Child Maltreatment, Unemployment, and Safety Nets*

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Abstract

We study the impact of unemployment on child abuse and neglect. By using restricted administrative data on reports of child maltreatment and a Bartik instrument, we find that a one percentage point increase in the unemployment rate leads to a 20 percent increase in neglect. This result captures an impact on the actual incidence and not change in reporting, and is not only driven by the manufacturing sector which explains most of the variation in our instrument. We find indication for a direct income channel: higher unemployment leads to a decrease in real expenditure on basic goods. Extensions to the duration of unemployment benefits implemented during the Great Recession mitigate the effect of unemployment on neglect.

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

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

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1 Introduction

The United States counted approximately 674,000 victims of child maltreatment in 2017 alone, equivalent to a rate of 9.1 victims per 1,000 children (U.S. Department of Health and Human Services, 2017). Child maltreatment includes the physical, sexual or emotional abuse and neglect of children under 18 years old, and has been found to be detrimental to its victims' health, wellbeing and socio-economic outcomes in the long-term.[1] In addition to direct costs for the victims, it is also a substantial economic burden for the entire U.S. population, with estimates that span from 428 billion dollars for annual substantiated incident cases to 2 trillion dollars for investigated cases (Peterson, Florence and Klevens, 2018). Despite being widespread and highly costly to society, economists have paid little attention to this social problem.

It is well-established that economic hardship is a strong predictor of child maltreatment. Nevertheless, causal empirical evidence is scarce.[2] This paper aims to fill the gap, by testing whether unemployment causes child maltreatment in the United States during the period from 2004 to 2012. We use a restricted-access dataset of the National Child Abuse and Neglect Data System (NCANDS), that contains one observation for every reported incident of child abuse and neglect made to the Child Protective Services for nearly every county.[3] We only consider substantiated cases which are cases for which the Child Protective Agencies' investigation has supportive evidence that the child has been abused or neglected. We link this dataset to the county-level unemployment rate and we identify the effect of unemployment on child maltreatment using an industry shift-share or Bartik instrument (Bartik, 1991; Blanchard and Katz, 1992).[4] Our instrument is a predicted county-level unemployment rate, which we create by interacting initial county industry shares and national industry unemployment rates.

[1] Being maltreated as a child has long-term consequences including mental health problems, substance abuse, lower levels of education, productivity or earnings, and a higher probability of engaging in crime (Dube et al., 2003; Springer et al., 2007; Currie and Widom, 2010; Currie and Tekin, 2012).

[2] See Berger and Waldfogel (2011) and Slack, Berger and Noyes (2017) for reviews of studies focusing on the economic determinants and consequences of child abuse and neglect. Correlational studies on the relationship between macroeconomic conditions and maltreatment is mixed (Paxson and Waldfogel, 1999, 2002; Bitler and Zavodny, 2004; Seiglie, 2004; Stephens-Davidowitz, 2013; Frioux et al., 2014; Raissian, 2015; Raissian and Bullinger, 2017; Schneider, Waldfogel and Brooks-Gunn, 2017).

[3] Most of the papers have used an earlier and more limited version of this dataset, except Benson, Bondurant and Fitzpatrick (2018), with information only available at the state level or only for counties where 1,000 or more reports are made leading to a serious censoring problem.

[4] These types of instruments have been widely used in the literature since pioneer studies of local labour market dynamics by Bartik (1991) and Blanchard and Katz (1992), including related research that investigates the effect of economic conditions on domestic violence (e.g., Aizer, 2010; Anderberg et al., 2016; Lindo, Schaller and Hansen, 2018). A growing body of work discusses the validity and interpretation of these instruments (e.g., Goldsmith-Pinkham, Sorkin and Swift, 2018; Borusyak, Hull and Jaravel, 2018; Adao, Kolesár and Morales, 2019).

We find that a one percentage point increase in the unemployment rate leads to a 20 percent increase in *neglect*. In the county with the median prevalence of neglect, which has 536 cases per year, this is equivalent to an additional 110 cases per year.

Given that the Bartik instrument is a combination of all sectors, it is important to understand what variation the instrument uses. If industry composition, or unobserved variables correlated with it, directly predict the outcomes of interest then the Bartik instrument is invalid (Baum-Snow and Ferreira, 2015). We then follow Goldsmith-Pinkham, Sorkin and Swift (2018), and run different tests to show the validity of our approach. We find that the manufacturing sector explains most of the variation in our Bartik instrument. Nonetheless, we show that the effect of unemployment on neglect is not solely driven by this industry, but is similar across the different sectors. We also construct the Bartik instrument in three different ways, consider two alternative instruments, and our results are robust.

Another common concern in this type of study is underreporting. First, the data we use improve on works that rely on self-reported retrospective measures of abuse or neglect in childhood or self-reported harsh parenting measures (as in Berger et al., 2016; Schneider, Waldfogel and Brooks-Gunn, 2017). Second, we show that the effect of unemployment captures an effect on the actual incidence of neglect and not reporting behaviour. We do this in three ways. Results are unchanged once we control for changes in employment in high-reporting sectors, or when we exclude cases where the reporter is a friend, neighbour or other relative (groups of reporters who, if they have been made unemployed, might spend more time around the child and so become more likely to observe child maltreatment for a given level of actual incidence). We also find no effect of unemployment on unsubstantiated reports, which are less likely to capture actual incidents of maltreatment.

We then discuss some of the mechanisms through which unemployment increases neglect. 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. Therefore, two sets of mechanisms can be identified. Firstly, child neglect might happen when there is a decrease in family income or an increase in the prices of children's goods. Secondly, indirect channels such as family instability, mental health or substance abuse resulting from an economic recession may prevent parents from providing adequate care and supervision to their children either because of incapacitation or directly via budget constraints.

We empirically investigate the direct income channel, and two indirect ones: substance abuse and family instability. By using state-level data on expenditure per capita available via the Bureau

of Economic Analysis, we find suggestive evidence that unemployment causes a decrease in real expenditure on basic goods, such as food and beverages (as in Griffith, O'Connell and Smith, 2016; Ganong and Noel, 2019), which could lead to neglect. Second, we use county-level prevalences of alcohol use based on the Behavioural Risk Factor Surveillance System dataset (estimated by Dwyer-Lindgren et al., 2015) but find no evidence that drinking is positively associated with neglect. Third, as a proxy for family instability, we derive county-level prevalences of divorce from the American Community Survey, and find divorce to increase when unemployment increases. However, when we control for divorce in our main specification, it does not mediate the effect of unemployment on neglect, suggesting that it is not a mechanism.

Finally, we study whether the generosity of state's welfare systems can prevent parents who are unemployed from neglecting their children. We focus on the Unemployment Insurance (UI) program which is one of the largest social safety net programs, and provides cash benefits to involuntarily unemployed persons who have sufficiently high earnings histories to qualify. In response to the Great Recession, UI was subjected to different extensions of the duration of unemployment benefits which vary considerably across states. We exploit this geographical and time variation in the UI generosity to study if this program mitigates the effect of unemployment on neglect. The effect is in fact smaller in states that introduce 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. Results are qualitatively similar when we adopt a county-border discontinuity design to address the potential endogeneity of the program.

Our paper advances our understanding of the causal impact of economic recessions on child abuse and neglect, adding to a very limited literature. Most closely related to our work is the paper by Lindo, Schaller and Hansen (2018) who study the impact of gender-specific employment on child maltreatment using predicted gender-specific employment rates in the spirit of a Bartik instrument, but in reduced-form analyses. The authors use administrative crime data for California and find both abuse and neglect to decrease when male employment increases, but to increase when female employment increases. The authors provide some evidence that time spent with children is the main channel for child abuse. The channels for their opposite gender-specific unemployment effects on neglect are harder to explain. Differently from Lindo, Schaller and Hansen (2018), we use data for the entire U.S., and find large impact of unemployment (male plus female) only on neglect, which

may be explained by a reduction of family expenditure on essential goods. This income channel is further supported by our novel result on the mitigating effect of the UI program.[5]

We then contribute to the vast literature on the impact of social safety net programs on children which focuses primarily on poverty, and cognitive or health outcomes both in the short and long-term (e.g., Almond, Hoynes and Schanzenbach, 2011; Dahl and Lochner, 2012; Hoynes, Miller and Simon, 2015; Hoynes, Schanzenbach and Almond, 2016; Bastian and Michelmore, 2018; Hoynes and Schanzenbach, 2018). Only few studies have looked at the direct link between welfare programs and child maltreatment (Paxson and Waldfogel, 2002, 2003; Berger et al., 2016; Carr and Packham, 2019). Instead, we are the first authors to test whether there is a role for government policy in *mitigating* the adverse effects of the Great Recession on child maltreatment. Bitler, Hoynes and Kuka (2017) ask a similar research question but focus on child poverty, and like us find that safety nets provide protection against unemployment.

Besides the papers that we have already mentioned, our work is related to the flourishing economic literature on child maltreatment: Sandner and Thomsen (2018) who study the impact of child care provision on child abuse and neglect, Benson, Bondurant and Fitzpatrick (2018) who consider the role of teachers reporting behaviour, and Doyle and Aizer (2018) who discuss the interplay between child maltreatment, foster care and intimate partner violence.

The paper is structured as follows. In Section 2, we present our empirical strategy and data. In Section 3, we outline the main results and several robustness checks. In Section 4, we explore mechanisms for our results. We study the role of a welfare program in Section 5, and conclude in Section 6.

[5]Two papers investigate the causal effect of income on child maltreatment. Berger et al. (2016) exploit differences between states and over time in the generosity of the Earned Income Tax Credit (EITC), and find that an increase in income is associated with a modest reduction in child neglect. However, this study uses data from the Fragile Families and Child Wellbeing Study which contains a small sample of children born between 1998 and 2000 into disadvantaged families, across only 20 U.S. cities, and self-reported measures of child maltreatment. Carr and Packham (2019) consider changes to the Supplemental Nutrition Assistance Program (SNAP) benefit issuance timing in Illinois as an income shock. They find that parents use violence after an influx of benefits, which oppose our findings, but they observe only extreme forms of child maltreatment, and use data only for Chicago. Finally, Fein and Lee (2003) and Cancian, Yang and Slack (2013) present suggestive but not conclusive evidence of a causal link between low income and child maltreatment focusing respectively on welfare reforms in Delaware and Wisconsin.

2 Identification Strategy and Data

2.1 Identification Strategy

We wish to understand the effect of the unemployment rate on the incidence of child abuse and neglect. However, changes in the unemployment rate depend both on changes in labour demand and labour supply. In particular, any 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 variable that is more likely to isolate demand-driven changes in the unemployment rate, and is often referred to as shift-share or Bartik-instrument (Bartik, 1991; Blanchard and Katz, 1992).

Our instrument is a predicted county-level unemployment rate constructed by taking the weighted average of the national-level unemployment rates across industries, where the weights or shares are the fraction of the employed working-age population in each industry and county the year before the start of the sample period as follows:

$$\widehat{Unemp}_{cst} = \Sigma_k N_{tk} w_{csk}. \tag{1}$$

Here, N_{tk} is the national-level unemployment rate, in each industry, k, in each time period, t. The weights or shares, w_{csk}, are the fractions of employed working-age individuals in each industry, k, in county c, state s, in the year 2003. Then, $\widehat{Unemp}_{cst}$ captures variation in unemployment that is driven by changes in the country's economy, but varies across counties because of historical differences in the distribution of nationwide industry employment by location. The identifying assumption is that counties that house industries with high unemployment rates do not experience other shocks that affect their number of children abused or neglected. We discuss threats to this identifying assumption in Sections 3.3-3.4 by following recent literature on the econometrics of the shift-share instrument (Goldsmith-Pinkham, Sorkin and Swift, 2018).

Our main equation is then:

$$Y_{cst} = \beta Unemp_{cst} + X'_{cst}\phi + \psi_{st} + \eta_c + \epsilon_{cst} \tag{2}$$

where Y_{cst} is the natural logarithm of the number of abuses per year, for the type of maltreatment of interest, and $Unemp_{cst}$ is the unemployment rate in county c, in state s, in year t whose effect

we identify in a 2SLS regression where the instrument, $\widehat{Unemp}_{cst}$, is estimated as in Equation (1). X'_{cst} include the natural logarithm of the child population, and the fractions of the population that are Black, Hispanic, and other race/ethnicity (non-White). County fixed effects, η_c, are included to control for any unobserved fixed differences between counties, while ψ_{st} captures state-year fixed effects which are needed to control for differences in the measurement of abuse and neglect (across states and over time within states).[6] We weight observations by the child population in the county-year, and we cluster standard errors at the state level. The coefficient of interest is β which indicates 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. In Section (3.6), we demonstrate the robustness of our results to a variety of changes to our main specification.

2.2 Data and Summary Statistics

Child Maltreatment Data All fifty states and the District of Columbia have a Child Protective Services (CPS) agency, which is responsible for investigating reports of child maltreatment (Appendix B reports details about CPS and the process of reporting). We use a dataset which contains every reported incident of child abuse and neglect made to the CPS in nearly every state in the U.S. for the years 2004-12. 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). Our restricted-access version of the data with information on all counties of the report is part of a unique pilot secure micro-data program.[7] 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.If multiple children are maltreated within a single report, we count an incident for

[6]Differences in measurement arise from several sources. First, the exact definitions of child abuse and neglect can vary slightly across states and over time within states (Child Welfare Information Gateway, 2014). For example, Washington state does not recognise emotional abuse. 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. Given these state-specific differences in child maltreatment measurement, we cannot perform the analysis focusing on commuting zones as often done in recent labour economic papers that use shift-share designs (e.g., David, Dorn and Hanson, 2013; Acemoglu and Restrepo, 2017). Some commuting zones cut across different states which would imply combining different forms of abuse or neglect together.

[7]These data were accessible through contractual arrangements with NDACAN, and are solely available through the Cornell Restricted Access Data Center at CRADC@Cornell.edu.

each of the children. We count only incidents where that specific type of maltreatment is found to be substantiated by the CPS. 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.[8] Our analysis focuses on a final sample of 2,803 counties from forty-six states (Appendix C reports further details on how we constructed the working dataset).[9]

Unemployment Data We focus on the annual unemployment rate at the county level, using data from the Local Area Unemployment Statistics (LAUS) produced by the Bureau of Labour Statistics (BLS).[10] This county-level unemployment rate is partially model-based, as the number of unemployed new entrants and re-entrants is estimated by combining state-level data on labour force entry with county-level demographic information.[11] This introduces a measurement error which is also addressed by our instrument.

To calculate the weights for the instrument (w_{csj}), we use county-level data from the Quarterly Census of Employment and Wages (QCEW) in 2003.[12]

We measure the national-level unemployment rates by industry using the Current Population Survey, where the industry of an unemployed person is the industry of their last job.[13] We consider the 2-digit industry classification provided by the North American Industry Classification System (NAICS), which consists of 20 industries.[14]

Extra Data We measure the child population, and the fraction of the population that are Black, Hispanic and Other Race using the Population and Housing Unit Estimates (PHUE), produced by the Census Bureau.

[8] This varies by the type of maltreatment, but, for example, 10% of county-years have no incidents of physical abuse and 8% of county-years have no incidents of neglect.

[9] In each year, a small number of states do not submit information to NCANDS, which is a voluntary reporting system. We exclude Alaska, South Dakota, Illinois, North Dakota and Oregon from the analysis because their reports are not available for several counties or years, or because their geographical boundaries are not counties.

[10] Data can be downloaded from: `https://www.bls.gov/lau/`.

[11] See `http://www.bls.gov/lau/laumthd.htm` for further details.

[12] Data can be downloaded from: `https://www.bls.gov/cew/`. We first sum the annual average number employed across all ownership sectors (government and private), for each industry 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.

[13] Data was sent to us by the BLS.

[14] We consider the following twenty industry categories: agriculture, forestry and fishing; mining; utilities; construction; manufacturing; wholesale trade; retail trade; transportation and warehousing; information; finance and insurance; real estate, rental and leasing; professional, scientific, and technical services; management of companies/enterprises; administrative, support, waste management and remediation services; educational services; health care and social assistance; entertainment and recreation services; accommodation and food services; other services; public administration.

Summary Statistics In Table I, we present unweighted means, standard deviations, 25th, 50th and 75th percentiles for the main variables we use in the regression analysis. Neglect is considerably more common than the other three types of maltreatment. The mean number of incidents of neglect is four times greater than that 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. Figure A1 in Appendix A instead shows a sharp increase in the unemployment rate between 2007 and 2009, following the onset of the Great Recession. The median county in our sample experienced an increase in the unemployment rate of 4.5 percentage points between 2007 and 2009. Both the size of the unemployment shock and the change in abuse rates vary considerably across counties within states as can be seen in Figures A2 and A3.

3 Main Results

3.1 First Stage and Reduced Form Estimates

The relevance of our instrument is tested in two ways. First, in Figure A4 we show the correlation between the unemployment rate and our instrument using pooled data from the period 2004-2012. The figure indicates a strong positive relationship between the two measures of unemployment. Second, in Table II, we present the first stage results. 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. The instrument is highly relevant, the Kleibergen-Paap F-statistic on the first stage is 34.9.

We now present the reduced form estimates by regressing the number of maltreatment cases on our instrument following our baseline specification. Table III reports the results. In column (1), we estimate the effect of unemployment on overall abuse. The measure of overall abuse combines any incident of neglect, physical, sexual or emotional abuse. In columns (2) to (5), we separate out the four different types of maltreatment. The results in column (1) show that a one percentage point increase in the estimated unemployment rate leads to an 8 percent increase in overall abuse, to a marginally statistically significant increase of 6 percent in physical abuse in column (2), and to a highly significant 15 percent increase in neglect in column (3).[15] These results, which are for the U.S. as a whole, contrast with the reduced form estimates presented by Lindo, Schaller and Hansen

[15]Results are robust if we exclude the top one percent counties with the highest predicted unemployment rate (available upon request).

(2018) who do not find a statistically significant effect of the overall predicted employment rate on child maltreatment, but are of a similar order of magnitude when they consider gender-specific employment rates (male predicted employment rate decreases abuse and neglect by respectively 7-11 percent, 5-9 percent; female predicted employment rate increases abuse and neglect by respectively 9-18 percent, 7-10 percent).

3.2 IV Estimates

In Table IV, we present the main IV results (Table A1 in Appendix reports the OLS estimates).[16] In column (1), we find that the coefficient on the unemployment rate is positive, and 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 results in column (3) 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, which has 536 cases per year, a one percentage point increase in the unemployment rate leads to an increase in neglect by 110 cases per year.[17]

We find no statistically significant effect on physical, sexual or emotional abuse. The effect of unemployment on neglect drives the effect on overall abuse. We therefore focus on understanding the effect on neglect in the rest of the paper.

3.3 The Role of the Manufacturing Sector

The identifying assumption is that counties that house industries with high unemployment rates do not experience other shocks affecting their number of children abused or neglected. Given that the Bartik instrument is a combination of all sectors, it is important to understand what variation the instrument uses (Goldsmith-Pinkham, Sorkin and Swift, 2018).

In Figure A5 we plot the industry-specific Bartik instrument and we can see that the manufacturing sector has the highest predicted unemployment rate of any sector. To more formally test whether the manufacturing sector indeed has the greatest weight in our instrument we follow Goldsmith-Pinkham, Sorkin and Swift (2018). Goldsmith-Pinkham, Sorkin and Swift (2018) discuss

[16] The associations of unemployment on child maltreatment outcomes are positive, except for emotional abuse, but never statistically significant, apart marginally for physical abuse. Lindo, Schaller and Hansen (2018)'s OLS estimate of the overall unemployment rate on overall abuse is also statistically insignificant.

[17] The weighted median county-year in the final sample (weighting by the child population) has 536 cases of neglect. Using the estimates in column (3) of Table IV, a 20.44 percent increase in neglect translates into an increase by $(0.2044 \times 536 = 110)$ cases.

how the Bartik instrument is equivalent to using the initial industry shares (multiplied with time fixed effects when multiple periods are used as in our case) as instruments in a weighted GMM estimation.[18] They decompose the Bartik instrument into a weighted combination of just-identified estimates, each using a single share as an instrument. The weights are called Rotemberg weights. We follow their approach and calculate these weights using our main specification for overall abuse and neglect. In Table A2 in Appendix A we report the Rotemberg weights only for industries with a weight above 3 percent. Manufacturing has the highest weight, about 73 percent, followed by Professional, Scientific, and Technical Services (11 percent), Construction (10 percent), Public Administration (6.4 percent), and Finance and Insurance (4.4 percent). This raises the concern that our estimates might be confounded by other changes affecting the manufacturing sector in particular.

To address this issue, in Table V we report reduced-form regressions based on our baseline specification where the Bartik is decomposed in a predicted unemployment rate only for the manufacturing sector and a predicted unemployment rate for all other industries (as in Acemoglu and Restrepo, 2017). We never reject the null hypothesis that the coefficients of these two variables are equal showing that the effects of the manufacturing Bartik are very similar to those of the non-manufacturing Bartik. This is reassuring as our main results do not seem to be only driven by the manufacturing industry, but suggest that the effects of unemployment are similar across the different sectors.

3.4 Alternative Choices of the Instrument

In this subsection, we demonstrate the robustness of the IV results to two alternative choices of the instrument, and three different constructions of the Bartik instrument.[19]

Results are reported in Table VI. 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 population in manufacturing at

[18] Borusyak, Hull and Jaravel (2018) instead offer a different interpretation of the Bartik where the identification assumption comes from the shocks - in our case, the national unemployment rate N_{tj} in each industry (see also Adao, Kolesár and Morales (2019) who develop the inference for Borusyak, Hull and Jaravel (2018)'s set-up). Goldsmith-Pinkham, Sorkin and Swift (2018) consider a setting with a large sample of locations, but a fixed number of time periods and industries, while Borusyak, Hull and Jaravel (2018)'s approach relies on the use of several industries. Therefore, our Bartik design follows Goldsmith-Pinkham, Sorkin and Swift (2018)'s reasoning, and is similar to recent applications based on a small number of shares (Card, 2001; Kovak, 2013; Acemoglu and Restrepo, 2017).

[19] In an early analysis we also used mass lay-off data as an exogenous driver of unemployment rate, but the first stage results were not robust.

the start of the sample period, in 2003 (Lin, 2008).[20] An appreciation of the real exchange rate harms the competitiveness of U.S. manufacturers who compete with foreign producers, thereby increasing unemployment. Counties with an initially larger reliance on manufacturing will experience greater unemployment shocks. Since exchange rates are determined in global markets, changes in the exchange rate will not be correlated with county-level unobservables that could be related to child maltreatment. To measure the real exchange rate, we use the broad real trade-weighted U.S. 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 Bureau of Economic Analysis.[21] 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. Using industry shares observed in 1995, about ten before the start of our sample period, further mitigates concerns about the potential correlation between our predicted unemployment rates and contemporaneous confounders of child maltreatment outcomes.

Finally, in columns (9) and (10), we use industry shares times year fixed effects as instruments as suggested by Goldsmith-Pinkham, Sorkin and Swift (2018).[22] These columns also report the overidentification tests for the validity of the source of variation coming from each industry.

The effect of unemployment on overall abuse is always positive, but is smaller in size and not always significant. The effect 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 0.06 to 0.24. The perfectly identified models (columns (1)-(8)) have instruments all 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)). In columns (9)-(10), the overidentification tests reject the null hypothesis that all industry shares are valid. However,

[20] Data can be downloaded from `https://fred.stlouisfed.org/series/TWEXBPA`.

[21] We use the total employed rather than the employment rate by industry, because the Current Population Survey does not ask about the industry of individuals who are out of the labour force. Data on real value added by industry and national-level employed by industry can be downloaded respectively from: `https://www.bea.gov/industry/gdpbyind_data.htm` and `https://www.bea.gov/industry/more.htm`.

[22] To be able to estimate this model, the standard errors need to be clustered at the county level instead of state level as the number of clusters needs to exceed the number of exogenous regressors and excluded instruments.

the identification assumption in these models is that each initial industry share is exogenous, a much stronger assumption than in our baseline specification which requires that counties where industries have a high unemployment rate are not experiencing other shocks which can affect child maltreatment. Based on the fact that the Bartik estimator combines many instruments, Goldsmith-Pinkham, Sorkin and Swift (2018) offer a way to investigate the failure of the overidentification. The idea is to explore whether the just-identified IV estimates associated with each industry share instrument (particularly the ones with high Rotemberg weight) are similar to the IV estimate obtained with the Bartik instrument. We follow this approach and find that the point estimates among the high-powered and the high-weight industries are clustered closely to 0.20 which is the effect of unemployment on neglect identified with the overall Bartik instrument. We can then be less concerned by the rejection of the overidentification test.[23]

3.5 An Effect on Actual Incidence or Reporting?

In this section we provide evidence that our results capture an effect of unemployment on the actual incidence of overall abuse and neglect and not on the reporting of maltreatment.

This is done in three ways as reported in Table VII. 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, Schaller and Hansen (2018). We use the American Community Survey data to compute the fraction of the working age (18-64) population who are employed in each sector.[24] 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 in the fraction employed in high-reporting sectors variables are statistically insignificant in all but one case.

[23] In Figure A6 we adopt the visual diagnostic offered by Goldsmith-Pinkham, Sorkin and Swift (2018) in order to see how disperse the just-identified IV estimates ($\hat{\beta}_k$) are around the overall Bartik instrument ($\hat{\beta}$). The x-axis is the first-stage F-statistic and the y-axis is the $\hat{\beta}_k$ associated with each instrument. The outcome is neglect. The figure only includes instruments with a first-stage above five. The horizontal line corresponds to the Bartik instrument. Every individual point of β_k is scaled by his respective Rotemberg weight, and presents a different shape depending on the sign of the Rotemberg weight. The Rotemberg weights can also be negative, but in our case the industries with negative weights are only a small fraction of the overall weight (the total share of overall $\hat{\beta}$ with negative weights is 0.051, versus 0.949 with positive weights) (see Section 4 of Goldsmith-Pinkham, Sorkin and Swift, 2018, for further details).

[24] 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. Since the geographic identifier in the ACS is the PUMA, we aggregate the data at county level by using the 2010 county to 2000 PUMA cross-walk.

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 (those three groups account for 10.7% of all reports in our working sample). We only count reports made by groups of reporters who are not affected by this concern. These are: professionals, parents and the alleged victim or perpetrator.[25] 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.[26] 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 of the size of the estimates for substantiated reports. In columns (7) and (8) we consider all reports (substantiated and unsubstantiated). As in Lindo, Schaller and Hansen (2018), we find our estimates to be qualitatively similar to our main results, even if the effects of the unemployment rate are smaller and only statistically significant for neglect.

[25] Professionals are those employed in the following professions: social services, medical, mental health, legal, law enforcement, criminal justice, education; as well as child day care providers and substitute care providers.

[26] Stephens-Davidowitz (2013) focuses on physical abuse using Google search data and finds that the Great Recession led to a decrease in the reporting of abuse, but an increase in the rate of child mortality due to neglect. He notes that national economic shocks like recessions can decrease government spending on child protection across the country and so lower reporting of both substantiated and unsubstantiated maltreatment. Unfortunately, NCANDS did not share with us data on deaths due to neglect to allow us to replicate Stephens-Davidowitz (2013)'s analysis. We also could not find a county-level dataset of CPS budgets covering our sample period to explore if unemployment rate leads to cuts in CPS staff and resources. However, even if it does our main IV estimates can be thought of as a lower bound for the true effect.

3.6 Other Robustness Checks

We check the robustness of the results to five alternative choices for the dependent variable in Table A3. 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. In columns (3) to (8), we use three alternative methods of dealing with the county-year cells with zero cases of maltreatment. In columns (3) and (4), we take the inverse hyperbolic sine transformation of the rate of maltreatment per 100,000 children. 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. The results are again robust.

We make several other changes to the specification, and present the results in Table A4. In columns (1) and (2), we control for linear county trends. In columns (3) and (4), we drop the state-year fixed effects. In columns (5) and (6), we weight observations using the child population in the county at the start of the sample period, in 2003. In columns (7) and (8), we weight observations by the total population of all ages in the county-year. Finally, in columns (9) and (10), we cluster standard errors at the county level. The results are still very similar to those in Table IV.

Finally, we show in Table A5 that our results are not driven by outlier counties, by dropping the counties with a child population of more than one million at the start of the sample period, in 2003 (columns (1)-(2)), and by dropping the smallest 10 percent of counties in terms of their child population at the start of the sample period, in 2003 (columns (3)-(4)).

4 Conceptual Framework and Empirical Mechanisms

In this section we describe the conceptual framework that helps us to understand the mechanisms through which unemployment can lead to child neglect. Two different mechanisms exist: a direct pecuniary or income mechanism and an indirect non-pecuniary mechanism. As in Paxson and Waldfogel (2002), we can think of a simple model of expenditure on children's goods and services and the time parents spend with children. Parents maximize their welfare given prices and endowments of time and money. Children's welfare and the basket of goods and services they consume depend on parental endowments, parental preferences and prices. In this simple framework, child neglect

happens when there is a decrease in income or an increase in the prices of children's goods.[27]

This model however ignores other important indirect mechanisms that can lead to neglect. Mental health and substance abuse have been found to be correlated with child neglect (Slack et al., 2011). Individuals may increase alcohol consumption or drug use to cope with stress after being made unemployed (Eliason and Storrie, 2009). Stress and depression also seem to increase with economic recessions (Ruhm, 2000, 2003; Hauksdóttir et al., 2013), particularly for disadvantaged or single-parent families (Paxson and Waldfogel, 1999; Currie, Duque and Garfinkel, 2015). Both higher substance abuse or worse mental health can limit a parent's ability to care for their children, for example by leaving the parent incapacitated or unable to provide adequate care and supervision. Alternatively, they may alternatively have a pecuniary effect on neglect if they drain resources that could otherwise be used to pay for the children's basic needs.

Family instability is another channel which is associated with poor child well-being (Fomby and Cherlin, 2007). A good proxy for family instability is divorce, which has been found to increase with a spouse's job displacement (Charles and Stephens, 2004). Divorce can in turn increase child neglect, because it may increase stress or substance abuse, or because children of divorced parents may grow up in single parent households, which may have fewer resources.

We test the direct income channel below by exploring whether unemployment causes a decrease in expenditure on basic goods and services. We also investigate whether substance abuse or divorce are potential mechanisms for our findings, bearing in mind that with the data at hand we cannot disentangle the effects of changes in economic conditions that work via changes in parental behaviours or directly via budget constraints (Paxson and Waldfogel, 2002).[28]

4.1 Unemployment Reduces Expenditure on Basic Goods

The literature identifies different forms of neglect which can be related to economic hardship or income shocks (DePanfilis, 2006; Hornor, 2014). They are: medical and dental neglect which is failure or delay in seeking medical care; nutritional neglect which is either failure to thrive or obesity; neglect due to poor hygiene and clothing; educational neglect which includes truancy or non-compliance; neglect related to housing instability such as homelessness, safe or clean home;

[27] Since for 86.8% of substantiated incidents at least one of the perpetrators is a parent, we refer to parents when discussing the mechanisms.

[28] Unfortunately, we could not find data on mental health for our sample period at county or state level to investigate the mental health mechanism. Lindo, Schaller and Hansen (2018) consider this channel using hospital data for California, but find mixed results. They also investigate parental time use as a potential mechanism through which gender specific unemployment leads to opposite effects on child maltreatment.

and neglect due to lack of available transportation. In this section we empirically test whether the inadequate allocations of goods and services is a potential mechanism for the effect that we estimate on neglect.

We use a state-year level dataset on Personal Consumption Expenditures (PCE) available from the Bureau of Economic Analysis. We focus on groups of expenditures on non-durable goods and services that could be related to neglect due to inadequate nutrition, lack of transportation, housing instability and lack or delay of medical care. The non-durable goods include expenditure per capita on food and beverages purchased for off-premises consumption, clothing and footwear, and gasoline and other energy goods. The services include housing and utilities, health care, and transportation services. We convert all expenditure items to be in real terms.[29] We estimate the following IV regression:

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

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

We run separate regressions for each expenditure item. 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 but with state-level weights, w_{sj}. 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.[30]

We present the results in Table VIII (Table A6 in the Appendix includes a more exhaustive list of items and overall consumption aggregated in the main categories).[31] The Table shows the effect of the unemployment rate on the three non-durable goods (columns (1)-(3)) and the three

[29] To do so, we use the All Items All Urban Consumers CPI, produced by the BLS.

[30] 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.

[31] Table A6 shows a clear decrease in total consumption (column (1) in Panel A) due to unemployment, which is driven by a decrease both in total expenditure on goods (column 2, Panel A), and services (column 5, Panel A). This is in line with recent work who looked at the effect of the Great Recession on consumption (Jensen and Johannesen, 2017). We also find a larger drop in durable expenditure compared to non-durables as predicted by Browning and Crossley (2009)'s model, however, durable goods seem to be less related to child maltreatment.

household consumption expenditures for services (columns (4)-(6)). We find that a one percentage point increase in the unemployment rate causes a 1.1 percent decrease in real expenditure on food and beverages per capita (column (1)), and a 2.2 and a 4.8 percent decrease real expenditure respectively on gasoline and other energy goods (column (3)), and transportation services (column (6)). To explore housing instability as a channel through which unemployment leads to higher neglect, we focus on expenditure for housing and utilities services (column (4)) which consists of rental consumption, water and sanitation, electricity, and gas. The effect of the unemployment rate on this expenditure item is not statistically significant. Finally, we find that real expenditure on health care services (column (5)), which include physicians, dental paramedical and hospital services is not significantly affected by the unemployment rate.

Overall, this analysis provides suggestive evidence that unemployment leads to a decrease in real expenditure on basic goods such as food and beverages, which could lead to nutritional neglect, and on gasoline, energy and transportation that can also put a child at greater risk of neglect (Goldman et al., 2003). Some of our findings are in line with Ganong and Noel (2019) who show that consumer spending during unemployment drops sharply on necessities such as groceries, and Griffith, O'Connell and Smith (2016) who find that over the Great Recession, British households cut real food expenditure.

4.2 Substance Abuse

To explore the substance abuse mechanism, we use data on county-level prevalence of heavy and binge drinking over the period from 2004 to 2012.[32] These measures are estimated by Dwyer-Lindgren et al. (2015), using the Behavioural Risk Factor Surveillance System (BRFSS) dataset.[33]

In Table IX, 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 in fact find that unemployment causes a decrease in female heavy and binge drinking. These effects are significant at the 5% and 1% levels respectively. A one standard deviation increase in the

[32]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.

[33]For details see: `http://www.cdc.gov/brfss/`. BRFSS is only representative at the state level, but Dwyer-Lindgren et al. (2015) use small area models to obtain county-level alcohol use prevalences.

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; 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.

Keeping in mind that the BRFSS is not representative at the county level, and the county prevalence of heavy and binge drinking used here are estimates, our results suggest that alcohol consumption is not a mechanism through which unemployment causes an increase in overall abuse or neglect.

4.3 Divorce

In Table X, we test whether divorce can explain the effect of unemployment on overall abuse or neglect. We use the American Community Survey (ACS), available for the period from 2005 to 2012, to measure the prevalence of divorce at the county level.[34]

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.

[34] 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.

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.

5 The Mitigating Effect of the Unemployment Insurance Program

In the previous section we have seen that the effects of parental unemployment on child neglect may operate directly through effects on family income by decreasing household expenditure. If parents can access welfare benefits, then this can prevent them from falling into poverty in response to a transitory negative income shock, and can help them to provide for their children's basic needs.[35]

We test this hypothesis by focusing on one of the most important social safety nets: Unemployment Insurance (UI).[36] UI provides cash benefits to involuntarily unemployed persons who have sufficiently high earnings histories to qualify. Since unemployment in the U.S. rose dramatically during the Great Recession, the policy response was to extend the duration of unemployment benefits from the standard 26 weeks up to 99 weeks. This was implemented through two programs: the Extended Benefits program, and the Emergency Unemployment Compensation program. States could then chose unemployment rate 'triggers', above which individuals became eligible for additional benefits.

We have a dataset on the duration of unemployment benefits, which comes from state-specific trigger reports provided by the Department of Labor.[37] Figure A7 shows the variation in the duration of benefits over time. In the period 2004-7, almost every state provided twenty-six weeks of unemployment benefits, in 2008, between thirty-three and thirty-five weeks, while after 2008, the variation in in the duration spanned between 48 and 99 weeks.

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 introduce longer extensions to the duration of benefits. We do this 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

[35] Paxson and Waldfogel (2002, 2003) find that reductions in welfare benefits in the 90s are associated with higher rates of foster care which is usually the outcome of the most extreme cases of child maltreatment.

[36] In an earlier version of this paper (Brown and De Cao, 2018), we also looked at two other social safety net programs for which we had a measure of state-level policy differences: the Supplemental Nutrition Assistance Program (SNAP), and the Earned Income Tax Credit (EITC). The first stages were not identified. Whilst this was unfortunate, the literature suggests that SNAP and EITC are less counter-cyclically responsive than UI (Bitler and Hoynes, 2016).

[37] The data is compiled in Hagedorn et al. (2015), and was kindly shared with us by Fatih Karahan (Federal Reserve Bank of New York).

unemployment benefits in Figure A7 to inform our choice of the thresholds. We use four bands in total: [0-48], (48-73], (73-86] and (86-99] weeks. The thresholds of the first band reflect the period 2004-8 where unemployment benefits are more limited, the other two thresholds correspond to the 33rd and 66th percentile of the 2009-12 distribution of benefits which is more variable.[38]

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 XI reports the second stage results (see Table A7 for the first stage results). 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 thirty-seven 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: parents who have been unemployed for more than ninety-nine weeks, and parents who earned less than a state-determined threshold level of earnings in their previous period of employment.

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.

[38] Results are robust to the selection of different sets of thresholds for the period 2009-2012.

5.1 Contiguous Border Counties Methodology

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 XI) might not capture a causal effect. Unobservable worker characteristics at the 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, Lester and Reich (2010).

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. We highlight those 987 counties in a map in Figure A8. 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.

Table XII report the results. In column (1), 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) of Table XI, 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 XI. This suggests that the estimates in Table XI may underestimate the true mitigating effect of extending the duration of benefits. We conduct the same analysis using the thresholds instead of the continuous measure of the duration of unemployment benefits in columns (4) and (5), where column (5) presents the results from the contiguous border counties methodology, and find similar results. Overall the effect of unemployment on neglect using this regression discontinuity approach is qualitatively similar to the analysis conducted in Table XI, even if no longer statistically significant.

6 Conclusion

Victims of abuse and neglect are more likely to be unhealthy, less productive, or to be enrolled in criminal activities which are all costly outcomes to society. Despite this, little causal empirical evidence on the determinants of child maltreatment currently exists. That evidence is clearly needed to guide child protection interventions. In this paper, we address this 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 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 which is formed by interacting initial county industry shares and national industry unemployment rates. We find that a one percentage point increase in the unemployment rate leads to a twenty percent increase in neglect.

As neglect is the failure to provide for a child's basic needs risking their health and well-being, we primarily explore a direct pecuniary channel. The likelihood of neglect might be higher if there is a decrease in income as a consequence of the recession. We find suggestive evidence that higher unemployment leads to a decrease in real expenditure on essential goods and services, like food and beverages, energy and transportation. This income channel is further supported by the results on the mitigating effect of unemployment induced by extensions of the Unemployment Insurance program.

These results suggest a potentially important benefit of the social safety net. The existing literature highlights the successful poverty-reducing impact of welfare programs (e.g., Ben-Shalom, Moffit and Scholz (2012); Moffitt (2013); Bitler, Hoynes and Kuka (2017)). Our paper quantifies an impact of the social safety net that has not been previously considered. Unemployment benefits can prevent children from being neglected when a parent is made unemployed, which may in turn have positive long-lasting consequences for those children. However, over the past 20 years, substantial changes to the U.S. welfare programs have led to increased spending on children near and above the poverty line, and less on the poorest ones (Hoynes and Schanzenbach, 2018). This might imply that the most disadvantaged children who are at greater risk of child maltreatment may have been left behind. It also emphasises how replacing lost income during an economic downturn might benefit children and society in general.

Given that childhood experiences are known to affect a large set of outcomes throughout the lifecycle, more research is needed to evaluate the impact of poverty reduction programs on children

protection, understanding why some low-income families are able to provide adequately for their children despite economic difficulties versus others whose economic conditions may lead to child neglect (Slack et al., 2011; Doyle and Aizer, 2018).

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Tables and Figures

Table I: Summary Statistics

	Mean	SD	25th percentile	Median	75th percentile
Number of Incidents of Physical Abuse	42.20	135.40	3	10.70	29
Number of Incidents of Sexual Abuse	22.58	67.79	2	7	20
Number of Incidents of Emotional Abuse	19.69	176.17	0	1	6
Number of Incidents of Neglect	185.21	621.47	11	43	126
Unemployment Rate %	6.85	2.99	4.6	6.1	8.5
Observations	24,181	24,181	24,181	24,181	24,181
Counties	2,803	2,803	2,803	2,803	2,803
States	46	46	46	46	46

Notes. In this Table, we present summary statistics for the full sample of 24,181 county-years included in the baseline regressions. All the summary statistics are unweighted and refer to substantiated cases.

Table II: First Stage Estimates

	Overall	Physical	Neglect	Sexual	Emotional
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
Bartik IV	**0.60*****	**0.63*****	**0.63*****	**0.63*****	**0.60*****
	(0.10)	**(0.11)**	**(0.11)**	**(0.11)**	**(0.10)**
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	23,468	24,181	24,181	24,181	23,468
Counties	2,771	2,803	2,803	2,803	2,771
States	45	46	46	46	45
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 first stage results for the baseline IV estimates in columns (1)-(5), (6) of Table IV. In each column, 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. There are fewer observations for overall and emotional abuse because some state-years are missing observations for emotional abuse.

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

Table III: The Effect of Unemployment on Child Maltreatment: Reduced Form Estimates

	Overall	Physical	Neglect	Sexual	Emotional
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	OLS
Bartik IV	**0.057*****	0.038	**0.128*****	0.002	0.017
	(0.028)	(0.038)	**(0.038)**	(0.039)	(0.146)
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	23,468	24,181	24,181	24,181	23,468
Counties	2,771	2,803	2,803	2,803	2,771
States	45	46	46	46	45

Notes. Standard errors (in parentheses) are clustered at the state level in all regressions. In this Table we regress the Bartik IV on a different outcome of child maltreatment. 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, 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. There are fewer observations for overall and emotional abuse because some state-years are missing observations for emotional abuse.

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

Table IV: The Effect of Unemployment on Child Maltreatment: IV Estimates

	Overall	Physical	Neglect	Sexual	Emotional
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
Unemployment Rate	**0.096****	0.060	**0.204*****	0.004	0.032
	(0.049)	(0.060)	**(0.068)**	(0.062)	(0.240)
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	23,468	24,181	24,181	24,181	23,468
Counties	2,771	2,803	2,803	2,803	2,771
States	45	46	46	46	45
Mean of outcome	3.77	1.65	3.05	0.94	-2.19
Mean of Unemployment Rate	6.86	6.85	6.85	6.85	6.86
Kleibergen-Paap F-stat	34.53	34.91	34.91	34.91	34.53

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, 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, 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. There are fewer observations for overall and emotional abuse because some state-years are missing observations for emotional abuse.

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

Table V: The Role of the Manufacturing Industry

	Overall	Physical	Neglect	Sexual	Emotional
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	OLS
Manufacturing Bartik IV	**0.052***	0.037	**0.125*****	-0.005	-0.013
	(0.027)	(0.036)	**(0.036)**	(0.041)	(0.141)
Non-Manufacturing Bartik IV	**0.084***	0.040	**0.143****	0.043	0.174
	(0.045)	(0.059)	**(0.057)**	(0.043)	(0.232)
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	23,468	24,181	24,181	24,181	23,468
Counties	2,771	2,803	2,803	2,803	2,771
States	45	46	46	46	45
Test for equality of coefficients (p-value)	0.299	0.929	0.628	0.0944	0.240

Notes. Standard errors (in parentheses) are clustered at the state level in all regressions. In this Table we regress the Manufacturing Bartik IV and the Non-Manufacturing Bartik IV on a different outcome of child maltreatment. 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, 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. There are fewer observations for overall and emotional abuse because some state-years are missing observations for emotional abuse.

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

Table VI: Robustness of Main Results: Alternative Instruments

	Real Exchange Rate		Alternative Bartik						Industry Shares	
			Value Added		Employment		1995 Weight			
	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	0.028	**0.15***	0.086	**0.24****	0.038	**0.21****	**0.073***	**0.17*****	0.023	**0.064***
	(0.065)	**(0.087)**	(0.078)	**(0.12)**	(0.085)	**(0.10)**	**(0.038)**	**(0.053)**	(0.035)	**(0.035)**
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	24,181	23,468	24,181	23,468	24,181	23,459	24,172	23,468	24,181
Counties	2,771	2,803	2,771	2,803	2,771	2,803	2,770	2,802	2,771	2,803
States	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
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	39.0	35.8	20.3	22.2	10.2	12.9	41.2	41.1		
Hansen Overid test p-value									0.000	0.007

Notes. Standard errors (in parentheses) are clustered at the state level except in columns (9)-(10) where they are clustered at the county 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 columns (9)-(10) we use the initial industry shares as instruments following Goldsmith-Pinkham, Sorkin and Swift (2018). 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, 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 VII: The Main Results Capture an Effect on the Actual Incidence of Maltreatment, Not Reporting

	Control Reporting		Professional/Parent Reporters		Unsubstantiated Reports		All Reports	
	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	**0.093***	**0.19*****	**0.098***	**0.23*****	0.035	0.079	0.049	**0.077****
	(0.048)	**(0.064)**	**(0.054)**	**(0.087)**	(0.070)	(0.085)	(0.035)	**(0.038)**
Fraction Employed in Schools	0.55	-0.27						
	(0.57)	(1.15)						
Fraction Employed in Social Services	0.88	1.06						
	(2.46)	(2.88)						
Fraction Employed in Health Care	0.27	1.03						
	(0.36)	(0.96)						
Fraction Employed in Police	-2.89	-0.24						
	(4.19)	(3.65)						
Fraction Employed in Clergy	3.70**	0.34						
	(1.62)	(2.86)						
Fraction Employed in Child Care	-0.091	-0.80						
	(1.36)	(1.65)						
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	20,766	21,354	22,686	23,399	23,468	24,181	23,468	24,181
Counties	2,767	2,799	2,736	2,768	2,771	2,803	2,771	2,803
States	45	46	43	44	45	46	45	46
Mean of outcome	3.75	3.04	3.35	2.53	4.71	4.13	5.35	4.85
Mean of Unemployment Rate	7.02	7.00	6.85	6.83	6.86	6.85	6.86	6.85
Kleibergen-Paap F-stat	32.5	33.0	31.2	31.3	34.5	34.9	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, while in columns (7) and (8) we include all reports, substantiated and unsubstantiated. 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, 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 VIII: Unemployment Decreases Real Expenditure on Essential Goods and Services

	Non-durable Goods			Services		
	Food	Clothing	Gasoline	Utilities	Healthcare	Transportation
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
Unemployment Rate	**-0.0109*** **	-0.0004	**-0.0220** **	-0.0001	-0.0006	**-0.0481*** **
	(0.0031)	(0.0090)	**(0.0100)**	(0.0058)	(0.0060)	**(0.0118)**
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Ethnic Group	Yes	Yes	Yes	Yes	Yes	Yes
Observations	400	400	400	400	400	400
States	46	46	46	46	46	46
Mean of outcome	7.92	6.97	7.18	8.70	8.62	6.85
Mean of Unemployment Rate	6.35	6.35	6.35	6.35	6.35	6.35
Kleibergen-Paap F-stat	18.5	18.5	18.5	18.5	18.5	18.5

Notes. Standard errors (in parentheses) are clustered at the state level in all regressions. Columns (1)-(3) report the effect of unemployment rate on non-durable goods which include food and beverages purchased for off-premises consumption, clothing and footwear, gasoline and other energy goods. Columns (4)-(6) report the effect of unemployment rate on household consumption expenditures for services which include housing and utilities, health care, and transportation services. For more details refer to Section 4.1. The unit of observation is the state-year. All outcomes are expressed in natural logarithm of each expenditure per capita item (measured in 2012 dollars). The unemployment rate is instrumented using state-level Bartik IV. All regressions 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 IX: Alcohol Consumption Does Not Drive the Effects

	Alcohol Consumption						Abuse and Neglect	
	Both		Male		Female		Overall	Neglect
	(1) Heavy	(2) Binge	(3) Heavy	(4) Binge	(5) Heavy	(6) Binge	(7)	(8)
	IV	IV	IV	IV	IV	IV	IV	IV
Unemployment Rate	-0.063	-0.10	-0.0091	0.042	**-0.12****	**-0.24*****	**0.093***	**0.19*****
	(0.051)	(0.068)	(0.068)	(0.12)	**(0.051)**	**(0.061)**	**(0.048)**	**(0.065)**
Heavy Drinking Prevalence							0.016	0.061
							(0.040)	(0.047)
Binge Drinking Prevalence							-0.044	-0.032
							(0.038)	(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 X: Divorce Does Not Drive the Effects

	Divorce		Maltreatment	
			Overall	Neglect
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Unemployment Rate	**0.0060*****	**0.0018****	**0.097****	**0.19*****
	(0.0017)	**(0.00084)**	**(0.049)**	**(0.066)**
Lag Unemployment Rate		**0.0062*****		
		(0.0020)		
Divorce Rate			-0.34	-0.38
			(0.37)	(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 XI: Unemployment Benefits Mitigate the Effect of Unemployment on Neglect - Second stage

	Continuous Benefit Duration	Set of thresholds
	(1)	(2)
	IV	IV
Unemployment Rate	**0.32*** **	**0.29*** **
	(0.12)	**(0.09)**
Unemployment Rate x Benefit Duration	**-0.0021***	
	(0.0012)	
Unemployment Rate x Thres (48-73]		**-0.11** **
		(0.047)
Unemployment Rate x Thres (73-86]		**-0.13*** **
		(0.049)
Unemployment Rate x Thres (86-99]		**-0.14*** **
		(0.045)
State-Year Fixed Effects	Yes	Yes
County Fixed Effects	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 benefits. 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, 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 XII: 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	**0.18***	**0.26***	0.18	**0.24***	0.083
	(0.10)	**(0.14)**	(0.18)	**(0.13)**	(0.17)
Unemployment Rate x Benefit Duration		-0.0025	-0.003		
		(0.0016)	(0.0025)		
Unemployment Rate x Thres (48-73]				-0.098	-0.047
				(0.066)	(0.14)
Unemployment Rate x Thres (73-86]				-0.13	-0.095
				(0.083)	(0.13)
Unemployment Rate x Thres (86-99]				**-0.071***	0.0016
				(0.041)	(0.094)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	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, 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. In columns (3) and (5), we additionally control for county-pair-year fixed effects.

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

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Appendix A: Additional Tables and Figures

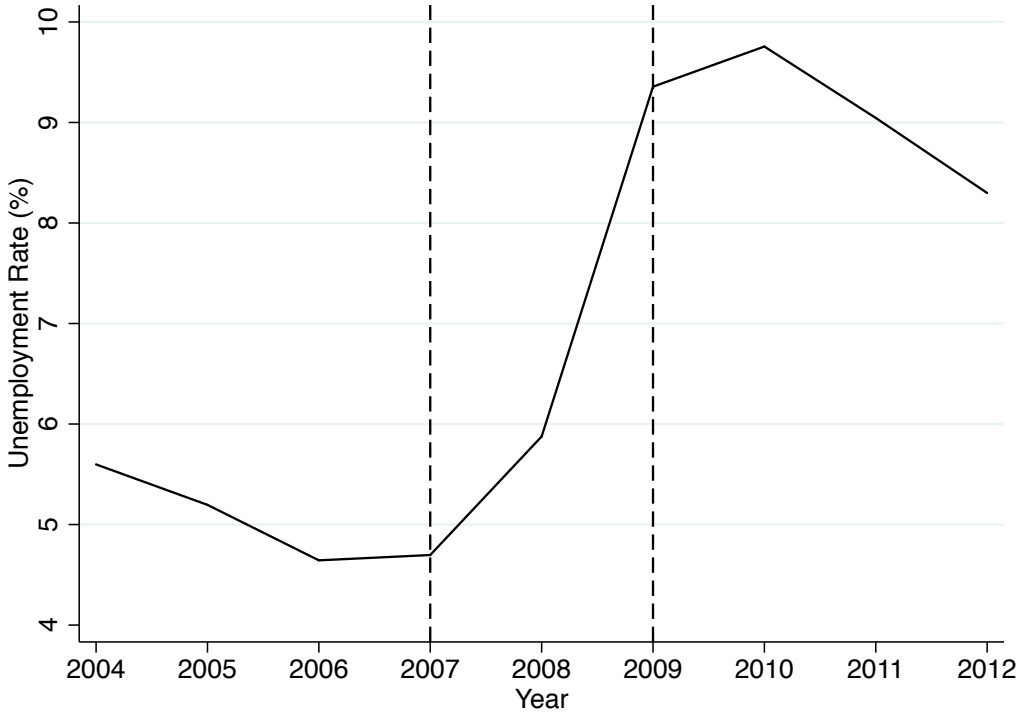


Figure A1: *Notes.* **Trend in the Unemployment Rate**. In this Figure, we present the trend in the weighted average unemployment rate across the 2,803 counties in our final sample. We weight observations by the child population, so that the unemployment rate is representative of where children reside in the U.S. Unemployment rates jumped during the period from 2007 to 2009, with the onset of the recession.

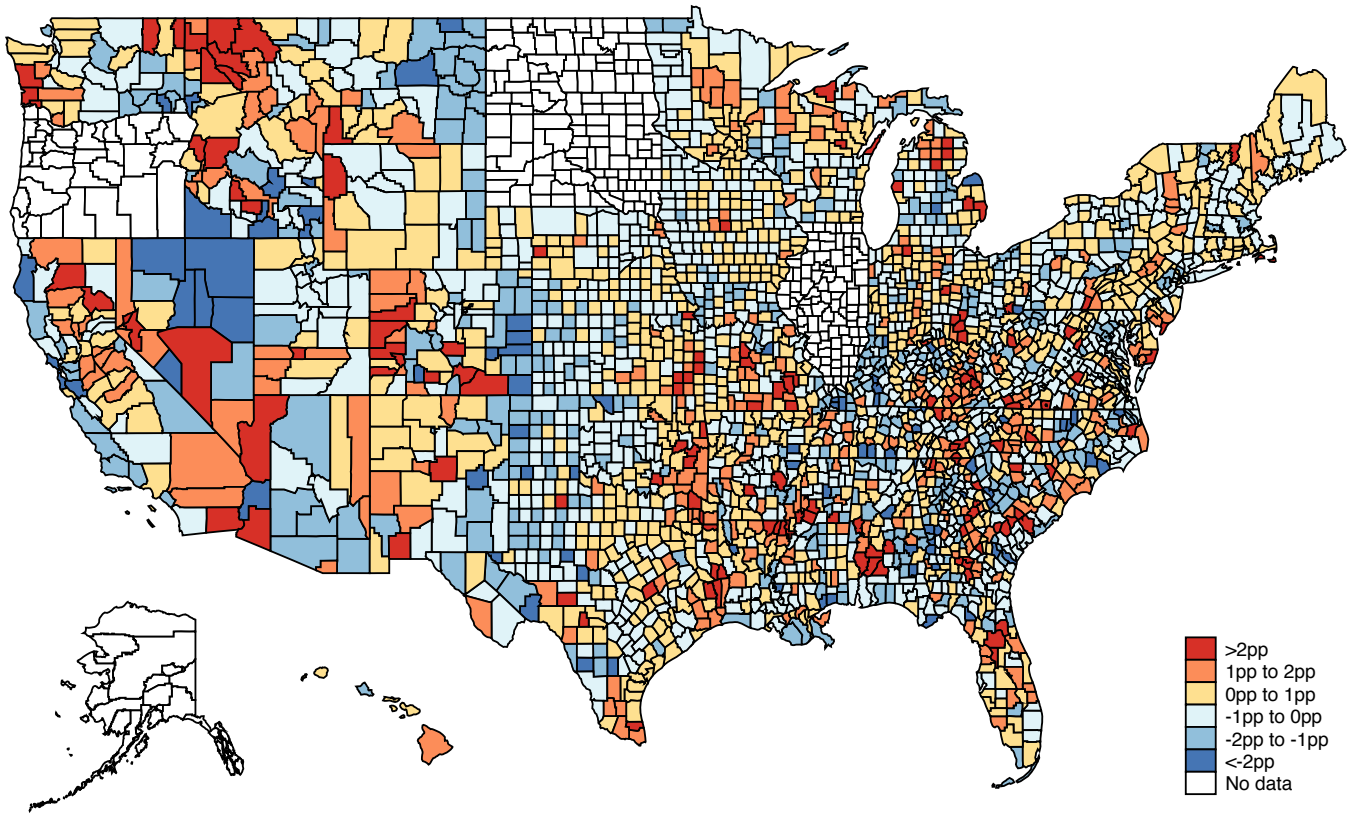


Figure A2: *Notes.* **Change in the Unemployment Rate Before and After the Recession**. This map presents the change in the unemployment rate before and after the recession. We regress the unemployment rate on state-year fixed effects. We take the residuals from this regression, and calculate the average 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.

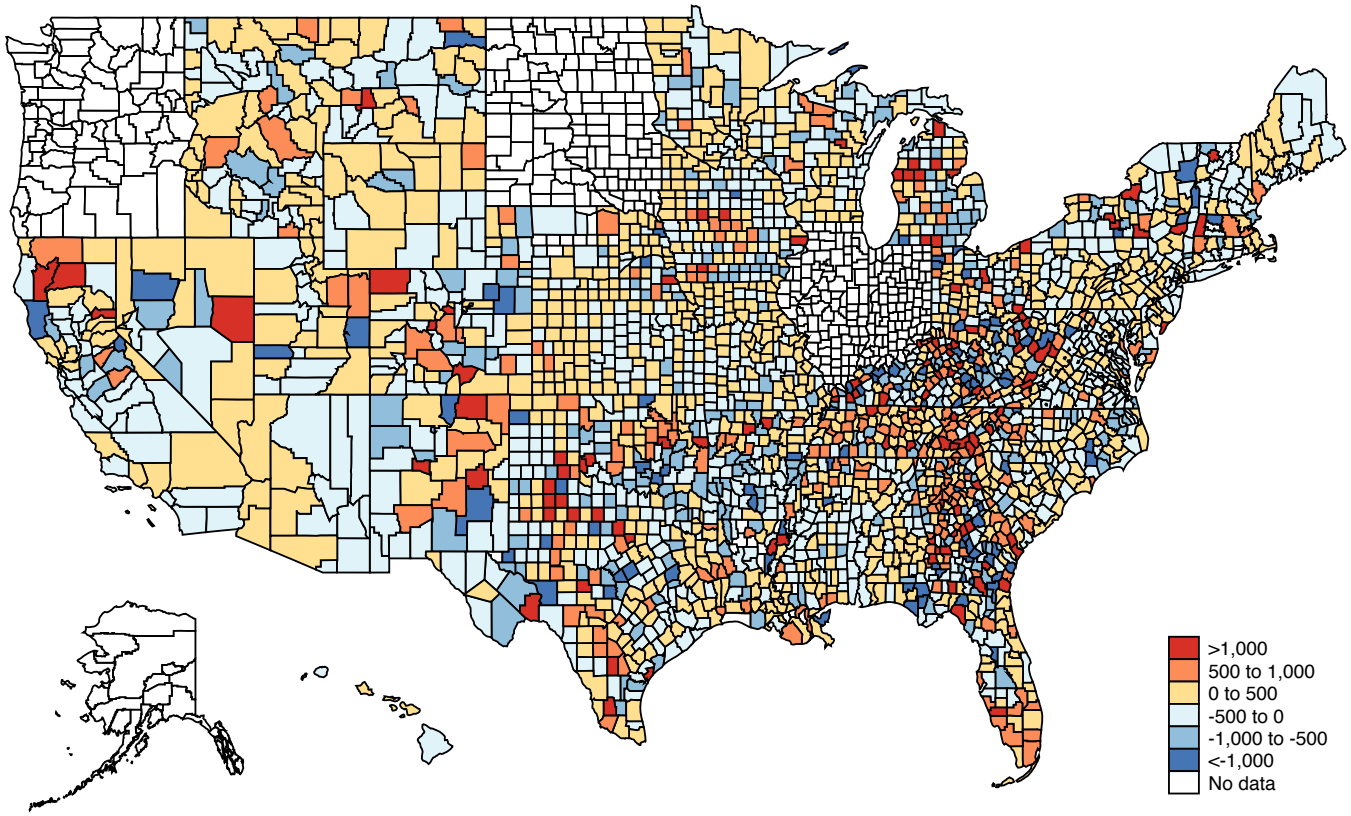


Figure A3: *Notes.* **Change in the Overall Abuse Rate Before and After the Recession**. This map presents the change in the overall abuse rate before and after the recession. We regress the overall abuse rate per 100,000 children on state-year fixed effects. We take the residuals from this regression, 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. Washington does not record emotional abuse in any year, and Indiana does not record emotional abuse for the years 2004-7. Therefore, we cannot calculate the change in the overall abuse rate for either state and so they are missing from this map.

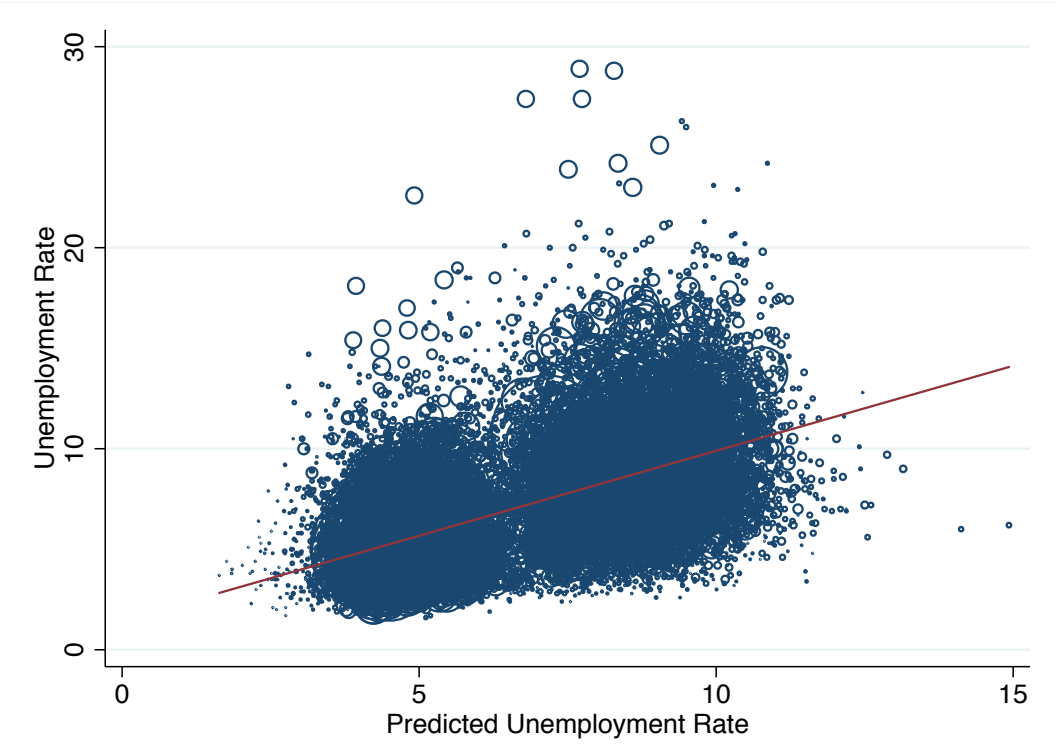


Figure A4: *Notes.* **Unemployment Rate and Predicted Unemployment Rate** The figure shows a scatterplot of unemployment rate and predicted unemployment rate (Bartik instrument) for the period 2004-2012 for all counties included in our main analysis, along with a regression line. Observations are weighted by child population.

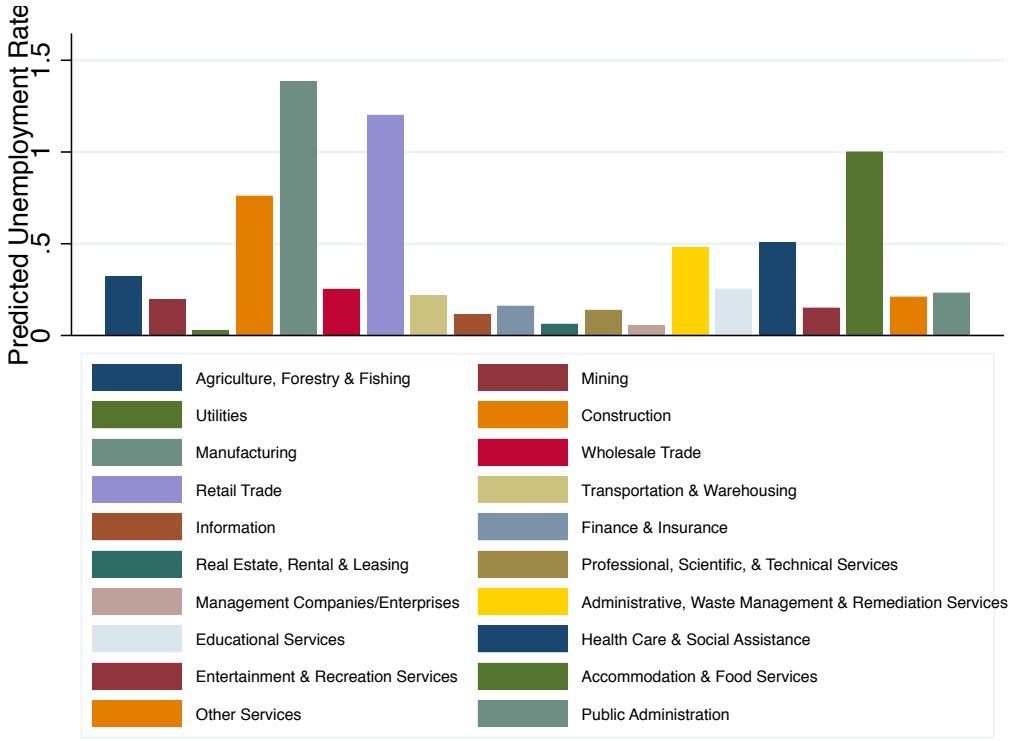


Figure A5: *Notes.* **Predicted Unemployment Rate by Industry.** The figure plots the predicted unemployment rate (Bartik instrument) for the period 2004-2012 for all the 20 industries.

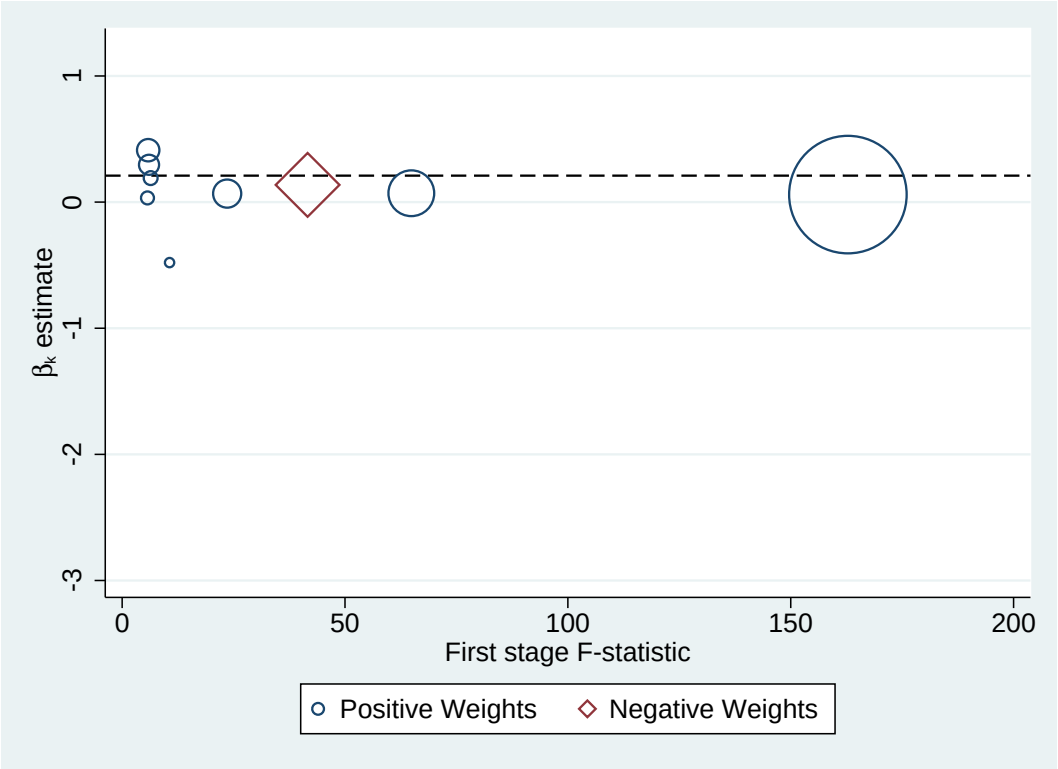


Figure A6: *Notes.* **Just-identified IV estimates based on the industry share instruments.** This figure plots the relationship between each industry share IV estimate $\hat{\beta}_k$, its first stage F-statistic, and its Rotemberg weight. Each point is a separate industry share instrument. The estimated $\hat{\beta}_k$ for each instrument is reported on the y-axis, while its estimated first-stage F-statistic is on the x-axis. The regressions outcome is neglect. Only instruments with first-stage F-statistics above five are reported. The size of the points corresponds to the magnitude of the Rotemberg weights, the circles indicate positive Rotemberg weights and the diamonds indicate negative weights. The horizontal dashed line corresponds to the overall $\hat{\beta}$ (Bartik IV) reported in column (3) of Table IV. For more details refer to footnote 23 and Goldsmith-Pinkham, Sorkin and Swift (2018).

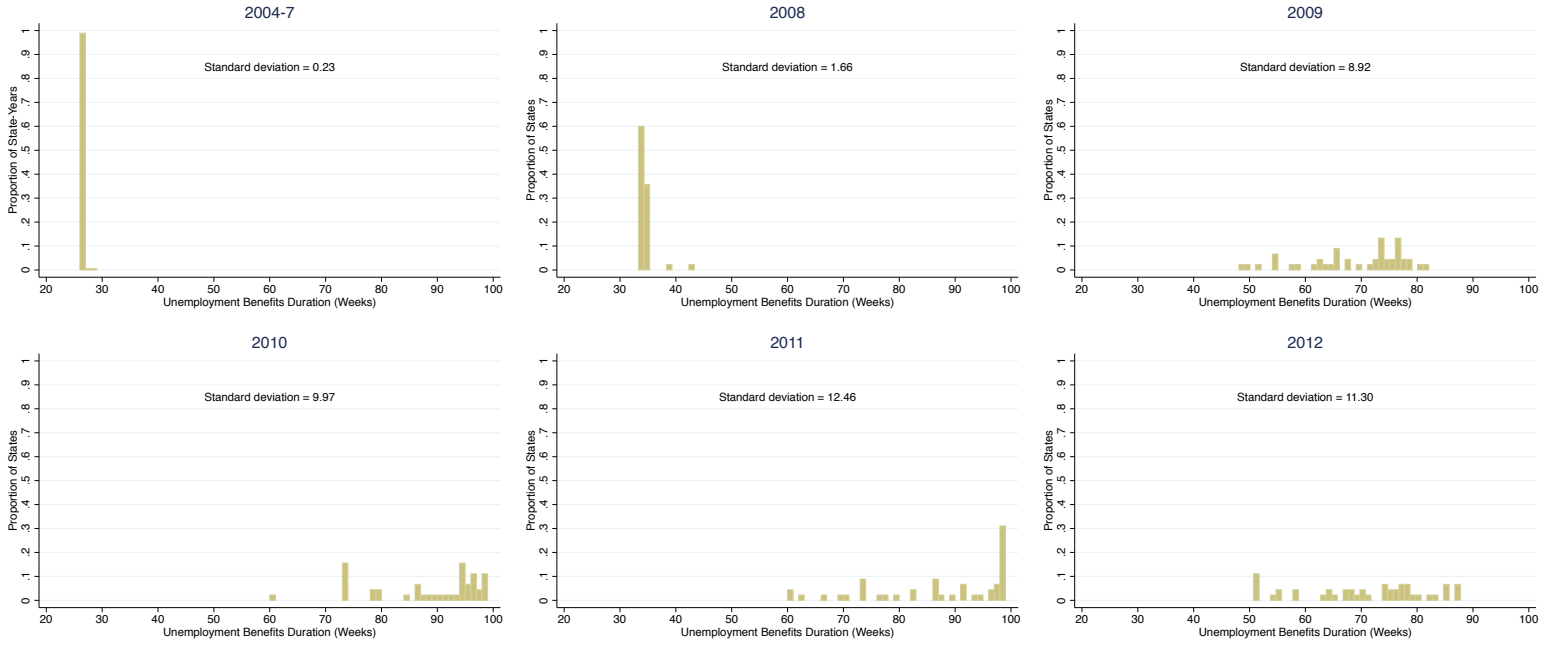


Figure A7: *Notes.* **The Duration of Unemployment Benefits Over Time.** In this Figure, we plot the duration of unemployment benefits over time. We choose bins with a width of one week (we were provided with a quarterly dataset, and we take the mean across quarters to make the dataset annual, so we 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 XI.

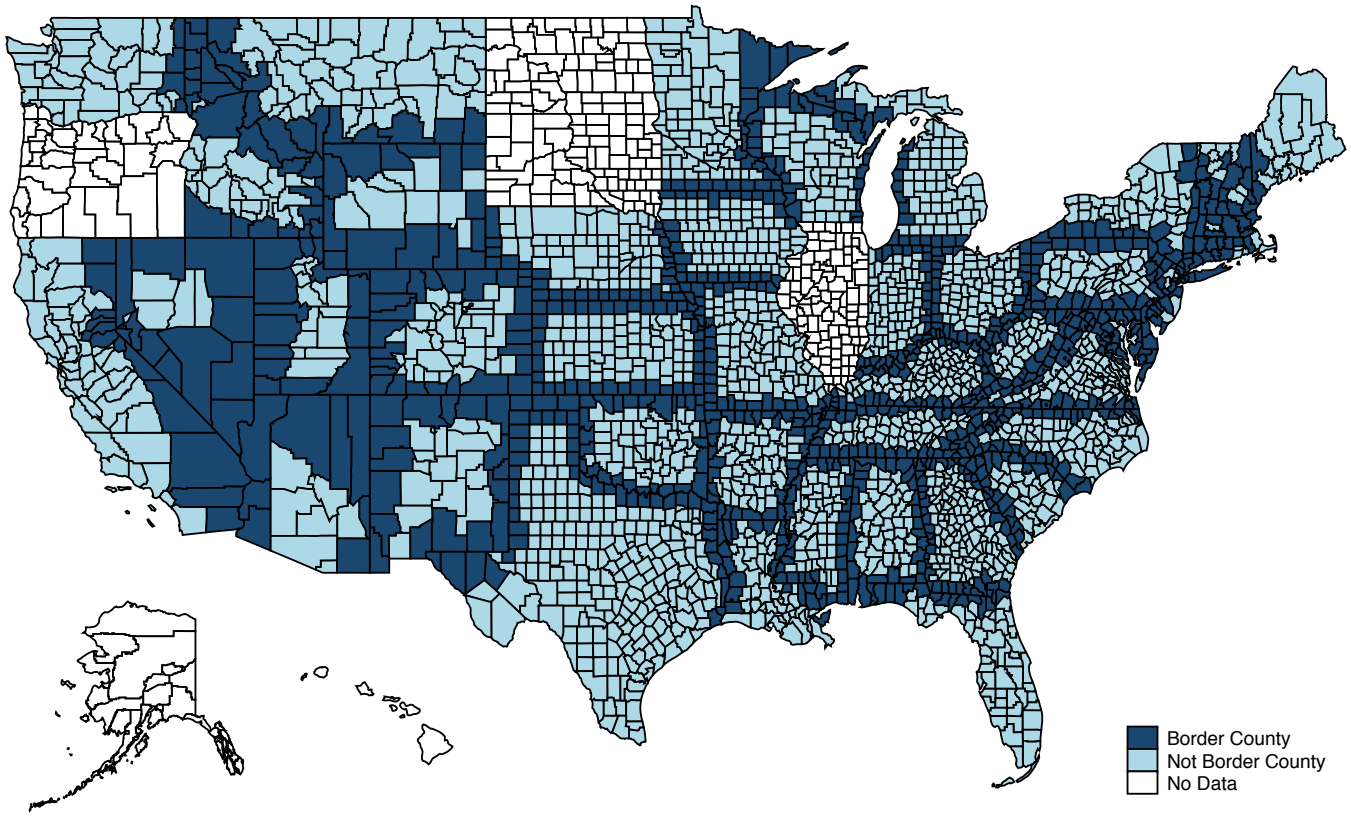


Figure A8: *Notes.* **Border Counties**. This map highlights the counties that are included in the sample for the regressions that use the contiguous border counties methodology in Table XII. These are counties that are part of a contiguous pair which straddles a state border, where both counties are 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 A1: The Effect of Unemployment on Child Maltreatment: OLS Estimates

	Overall	Physical	Neglect	Sexual	Emotional
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	OLS
Unemployment Rate	0.013	**0.026****	0.002	0.010	-0.045
	(0.010)	**(0.010)**	(0.012)	(0.014)	(0.044)
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	23,468	24,181	24,181	24,181	23,468
Counties	2,771	2,803	2,803	2,803	2,771
States	45	46	46	46	45
Mean of outcome	3.77	1.65	3.05	0.94	-2.19
Mean of Unemployment Rate	6.86	6.85	6.85	6.85	6.86

Notes. Standard errors (in parentheses) are clustered at the state level in all regressions. This Table contains the results of the OLS 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, 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. There are fewer observations for overall and emotional abuse because some state-years are missing observations for emotional abuse.

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

Table A2: Rotemberg Weights

	Top 5 Rotemberg weight industries	
	Overall	Neglect
Industry	(1)	(2)
Manufacturing	0.734	0.732
Professional, Scientific, and Technical Services	0.116	0.111
Construction	0.109	0.104
Public Administration	0.064	0.074
Finance and Insurance	0.044	0.042

Notes. The table presents Rotemberg weights for the industries used in the construction of the Bartik instrument, as explained in Goldsmith-Pinkham, Sorkin and Swift (2018). We report the Rotemberg weights for outcomes overall abuse and neglect, and only for industries with a weight above 3 percent.

Table A3: Robustness of Main Results: Alternative Choices for the LHS Variable

	Ln(Rate)		IHS Rate		Add 0.01		Add 0.0001		Ln(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	**0.096***	**0.21*****	**0.094***	**0.18*****	**0.093***	**0.18*****	**0.099****	**0.23*****	0.11	**0.25*****
	(0.053)	**(0.073)**	**(0.053)**	**(0.066)**	**(0.048)**	**(0.063)**	**(0.050)**	**(0.074)**	(0.067)	**(0.092)**
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 take the natural logarithm of the number of children who are victims of overall abuse and neglect (after adding 0.001), 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, state-year fixed effects, 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. We weight observations by the child population in the county-year.

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

Table A4: Robustness of Main Results: Specification Changes

	Control County Trends		Drop State-Year FE		2003 Child Weight		Total Pop Weight		Cluster County Level	
	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	0.042	**0.098****	0.067	**0.13****	**0.095***	**0.20*****	**0.096****	**0.20*****	**0.096***	**0.20*****
	(0.041)	**(0.046)**	(0.052)	**(0.059)**	**(0.049)**	**(0.067)**	**(0.045)**	**(0.063)**	**(0.051)**	**(0.063)**
State-Year Fixed Effects	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear County Trends	Yes	Yes	No	No	No	No	No	No	No	No
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	24,181	23,468	24,181	23,480	24,193	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
States	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
Mean of Unemployment Rate	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 (8), and at the county level in columns (9) and (10). 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 weight observations using the child population in the county in 2003. In columns (7) and (8), we weight observations using the total population in the county-year. Finally, in columns (9) and (10), 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. 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. In columns (1)-(4) and (9)-(10), we weight observations by the child population in the county-year.

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

Table A5: 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	**0.12*****	**0.22*****	**0.089***	**0.20*****
	(0.043)	**(0.065)**	**(0.050)**	**(0.069)**
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 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 A6: Unemployment Decreases Real Expenditure

Panel A: Main expenditure items

	Total PCE	1. Goods	1.1. Durable Goods	1.2. Non-durable Goods	2. Services	2.1. HH consumption	2.2. NPISHs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	IV	IV	IV	IV	IV	IV	IV
Unemployment Rate	**-0.0145*** **	**-0.0155*** **	**-0.0379*** **	-0.0043	**-0.0106*** **	**-0.0116*** **	0.0051
	(0.0033)	**(0.0057)**	**(0.0099)**	(0.0036)	**(0.0023)**	**(0.0023)**	(0.0291)
Mean of outcome	10.45	9.400	8.296	8.994	10.02	9.981	6.698

Panel B: Expenditure on 1. Goods

	1.1. Durable Goods				1.2. Non-durable Goods			
	1.1.1. Motor	1.1.2. Furnishings	1.1.3. Recreational	1.1.4. Other durable	1.2.1. Food	1.2.2. Clothing	1.2.3. Gasoline	1.2.4. Other non-durable
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IV	IV	IV	IV	IV	IV	IV	IV
Unemployment Rate	**-0.0481*** **	**-0.0478*** **	**-0.0302*** **	0.0003	**-0.0109*** **	-0.0004	**-0.0220** **	**0.0108* **
	(0.0135)	**(0.0139)**	**(0.0090)**	(0.0089)	**(0.0031)**	(0.0090)	**(0.0100)**	**(0.0056)**
Mean Outcome	7.226	6.839	7.006	6.335	7.924	6.973	7.178	7.951

Panel C: Expenditure on 2.1 HH consumption

	2.1.1. Utilities	2.1.2. Healthcare	2.1.3. Transportation	2.1.4. Recreational Serv	2.1.5. Food-Accomodation	2.1.6. Financial	2.1.7. Other Serv
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	IV	IV	IV	IV	IV	IV	IV
Unemployment Rate	-0.0001	-0.0006	**-0.0481*** **	**-0.0190** **	**-0.0247*** **	**-0.0280*** **	**-0.0148** **
	(0.0058)	(0.0060)	**(0.0118)**	**(0.0076)**	**(0.0063)**	**(0.0047)**	**(0.0068)**
Mean Outcome	8.705	8.624	6.852	7.139	7.631	7.862	8.012

Notes. Standard errors (in parentheses) are clustered at the state level in all regressions. Total PCE - Personal Consumption Expenditure - is given by the sum of expenditure on goods and services. Expenditure on Goods include both durable and non-durable goods. Expenditure on Services include HH consumption that corresponds to Household consumption expenditures (for services), and NPISHs that is final consumption expenditures of nonprofit institutions serving households. In Panel B we report the effect of unemployment rate on expenditure on Durable goods which include motor vehicles and parts, furnishings and durable household equipment, recreational goods and vehicles and other durable goods, and expenditure on Non-durable goods which include good and beverages purchased for off-premises consumption, clothing and footwear, gasoline and other energy goods, and other non-durable goods. In Panel B we report the effect of unemployment rate on Household consumption expenditures for services which include housing and utilities, health care, transportation services, recreation services, food services and accommodations, financial services and insurance, and other services. For more details refer to Section 4.1. The unit of observation is the state-year. All outcomes are expressed in natural logarithm of each expenditure per capita item (measured in 2012 dollars). The unemployment rate is instrumented using state-level Bartik IV. All regressions 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. The Kleibergen-Paap F-stat is 18.52, and the mean of unemployment rate is 6.35.

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

Table A7: 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	**1.35*** **	**153.21*** **	**-0.26*** **	**-0.41*** **	**-0.65*** **	**-0.53*** **
	(0.26)	**(37.08)**	**(0.07)**	**(0.09)**	**(0.11)**	**(0.075)**
Bartik IV x Benefit Duration	**-0.007*** **	**-1.18*** **				
	(0.002)	**(0.40)**				
Bartik IV x Thres (48-73]			**0.052*** **	**0.11*** **	**0.0055*** **	**0.0051*** **
			(0.0027)	**(0.0037)**	**(0.0018)**	**(0.0012)**
Bartik IV x Thres (73-86]			**0.052*** **	**0.0028*** **	**0.11*** **	**0.0034*** **
			(0.0039)	**(0.0009)**	**(0.0040)**	**(0.0009)**
Bartik IV x Thres (86-99]			**0.049*** **	**0.0015*** **	0.0006	**0.11*** **
			(0.0029)	**(0.0005)**	(0.0008)	**(0.0027)**
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	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 reported in Table XI in which we ask whether the effect of unemployment on neglect is mitigated by unemployment benefits. 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. Data on the duration of benefits is not available for Hawaii.

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

Appendix B: The Child Protective Services and the Process of Child Maltreatment Reporting

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. The process of child maltreatment reporting varies by state, but typically works as follows. All but ten states have a centralised statewide hotline that reporters can call if they suspect child abuse or neglect.[39] 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.

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. 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

[39] 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.

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 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 2017, the CPS agencies received approximately 4.1 million reports of child abuse or neglect involving approximately 7.5 million children, of which 3.5 million were investigated (U.S. Department of Health and Human Services, 2017). Professionals made 65.7% of reports, with 19.4% by education personnel, 18.3% made by legal and law enforcement personnel and 11.7% by social services personnel (U.S. Department of Health and Human Services, 2017). Of the child-reports that are investigated, 17% of cases are found to be substantiated.

Online References

Child Welfare Information Gateway. 2014. "Definitions of Child Abuse and Neglect." U.S. Department of Health and Human Services.

US Department of Health and Human Services. 2017. "Child Maltreatment 2017'.' US Department of Health and Human Services, Administration for Children and Families.

Appendix C. Description of the Construction of the Outcomes Dataset

C.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 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 C1. 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 C1, 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

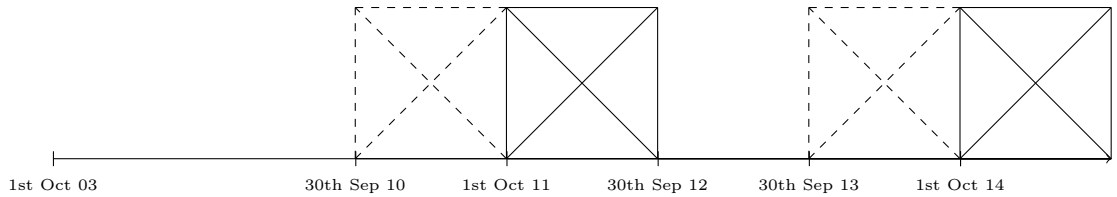


Figure C1: Missing Years by Report Date for Indiana. The solid crosses indicate the missing periods of data for Indiana by investigation disposition date (the Federal Fiscal Year 2012, and from 1st October 2014 onwards). To organise the data by report date, we treat both the solid and dashed time periods 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.

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 2.2.

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