-year. We check whether the effect of unemployment on neglect still exists in this sub-sample of county-pairs (column (1)), and whether the heterogeneity by benefits duration exists (column (2)). In column (3), we use the contiguous border counties methodology to ask whether extending the duration of benefits causes a reduction in the effect of unemployment on neglect. In columns (2)-(3), we consider a continuous measure of unemployment benefit. Column (4) replicates column (2), while column (5) replicates column (3) but with discrete unemployment benefit duration expressed in thresholds. In columns (4)-(5), we consider a set of thresholds [0-48], (48-73], (73-86], (86-99] where each threshold is interacted with the unemployment rate. Data on the duration of benefits is not available for Hawaii. In each case, the dependent variable is the natural logarithm of the number of incidents of neglect per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of neglect across states. We include state-year fixed effects to control for any changes in the definitions of neglect over time within states. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year. In columns (3) and (5), we additionally control for county-pair-year fixed effects.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.6: SNAP and EITC: First Stages

	SNAP		EITC	
	Unemployment Rate	SNAP x Unemployment Rate	Unemployment Rate	EITC x Unemployment Rate
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Bartik IV	0.87*** (0.14)	0.41*** (0.16)	0.75*** (0.12)	1.64* (0.98)
SNAP x Bartik IV	-0.36*** (0.13)	-0.50* (0.28)		
EITC x Bartik IV			-0.017** (0.0077)	0.068 (0.21)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes
Observations	21,833	21,833	19,494	19,494
Counties	2,803	2,803	2,803	2,803
States	46	46	46	46
SW F-stat for Unemployment Rate	30.6		4.53	
SW F-stat for Interaction		2.38		0.37

*Notes.* Standard errors (in parentheses) are clustered at the state level. This Table presents the first stage results for IV regressions which ask whether the Supplemental Nutritional Assistance Program (SNAP) or the state-level Earned Income Tax Credit (EITC) mitigated the effect of unemployment on neglect. In columns (1) and (2), we consider SNAP. We create a variable that tells us what fraction of the twelve months of the year each state had relaxed asset test eligibility requirements for SNAP. We interact this variable with the unemployment rate, and create a second instrument by interacting this variable with the Bartik instrument. In column (1), we present the results from the first stage for the unemployment rate, and in column (2), the results from the first stage for the interaction. In columns (3) and (4), we consider EITC. We create a variable that tells us the percentage of the federal EITC offered by each state in each year in its own state-level EITC. As with SNAP, we interact this variable with the unemployment rate and create a second instrument by interacting this variable with the Bartik instrument. In column (3), we present results from the first stage for the unemployment rate, whilst in column (4), we present results from the first stage for the interaction. Our measure of SNAP is only available for the years 2004-11, whilst our measure of EITC is only available for the years 2004-10. We weight observations by the child population in the county-year in all regressions.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.7: Effect on Neglect for Children Living in Inadequate Versus Adequate Housing

		Inadequate	Adequate
	(1)	(2)	(3)
	IV	IV	IV
Unemployment Rate	<b>0.19**</b> <b>(0.076)</b>	-0.11 (0.15)	<b>0.20**</b> <b>(0.084)</b>
State-Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes
Observations	11,152	11,152	11,152
Counties	1,514	1,514	1,514
States	23	23	23
Mean of outcome	3.38	0.15	2.96
Mean of Unemployment Rate	7.22	7.22	7.22
Kleibergen-Paap F-stat	21.3	21.3	21.3

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. To calculate p-values for the tests of statistical significance, we compare test statistics to the T(G-1) distribution due to the small number of clusters (where G is the number of clusters). This Table contains the results of IV regressions which look at the effect of unemployment on neglect, allowing for different effects among children living in inadequate and adequate housing. In column (2), we count incidents where the child is described as living in inadequate housing by the Child Protection Services officer. In column (3), we count incidents where the child is described as living in adequate housing by the Child Protection Services officer. We only include state-years for which more than 80% of reports had a non-missing value for this housing measure. We are left with a sub-sample of 23 states. In column (1), we therefore run the baseline regression on that sub-sample of states to check whether the main effects still exist. In each case, the dependent variable is the natural logarithm of the number of incidents of neglect per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of neglect across states. We include state-year fixed effects to control for any changes in the definitions of neglect over time within states. Finally, we control for the natural logarithm of the child population and the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.8: Correlation Between Poverty Rate and Private Safety Net Coverage

	Fraction Uninsured Children	Fraction Children Two Employed Parents
	(1)	(2)
	OLS	OLS
Poverty Rate	<b>0.72***</b> <b>(0.099)</b>	<b>-1.29***</b> <b>(0.11)</b>
Observations	2,802	2,802
States	46	46
Mean of outcome	0.12	0.44
Mean of Poverty Rate	13.4	13.4

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. In this Table, we ask whether the poverty rate in 2000 is unconditionally correlated with each of the two private safety net measures used in Table 4. The poverty rate is measured as a fraction of the population. In column (1), the dependent variable is the fraction of children who are not covered by health insurance in 2000. In column (2), the dependent variable is the fraction of children who have two employed parents in 2000. We restrict the sample to the 2,803 counties included in the baseline regressions. For one county the poverty rate in 2000 is missing. We weight observations by the child population in the county in 2000.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.9: Probability of Having Two Employed Parents Present in the Household by Ethnic Group

	Sample Includes Black, Hispanic and White	Sample Includes Black and Hispanic
	(1)	(2)
	OLS	OLS
Black	<b>-0.23***</b> (0.011)	<b>-0.074***</b> (0.0097)
Hispanic	<b>-0.16***</b> (0.0098)	
Observations	277,810	74,074
Mean of outcome	0.39	0.25

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. In this Table, we present results from OLS regressions which ask whether there is a difference in the probability that children have two employed parents present in the household by ethnic group. In both regressions, the dependent variable is a dummy variable that takes the value 1 if a child has two employed parents living in the household with them, and 0 otherwise. The unit of observation is the child, and we include children aged 0-17 from the 2003 round of the American Community Survey in the sample. In column (1), we restrict the sample to White, Black and Hispanic children, and compare the probability for Black and Hispanic children to the White base group. In column (2), we restrict the sample to Black and Hispanic children, and compare the probability for Black children to the Hispanic base group. We define Black and White as Black-not-Hispanic and White-not-Hispanic respectively. We weight observations using the person weights provided in the ACS.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



Table A.10: Robustness of Main Results: Alternative Choices for the LHS Variable

	ln(Rate)		IHS Rate		Add 0.01		Add 0.0001		Number of Children	
	Overall	Neglect	Overall	Neglect	Overall	Neglect	Overall	Neglect	Overall	Neglect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Unemployment Rate	<b>0.096*</b>	<b>0.21***</b>	<b>0.094*</b>	<b>0.18***</b>	<b>0.093*</b>	<b>0.18***</b>	<b>0.099**</b>	<b>0.23***</b>	0.11	<b>0.25***</b>
	<b>(0.053)</b>	<b>(0.073)</b>	<b>(0.053)</b>	<b>(0.066)</b>	<b>(0.048)</b>	<b>(0.063)</b>	<b>(0.050)</b>	<b>(0.074)</b>	(0.067)	<b>(0.092)</b>
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,468	24,181	23,468	24,181	23,468	24,181	23,468	24,181	17,403	17,748
Counties	2,771	2,803	2,771	2,803	2,771	2,803	2,771	2,803	2,046	2,078
States	45	46	45	46	45	46	45	46	37	38
Mean of outcome	6.41	5.67	7.15	6.51	3.86	3.23	3.68	2.87	3.66	2.88
Mean of Unemployment Rate	6.86	6.85	6.86	6.85	6.86	6.85	6.86	6.85	6.84	6.86
Kleibergen-Paap F-stat	32.7	33.1	32.7	33.1	34.5	34.9	34.5	34.9	25.2	26.7

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. This Table contains the results of IV regressions which look at the robustness of the effect of unemployment on the incidence of overall abuse and neglect with respect to the choice of the dependent variable. In columns (1) and (2), we take the natural logarithm of the rate of abuse per 100,000 children. We first add 0.001 incidents to the number of abuses before taking the rate, to ensure that no county-years have a rate of zero before taking the natural logarithm. In columns (3) and (4), we take the inverse hyperbolic sine transformation of the rate of abuse per 100,000 children. In columns (1)-(4), we do not control for the natural logarithm of the child population, as the left hand side variable is already based on a rate per 100,000 children. In columns (5) and (6), the dependent variable is the natural logarithm of the number of incidents of overall abuse or neglect, after first adding 0.01. In columns (7) and (8), we again take the natural logarithm of the number of incidents, but now add 0.0001. In columns (9) and (10), we count the number of children who are victims of overall abuse and neglect, rather than the number of incidents. Only 38 states record a unique child ID variable, and so can be included in these two regressions. In columns (5)-(10), we control for the natural logarithm of the child population. We control for county fixed effects in all regressions. We have therefore implicitly controlled for state fixed effects, and so control for differences in definitions of abuse across states. We include state-year fixed effects to control for any changes in the definitions of abuse over time within states. Finally, we control for the fraction of the overall population that is Black, Hispanic or Other Race. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.11: Robustness of Main Results: Specification Changes

	Control County Trends		Drop State-Year FE		Reduced Form		2003 Child Weight		Total Pop Weight		Cluster County Level	
	Overall	Neglect	Overall	Neglect	Overall	Neglect	Overall	Neglect	Overall	Neglect	Overall	Neglect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	IV	IV	IV	IV	OLS	OLS	IV	IV	IV	IV	IV	IV
Unemployment Rate	0.042	<b>0.098**</b>	0.067	<b>0.13**</b>			<b>0.095*</b>	<b>0.20***</b>	<b>0.096**</b>	<b>0.20***</b>	<b>0.096*</b>	<b>0.20***</b>
	(0.041)	<b>(0.046)</b>	(0.052)	<b>(0.059)</b>			<b>(0.049)</b>	<b>(0.067)</b>	<b>(0.045)</b>	<b>(0.063)</b>	<b>(0.051)</b>	<b>(0.063)</b>
Bartik IV					<b>0.054*</b>	<b>0.13***</b>						
					<b>(0.028)</b>	<b>(0.038)</b>						
State-Year Fixed Effects	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear County Trends	Yes	Yes	No	No	No	No	No	No	No	No	No	No
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,468	24,181	23,468	24,181	23,480	24,193	23,468	24,181	23,468	24,181	23,468	24,181
Counties	2,771	2,803	2,771	2,803	2,771	2,803	2,771	2,803	2,771	2,803	2,771	2,803
States	45	46	45	46	45	46	45	46	45	46	45	46
Mean of outcome	3.77	3.05	3.77	3.05	3.77	3.05	3.77	3.05	3.77	3.05	3.77	3.05
Mean of Unemployment Rate	6.86	6.85	6.86	6.85			6.86	6.85	6.86	6.85	6.86	6.85
Kleibergen-Paap F-stat	33.6	30.5	21.0	22.6			32.3	33.0	40.0	40.8	67.4	70.5

*Notes.* Standard errors (in parentheses) are clustered at the state level in the regressions in columns (1) to (10), and at the county level in columns (11) and (12). In this Table, we test the robustness of the main results for overall abuse and neglect. In columns (1) and (2), we control for linear county trends. In columns (3) and (4), we drop state-year fixed effects and add year fixed effects. In columns (5) and (6), we run reduced form OLS regressions with the instrument as the right hand side variable. In columns (7) and (8), we weight observations using the child population in the county in 2003. In columns (9) and (10), we weight observations using the total population in the county-year. Finally, in columns (11) and (12), we cluster standard errors at the level of the county. In each case, the dependent variable is the natural logarithm of the number of incidents of that abuse type per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. In columns (1)-(6) and (11)-(12), we weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A.12: Robustness of Main Results: Sample Changes

	Drop Large Counties		Drop Small Counties	
	Overall	Neglect	Overall	Neglect
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Unemployment Rate	<b>0.12***</b> <b>(0.043)</b>	<b>0.22***</b> <b>(0.065)</b>	<b>0.089*</b> <b>(0.050)</b>	<b>0.20***</b> <b>(0.069)</b>
State-Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes
ln(Child Population)	Yes	Yes	Yes	Yes
Observations	23,450	24,163	21,480	22,186
Counties	2,769	2,801	2,545	2,577
States	45	46	45	46
Mean of outcome	3.76	3.05	4.20	3.52
Mean of Unemployment Rate	6.86	6.85	7.02	7.00
Kleibergen-Paap F-stat	35.6	36.1	33.6	34.1

*Notes.* Standard errors (in parentheses) are clustered at the state level in all regressions. In this Table, we test the robustness of the main results for overall abuse and neglect. In columns (1) and (2), we drop the two counties with a population of more than one million children in 2003 (Harris, Texas; and Los Angeles, California). In columns (3) and (4), we drop the counties in the smallest 10% of counties in the United States in terms of 2003 child population, which are the counties with fewer than 1,232 children. In each case, the dependent variable is the natural logarithm of the number of incidents of that abuse type per year, after first adding 0.001 incidents to every county-year to ensure that no county-years have zero incidents. We weight observations by the child population in the county-year.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## B Safety Nets

### B.1 Social Safety Nets

#### B.1.1 Contiguous Border Counties Methodology

We restrict the sample to pairs of contiguous counties that straddle a state border. We include only county-pair-years where both counties are observed in the NCANDS dataset. We have 1,090 county-pairs, involving 987 different counties, for whom all the variables for the regression analysis are measured.<sup>45</sup> We highlight those 987 counties in the map in Figure A.4. We run the baseline IV regressions on this sample of county-pairs, but now control for county-pair-year fixed effects. We then identify effects by comparing the variation in neglect over time within counties between contiguous counties that straddle a state border. If unobservable worker characteristics are similar across contiguous counties, this methodology allows us to overcome the identification concern expressed in Section 5.1. We therefore estimate the following, where equations (6) and (7) are the first stages, and equations (5) is the second stage of the IV procedure:

$$Y_{cspt} = \beta_1 Unemp_{cst} + \beta_2 (Ben_{st} \times Unemp_{cst}) + X'_{cst} \phi + \psi_{st} + \eta_c + \gamma_{pt} + \epsilon_{cspt} \quad (5)$$

$$Unemp_{cspt} = \delta_1 (\Sigma_j w_{csj} N_{tj}) + \delta_2 (Ben_{st} \times (\Sigma_j w_{csj} N_{tj})) + X'_{cst} \theta + \pi_{st} + \tau_c + \omega_{pt} + \nu_{cspt} \quad (6)$$

$$(Ben_{st} \times Unemp_{cst}) = \mu_1 (\Sigma_j w_{csj} N_{tj}) + \mu_2 (Ben_{st} \times (\Sigma_j w_{csj} N_{tj})) + X'_{cst} \vartheta + \kappa_{st} + \nu_c + \iota_{pt} + \varepsilon_{cspt} \quad (7)$$

Here,  $p$  denotes the county-pair. The difference from regression equation (1) is the addition of the county-pair-year fixed effects,  $\gamma_{pt}$ . The unit of observation is now the county-pair-year<sup>46</sup>. Each county therefore enters the regression as many times as it has border pairs, in a given year. The modal number of contiguous border pairs a county

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<sup>45</sup>In the United States as a whole, there are 1,308 contiguous county-pairs that straddle a state border.

<sup>46</sup>As in Dube et al. (2010), we must organise the data at the county-pair-year level rather than the county-year level for the following reason. There are more county-pair-year fixed effects than we have degrees of freedom. If we included a dummy variable for every county-pair-year we would be unable to estimate all these coefficients. However, if we organise the data at the county-pair-year level, there is only a single non-zero county-pair-year dummy variable for each observation. This property allows us to demean all the observations within the county-pair-year, treating the county-pair-year dummies as nuisance parameters rather than explicitly including them in the regression. We can then estimate the model using these county-pair-year-demeaned variables. To estimate equation (5), we first demean all the observations within counties (to implicitly control for county fixed effects), and then demean the resulting variables within county-pair-years.

has is two, and the maximum is nine. A county’s inclusion in multiple county-pairs will induce a correlation in the error term between those county-pairs. To deal with this, we follow Dube et al. (2010) and use two-way clustering of the standard errors.<sup>47</sup> We cluster at the state level as before, and at the level of the border segment.<sup>48</sup>

In column (1) Table A.5, we first check whether the main effect of unemployment on neglect still exists in the sub-sample of contiguous county-pairs, which it does. In column (2), we check whether the policy heterogeneity still exists in this sub-sample. The point estimate on the interaction term in column (2) is very similar in size to the estimate for the main sample in column (1) Table 3, but the effect is no longer statistically significant. This may be due to low statistical power since the border counties constitute a little over one third of the number of counties in the main sample. Bearing that in mind, we present the results from the regression which uses the contiguous border counties methodology, in column (3). We find that the absolute size of the coefficient on *Unemployment Rate*  $\times$  *Benefit Duration* increases. The point estimate is nearly fifty percent larger than the estimate in the main sample in column (1) of Table 3. This suggests that the estimates in Table 3 may underestimate the true mitigating effect of extending the duration of benefits. However, the effects are not statistically significant and so the results are not conclusive. The same is true when we use the thresholds instead of the continuous measure of the duration of unemployment benefits. This is perhaps unsurprising given that the policy heterogeneity is not significant in column (2), and only just significant in column (4), and may be explained by low statistical power.

### **B.1.2 The Supplemental Nutrition Assistance Program (SNAP) and the Earned Income Tax Credit (EITC)**

The US Department of Agriculture’s (USDA) SNAP, previously known as the Food Stamp program, offers financial assistance for food purchases. Like UI, SNAP is also expanded after the recession (Mulligan, 2012). The federal government allowed states to increase or eliminate asset tests for food stamps, thereby widening eligibility. As a consequence, by 2010 half of non-elderly households with an unemployed head or spouse participated in the program (Hagedorn et al., 2013). We create a variable, *SNAP*, that measures the

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<sup>47</sup>In addition, we ran regressions in which we adjusted the weights to mechanically ensure that the weight received by each county-year does not depend on the number of contiguous border pairs it has. To do this, we multiplied the original weights by the inverse of the number of contiguous border pairs a county has in each year. The results are very similar and are available upon request.

<sup>48</sup>A border segment is a group of county-pairs that run along the same boundary between two states. We have 45 states and 94 border segments in the regressions in Table A.5.

fraction of months in the year for which each state increased or eliminated asset tests, using monthly data from the USDA’s SNAP Policy Database for the years 2004-11.

The EITC is a tax credit that is primarily targeted towards families with children (Bitler and Hoynes, 2016). The tax credit is refundable, meaning that any surplus credit over tax liabilities can be claimed as a benefit. Though it is targeted at people in work, unemployed individuals can claim EITC providing they have worked at some point during the tax year. The main EITC is federal, but some states additionally offer their own EITC (Williams, 2017). By the end of the sample period, twenty-three states had their own EITC, which varied in generosity from 3.5 to 45% of the federal EITC.<sup>49</sup> We create a variable *EITC*, which measures the percentage of the federal EITC offered by each state in each year.<sup>50</sup> This dataset comes from the Tax Policy Center, and is available for the years 2004-10.

We ask whether the effect of unemployment on neglect is smaller in states that increased or eliminated asset tests for SNAP, or offered a larger tax credit in the state EITC. In separate regressions, we interact the unemployment rate with *SNAP* and *EITC* respectively, and interact those variables with the Bartik instrument to create a second instrument. However, we cannot learn much about the effect of either program, because the instruments do not identify separate variation in each of the endogenous variables well. The Sanderson-Windmeijer F-statistic on the first stage for the interaction term is just 2.38 and 0.37 for SNAP and EITC respectively. We present the first stage results in Table A.6.

## B.2 Private Safety Nets and Poverty

In Section 5.2.1, we asked whether the effect of unemployment occurs in the poorest households. To do this, we used a variable from the NCANDS which indicates whether the house in which the child resides is inadequate. This indicator is not recorder for every child-report. For the regressions in Table A.7, we restrict the sample to state-years in which this indicator is non-missing for over 80% of substantiated reports. This gives us a sub-sample of twenty-three states, and so in column (1) of Table A.7, we first check

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<sup>49</sup>Every state other than Minnesota directly set its state EITC as a percentage of the federal EITC. Depending on the family’s income level, the tax credit in Minnesota ranged between 25 and 45% of the federal credit.

<sup>50</sup>For the vast majority of states, the state EITC is refundable like the federal EITC. However, five states had a non-refundable state EITC for at least some years during the sample period. We therefore also create a variable that treats any state-year with a non-refundable EITC as missing and the results are very similar. Further details are in the Data Appendix.

that the effect of unemployment on neglect still exists in that sub-sample, which it does. Since we cluster standard errors at the state level, Wald tests may now overreject as we have a small number of clusters. To deal with this, we compare test statistics to the  $T(G-1)$  distribution, where  $G = 23$  (the number of clusters), following Cameron and Miller (2015). We cannot use a wild bootstrap, the preferred method of dealing with a small number of clusters, as the properties of the wild bootstrap have not been derived for the IV estimator.

## C Other Robustness Tests

### C.1 Alternative Choices of the Dependent Variable

We check the robustness of the results to five alternative choices for the dependent variable in Table A.10. In columns (1) and (2), we use the natural logarithm of the rate of maltreatment per 100,000 children, and accordingly no longer control for the natural logarithm of the child population on the right hand side.<sup>51</sup> This is a restricted version of the baseline regression in which the coefficient on the natural logarithm of the child population equals one. In columns (3) to (8), we use three alternative methods of dealing with zeros. In columns (3) and (4), we take the inverse hyperbolic sine transformation of the rate of maltreatment per 100,000 children, following Woolley (2011). In columns (5) and (6), we add 0.01 to the number of incidents before taking the natural logarithm, and in columns (7) and (8), we add 0.0001. Finally, in columns (9) and (10), we count the number of children who are victims of maltreatment in each county-year, rather than the number of incidents. In those regressions, we must restrict the sample to the states that report a unique child ID.

The results are again robust. The effect of unemployment on neglect is statistically significant at a 1% level in every case, and the size of the point estimate is very similar to the main results. The effect on overall abuse is statistically significant at least at a 10% level in all but one case (in column (9), the p-value is 0.113), and again the point estimates are similar in size to the main results.

### C.2 Other Changes to the Specification

We make several other changes to the specification, and present the results in Table A.11. In columns (1) and (2), we control for linear county trends. In columns (3) and (4), we

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<sup>51</sup>To do this, we add 0.001 to the number of incidents before calculating the rate.

drop the state-year fixed effects. In columns (5) and (6), we run reduced form OLS regressions with the Bartik IV as the right hand side variable. In columns (7) and (8), we weight observations using the child population in the county at the start of the sample period, in 2003. In columns (9) and (10), we weight observations by the total population of all ages in the county-year. Finally, in columns (11) and (12), we cluster standard errors at the county level. The results are robust. The effect on neglect is statistically significant at least at the 5% level in every case, whilst the effect on overall abuse is significant at least at the 10% level in all but two cases.

### C.3 Changes to the Sample

As a final set of robustness tests, we check in Table A.12 that the results are not driven by outlier counties with very small or large populations of children. In columns (1) and (2), we drop the counties with a child population of more than one million at the start of the sample period, in 2003.<sup>52</sup> In columns (3) and (4), we drop the smallest 10 percent of counties in terms of their child population at the start of the sample period, in 2003.<sup>53</sup> In both cases, the results for overall abuse and neglect are robust, both in terms of the size and statistical significance of the effect.

## D Description of the Construction of the Variables

### D.1 Organising the Data by Calendar Year and Report Date

The NCANDS data is released annually, and is organised by Federal Fiscal Year (FFY) (running from 1st October to 30th September), and by the investigation disposition date (the date of the outcome of the CPS investigation). For example, the NCANDS dataset for 2012 contains every child-report for which the outcome of the investigation occurred between 1st October 2011 and 30th September 2012. We would ideally like to organise the data by the date of the incident of abuse, but that is unobserved. The closest that we can get to the date of the incident is the date of report, which is also contained in the dataset. We therefore reorganise the data by the date of report and calendar year. This seems straightforward. However, an issue arises because seventeen states are not observed in at least one year during the sample period. The problem is that one missing

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<sup>52</sup>There are two counties with a child population of more than one million in 2003. These are: Los Angeles, California (2,678,788 children), and Harris, Texas (1,043,580 children).

<sup>53</sup>We drop the counties with a population of less than 1,231 children in 2003 (the 10th percentile among all counties in the U.S. in that year).



year of NCANDS data by the federal fiscal year and investigation disposition date does not translate into only one missing year by calendar year and report date. To see this, take the example of Indiana, as demonstrated in Figure D.1. Indiana is missing the FFY 2012. Our dataset for this state then does not include any incident whose investigation is concluded between 1st October 2011 and 30th September 2012, as indicated by the solid cross. Now suppose that an incident is reported on 15th September 2011. Whilst this incident is reported within a ‘non-missing’ FFY of the dataset, if the investigation is concluded more than 15 days after the report is made, then the investigation disposition date falls in a missing FFY and this incident will be missing from the data. To deal with this, for each missing FFY of the data, we extend the missing dates to twelve months before the start of the missing FFY, as demonstrated by the dashed cross in the Figure. Over 99% of reports reach an investigation disposition within twelve months of the report date, and so doing this we can claim to capture over 99% of all cases of child abuse in the final sample period.

As can be seen in Figure D.1, for some state-years we then only observe reports for part of the calendar year. For example, for Indiana in 2010 we only observe reports from 1st January until 30th September 2010. To deal with this, we firstly restrict the sample period to 2004 to 2012 (when the majority of states have a complete year’s worth of data). Secondly, for the states with missing years, we calculate the number of abuses per year as:  $A_{cst}^* = A_{cst}/(O_{cst}/D_t)$ , where  $A_{cst}^*$  is the number of abuses per year for county  $c$  in state  $s$  in year  $t$ ,  $A_{cst}$  is the number of abuses over the part of the year that we observe,  $O_t$  is the number of days in year  $t$  that we observe for county  $c$  in state  $s$ , and  $D_t$  is the total number of days in year  $t$ . After creating the measure of the number of abuses per year in this way, we take the natural logarithm transformation as explained in Section 3.2.1.

## D.2 Dealing with Missing Counties

The county of report is typically the county in which the victim resides. However, in some states (for example Utah), it is the county where the office investigating the report of child abuse lies. In general, the two are the same. However it is possible that a county is missing from the dataset because there is no CPS office located in the county, rather than because there are truly no incidents of child abuse in that county. We assume that the former is true only if a county is missing from the dataset for every type of abuse for every year of the dataset, which the case for 54 counties. We treat these 54 counties as missing from the dataset throughout, and treat any other county that does not appear

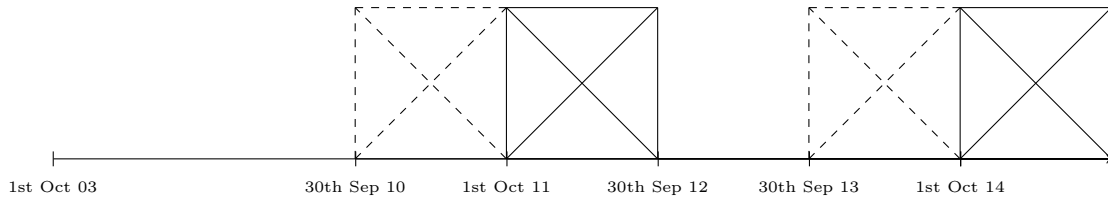


Figure D.1: Missing Years by Report Date for Indiana. The solid crosses indicate the missing periods of data for Indiana by investigation disposition date (the Federal Fiscal Year 2012, and from 1st October 2014 onwards). To organise the data by report date, we treat both the solid and dashed time periods as missing. In other words, we extend the missing period by twelve months before the start of the missing FFY by investigation disposition date. To see the intuition: for the first missing year in the Figure, we know that more than 99% of all incidents reported before 30th September 2010 will have had their investigation disposition before the start of the missing year (1st October 2011), and will therefore appear in the dataset.

in the dataset in a particular year for a particular abuse type as having zero incidents of that abuse type in that year.

Table D.1: Data Appendix

Variable	Data Source	Method
Overall Abuse, Neglect, Physical Abuse, Sexual Abuse, Emotional Abuse	National Child Abuse and Neglect Data System (NCANDS)	We keep only child-reports where at least one allegation of child maltreatment is found to be substantiated. There can be up to four allegations of child maltreatment on any given child-report. To do this, we keep any child-report for which at least one of the variables Mal1Lev, Mal2Lev, Mal3Lev, Mal4Lev is equal to 1 (substantiated), 2 (indicated or reason to suspect), or 3 (alternative response victim). We keep only child-reports for which the child is aged from 0 to 17 years inclusive. Within each county-year we then sum the total number of substantiated incidents of each type of abuse. To do so we use the report date (RptDt) and calendar year, as explained in Section D.1. For the measures of overall abuse, we treat a child-report with more than one substantiated type of abuse as a single case. For example, if a child is both physically and sexually abused, we treat this as only one incident of overall abuse. We create a measure of the overall number of abuses per year as: $A_{cst}^* = A_{cst}/(O_{cst}/D_t)$ , where $A_{cst}^*$ is the number of abuses per year for county $c$ in state $s$ in year $t$ , $A_{cst}$ is the number of abuses over the part of the year that we observe, $O_t$ is the number of days in year $t$ that we observe for county $c$ in state $s$ , and $D_t$ is the total number of days in year $t$ . We then take the natural logarithm of the number of abuses per year. For all county-years, we first add 0.001 to the number of abuses before taking the natural logarithm. Further explanation about the creation of the left hand side variables is given in Section D. The measure of neglect includes medical neglect.
Unemployment Rate	Local Area Unemployment Statistics (LAUS)	We use the annual average unemployment rate at a county-level, produced by the Bureau of Labor Statistics. This dataset can be downloaded from: <a href="https://www.bls.gov/lau/">https://www.bls.gov/lau/</a> .
Predicted Unemployment Rate (Instrument)	Quarterly Census of Employment and Wages (QCEW) and Current Population Survey (CPS-BLS)	The weights for the instrument are the fraction of all employed individuals working in each industry at the county-level in 2003. To calculate this, we use the QCEW. We first sum the annual average number employed ‘annual_avg_emplvl’ across all ownership sectors (government and private), for each of the 20 NAICS industries at a county level in 2003. This tells us the total number employed in each industry in 2003. We then sum these totals across all industries, and then divide the total employed in each industry by that sum to give the fraction of employed individuals in each industry. We calculate national level unemployment rates in each year using the CPS-BLS. We divide the total unemployed by the sum of the total employed and unemployed in each of the 20 NAICS industries. To calculate the instrument we then take a weighted average of these national level unemployment rates across industries using the weights previously described, which capture the initial industrial structure in each county. The industry of an unemployed person is the industry of their last job. Data from the QCEW can be downloaded from: <a href="https://www.bls.gov/cew/">https://www.bls.gov/cew/</a> . National-level unemployment files from the CPS-BLS were sent to us by the BLS.

Data Appendix: Continued

Variable	Data Source	Method
Fraction Black, Hispanic and Other Race	Population and Housing Unit Estimates (PHUE)	We calculate the fraction of the population who are Black, Hispanic and Other Race. The PHUE contains a breakdown of the total population by race, where an individual can be classified as Black Alone, White Alone, Asian Alone, American Indian Alone, Alaskan Native Alone, Native Hawaiian or Other Pacific Islander Alone or Two or More Races. The PHUE treats being Hispanic as an ethnic group, rather than a race. For the fraction Black, we use Black Alone (non-Hispanic). For the fraction Other Race, we use the sum of American Indian Alone, Asian Alone, Alaskan Native Alone, Native Hawaiian or Other Pacific Islander Alone and Two or More Races (non-Hispanic). For the years 2004-9, we use the 2009 Vintage, and for the years 2010-12 we use the 2014 Vintage. Data from the PHUE can be downloaded from: <a href="https://www.census.gov/programs-surveys/popest.html">https://www.census.gov/programs-surveys/popest.html</a> .
Child Population	Population and Housing Unit Estimates (PHUE)	We take the total number of boys and girls aged 0-17 from the PHUE. For the years 2004-9, we use the 2009 Vintage, and for the years 2010-12 we use the 2014 Vintage. This is also the dataset we use to create the weight for our regressions. Data from the PHUE can be downloaded from: <a href="https://www.census.gov/programs-surveys/popest.html">https://www.census.gov/programs-surveys/popest.html</a> .
Benefit Duration	Fatih Karahan, compiled for the paper Hagedorn et al. (2013).	We use a quarterly dataset of the maximum duration of unemployment benefits an individual can claim at the state level, provided to us by Fatih Karahan at the Federal Reserve Bank of New York. We take an unweighted mean across the four quarters to make the data annual. Hawaii is missing from this dataset.
SNAP	USDA SNAP Policy Database	We use the dummy variable 'bbce' from the SNAP Policy Database, which takes the value 1 if the state uses broad-based categorical eligibility to increase or eliminate the asset test and/or increases the gross income limit for virtually all SNAP applicants. The dataset is provided monthly, and so for each state-year we calculate the fraction of the twelve months of the year for which this dummy variable takes the value 1. This dataset is available for the years 2004-11. It can be downloaded from: <a href="https://www.ers.usda.gov/data-products/snap-policy-database/">https://www.ers.usda.gov/data-products/snap-policy-database/</a> .

Data Appendix: Continued

Variable	Data Source	Method
EITC	State EITC Based on the Federal EITC, Tax Policy Center	We create a variable at the state-year level which measures the percentage of the federal EITC offered by each state in its own EITC. If a state does not offer its own EITC, this percentage is equal to zero. We treat refundable and non-refundable EITCs equivalently (but check the robustness of results to dropping state-years with a non-refundable EITC). Five states have a non-refundable EITC at some point during the sample period, which are: Delaware, Iowa, Maine, Rhode Island and Virginia. Maryland offers two separate rates, one refundable and one non-refundable. We always use the refundable rate. Minnesota's credit is not expressed as a fraction of the federal EITC, but depending on the family's income the credit can range from 25 to 45% of the federal EITC. We use the highest possible tax credit of 45%. New Jersey's EITC is only available to families with incomes less than \$20,000. Wisconsin has three separate rates depending on the number of children a family has, and so we again consider the highest rate, which is 43%. This dataset is available for the years 2004-10. It can be downloaded from: <a href="http://www.taxpolicycenter.org/statistics/state-eitc-based-federal-eitc">http://www.taxpolicycenter.org/statistics/state-eitc-based-federal-eitc</a> .
Fraction Uninsured Children	Small Area Health Insurance Estimates (SAHIE)	We take the county-level count of the number of children under 18 who are not covered by health insurance in 2000, estimated in the SAHIE. We divide this by the child population from the PHUE in 2000, to create a measure of the fraction of children under 18 who are not covered by health insurance in 2000. The SAHIE for 2000 can be downloaded from: <a href="https://www.census.gov/support/USACdataDownloads.html">https://www.census.gov/support/USACdataDownloads.html</a> . Details on the methodology used in the SAHIE are available from: <a href="https://www.census.gov/did/www/sahie/methods/2000/model.html">https://www.census.gov/did/www/sahie/methods/2000/model.html</a> .
Poverty Rate	Small Area Income and Poverty Estimates (SAIPE)	We take the county-level percentage of all ages in poverty in 2000. We divide by 100 to convert this variable to the fraction of all ages in poverty in 2000. This dataset can be downloaded from: <a href="https://www.census.gov/did/www/saipe/data/statecounty/">https://www.census.gov/did/www/saipe/data/statecounty/</a> . Details on the methodology used in the SAIPE are available from: <a href="https://www.census.gov/did/www/saipe/methods/statecounty/2000county.html">https://www.census.gov/did/www/saipe/methods/statecounty/2000county.html</a> .
Expenditure Per Capita on Food and Beverages for Off-Premises Consumption, and Clothing and Footwear	Per Capita Expenditure-By-State Statistics, Bureau of Economic Analysis (BEA)	We have a dataset of per capita expenditure on two sets of basic goods: food and beverages for off-premises consumption, and clothing and footwear. This dataset is at the state-year level. It was sent to us by the BEA. The dataset is in nominal terms, and so we convert to 2012 dollars using the All Items All Urban Consumers (Current Series) Consumer Price Index (U.S. City Average) from the BLS. The CPI can be downloaded from: <a href="https://www.bls.gov/cpi/#data">https://www.bls.gov/cpi/#data</a> .

Data Appendix: Continued

Variable	Data Source	Method
Inadequate and Adequate Housing Neglect	National Child Abuse and Neglect Data System (NCANDS)	<p>The NCANDS dataset includes a caretaker risk factor with the variable name FCHouse. FCHouse is a ‘risk factor related to substandard, overcrowded, unsafe or otherwise inadequate housing conditions, including homelessness’. This variable is often not recorded by the Child Protection Services officer. We keep only state-years in which this variable is non-missing for more than 80% of substantiated cases. Among those state-years, there are four states where the FCHouse variable is either always equal to 1 (inadequate housing) or 2 (adequate housing) for at least all but one year in the sample period. These are: Colorado, Virginia, Florida and Indiana. We therefore additionally drop these states. We drop observations from Ohio in 2011 for the same reason. We are left with a final sample of 23 states: Alabama, Arizona, Arkansas, Delaware, D.C., Georgia, Idaho, Iowa, Kentucky, Maine, Minnesota, Mississippi, Missouri, Nevada, New Jersey, New Mexico, Ohio, South Carolina, Tennessee, Texas, Utah, Washington and Wyoming. For these states, we separately count substantiated cases for children living in inadequate and adequate housing at the county-year level, and create the natural logarithm of each, after adding 0.001 incidents as with the main dependent variable.</p>
Fraction Children Two Employed Parents	Census 2000	<p>For the regression analysis in Table 4, we use the ‘empstat_mom’ and ‘empstat_pop’ variables from the census data in 2000 (available from IPUMS), to calculate the fraction of children, aged 0-17, with an employed mother and father living in the household with them in the PUMA. To reflect the sampling design of the census, we use the person weights (‘perwt’) to create this fraction. The measure counts only children with heterosexual parents. We then use the 2010 county to 2000 PUMA cross-walk provided by the Missouri Census Data Center (Mable/GeoCorr14, available from: <a href="http://mcdc.missouri.edu/websas/geocorr14.html">http://mcdc.missouri.edu/websas/geocorr14.html</a>) to crosswalk data to the county-level. For counties that are entirely contained within a single PUMA, we assign to the county the value for the PUMA in which it is contained. For counties that cut across more than one PUMA, we assign a weighted average of the values for the PUMAs that intersect with that county, where the weights are the fraction of the county population in each PUMA. For the analysis in Table A.9, we do exactly the same but using the more recent American Community Survey (2003). In both cases, we wished to use the most recent measure available before the start of the sample period, but the regression analysis in Table 4 is at the county level, and so to improve precision we used the census from 2000, since it has a larger sample. Data from both the census and the ACS can be downloaded from: <a href="https://usa.ipums.org/usa/">https://usa.ipums.org/usa/</a>.</p>

Data Appendix: Continued

Variable	Data Source	Method
First Time, Second Time, Third Time, Fourth or More Time Neglect	National Child Abuse and Neglect Data System (NCANDS)	We first keep only the states that we can verify record a unique Child ID variable consistently over time. This variable that allows us to track the same child through the panel. Each state provides mapping files to NCANDS, which outline how each variable is recorded. Alabama, Minnesota and South Carolina do not record a unique Child ID variable, and so are dropped. Indiana does not provide mapping files, whilst Arkansas, New Mexico, Texas and Wisconsin do not provide the Child ID file of the mapping file. For these five states we cannot know whether the child ID variable is recorded consistently over time, and so to be cautious we drop these states too. We are therefore left with 38 states for the analysis. For these states, we do the following. We keep only substantiated cases of neglect. For each child-report, we use the child ID variable to record how many times that child has been the victim of neglect in the past. We can therefore understand whether each child-report is the first, second, third, fourth or more time that the child has been the victim of neglect. We then sum the total number of first time cases, second time cases, third time cases, and fourth or more time cases at the county-year level, and create the natural logarithm of each, after adding 0.001 incidents as with the main dependent variable.
Heavy Drinking Prevalence	Behavioural Risk Factor Surveillance System	We take the heavy drinking prevalence rate from the Dwyer-Lindgren et al. (2015) paper. Heavy drinking is defined as consuming more than one drink per day for women and two drinks per day for men for the past thirty days. This measure is only available from 2005 onwards.
Binge Drinking Prevalence	Behavioural Risk Factor Surveillance System	We take the binge drinking prevalence rate from the Dwyer-Lindgren et al. (2015) paper. Binge drinking is defined as consuming more than four drinks in a single day for women and five drinks for men at least once during the past thirty days.
Divorce Rate	American Community Survey (ACS)	We calculate the fraction of the total population aged 18 and over who are divorced. To reflect the sampling design of the ACS, we sum the individual person weights to create the total (weighted) number divorced, and divide this by the total (weighted) number of individuals aged 18 and over. The geographic identifier in the ACS is the PUMA, not the county. We therefore use the 2010 county to 2000 PUMA cross-walk provided by the Missouri Census Data Center (Mable/GeoCorr14, available from: <a href="http://medc.missouri.edu/websas/geocorr14.html">http://medc.missouri.edu/websas/geocorr14.html</a> ), as explained above in the description for the Fraction Children Two Employed Parents. The ACS only contains PUMA information for the years 2005 to 2012, and so the divorce rate is only defined for those years. Data from the ACS can be downloaded from: <a href="https://usa.ipums.org/usa/">https://usa.ipums.org/usa/</a> .

Data Appendix: Continued

Variable	Data Source	Method
Fraction Employed in Schools, Health Care, Social Services, Police, Clergy, Childcare	American Community Survey (ACS)	For each high-reporting sector, we calculate the fraction of the working age (18-64) population who are employed in each sector. To reflect the sampling design of the ACS, we sum the individual person weights to create the total (weighted) number employed in each sector, and divide this by the total (weighted) number of working age individuals. The geographic identifier in the ACS is the PUMA, not the county. We therefore use the 2010 county to 2000 PUMA cross-walk provided by the Missouri Census Data Center (Mable/GeoCorr14, available from: <a href="http://mcdc.missouri.edu/websas/geocorr14.html">http://mcdc.missouri.edu/websas/geocorr14.html</a> ), as explained above in the description for the Fraction Children Two Employed Parents. The ACS only contains PUMA information for the years 2005 to 2012, and so these variables are only defined for those years. Data from the ACS can be downloaded from: <a href="https://usa.ipums.org/usa/">https://usa.ipums.org/usa/</a> .
Substantiated Incidents Reported by Professionals/Parents	National Child Abuse and Neglect Data System (NCANDS)	We create a measure of the total number of incidents of overall abuse and neglect in the county-year as for the baseline dependent variables, except now only counting incidents reported by a professional, parent, alleged victim or perpetrator. To do this, we use the variable RptSrc in the NCANDS dataset, and keep only child-reports for which RptSrc is equal to 1, 2, 3, 4, 5, 6, 7, 8, 9 or 12. We do not count incidents reported by Other Relative (RptSrc = 10), Friend/Neighbour (RptSrc = 11), Anonymous Reporter (RptSrc = 13), Other (RptSrc = 88) or Unknown (RptSrc = 99). We keep state-years in which more than 80% of substantiated reports have a non-missing reporter (for which RptSrc = 13 or 88 are treated as non-missing).
Unsubstantiated Incidents	National Child Abuse and Neglect Data System (NCANDS)	We create a measure of the total number of unsubstantiated incidents of overall abuse and neglect in the county-year. We use the same approach as for the baseline dependent variables, except that we now count observations for which the relevant MalLev variable is equal to 4, 5, 6, 7, 8 or 88. Again, we consider the disposition with respect to the specific type of maltreatment of interest. A child may be a substantiated victim of one type of maltreatment and an unsubstantiated victim of another type of maltreatment on the same report.
Real Exchange Rate Instrument	Real Trade Weighted Dollar Index: Broad (TWEXBPA), Federal Reserve Bank of St. Louis; QCEW	We take the real exchange rate as published by the Federal Reserve Bank of St. Louis. This can be downloaded from: <a href="https://fred.stlouisfed.org/series/TWEXBPA">https://fred.stlouisfed.org/series/TWEXBPA</a> . With this, we calculate the change in the real exchange rate on the previous year as: $(RER_t - RER_{t-1})/RER_{t-1}$ . We then multiply the change in the real exchange rate by the county-level fraction of the employed working-age population in manufacturing at the start of the sample period, in 2003 (the manufacturing industry weight used to create the original Bartik instrument), from the QCEW.



Data Appendix: Continued

Variable	Data Source	Method
Real Value Added Bartik Instrument	GDP-by-Industry, Bureau of Economic Analysis (BEA); QCEW	We take a weighted average of the national-level real value added in each of the 20 NAICS industries, where the weights are the fraction of the employed working-age population in each industry in 2003 as in the original Bartik instrument. Real value added by industry is measured in billions of 2009 dollars, and can be downloaded from: <a href="https://www.bea.gov/industry/gdpbyind_data.htm">https://www.bea.gov/industry/gdpbyind_data.htm</a> .
Employment Bartik Instrument	GDP-by-Industry/ Employment, Bureau of Economic Analysis (BEA); QCEW	We take a weighted average of the national-level total employed in each of the 20 NAICS industries (obtained from Table 6.4D of the NIPA Tables, for each year 2004-12), where the weights are the fraction of the employed working-age population in each industry in 2003 as in the original Bartik instrument. The total employed variable is measured in thousands and can be downloaded from: <a href="https://www.bea.gov/industry/more.htm">https://www.bea.gov/industry/more.htm</a> .
Number of Children Maltreated	National Child Abuse and Neglect Data System (NCANDS)	We first keep only the states that we can verify record a unique Child ID variable consistently over time, as explained above for the description of the First Time, Second Time, Third Time and Fourth or More Time variables. We are therefore left with 38 states for the analysis. For these states, we do the following. For each type of abuse or neglect, we keep only substantiated cases. For each child-report, we use the child ID variable to record how many times that child has been the victim of overall abuse or neglect in that year. We then keep only the first time cases within each county-year, and sum these. This gives us a measure of the total number of children who have been the victim of overall abuse or neglect in each county-year. We create the natural logarithm of the number of children after first adding 0.001 incidents as with the main dependent variable.