Homes in Limbo, Children at Risk: Exploring the Link between Housing Instability and Child Maltreatment Using the Discontinuity of the Protecting Tenants at Foreclosure Act

Wei Fu[1] and Muzhe Yang[2]

November 2024

Preliminary Draft: Please do not cite or quote without the authors' permission.

[1] Department of Health Management and Systems Sciences, University of Louisville, Louisville, KY 40202, wei.fu@louisville.edu. Declarations of relevant or material financial interests that relate to the research described in this paper: none. Declarations of conflicts of interest: none.

[2] Corresponding Author, Department of Economics, Lehigh University, Bethlehem, PA 18015, muzheyang@lehigh.edu. Declarations of relevant or material financial interests that relate to the research described in this paper: none. Declarations of conflicts of interest: none.

Abstract

This study uses the discontinuity of the Protecting Tenants at Foreclosure Act (PTFA), which resulted in a lapse in federal protections and variations in state-level tenant protections, to examine the causal link between housing instability and child maltreatment. Using restricted administrative data and a difference-in-differences approach, we find an increase of as much as 18% in housing instability-induced child maltreatment in states lacking PTFA protection during the gap period. Our findings highlight the need to incorporate stable housing as a key component into prevention and intervention strategies, to tackle the wide range of risk factors of child maltreatment effectively.

JEL codes: I12, I18, I31, J12, J13, R38
Keywords: child maltreatment, housing instability, foreclosure, tenant protection

1. Introduction

Child maltreatment, including all forms of physical, emotional and sexual abuse, as well as neglect and exploitation of children by a parent or caregiver, is one of the most critical issues in the United States. Recent data indicate a national rate of 7.7 victims per 1,000 children between 2021 and 2022 (Children's Bureau, 2022), with young children at the greatest risk—70 percent of these victims were between birth and age 10 in 2022.[1] These alarming statistics highlight an urgent need for greater awareness, prevention and intervention efforts to protect children.

The consequences of child maltreatment are severe and enduring, linked to a range of adverse outcomes, including physical and psychological harms, cognitive impairments, chronic health conditions, criminal behaviors, and significant economic costs (Currie and Tekin, 2012; Currie and Widom, 2010; Leeb et al., 2011; Strathearn et al., 2020; Vachon et al., 2015). The average lifetime cost per victim of nonfatal child maltreatment is estimated at approximately $210,000 in 2010 dollars (Fang et al., 2012).

Given these significant and long-lasting consequences, extensive research has examined contributing factors at individual, interpersonal, community and societal levels (Austin et al., 2020). These include a child's characteristics, parental characteristics, socioeconomic status, as well as broad economic and policy environments.[2] A recent evidence review by Higgins and Hunt (2024) finds that parental characteristics are consistently identified as key factors associated with child maltreatment across various studies. That review also highlights the necessity

[1] These statistics are based on administrative data from the National Child Abuse and Neglect Data System. These data only include children reported to authorities (e.g., the state's child protective services agency) for maltreatment. As a result, the actual number of abused or neglected children can be greater. Detailed statistics are provided at https://datacenter.aecf.org/data/tables/9904-children-who-are-confirmed-by-child-protective-services-as-victims-of-maltreatment-by-age-group#detailed/1/any/false/1095/62,2594,2595,113,36/19235,19236 (accessed in July 2024).
[2] We give a detailed review of this literature in Section 2.1.

for further research on factors affecting parenting skills and capacity, such as housing instability. Our study aims to address this gap in the literature.

In general, housing instability refers to challenges such as difficulty in paying rent, spending a significant portion of income on housing, living in substandard or overcrowded conditions, and frequent relocations (Gu et al., 2023). Housing instability is prevalent among children. Nationwide, it is estimated that in 2022, 17.1 percent of U.S. children (more than one in six), representing more than 12.1 million children, experienced unstable housing, with higher prevalence (27 to 30 percent) among racial minorities (Lebrun-Harris et al., 2024).

Research has documented an association between housing instability and child maltreatment (Chandler et al., 2022), with housing instability ranking second only to poverty among social determinants of health that are correlated with child maltreatment (Hunter and Flores, 2021). However, establishing a causal link remains challenging, mainly due to unobserved variables (e.g., hidden substance abuse) that affect both housing instability and child maltreatment. Our study aims to fill this gap in the literature by focusing on one specific aspect of housing instability: the uncertainty that tenants face regarding their ability to remain in their rented homes for the full lease term due to unexpected property foreclosures, often without prior notice from landlords.

Indeed, such situations often arise through no fault of the tenants. The Protecting Tenants at Foreclosure Act (PTFA), a federal law established in 2009, was intended to shield tenants from the repercussions of foreclosures. However, in December 2014, the U.S. Congress failed to renew this law, resulting in a significant gap in federal protection. During this period, some states enacted their own laws to continue the protection previously offered by the PTFA (a control group), while residents in other states experienced the lapse of the PTFA protection (a treatment group).

The central idea of our research design is comparing two groups, both of which include tenants who can face uncertainty about staying in their rented homes for the full lease term due to unexpected foreclosures. In one group, this uncertainty is reduced during the relocation process, while the other group does not experience this reduction, leading to greater housing instability. In our study, we focus on one kind of reduced uncertainty, which is due to the protection provided by the PTFA to tenants who face forced displacement. We give detailed explanations about the PTFA and our identification strategies in Section 2.2 and Section 3.2, respectively.

It is important to note that the kind of housing instability we focus on is specific to unexpected eviction that happens to tenants living in foreclosed properties who themselves did nothing wrong. This distinguishes our study from those focusing on evictions in which tenants' own behaviors or actions (e.g., not paying rent) led to those evictions. While our focus seems limited, we want to emphasize that forced displacement (e.g., due to foreclosure-related evictions) can exacerbate housing instability. A forced move can make renters accept substandard housing, which can lead to another move soon after, especially for low-income renters (Desmond et al., 2015). In addition, forced displacement can compel renters to move to lower-quality housing and neighborhoods, putting them in a precarious housing situation (Desmond and Shollenberger, 2015; Evans, 2021). Thus, our focus extends beyond a particular type of eviction to examine a form of eviction that can trigger a vicious cycle of housing instability. In this regard, our study provides important insights into the broad implications of such housing instability.

Focusing on the PTFA's gap period, our empirical analysis uses restricted administrative data from the National Child Abuse and Neglect Data System (NCANDS) and a heterogeneity-robust difference-in-differences (DID) estimator. We provide compelling evidence that incidents of child maltreatment due to housing instability increase by as much as 18% in the treatment group in the year following the PTFA's expiration; this effect persists until the PTFA was reinstated in June 2018. Furthermore, using data from the Behavioral Risk Factor Surveillance System (BRFSS), we find an increase in mental health issues among renters, but not homeowners, in the treatment group during the PTFA's gap period. This finding on mental health issues suggests a mechanism through which housing instability affects child maltreatment. Moreover, it aligns with the hypothesis that the discontinuity of the PTFA, which resulted in a significant lapse in federal protection for tenants, mostly impacts renters rather than homeowners.

In all our DID estimations, we find support for the parallel trend (PT) assumption on the basis that differences in the outcome variable's trends between the treatment and control groups are not detected in the pre-treatment period. In our study we also conduct falsification checks by examining maltreatment cases related to inherent risk factors of the child and the caregiver, such as intellectual disability or chronic emotional disorder—outcomes that are unlikely to be influenced by the loss of federal protection of tenants living in foreclosed properties. As expected, all these falsification checks confirm null results.

To the best of our knowledge, we provide the first study that utilizes exogenous changes in PTFA to examine the causal relationship between housing instability and child maltreatment. Given the current housing market pressures, such as the affordability crisis, which contributes to unstable housing, our findings highlight an important causal link. These housing pressures can create stressful environments that are not conducive to a child's safety and well-being. Therefore, it is crucial that prevention and intervention strategies incorporate housing stability as a key component to effectively address the wide range of risk factors associated with child maltreatment.

The rest of the paper proceeds as follows. Section 2 reviews the literature, including an overview of the PTFA. Section 3 details the empirical framework of our study. We discuss the findings in Section 4, with concluding remarks provided in Section 5.

2. Background

2.1. Child Maltreatment, Contributing Factors, and Housing Instability

Our study focuses on the causal relationship between child maltreatment and one of its risk factors—housing instability. More broadly, our study bridges two strands of literature: one focusing on the risk factors of child maltreatment, and the other on the impact of housing instability.[3]

Child maltreatment has significant and enduring effects on its victims. It is linked to a variety of adverse physical and mental health outcomes that affect individuals throughout their lives, placing a significant burden on both victims and society (e.g., Leeb et al., 2011). What is worth noting is that different forms of maltreatment (e.g., physical, emotional, sexual abuse, and neglect) can have equivalent, broad effects on psychiatric and behavioral outcomes, such as anxiety and depression, rule-breaking and aggression (Vachon et al., 2015). The adverse effects of maltreatment can persist over a long period of time, affecting a wide range of outcomes such as attention problems, significant cognitive delays, posttraumatic stress disorder symptoms, delinquency, drug abuse, and youth pregnancy (Strathearn et al., 2020). Victims of maltreatment during childhood are also found to have lower levels of education, employment, earnings, and fewer assets as adults, with larger adverse effects for women (Currie and Widom, 2010). Furthermore, Currie and Tekin's study (2012) finds that child maltreatment is a major determinant of future criminal behavior, and the impact is substantial compared with other factors that have been studied in the economics literature (e.g., unemployment, education, gun ownership, crack cocaine introduction, abortion legalization, and lead exposure). The average lifetime cost per victim of nonfatal child maltreatment is estimated to be about $210,000 in 2010 dollars; this estimate includes costs such

[3] The scope of our review does not include foster care, given that the study by Doyle and Aizer (2018) shows that the rates of child maltreatment and foster care entry are almost uncorrelated with each other (with more details provided in their figure 2).

as childhood health care costs, adult medical costs, productivity losses, child welfare costs, criminal justice costs, and special education costs (Fang et al., 2012).

Given the serious and far-reaching consequences of child maltreatment, existing literature has examined contributing factors at individual, interpersonal, community and societal levels (Austin et al., 2020). These factors include, for example, child's age (Children's Bureau, 2022); maternal age at the child's birth (Mersky et al., 2009); special health care needs or disabilities (Jaudes and Mackey-Bilaver 2008; Van Horne et al., 2015; Van Horne et al., 2018); parental work status and single parenthood (Paxson and Waldfogel, 2002); parental employment and welfare receipt (Slack et al., 2003); parental mental health disorders (Chemtob et al., 2013; Lee et al., 2012); parental substance use (Kepple, 2017); parental intimate partner violence (Taylor et al., 2009); parental resources including income, parental time and the quality of parental time (Paxson and Waldfogel, 1999); relationship transition (Schneider, 2016); income and poverty status (Berger et al., 2017; Maguire-Jack and Font, 2017); access to abortion (Aslim et al., 2024; Bitler and Zavodny, 2002, 2004; Seiglie, 2004; Sen, 2007); access to mental health and substance use treatment (Ali et al., 2024); crime and neighborhood disadvantage (Morris et al., 2019); social norms regarding gender inequity (Klevens and Ports, 2017); and economic conditions and policies[4] such as county-level economic indicators (Frioux et al., 2014), the Great Recession (Schneider et al., 2017), gender-specific labor market conditions (Lindo et al., 2018), state-level child

[4] The Child Abuse Prevention and Treatment Act (CAPTA) is a key piece of federal legislation in the United States, aiming to address child abuse and neglect. CAPTA was enacted in 1974, and it has been amended several times. CAPTA provides federal funding to states to support prevention, assessment, investigation, prosecution, and treatment activities related to child abuse and neglect (https://www.childwelfare.gov/resources/about-capta-legislative-history, accessed in July 2024). However, the effectiveness of CAPTA has been questioned (https://www.childrensrights.org/news-voices/50-years-of-capta-what-you-need-to-know-about-this-harmful-law, accessed in July 2024). A national survey conducted by *The Boston Globe* and *ProPublica* in 2019 finds that no state fully complies with the CAPTA requirements (https://www.bostonglobe.com/metro/2019/12/13/cry-for-help/prT5xvp27BGZK6AZQWRNVL/story.html, accessed in July 2024).

welfare expenditures (Malcolm, 2012), the Temporary Assistance for Needy Families program (Ginther and Johnson-Motoyama, 2022), state supplemental nutrition assistance program (SNAP) (Johnson-Motoyama et al., 2022) and state expansion of the SNAP's eligibility (Austin et al., 2023), proximity to SNAP-authorized retailers (Bullinger et al., 2021), beer taxes (Markowitz and Grossman, 2000), cigarette taxes (McLaughlin, 2018), minimum wage (Raissian and Bullinger, 2017; Schneider et al., 2022), child tax credit (Bullinger and Boy, 2023), welfare reforms (Paxson and Waldfogel, 2003), supply of prescription opioids (Evans et al., 2022), and public childcare provision (Sandner et al., forthcoming).

As Austin et al. (2020) points out, existing approaches for preventing child maltreatment have primarily focused on reducing risks and enhancing protective measures at the interpersonal level, with the assumption that the child's family has control over all factors contributing to the maltreatment of a child; this focus is limited given that there are risk factors at the community and societal levels that are beyond the family's control. One of those factors is related to housing instability, which our study focuses on. A recent evidence review conducted by Higgins and Hunt (2024) finds that across the included studies parental characteristics are consistently identified as key factors associated with child maltreatment. This review also underscores the need for further research on factors affecting parenting skills and capacity, such as housing instability. Our study aims to address this gap in the literature.

There is a body of literature documenting an association between housing instability and child maltreatment (Chandler et al., 2022). As one of the social determinants of health that correlate with child maltreatment, housing instability is found to rank second among those factors, next to poverty (Hunter and Flores, 2021). Although housing instability has been recognized as one important risk factor for child maltreatment, establishing a causal link remains challenging. For example, a study by Marcal (2018) aims to confirm that causal link using matching and propensity score weighting methods, which rely on the selection-on-observables assumption meaning that all factors affecting housing instability and child maltreatment are observed and controlled for. This assumption may not hold in practice. For instance, families experiencing unstable housing may receive some forms of assistance (e.g., through the emergency rental assistance program), which could potentially lower the risk of child maltreatment. If the assistance received is uncontrolled for, then the effect of housing instability on child maltreatment can be under-estimated, and this lack of control may explain why Marcal (2018) only finds a small effect of housing instability on child maltreatment.

To the best of our knowledge, we provide the first study that utilizes the change in PTFA, which occurs exogenously to individuals, to explore a causal link between housing instability and child well-being. Our identification strategy aligns with studies that leverage exogenous variation in the causal variable, such as Evans et al. (2023), which estimates the impact of temperature on child maltreatment, and Erten and Keskin (2020), which uses a regression discontinuity design to estimate the effect of education on child physical abuse. More broadly, our study adds to the literature summarized by Doyle and Aizer (2018) on utilizing quasi-experimental variation for causal inference in assessing the impact of public policies and social programs on child well-being.

2.2. The Protecting Tenants at Foreclosure Act

The PTFA is a federal law that provides protection to tenants who live in properties that have been foreclosed upon. In this section we give an overview based on OCC (2020).

The PTFA requires any new owner who takes over a foreclosed residential property (which can be any type, including single-family homes and multi-unit properties) to honor the rights of existing tenants. Specifically, the new owner is required to give notice to tenants at least 90 days before eviction, starting from the date when tenants receive the notice to vacate. Furthermore, the PTFA allows tenants to stay in their homes until the end of their leases, unless the new owner intends to use the property as primary residence or if there is no lease or the lease can be terminated under state laws. Even under these exceptions, the PTFA requires that tenants should still receive a 90-day notice prior to eviction. The PTFA aims to provide tenants who must vacate their homes with sufficient time to find new places to live, helping them maintain a certain degree of housing stability and protecting them from being abruptly displaced or even becoming homeless.

The PTFA was enacted in May 2009, intending to shield tenants from being inadvertently affected by foreclosures through no fault of their own. However, the law was passed with a sunset clause because of the intention of making the law only as a temporary help to tenants during the peak of the foreclosure crisis in 2009. Despite the recognition of ongoing need for tenant protections and the legislation introduced in 2013 in the U.S. Congress (NLIHC, 2015), there was little bipartisan support in passing the bill, eventually leading to the expiration of the PTFA on December 31, 2014.[5]

The PTFA is the only law that provides the federal-level standardized protection to tenants living in foreclosed properties. This federal law does not override state laws that are more protective. However, prior to the expiration, the PTFA remained more protective than state laws in most states, especially in areas regarding the survival of a lease beyond a foreclosure and the advance notice a tenant must receive before being required to vacate the foreclose property (NLCHP, 2012)—areas that can significantly affect the tenant's housing stability.[6] Therefore, the

---

[5] For more details, see https://safeguardproperties.com/protecting-tenants-at-foreclosure-act-ptfa-scheduled-to-expire-at-end-of-2014/ (accessed in July 2024).

[6] "Since its enactment, the PTFA has been recognized as an extremely effective tool in preventing the eviction of renters due to a foreclosure. In the fall of 2011, NLIHC conducted surveys of legal service providers and housing counselors who work directly with tenants in foreclosure. Ninety-two percent of the legal service providers responded that they have used the PTFA in their advocacy for a client. Close to 90 percent of the lawyers who have used the PTFA stated that it has helped to halt or otherwise avoid an eviction in their cases. Likewise, 90 percent of housing counselors said that the PTFA has been useful in one or more of their eff orts to assist tenants in foreclosure-related

expiration of the PTFA means losing the best protection for tenants in foreclosed properties across the country, and tenants were left on their own to rely on a patchwork of state laws governing what landlords must do for tenants in foreclosed properties to protect themselves from abrupt displacement. Furthermore, unlike homeowners who may anticipate a foreclosure and make preparations accordingly, tenants may not know if their landlords are missing mortgage payments and therefore can be caught entirely off guard by a foreclosure.[7] Thus, losing the PTFA can create significant housing instability, placing an undue burden on tenants who are not responsible for the default that causes the foreclosure but end up having little or no time to prepare to relocate.

In response to the ongoing challenges faced by tenants and recognizing the importance of protecting tenants' rights during the foreclosure process, the U.S. Congress reinstated the PTFA in June 2018, making it permanent (OCC, 2020).[8] In retrospect, the discontinuity of PTFA created a significant lapse in federal protection, putting tenants in foreclosed properties in a precarious situation, such as being evicted with just a few days' notice. During the period from the expiration of the PTFA to its reinstatement, some states enacted their own laws to offer protections that are similar to what the PTFA previously offered, whereas other states did not do so. These differences in state-level laws protecting tenants in foreclose properties before and after the expiration of the PTFA (and prior to its reinstatement) form the basis of our DID research design, which we will discuss in the identification strategy section.

---

evictions, and 73.7 percent responded that the PTFA has helped halt or otherwise avoid an eviction of at least one of their clients" (NLIHC, 2012, p. 9).

[7] According to the estimate provided by NLIHC (2012), nationwide at least 40 percent of foreclosed housing units were rentals, as of September 2012.

[8] See https://www.nhlp.org/wp-content/uploads/2018.08-Restoration-of-the-PTFA-1.pdf (accessed in July 2014) for details.

3. Empirical Framework

3.1. Data

Child File Data. Our main data source is the NCANDS Child File provided by the National Data Archive on Child Abuse and Neglect (NDACAN). Access to the Child File data is restricted. We obtained these data through the Restricted Data License[9] approved by the NDACAN.[10]

Our study uses the NCANDS Child File data that consist of administrative records collected by all 50 states, plus the District of Columbia. Each state collects case-level data, which are child-specific records for each report of alleged child maltreatment (e.g., abuse and neglect) that received a response from the state's child protective services (CPS) agency.[11] In each reporting year (also called submission year), which uses the federal fiscal year (i.e., October 1 of the year before through September 30 of the current year), states send their data to the NCANDS.[12] These data include completed reports that receive a disposition, that is, the final decision on whether the allegation is substantiated,[13] as a result of the CPS investigation

[9] See https://www.ndacan.acf.hhs.gov/datasets/request-restricted-data.cfm (accessed in May 2024) for more details.

[10] The NCANDS Child File data used by our study (FFY2010v7, FFY2011v7, FFY2012v7, FFY2013v7, FFY2014v6, FFY2015v6, FFY2016v5, FFY2017v5, FFY2018v6, FFY2019v6, FFY2020v4, FFY2021v3) were provided by the NDACAN and have been used with permission. The data were originally collected under the auspices of the Children's Bureau. Funding was provided by the Children's Bureau, Administration on Children, Youth and Families, Administration for Children and Families, U.S. Department of Health and Human Services. The collector of the original data, the funding agency, NDACAN, Duke University, Cornell University, and the agents or employees of these institutions bear no responsibility for the analyses or interpretations presented here. The information and opinions expressed reflect solely the opinions of the authors.

[11] Here, alleged maltreatment means suspected maltreatment (Children's Bureau, 2022, p. 111). In the NCANDS Child File data, such suspected cases are included in a referral to a CPS agency. The initial notification that a CPS agency receives is called a referral (Children's Bureau, 2022, p. 6). Referrals come from family members and community members, such as physicians, educators, and neighbors (Doyle and Aizer, 2018).

[12] For example, Child File data of reporting (a.k.a. submission) year X (i.e., federal fiscal year X) include data collected between October 1, year X-1 to September 30, year X.

[13] A substantiated maltreatment means that "the allegation of maltreatment or risk of maltreatment is supported or founded by state law or policy" (Children's Bureau, 2022, p. 17).

regarding whether the alleged maltreatment occurred.[14] These data eventually become the NCANDS Child File data of that reporting year, which have consistent coding structures and layouts, after these four processes are completed sequentially: state mapping, state data submission, NCANDS validation of data submission, and technical assistance review of data.[15]

Each record of the NCANDS Child File contains information for a child-report pair; that is, information regarding a child associated with a report of alleged maltreatment. Specifically, the following categories of information are provided by the Child File: (a) maltreatment types and maltreatment disposition levels; (b) child's demographic data and risk factors (i.e., the child's characteristics that may trigger a maltreatment); (c) caregiver risk factors (i.e., characteristics of the child's caregiver that may trigger a maltreatment); and (d) time, geographic identifiers and report source related to the alleged child maltreatment.

Maltreatment Types and Maltreatment Disposition Levels: We include all types of child maltreatment covered by the Child File: physical abuse, neglect or deprivation of necessities, medical neglect, sexual abuse, psychological or emotional maltreatment, and sex trafficking. We also have information on whether a child maltreatment is substantiated (an outcome of the CPS investigation).[16]

Child's Demographic Data and Risk Factors: We use information on the child's age (at the time of the report of alleged maltreatment) to limit our sample to children who are 17 years old or younger.[17] We also use information on the risk factors of

---

[14] In the Child File data we received, about 98 percent of the reports of alleged child maltreatments received dispositions within two years of being reported.

[15] https://www.ndacan.acf.hhs.gov/datasets/pdfs_user_guides/dataset279usersguide.pdf provides more details (accessed in August 2024).

[16] If several allegations are combined into one report, the report's disposition represents the most severe maltreatment disposition of all allegations within that report. Here, a maltreatment that has a disposition coded being substantiated is viewed as more severe than a maltreatment that has a disposition coded being unsubstantiated.

[17] We use the same age cutoff used by the World health Organization in the definition of child maltreatment: "Child maltreatment is the abuse and neglect that occurs to children under 18 years

the child and the caregiver to identify maltreatments that are related to clinically diagnosed conditions, such as intellectual disability and chronic emotional disorder.[18]

Caregiver Risk Factors: We use information on the caregiver risk factors to identify maltreatments that are related to inadequate housing or financial problems of the child's family.[19]

Time, Geographic Identifiers and Report Source Related to the Alleged Child Maltreatment: We have information on the calendar year and month in which the state's CPS agency received the report of the alleged child maltreatment.[20] In our main analysis we focus on cases reported between 2014 and 2015. Note that the Child File provides the state and county identifiers of the jurisdiction, not the child's residence, to which the report of alleged child maltreatment was assigned for a CPS response. In our study we use the county of a jurisdiction that received the report as a proxy for the county where the child maltreatment occurred. In the Child File

---

of age" (source: https://www.who.int/news-room/fact-sheets/detail/child-maltreatment, accessed in July 2024).

[18] Here are the definitions of these variables given by the codebook, which is available at https://www.ndacan.acf.hhs.gov/datasets/datasets-list-ncands-child-file.cfm (accessed in July 2024). For the child's intellectual disability: "A clinically diagnosed condition of reduced general cognitive and motor functioning existing concurrently with deficits in adaptive behavior and manifested during the developmental period that adversely affects socialization and learning." For the caregiver's intellectual disability: "A clinically diagnosed condition of significantly less than average intellectual functioning existing concurrently with deficits in adaptive behavior that adversely affect socialization and learning." For chronic emotional disorder of the child and the caregiver: "A clinically diagnosed condition exhibiting one or more of the following characteristics over a long period of time and to a marked degree: an inability to build or maintain satisfactory interpersonal relationships; inappropriate types of behavior or feelings under normal circumstances; a general pervasive mood of unhappiness or depression; or a tendency to develop physical symptoms or fears associated with personal problems. The term includes schizophrenia and autism."

[19] Here are the definitions of these variables given by the codebook, which is available at https://www.ndacan.acf.hhs.gov/datasets/datasets-list-ncands-child-file.cfm (accessed in July 2024). For inadequate housing: "Housing facilities that are substandard, overcrowded, unsafe or otherwise inadequate for the child to reside in, including homelessness." For financial problems of the child's family: "the family's inability to provide sufficient financial resources to meet minimum needs."

[20] If several allegations are combined into one report, the date of the report will use the date of the initial allegation.

data, only state identifiers, not any county identifiers, of the jurisdictions are released by the NCANDS for counties that have fewer than 700 child maltreatment cases during a federal fiscal year (for confidentiality protections).[21] Hereafter, we refer to these counties as masked counties. For any maltreatment case in which the child died, all geographic identifiers are masked by the NCANDS in the Child File data (for confidentiality protections). In addition, we use information provided by the Child File to create indicators regarding who made the reports: reported by professionals or reported by non-professionals. Using the county, year and month previously explained, we aggregate the Child File data to the county-year-month level, specifically taking the sum for each trigger (e.g., inadequate housing) of child maltreatment explained in (a), (b) and (c) above and for each county-year-month pair observed in the Child File data.[22]

SEER, ACS and Census Data. For our empirical analysis we merge the aggregate Child File data explained above with county-year-level data obtained from the following data sources: the Surveillance, Epidemiology, and End Results (SEER) program, the American Community Survey (ACS) 2010–2014 five-year county-level estimates, and the 2010 census.[23]

From SEER we obtain variables on proportions of individuals of different age groups and of different races, as well as indicators of different county population sizes. From ACS we obtain variables on educational attainment, family poverty rate, housing units with a mortgage, rental burden (i.e., rent divided by household

---

[21] For details, please see the section titled "State/County FIPS Code" of the NCANDS Child File Codebook (available at https://www.ndacan.acf.hhs.gov/datasets/pdfs_user_guides/ncands-child-file-codebook.pdf, accessed in September 2024).

[22] For the same maltreatment case (identified by the child-report pair in the Child File data) that appeared repeatedly in multiple Child Files (i.e., in multiple submission years), we keep only the case that appeared in the most recent submission year. This is the same procedure used by Evans et al. (2022), who also use the Child File data.

[23] For details and data access, see https://seer.cancer.gov/popdata/download.html (accessed in May 2024) and https://data.census.gov/table/ (accessed in August 2024).

income), and health insurance coverage.[24] From the 2010 census we obtain the variable on the percentage of county population living in rural areas.[25] Furthermore, we calculate the number of child maltreatment cases (alleged or substantiated) per 100,000 children aged 0–17 measured in 2010 from SEER for each trigger (e.g., inadequate housing) of the maltreatment explained in (a), (b) and (c) and for each observed county-year-month pair in the Child File data.

Masked Counties in the Child File Data. For those masked counties previously discussed, we use their state identifiers to group them into a whole "composite county" for each state. Regarding the county-level variables, obtained from the ACS five-year (2010–2014) estimates data, on educational attainment, family poverty rates, housing units with a mortgage, rental burden, and health insurance coverage, we use the weighted average value for a composite county, where the weighted average is taken over all counties constituting that composite county within a state[26] with the weight being the county population provided by ACS. Regarding the county-level demographic variables on age and racial groups obtained from SEER, as well as the variable on county population living in rural areas obtained from the 2010 census, we use the sum value for each of those

---

[24] SEER provides yearly data for each U.S. county. In contrast, ACS provides data for each U.S. county only through its five-year estimates data. The ACS data used by our study is the 2010–2014 five-year county-level estimates data. This five-year period overlaps the pre-treatment period of our study, which we will explain in the identification strategy section.

[25] See https://www2.census.gov/geo/docs/reference/ua/County_Rural_Lookup.xlsx for more details (accessed in May 2024).

[26] Note that in the Child File data, masked counties are not necessarily the same across different years. This is because counties that have fewer than 700 child maltreatment cases or child fatalities can change year by year, meaning that the makeup of a composite county within a state can change over time. Because of these different makeups, variables on a composite county's characteristics will vary over time. The conditional parallel trend assumption (which we will explain in the identification strategy section) required by the DID to estimate a causal effect only allows the use of time-invariant covariates. As a result, to ensure that the composite-county-level covariates are time-invariant, we fix the makeup of a composite county within a state by using the same set of masked counties. To do so, we use the Child File data in which the year of reported alleged child maltreatments is 2014 (the year that is in the pre-treatment period of DID analysis). We give detailed explanations in Appendix A.

population segments for a composite county, where the sum is based on the year 2010 and over all counties constituting that composite county within a state. We divide this value by the population of that composite county measured in 2010 to obtain the proportion of a specific population segment for that composite county.[27]

BRFSS Data. To explore potential mechanisms underlying the estimated effects on child maltreatment, we utilize BRFSS to obtain variables on respondents' mental health as well as their associated characteristics, including age, gender, race, educational attainment, marital status, and homeownership.[28]

In our study we consider all counties of each state, including those masked counties. However, as we previously discussed, for any maltreatment case resulting in the death of a child, no geographic identifiers (such as state identifiers) are released by the NCANDS. We omit these cases[29] in our empirical analysis because our identification strategy (explained in the next section) requires state identifiers used for defining the treatment and control groups. Omitting these cases also means that our study undercounts the actual number of child maltreatment cases. To the extent that the fatality occurs randomly between the treatment and control groups, those undercounts may be similar between the treatment and control groups, or the difference in those undercounts between the two groups may remain constant over time. If this is true, then the time-invariant difference will be removed by DID.

3.2. Identification Strategy

Our identification strategy uses a DID design. Specifically, we compare two groups before and after the expiration of the PTFA (and prior to its reinstatement). The PTFA expired on December 31, 2014, which we use as a cutoff point for the pre-treatment and post-treatment periods. The treatment (control) group is the one that

[27] We give detailed explanations in Appendix A.

[28] See https://www.cdc.gov/brfss/annual_data/annual_data.htm (accessed in July 2024) for details and data access.

[29] These cases account for only 0.05 percent of all child maltreatment incidents reported between 2011 and 2019.

lost (did not lose) the federal-level protection provided by the PTFA in the post-treatment period.

In our study the treatment group consists of states that provide tenants with either no specific protection or an eviction notice that gives tenants no more than 5 days, which is less than one week, before vacating a foreclosed property. The control group includes states that provide tenants with the right to an eviction notice that gives them at least 10 days before vacating a foreclosed property.[30] More detailed information is presented in Figure 1. Our treatment-control group designation is for the purpose of capturing a difference in housing instability resulting from the PTFA's expiration between the two groups. The treatment group is arguably exposed to greater housing instability than the control group. The treatment effect we estimate is not necessarily the effect of losing the PTFA alone, but rather a composite effect of losing the PTFA and meanwhile having some states adopt PTFA-like laws; that is, the effect of the treatment group losing the PTFA and the control group gaining PTFA-like laws. Furthermore, since the data we use for child maltreatment analysis do not have information on homeownership or foreclosure status, the treatment effect we estimate is an intent-to-treat effect.

In our main specification, we use January 2014–December 2014 and January 2015–December 2015 as the pre-treatment and post-treatment periods, hereafter referred to as the pre-period and post-period, respectively. We focus on this relatively short period, which is one year before and one year after the expiration of the PTFA for the following reasons. Our treatment-control group determination

[30] Specifically, the treatment group includes the following 30 states: Alabama, Alaska, Arizona, Arkansas, Colorado, Delaware, Florida, Georgia, Hawaii, Iowa, Kansas, Kentucky, Indiana, Louisiana, Maine, Michigan, Mississippi, New Hampshire, New Mexico, North Dakota, Ohio, Oklahoma, Pennsylvania, South Carolina, South Dakota, Tennessee, Utah, Virginia, Wisconsin, and Wyoming. The control group includes the following 17 states, plus the District of Columbia: California, Connecticut, Illinois, Maryland, Massachusetts, Minnesota, Missouri, Montana, New Jersey, New York, Nevada, North Carolina, Oregon, Rhode Island, Texas, West Virginia, and Washington (see https://nlihc.org/sites/default/files/FactSheet_PTFA_2015.pdf, accessed in June 2024, for more details).

relies on the June 2015 factsheet provided by the National Low Income Housing Coalition (NLIHC, 2015), which is based on data from the National Housing Law Project and data from the National Law Center on Homeless and Poverty (NLCHP, 2012, p. 11). To the best of our knowledge, this June 2015 factsheet provides the most relevant information about state laws protecting tenants in foreclosed properties at the time of the PTFA expiration.[31] By using a window of time that is relatively close to the expiration of the PTFA, we aim to reduce errors in defining the treatment and control groups because of the reliance on the June 2015 factsheet. Also note that in the June 2015 factsheet, there is no information regarding the treatment or control group determination for these three states: Idaho, Nebraska and Vermont. As a result, these three states are excluded from the samples used for our DID estimations.[32]

3.3. Econometric Specification

Our DID is applied to a two-group, multiple-period setting. There is no staggered treatment timing, since in our empirical setting the loss of federal-level protection for tenants living in foreclosed properties occurred at a single point in time, which is December 31, 2014. However, there can be treatment effect dynamics, meaning that treatment effects vary over time in the post-period. To accommodate this treatment effect heterogeneity in time, for which Roth et al. (2023) provides detailed discussion, we use the heterogeneity-robust DID estimator developed by Callaway and Sant'Anna (2021).[33] We refer to this estimator as the CS estimator,

[31] We spent significant effort on searching information related to state laws regarding protecting tenants at foreclose properties for periods that includes more years before and after the expiration of the PTFA. We used the Wayback Machine, which is the digital archive of the World Web, to conduct the search, given that it is often necessary to examine websites that display information in the past. We did not find any new information that adds to the June 2015 factsheet.

[32] In our robustness check, we added these three states back to the estimation sample, using information from NLCHP (2012) to approximate the treatment-control status of each of the three states. Our estimation results are similar whether or not we include these three states. We give detailed explanations in Appendix B.

[33] Roth et al. (2023) provides a comprehensive summary of recently developed heterogeneity-robust DID estimators. In our view, the estimator developed by Callaway and Sant'Anna (2021) provides

hereafter. When using the CS estimator, we use the version that has the double-robustness property, namely the augmented inverse probability weighting estimator (AIPW), explained by Callaway and Sant'Anna (2021) and Roth et al. (2023).[34]

Unlike any conventional DID estimator that is directly applied to a linear regression model, such as the conventional two-way fixed-effect (TWFE) model, in which a specific coefficient in that model is intended to represent a target parameter, the CS estimator is derived directly from the conditional PT assumption to estimate the average treatment effect on the treated (ATT). The estimand of this estimator, which is $ATT(g, t)$ that uses AIPW to achieve the double-robustness property, implements a transformation (Callaway and Sant'Anna, 2021, p. 228) of the outcome variable (y), the cohort indicator (G_g) where a cohort is defined by the time when a group is first treated (g), and covariates ($\boldsymbol{x}$), for each combination of cohort (g) and time (t).[35]

To identify $ATT(g, t)$, the CS estimator requires the no-anticipation assumption, meaning that $y_t(g) = y_t(0)$ in the pre-period (i.e., $t < g$), as well as the following conditional PT assumption:

$$E\left(y_t(0)|G_g = 1, \boldsymbol{x}\right) - E\left(y_{g-1}(0)|G_g = 1, \boldsymbol{x}\right) =$$

the most flexible and transparent way of incorporating covariates into the DID estimation that is directly derived from the conditional parallel trend assumption (which is needed for DID to identify the average treatment effect on the treated). Furthermore, the estimator developed by Callaway and Sant'Anna (2021) has the double-robustness property, and it requires the parallel trend assumption to hold only between $g - 1$ and t (where $t \geq g$), where g denotes the time when a group is first treated. By comparison, this estimator arguably invokes the least stringent version of the parallel trend assumption among the many recently developed heterogeneity-robust DID estimators that require the PT assumption to hold for all pre-treatment periods (i.e., between s and t, where $s \leq g - 1$ and $t \geq g$).

[34] In Appendix C we give more detailed explanations about the CS estimator. In our robustness checks, we use two alternative versions of the CS estimator that have only the single-robustness property, namely the regression adjustment (RA) estimator and the inverse probability weighting (IPW) estimator.

[35] In our empirical analysis about child maltreatment, the unit of observation is a county-year-month pair. Although there are repeated observations on a county over year and month, the resulting panel data are unbalanced. In this case, the CS estimator treats the data as repeated cross-sections.

$$E(y_t(0)|G_0 = 1, \boldsymbol{x}) - E\big(y_{g-1}(0)|G_0 = 1, \boldsymbol{x}\big) \text{ for all } t \geq g. \quad (1)$$

Here, t denotes a year-month pair, and g takes two values: 01/2015 (the month immediately after the PTFA's expiration on December 31, 2014) and zero, for the treatment and control groups, respectively; that is, $G_{01/2015} = 1$ for the treatment group, and $G_0 = 1$ for the control group. The baseline period used by the CS estimator is $g - 1$, which in our study is December 2014. In our child maltreatment analysis, we use the following set of variables, denoted by $\boldsymbol{x}$, measured in the pre-treatment period (with details provided in Table 1) and at the county level as the conditioning variables (i.e., covariates) for the conditional PT assumption described by equation (1): proportions of individuals of different age groups (ages 0–19 and 20–64) and of different racial groups (White and Black), indicators of different county population sizes, educational attainment, family poverty rate, housing units with a mortgage, rental burden (i.e., rent divided by household income), health insurance coverage, and the percentage of county population living in rural areas.[36] In Table 1 we list these outcome variables and covariates. The CS estimator computes cluster-robust standard errors with the clustering being at the level of treatment assignment, which in our case is the state level.[37]

[36] In our DID estimations using mental health as the outcome variable, the data are also repeated cross-sectional, so the estimations are based on the formula reported in Appendix C. Specifically, the BRFSS data contain variables measured on an annual basis and information on interview year and interview month. In those estimations we use the following variables as the covariates for the conditional PT assumption: age measured in 2014 (which is the time period immediately prior to the treatment start year), gender, race and ethnicity, educational attainment, and marital status, and standard errors are clustered at the state level. For subsample estimation by BRFSS respondent's age, the age is a time-varying variable measured at the time of the BRFSS interview.

[37] The CS estimator allows the use of weight. In our estimation regarding child maltreatment outcomes, we use the population of individuals aged 0–17 (measured in 2010 from SEER) in a county as the weight. In our estimation regarding mental health, we use the sampling weight provided by BRFSS as the weight.

4. Results

4.1. Main Results

When examining child maltreatment, we consider both alleged and substantiated cases for several reasons. First, focusing only on substantiated cases may lead to an under-estimation of the true prevalence of maltreatment. A substantiated case of maltreatment means that "the allegation of maltreatment or risk of maltreatment is supported or founded by state law or policy" (Children's Bureau, 2022, p. 17). However, there is always the possibility of errors in the determination process. Second, research suggests that children involved in any CPS investigation, whether substantiated or not, could be at increased risk for adverse outcomes, such as teen pregnancy (Kugler et al., 2019). Therefore, examining both alleged and substantiated cases could help us obtain a more comprehensive understanding of the extent of child maltreatment.

Treatment and Control Groups Comparison. Table 1 presents the summary statistics separately for the treatment and control groups and for the pre-treatment period (i.e., January 2014 through December 2014). The unconditional PT assumption does not require characteristics between the treatment and control groups in the pre-treatment period to be balanced (i.e., similar on average). Instead, it requires average differences in those characteristics between the two groups remain constant between the pre-treatment and post-treatment periods. In Table 1 we observe that the treatment group appears to have lower socioeconomic status (e.g., higher prevalence of family poverty), lower rental burden, and lower urbanicity. While these differences do not necessarily invalidate the PT assumption, they do challenge its plausibility. In our DID analysis, we rely on the conditional PT assumption explained in Section 3.3. We also compare the estimates obtained under unconditional and conditional PT assumptions, respectively. Results are reported in Figure 2 and Table 2.

DID Estimates for Child Maltreatment. Panels A and B of Figure 2 and Table 2 present findings on child maltreatment related to inadequate housing and financial problems of the child's family. Housing instability is generally characterized by (a) difficulties in paying rent, (b) spending a significant portion of income on housing, (c) living in substandard or overcrowded conditions, and (d) frequent relocations (Gu et al., 2023). The Child File data contain information on whether incidents of child maltreatment are related to inadequate housing or financial issues within the child's family, as discussed in Section 3.1. Our study utilizes this information to capture aspects (a), (b) and (c), but it does not allow us to directly account for (d) unless we assume inadequate housing triggers frequent moves. Also note that the financial problems of the child's family may extend beyond housing-related issues. Therefore, given the limitation of the Child File data, we also specifically examine child maltreatment related solely to inadequate housing, and the results are reported in Panels C and D of Figure 2 and in Table 2.

Table 2 presents the estimates of the overall ATT obtained by aggregating $ATT(g,t)$'s across time and cohorts.[38] Results in Table 2 suggest that incidents of child maltreatment due to housing instability could increase by as much as 18% in the treatment group in the year following the PTFA's expiration. This table also shows that the point estimates of the overall ATT are similar (between columns 1 and 2), whether or not covariates (discussed in Section 3.3) are included. The

[38] Specifically, this aggregation is a weighted average of $ATT(g,t)$'s, where $g = 01/2015$ and $02/2014 \leq t \leq 12/2015$), and the weight is determined by the sample size used for estimating $ATT(g,t)$. See Callaway and Sant'Anna (2021, section 3.2, p. 211) for more details about this aggregation into the overall ATT. Note that the aggregation starts from the second period of our sample, which is February 2014. Our sample starts from January 2014, but there is no $ATT(g,t)$ estimated for January 2014. This is because, as we explained in Section 3.3, the base period for $ATT(g,t)$ estimated for the pre-period (i.e., $t < g$) is $t-1$. The earliest month in which $ATT(g,t)$ can be estimated in the pre-period is February 2014. In contrast, $ATT(g,t)$ can be estimated for every month in the post-period. This is because the base period for the post-period (i.e., $t \geq g$) is December 2014 (i.e., $g-1$).

similarity in the point estimates suggests that the unconditional PT assumption might not be entirely implausible.[39]

Detecting Pre-Trends and Treatment Effect Dynamics. In Figure 2, the $ATT(g,t)$ estimates for the pre-period are used for detecting the presence of pre-trends between the treatment and control groups, while the post-period $ATT(g,t)$ estimates are used for assessing the treatment effect dynamics. Overall, we observe no pre-trends between the treatment and control groups in the pre-period when covariates are included, which suggests the validity of the conditional PT assumption. For most of the pre-period, we also find no pre-trends even without covariates, suggesting that the unconditional PT assumption is not entirely implausible. Furthermore, the point estimates are similar, whether or not covariates are included. All these findings add support to the plausibility of the unconditional PT assumption.[40]

Figure 2 also indicates a persistent treatment effect. This persistence could be attributed to the expiration of the PTFA, which, although a one-time event, could trigger a cycle of housing instability. While the PTFA does not directly improve housing quality, it provides crucial protection by preventing immediate eviction, allowing tenants to stay in their homes until the end of their leases. Without the PTFA, tenants become vulnerable to immediate eviction by purchasers of foreclosed properties, such as banks or new homeowners. Forced evictions that do

---

[39] Note that without using any covariates, estimation of the $ATT(g,t)$ by the CS estimator is simplified to the standard two-group two-period differencing of two differences. If the unconditional PT assumption is true, we would expect the inclusion of covariates in the CS estimator not to change the ATT point estimates substantially but to yield efficiency gains (i.e., smaller standard errors). Results in Table 2 are consistent with this expectation.

[40] If the unconditional PT assumption is true, incorporating covariates (under the conditional PT assumption) will enhance the efficiency of estimating ATT. (This is analogous to clinical trials, where the effect of a randomly assigned treatment is estimated with improved precision when covariates are included.) Although this efficiency gain may not be evident in each estimated $ATT(g,t)$ and appears more pronounced in the later months of the post-period, it is eventually realized in the estimation of the overall ATT, as demonstrated in Table 2.

not allow tenants sufficient time to prepare for relocation can compel them to move hastily into substandard housing. Homes that are substandard may also be mismanaged by property owners. When owners neglect property maintenance, they may also fail in meeting financial obligations such as mortgage payments, thus increasing the risk of foreclosure and further exacerbating the cycle of housing instability.

Caveats and Limitations. Regarding the treatment effect dynamics indicated by the estimated $ATT(g,t)$'s in the post-period (in each panel of Figure 2), as we explained in the identification strategy section, the estimated treatment effect in the post-period reflects not necessarily the impact of losing the PTFA alone but rather a combined effect of its expiration and the simultaneous adoption of PTFA-like laws in some states. Ideally, we would separate these two effects, but we are restricted by the conditional PT assumption. This assumption, while weaker than the assumption of conditional randomization of the treatment, has a trade-off: it does not allow us to disentangle the treatment effect dynamics in the post-period from the effects of time-varying variables affected by the treatment in the treated group. Under the conditional PT assumption, adding time-varying variables to the CS estimator as the conditioning variables, such as a variable representing a state policy change in response to the PTFA's expiration, in order to isolate the treatment effect dynamics is not a viable solution. As Roth et al. (2023) points out, a covariate used as the conditioning variable of the PT assumption cannot be the one that is affected by the treatment and "conditioning on it induces a 'bad control' problem that can induce bias" (p. 2232).

4.2. Robustness Checks

In this section we briefly discuss the robustness checks we conducted. Detailed results and discussions are provided in the appendices.

Results Remaining Similar When Using Alternative Samples, Estimators and Inference Procedures. In Appendix B we explained how we added these three states—Idaho, Nebraska and Vermont—back to the estimation sample, using information from NLCHP (2012) to approximate the treatment-control status of each of the three states. In Appendix C we reported the results obtained from two alternative versions of the CS estimator, which use RA and IPW, respectively.[41] In Appendix D we reported the simultaneous confidence intervals (a.k.a. uniform confidence bands) for ATTs estimated for each month from the pre-period through the post-period.

No Pre-Trends in the Extended Pre-Period and Presence of Treatment Effect Dynamics in the Extended Post-Period. Results are reported in Appendix E, with the extended pre-period being from January 2011 to December 2014, and the extended post-period that being from January 2015 to December 2019.[42]

Results Not Disproportionately Influenced by Any Single State. We conducted a series of "leave-one-out" estimations. Results are reported in Appendix F.

Results Remaining the Same When Using Only Counties Identified in the Child File Data. Results are reported in Appendix G. This robustness check confirmed that our estimates reported in the main results section were not affected by the way we constructed the composite counties and determined the numeric values of their characteristics (explained in Appendix A).

---

[41] The RA and IPW estimation results are reported in Appendix C, and they are similar to the AIPW estimation results. Note that without using any covariates, estimations of $ATT(g,t)$ by AIPW, RA and IPW are all simplified to the standard two-group two-period differencing of two differences. Thus, differences in the estimates obtained by AIPW, RA and IPW come from the different ways in which AIPW, RA and IPW incorporate covariates. If the unconditional PT assumption is true, we would expect that covariates play little role in the ATT estimation, which implies that estimates obtained by AIPW, RA and IPW that incorporate covariates should be similar. This is exactly what we found, and this finding lends support to the unconditional PT assumption, although it does not conclusively prove it.

[42] We ended the post-period prior to the year of 2020, the year of the COVID-19 outbreak. Regarding the persistence of the treatment effect over the extended post-period, we find suggestive evidence of a decline in the effect, which could be attributed to the reinstatement of the PTFA in 2018.

Results Remaining Similar When Using Alternative Definitions of the Treatment and Control Groups. We explained the alternative definitions and reported the results in Appendix H.

Results Remaining Similar When Using a Dynamic TWFE Model. In Appendix I, we discussed our estimation results using a dynamic TWFE model, which is often referred to as the (conventional) event-study model (Roth et al., 2023).[43] The similarity in the results between the event-study estimates and the CS estimates, to some extent, mitigates concerns that the treatment effect dynamics indicated by the CS estimates could be driven entirely by post-treatment responses, such as state housing policies that affect eviction filings (Bradford and Bradford, forthcoming).

Validation Checks: Reporting Source. We considered the possibility that families experiencing housing instability have increased interactions with public agencies and professionals who are mandated reporters of suspected child maltreatment. If this is true, then the heightened visibility may lead to increased likelihood of maltreatment being officially reported. Furthermore, professionals can be better trained to recognize signs of child maltreatment, which could result in more accurate reporting. Consequently, we would expect the effect of housing instability on child maltreatment to be more pronounced in reports done by professionals compared with reports done by non-professionals. We find support for this hypothesis, and the results are reported in Appendix J.

[43] In our empirical setting, where there is no staggered treatment timing, the dynamic TWFE model, estimated by the ordinary least squares (OLS), is adequate for capturing treatment effect heterogeneity solely in time since treatment (Roth et al., 2023). In this regard, the reason for us to choose the CS estimator instead of the OLS is the double-robustness property provided by the AIPW version of the CS estimator. However, as we previously explained, this double-robustness comes at a cost: it does not allow us to separately estimate the treatment effect dynamics and the effects of time-varying covariates. In contrast, the conventional event-study model allows us to do so, as long as those covariates are correctly specified in the model. However, this advantage of the event-study model requires the assumption that the treatment is exogenous, conditional on all covariates (both time-invariant and time-varying) used in the model—a stronger assumption than the conditional PT assumption.

Validation Checks: Counties with High vs. Low Delinquency Rates. Since losing the PTFA protection directly affects tenants in foreclosed properties, we expect its impact to be more salient in areas that have high foreclosure rates measured prior to the PTFA's expiration. However, we are unable to access foreclosure data. Instead, we use delinquency as a leading indicator of foreclosure. The findings of this analysis are reported in Appendix K, and they align with our expectation.

Validation Checks: Pro-Renter vs. Pro-Business States. We examined the possibility that the impact of the PTFA's expiration on child maltreatment varies based on a state's stance toward business (or landlord) and tenant protections. In pro-business (i.e., pro-landlord) states, where economic priorities may lead to weaker tenant protections and a greater emphasis on property rights, the loss of PTFA protection could result in increased housing instability for vulnerable families. This increased instability could exacerbate the stress and challenges faced by these families, potentially leading to a higher incidence of child maltreatment. In contrast, pro-renter states typically have stronger tenant protections and more comprehensive support systems, which could mitigate the impact of forced displacement, such as foreclosure-related evictions. Therefore, we would expect the impact of the PTFA's expiration on child maltreatment to be more pronounced in pro-business states than in pro-renter states. We find support for this hypothesis, and the results are reported in Appendix L.

4.3. Potential Mechanism

As discussed in Section 2.1, multiple factors can contribute to child maltreatment. Among these, poor parental mental health may serve as a link between the expiration of the PTFA and the increased risk of child maltreatment. Mental health issues, exacerbated by increased housing instability following the loss of PTFA protection, may impair parents' ability to provide a stable and nurturing environment for their children, therefore increasing the risk of maltreatment. To investigate this potential mechanism, we use individual-level data from BRFSS and the CS estimator to estimate the ATT on respondents' mental health, incorporating the covariates listed in Table 1.

Table 3 shows that there is a significant increase of approximately 15% in the likelihood of having mental health problems in the past 30 days[44] among renters (Panel A), but not among homeowners (Panel B), in the treatment group during the PTFA gap period. Similar results were obtained from using two age groups (columns 1 and 2). The difference in the estimation results between renters and homeowners is expected and reasonable, given that the PTFA specifically targets tenants, not homeowners, and therefore, its expiration should mostly affect renters. In this sense, the estimation results for homeowners serve as a falsification check.

Figure 3 presents the ATTs estimated monthly from the pre-period to the post-period. The overall pattern aligns with the findings in Table 3. For most of the pre-period, we find no statistically significant pre-trends between the treatment and control groups, suggesting the plausibility of the conditional PT assumption. In addition, there appears to be no evidence suggesting an anticipation effect, where renters might have experienced increased mental health problems in expectation of the PTFA's expiration. If such an effect existed, we would expect to see an increase in mental health issues among renters. However, as shown in Figure 3 (Panel A), the results indicate a decrease in the likelihood of having mental health problems among renters interviewed by BRFSS around October and December 2014; this finding does not support the presence of the anticipation effect discussed above. Furthermore, the impact of losing the PTFA protection on renters' mental health appears to be persistent, which is consistent with the potential cycle of housing instability triggered by the expiration of the PTFA we previously discussed.

[44] Here, having mental health problems is defined as experiencing at least one day of such problems in the past 30 days. In Appendix M we show that our results remain qualitatively the same across different cutoff values ranging from one day to 30 days.

If mental health mediates the relationship between housing instability and child maltreatment, and housing instability affects mental health through universal responses to adversity, we would expect the mental health-mediated effect on child maltreatment to be present in various racial and gender groups. Figure 4 supports this, showing similar effects across genders (of the child and of the perpetrator). In the Child File data, about 80 percent of perpetrators are the victims' parents, but perpetrator information is only available for substantiated cases. For alleged maltreatments, we infer the parent's race from the child's race. Under this assumption, Figure 4 suggests that the effects could be stronger for White parents. This aligns with Al-Amin et al.'s (2024) study, which finds that White adults experience greater psychological distress from housing instability than Black adults.

4.4. Falsification Checks

To assess whether the previously discussed ATT estimates are indeed driven by the PTFA change and not by other confounding factors, we conducted the following falsification checks. Specifically, these checks focus on maltreatments related to inherent risk factors of the child and the caregiver, such as intellectual disability or chronic emotional disorder—outcomes that are unlikely to be influenced by the loss of federal protection of tenants living in foreclosed properties. We use these outcomes as the dependent variables, repeating the estimations previously done in Table 2 and Figure 2. The results of these falsification checks are reported in Table 4 (for the overall ATT estimates) and Figure 5 (for the monthly ATT estimates from the pre-period to the post-period). The findings from both Table 4 and Figure 5 confirm null results, indicating no effect of the PTFA's expiration on the child's or the caregiver's intellectual disability or chronic emotional disorders, as expected. These null results provide supporting, although not conclusive, evidence that the previously estimated ATTs were driven by the PTFA change rather than other confounding factors.[45]

5. Conclusion

In this study we use the discontinuity of the PTFA during a specific period, which generated a lapse in federal protection for tenants living in foreclosed properties, to examine the causal link between housing instability and child maltreatment. To the best of our knowledge, we provide the first study that utilizes such a change in the PTFA to explore this causal relationship. Our findings indicate that incidents of child maltreatment driven by housing instability could increase by as much as 18% in states that lost the PTFA protection compared with states that maintained similar protections during the PTFA gap period. Furthermore, our analysis of mental health using data from BRFSS suggests a potential mechanism underlying this link: the expiration of the PTFA appears to induce an increase in mental health issues among renters in states that lost the PTFA protection. Poor parental mental health, exacerbated by housing instability, may impair caregivers' ability to provide a stable and nurturing environment, thereby increasing the risk of child maltreatment.

While our study specifically examines the impact of housing instability triggered by the uncertainty faced by tenants in foreclosed properties, its implications extend beyond this context. Forced displacement, such as foreclosure-related evictions, can initiate a vicious cycle of housing instability. For low-income renters, a forced move can lead to accepting substandard housing, often resulting in another relocation shortly thereafter (Desmond et al., 2015). In addition, forced displacement often compels renters to relocate to lower-quality housing and neighborhoods, placing them in a precarious housing situation (Desmond and Shollenberger, 2015; Evans, 2021). This ongoing cycle of instability further

[45] Additionally, we find no direct evidence of children moving between the treatment and control groups throughout the sample period of our study. Results are reported in Appendix N.

exacerbates the risks to child well-being, highlighting the critical need for protective measures like the PTFA. The reinstatement of the PTFA in 2018 as a permanent federal law is a clear recognition of the need for continuous and long-term protection for tenants.

Today, more people in the U.S. are renting their homes than at any time since at least 1965.[46] In 2024, 34 percent of Americans were renters.[47] Among these renters, a disproportionate number are individuals of low socioeconomic status, for whom the stability of rental housing is particularly important. The unique period of PTFA discontinuity that we examined provides valuable insights into the critical role of federal-level tenant protections, especially for vulnerable populations. The findings from this period highlight the broader implications of housing policy and emphasize the critical need for continuous protections to prevent further exacerbation of existing social inequities.

In light of ongoing housing market pressures, such as affordability crises,[48] our study emphasizes the need to incorporate housing stability into child protection and welfare policies. The reinstatement and permanence of the PTFA should be seen as a critical component of a broader strategy to address housing-related vulnerabilities. Continued research and policy initiatives that tackle the social determinants of child maltreatment, such as housing instability, will be essential in fostering safe and nurturing environments for all children. Our study underscores the importance of considering macro-level factors and implementing comprehensive polices to effectively prevent child maltreatment and improve child well-being.

---

[46] For more details, see https://www.pewresearch.org/short-reads/2017/07/19/more-u-s-households-are-renting-than-at-any-point-in-50-years (accessed in August 2014).

[47] For more details, see https://www.thezebra.com/resources/research/renting-statistics (accessed in August 2024).

[48] For example, according to data from the U.S. Census Bureau, during the 2017–2021 period, more than 40 percent (i.e., 19 million) of renters in the country spent more than 30 percent of their income on housing costs, which is considered as housing cost burdened. More details are provided at https://www.census.gov/newsroom/press-releases/2022/renters-burdened-by-housing-costs.html (accessed in August 2024).

References

Al-Amin, N. S., A. McBryde-Redzovic, M. Gutierrez-Kapheim and U. A. Mitchell (2024). "COVID-Related Stressors and Psychological Distress Among Chicago Residents: the Moderating Role of Race." *Journal of Racial and Ethnic Health Disparities* 11 (2): 598–610.

Ali, M. M., T. Lu, J. C. Maclean and A. Meinhofer (2024). "Mental Health, Substance Use, and Child Maltreatment." National Bureau of Economic Research Working Paper No. 32895.

Aslim, E. G., W. Fu and E. Tekin (2024). "Proximity to Abortion Services and Child Maltreatment." National Bureau of Economic Research Working Paper No. 32771.

Austin, A. E., A. M. Lesak and M. E. Shanahan (2020). "Risk and Protective Factors for Child Maltreatment: A Review." *Current Epidemiology Reports* 7 (4): 334–342.

Austin, A. E., M. E. Shanahan, M. Frank, R. B. Naumann, H. L. M. Reyes, G. Corbie and A. S. Ammerman (2023). "Association of State Expansion of Supplemental Nutrition Assistance Program Eligibility with Rates of Child Protective Services-Investigated Reports." *JAMA Pediatrics* 177 (3): 294–302.

Berger, L. M., S. A. Font, K. S. Slack and J. Waldfogel (2017). "Income and Child Maltreatment in Unmarried Families: Evidence from the Earned Income Tax Credit." *Review of Economics of the Household* 15 (4): 1345–1372.

Bitler, M. P. and M. Zavodny (2002). "Child Abuse and Abortion Availability." *American Economic Review Papers and Proceedings* 92 (2): 363–367.

— and — (2004). "Child Maltreatment, Abortion Availability, and Economic Conditions." *Review of Economics of the Household* 2 (2): 119–141.

Bradford, A. C. and W. D. Bradford (forthcoming). "The Effect of State Housing Policies on Eviction Filings and Judgments in the United States, 2001–2018." *Housing Policy Debate*: 1–21.

Bullinger, L. R. and A. Boy (2023). "Association of Expanded Child Tax Credit Payments with Child Abuse and Neglect Emergency Department Visits." *JAMA Network Open* 6 (2): e2255639.

Bullinger, L. R., J. M. Fleckman and K. Fong (2021). "Proximity to SNAP-Authorized Retailers and Child Maltreatment Reports." *Economics & Human Biology* 42: 101015.

Callaway, B. and P. H. C. Sant'Anna (2021). "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics* 225 (2): 200–230.

Chandler, C. E., A. E. Austin and M. E. Shanahan (2022). "Association of Housing Stress with Child Maltreatment: A Systematic Review." *Trauma, Violence, & Abuse* 23 (2): 639–659.

Chemtob, C. M., O. G. Gudiño and D. Laraque (2013). "Maternal Posttraumatic Stress Disorder and Depression in Pediatric Primary Care: Association with Child Maltreatment and Frequency of Child Exposure to Traumatic Events." *JAMA Pediatrics* 167 (11): 1011–1018.

Children's Bureau (2022). "Child Maltreatment 2022." U.S. Department of Health & Human Services. Retrieved from https://www.acf.hhs.gov/cb/report/child-maltreatment-2022 in July 2024.

Currie, J. and E. Tekin (2012). "Understanding the Cycle: Childhood Maltreatment and Future Crime." *Journal of Human Resources* 47 (2): 509–549.

Currie, J. and C. S. Widom (2010). "Long-Term Consequences of Child Abuse and Neglect on Adult Economic Well-Being." *Child Maltreatment* 15 (2): 111–120.

Desmond, M., C. Gershenson and B. Kiviat (2015). "Forced Relocation and Residential Instability among Urban Renters." *Social Service Review* 89 (2): 227–262.

Desmond, M. and T. Shollenberger (2015). "Forced Displacement from Rental Housing: Prevalence and Neighborhood Consequences." *Demography* 52 (5): 1751–1772.

Doyle, J. J. and A. Aizer (2018). "Economics of Child Protection: Maltreatment, Foster Care, and Intimate Partner Violence." *Annual Review of Economics* 10: 87–108.

Erten, B. and P. Keskin (2020). "Breaking the Cycle? Education and the Intergenerational Transmission of Violence." *The Review of Economics and Statistics* 102 (2): 252–268.

Evans, M. (2021). "The Unequal Housing and Neighborhood Outcomes of Displaced Movers." *Journal of Urban Affairs* 43 (9): 1214–1234.

Evans, M. F., L. Gazze and J. Schaller (2023). "Temperature and Maltreatment of Young Children." National Bureau of Economic Research Working Paper No. 31522.

Evans, M. F., M. C. Harris and L. M. Kessler (2022). "The Hazards of Unwinding the Prescription Opioid Epidemic: Implications for Child Maltreatment." *American Economic Journal: Economic Policy* 14 (4): 192–231.

Fang, X., D. S. Brown, C. S. Florence and J. A. Mercy (2012). "The Economic Burden of Child Maltreatment in the United States and Implications for Prevention." *Child Abuse & Neglect* 36 (2): 156–165.

Frioux, S., J. N. Wood, O. Fakeye, X. Luan, R. Localio and D. M. Rubin (2014). "Longitudinal Association of County-Level Economic Indicators and Child Maltreatment Incidents." *Maternal and Child Health Journal* 18 (9): 2202–2208.

Ginther, D. K. and M. Johnson-Motoyama (2022). "Associations between State TANF Policies, Child Protective Services Involvement, and Foster Care Placement." *Health Affairs* 41 (12): 1744–1753.

Gu, K. D., K. C. Faulkner and A. N. Thorndike (2023). "Housing Instability and Cardiometabolic Health in the United States: A Narrative Review of the Literature." *BMC Public Health* 23: 931.

Higgins, D. J. and G. R. Hunt (2024). "Child, Parent and Contextual Factors Associated with Child Protection System Involvement and Child Maltreatment in the Family: A Rapid Evidence Review." *Australian Journal of Social Issues* 59 (2): 358–400.

Hunter, A. A. and G. Flores (2021). "Social Determinants of Health and Child Maltreatment: A Systematic Review." *Pediatric Research* 89: 269–274.

Jaudes, P. K. and L. Mackey-Bilaver (2008). "Do Chronic Conditions Increase Young Children's Risk of Being Maltreated?" *Child Abuse & Neglect* 32 (7): 671–681.

Johnson-Motoyama, M., D. K. Ginther, P. Oslund, L. Jorgenson, Y. Chung, R. Phillips, O. W. J. Beer, S. Davis and P. L. Sattler (2022). "Association between State Supplemental Nutrition Assistance Program Policies, Child Protective Services Involvement, and Foster Care in the US, 2004–2016." *JAMA Network Open* 5 (7): e2221509.

Kepple, N. J. (2017). "The Complex Nature of Parental Substance Use: Examining Past Year and Prior Use Behaviors as Correlates of Child Maltreatment Frequency." *Substance Use & Misuse* 52 (6): 811–821.

Klevens, J. and K. A. Ports (2017). "Gender Inequity Associated with Increased Child Physical Abuse and Neglect: A Cross-Country Analysis of Population-Based Surveys and Country-Level Statistics." *Journal of Family Violence* 32 (8): 799–806.

Kugler, K. C., K. Guastaferro, C. E. Shenk, S. J. Beal, K. M. Zadzora and J. G. Noll (2019). "The Effect of Substantiated and Unsubstantiated Investigations of Child Maltreatment and Subsequent Adolescent Health." *Child Abuse & Neglect* 87: 112–119.

Lebrun-Harris, L. A., M. Sandel, R. Sheward, A. Poblacion and S. Ettinger de Cuba (2024). "Prevalence and Correlates of Unstable Housing among US Children." *JAMA Pediatrics* 178 (7): 707–717.

Lee, S. J., C. A. Taylor and J. L. Bellamy (2012). "Paternal Depression and Risk for Child Neglect in Father-Involved Families of Young Children." *Child Abuse & Neglect* 36 (5): 461–469.

Leeb, R. T., T. Lewis and A. J. Zolotor (2011). "A Review of Physical and Mental Health Consequences of Child Abuse and Neglect and Implications for Practice." *American Journal of Lifestyle Medicine* 5 (5): 454–468.

Lindo, J. M., J. Schaller and B. Hansen (2018). "Caution! Men Not at Work: Gender-Specific Labor Market Conditions and Child Maltreatment." *Journal of Public Economics* 163: 77-98.

Maguire-Jack, K. and S. A. Font (2017). "Community and Individual Risk Factors for Physical Child Abuse and Child Neglect: Variations by Poverty Status." *Child Maltreatment* 22 (3): 215–226.

Malcolm, M. (2012). "Can Buy Me Love: The Effect of Child Welfare Expenditures on Maltreatment Outcomes." *Applied Economics* 44 (28): 3725–3736.

Marcal, K. E. (2018). "The Impact of Housing Instability on Child Maltreatment: A Causal Investigation." *Journal of Family Social Work* 21 (4-5): 331–347.

Markowitz, S. and M. Grossman (2000). "The Effects of Beer Taxes on Physical Child Abuse." *Journal of Health Economics* 19 (2): 271–282.

McLaughlin, M. (2018). "The Relationship between Cigarette Taxes and Child Maltreatment." *Child Abuse & Neglect* 79: 339–349.

Mersky, J. P., L. M. Berger, A. J. Reynolds and A. N. Gromoske (2009). "Risk Factors for Child and Adolescent Maltreatment: A Longitudinal Investigation of a Cohort of Inner-City Youth." *Child Maltreatment* 14 (1): 73–88.

Morris, M. C., M. Marco, K. Maguire-Jack, C. D. Kouros, B. Bailey, E. Ruiz and W. Im (2019). "Connecting Child Maltreatment Risk with Crime and Neighborhood Disadvantage across Time and Place: A Bayesian Spatiotemporal Analysis." *Child Maltreatment* 24 (2): 181–192.

National Law Center on Homelessness & Poverty (NLCHP) (2012). "Eviction (Without) Notice: Renters and the Foreclosure Crisis." Retrieved from https://homelesslaw.org/wp-content/uploads/2018/10/Eviction_Without_Notice.pdf in July 2024.

National Low Income Housing Coalition (NLIHC) (2012). "Renters in Foreclosure: A Fresh Look at an Ongoing Problem." Retrieved from https://nlihc.org/sites/default/files/rentersinforeclosure2012.pdf in July 2024.

National Low Income Housing Coalition (NLIHC) (2015). "Protecting Tenants at Foreclosure Act." Retrieved from https://nlihc.org/sites/default/files/FactSheet_PTFA_2015.pdf in July 2024.

Office of the Comptroller of the Currency (OCC) (2020). "Protecting Tenants at Foreclosure Act" (Version 1.0, March 2020). Retrieved from https://www.occ.treas.gov/publications-and-resources/publications/comptrollers-handbook/files/protecting-tenants-foreclosure/pub-ch-ptfa.pdf in July 2024.

Paxson, C. and J. Waldfogel (1999). "Parental Resources and Child Abuse and Neglect." *American Economic Review Papers and Proceedings* 89 (2): 239–244.

— and — (2002). "Work, Welfare, and Child Maltreatment." *Journal of Labor Economics* 20 (3): 435–474.

— and — (2003). "Welfare Reforms, Family Resources, and Child Maltreatment." *Journal of Policy Analysis and Management* 22 (1): 85–113.

Raissian, K. M. and L. R. Bullinger (2017). "Money Matters: Does the Minimum Wage Affect Child Maltreatment Rates?" *Children and Youth Services Review* 72: 60–70.

Roth, J., P. H. C. Sant'Anna, A. Bilinski and J. Poe (2023). "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature." *Journal of Econometrics* 235 (2): 2218–2244.

Sandner, M., S. L. Thomsen and L. González (forthcoming). "Preventing Child Maltreatment: Beneficial Side Effects of Public Childcare." *The Economic Journal*.

Schneider, W. (2016). "Relationship Transitions and the Risk for Child Maltreatment." *Demography* 53 (6): 1771–1800.

Schneider, W., L. R. Bullinger and K. M. Raissian (2022). "How Does the Minimum Wage Affect Child Maltreatment and Parenting Behaviors? An Analysis of the Mechanisms." *Review of Economics of the Household* 20 (4): 1119–1154.

Schneider, W., J. Waldfogel and J. Brooks-Gunn (2017). "The Great Recession and Risk for Child Abuse and Neglect." *Children and Youth Services Review* 72: 71–81.

Seiglie, C. (2004). “Understanding Child Outcomes: An Application to Child Abuse and Neglect.” *Review of Economics of the Household* 2 (2): 143–160.

Sen, B. (2007). “State Abortion Restrictions and Child Fatal-Injury: An Exploratory Study.” *Southern Economic Journal* 73 (3): 553–574.

Slack, K. S., J. L. Holl, B. J. Lee, M. McDaniel, L. Altenbernd and A. B. Stevens (2003). “Child Protective Intervention in the Context of Welfare Reform: The Effects of Work and Welfare on Maltreatment Reports.” *Journal of Policy Analysis and Management* 22 (4): 517–536.

Strathearn, L., M. Giannotti, R. Mills, S. Kisely, J. Najman and A. Abajobir (2020). “Long-Term Cognitive, Psychological, and Health Outcomes Associated with Child Abuse and Neglect.” *Pediatrics* 146(4): e20200438.

Taylor, C. A., N. B. Guterman, S. J. Lee and P. J. Rathouz (2009). “Intimate Partner Violence, Maternal Stress, Nativity, and Risk for Maternal Maltreatment of Young Children.” *American Journal of Public Health* 99 (1): 175–183.

Vachon, D. D., R. F. Krueger, F. A. Rogosch and D. Cicchetti (2015). “Assessment of the Harmful Psychiatric and Behavioral Effects of Different Forms of Child Maltreatment.” *JAMA Psychiatry* 72 (11): 1135–1142.

Van Horne, B. S., M. O. Caughy, M. Canfield, A. P. Case, C. S. Greeley, R. Morgan and L. E. Mitchell (2018). “First-Time Maltreatment in Children Ages 2–10 with and without Specific Birth Defects: A Population-Based Study.” *Child Abuse & Neglect* 84: 53–63.

Van Horne, B. S., K. B. Moffitt, M. A. Canfield, A. P. Case, C. S. Greeley, R. Morgan and L. E. Mitchell (2015). “Maltreatment of Children under Age 2 with Specific Birth Defects: A Population-Based Study.” *Pediatrics* 136 (6): e1504–e1512.

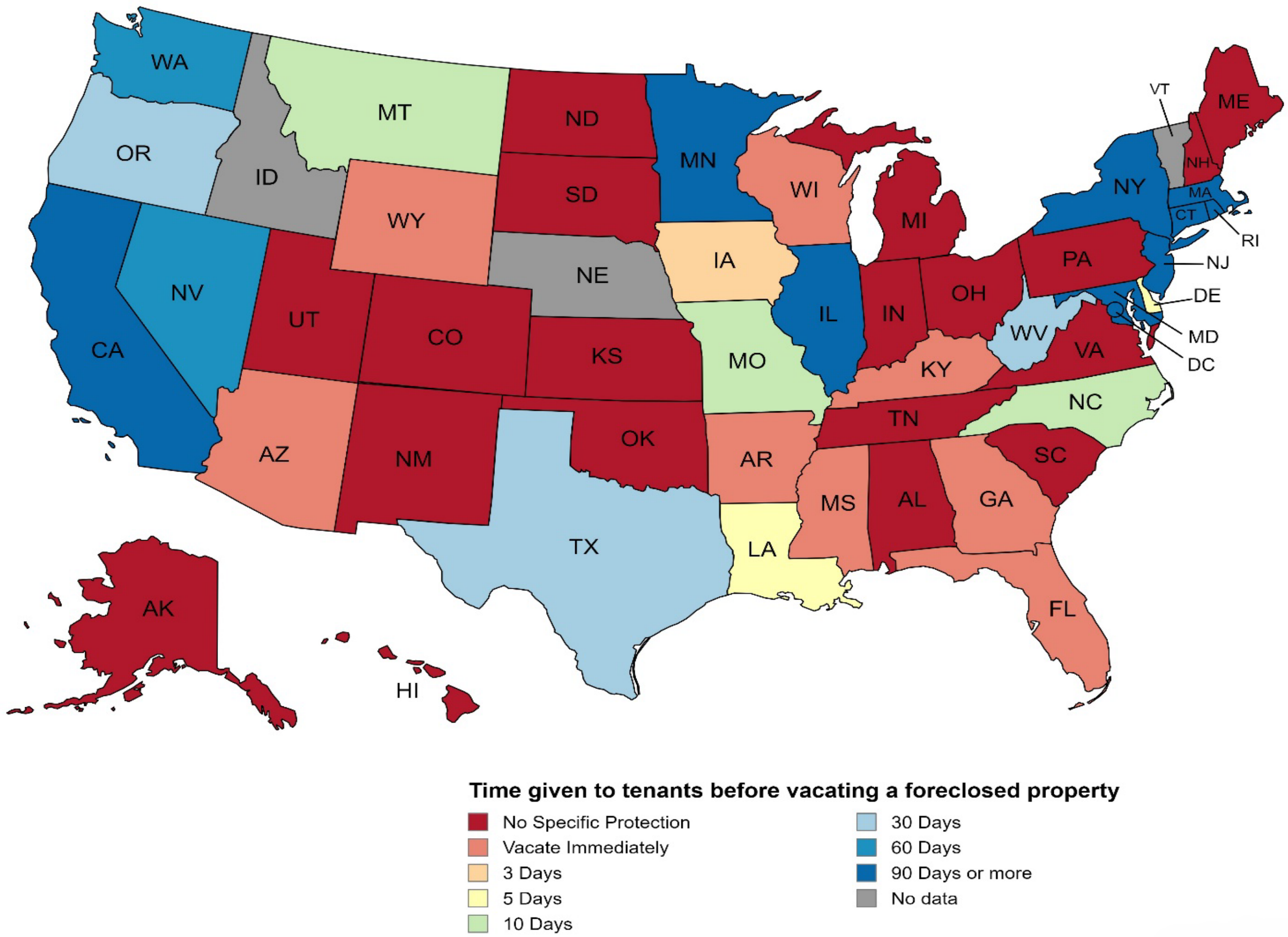


Figure 1: State-Level Protections for Tenants at Foreclosure

Notes: The map was created based on information from the National Housing Law Project and the National Law Center on Homelessness and Poverty (https://nlihc.org/sites/default/files/FactSheet_PTFA_2015.pdf, accessed in June 2024). The map was created with mapchart.net.

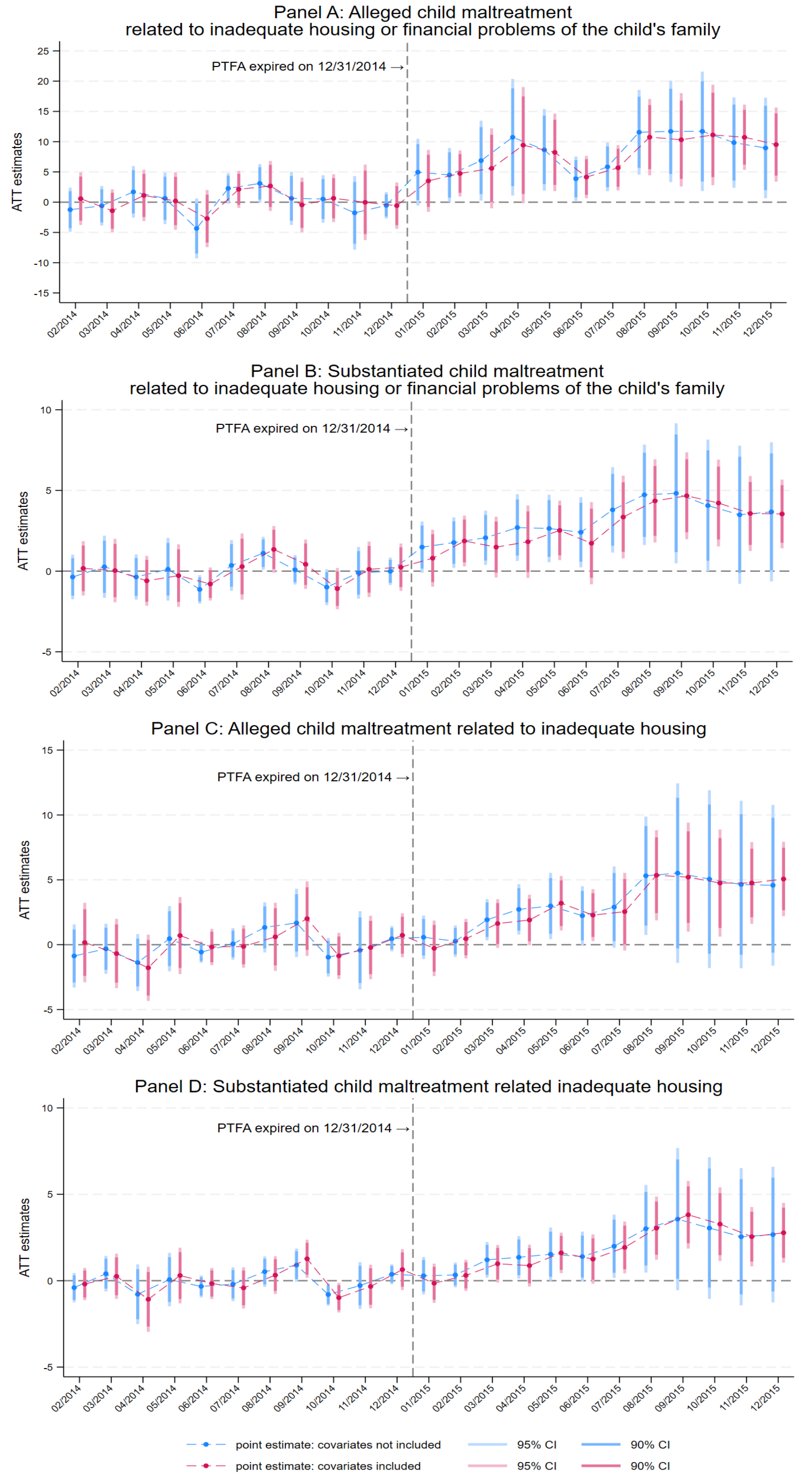


Figure 2: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family

Notes: The sample period is January 2014 through December 2015. All estimations use the augmented inverse probability weighting (AIPW) version of the Callaway and Sant'Anna (2021) difference-in-differences (DID) estimator. Covariates used in the estimations are: proportions of individuals aged 0–19 (measured in 2010 from SEER) in a county, proportions of individuals aged 20–64 (measured in 2010 from SEER) in a county, proportions of individuals whose race is white (measured in 2010 from SEER) in a county, proportions of individuals whose race is black (measured in 2010 from SEER) in a county, proportions of individuals who completed at least some college (2010–2014 ACS five-year estimates) in a county, proportions of families below poverty level (2010–2014 ACS five-year estimates) in a county, proportions of owner-occupied housing units with a mortgage (2010–2014 ACS five-year estimates) in a county, proportions of occupied units paying rent with GRAPI (gross rent as a percentage of household income) ≥ 25% (2010–2014 ACS five-year estimates) in a county, proportions of individuals (of the civilian noninstitutionalized population) without health insurance coverage (2010–2014 ACS five-year estimates) in a county, proportions of county population living in rural areas as of the 2010 census, an indicator (1/0) for county population (measured in 2010 from SEER) < 25,000, an indicator (1/0) for 25,000 ≤ county population (measured in 2010 from SEER) < 50,000, an indicator (1/0) for 50,000 ≤ county population (measured in 2010 from SEER) < 100,000, and an indicator (1/0) for 100,000 ≤ county population (measured in 2010 from SEER) < 250,000. Standard errors (reported in parentheses) are clustered at the state level.

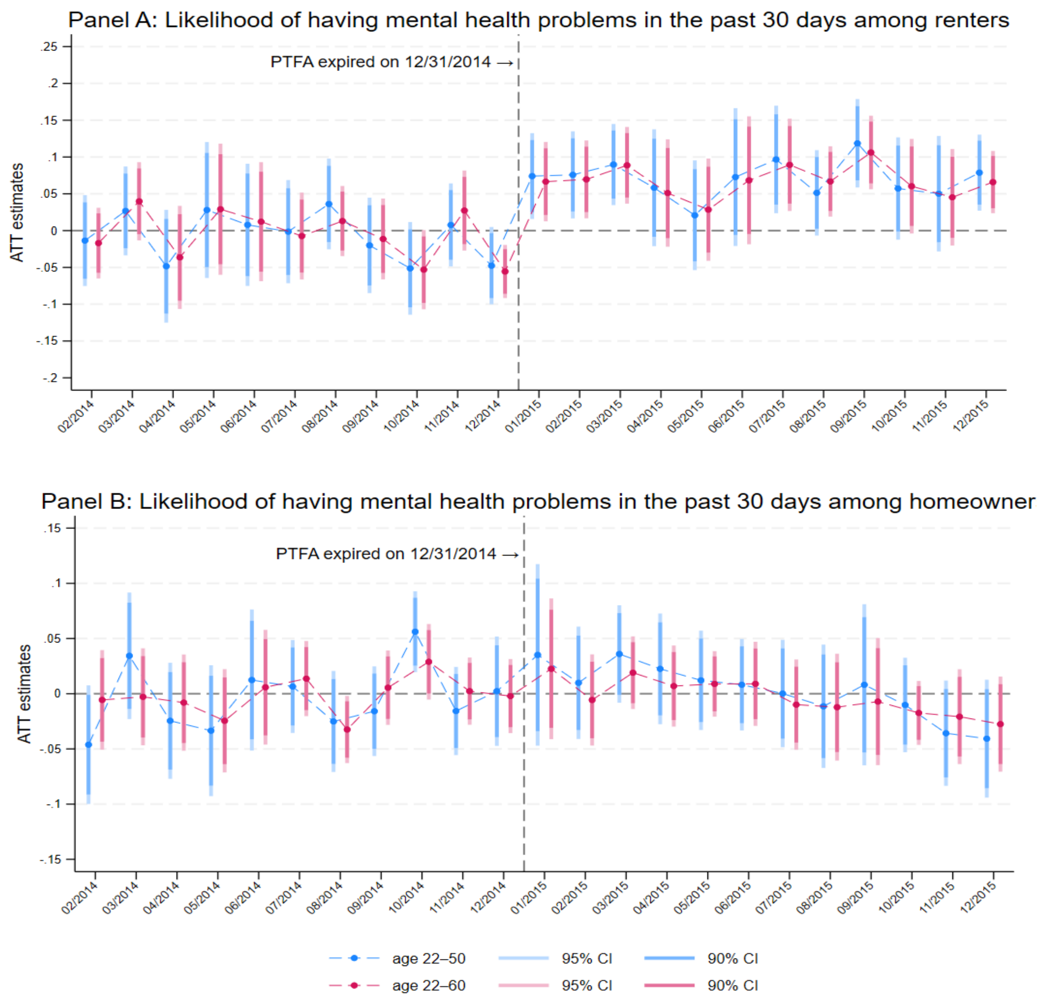


Figure 3: DID Estimates for Having Mental Health Problems by Homeownership

Notes: The sample period is January 2014 through December 2015. All estimations use the augmented inverse probability weighting (AIPW) version of the Callaway and Sant'Anna (2021) difference-in-differences (DID) estimator. Having mental health problems is defined as experiencing at least one day of such problems in the past 30 days. Covariates used in the estimations are: age (measured in 2014), male (1/0), White (1/0), Black (1/0), less than high school education (1/0), high school education (1/0), some college (1/0), and married (1/0). For subsample estimation by BRFSS respondent's age, the age is a time-varying variable measured at the time of the BRFSS interview. Standard errors (reported in parentheses) are clustered at the state level.

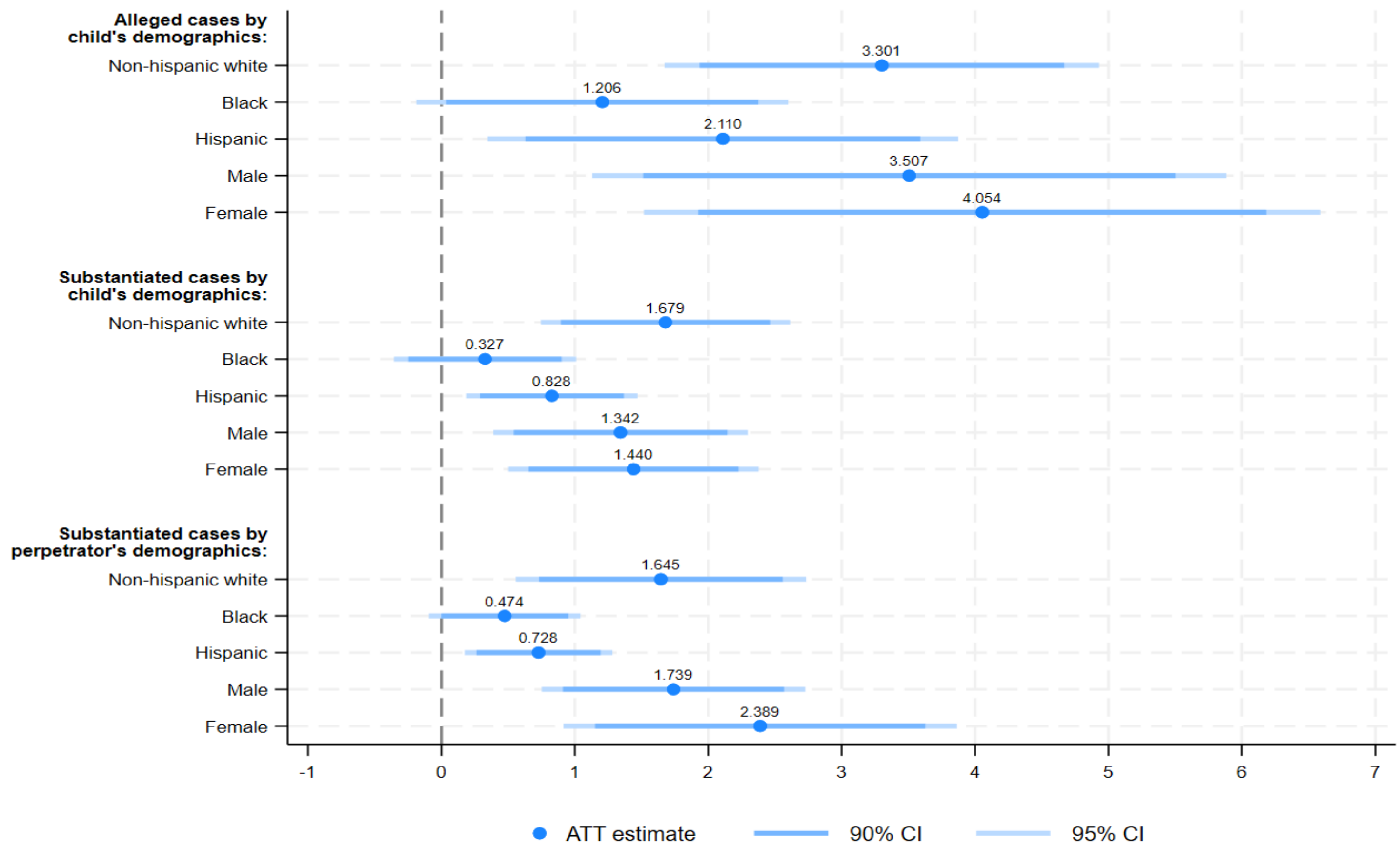


Figure 4: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family, by the Demographics of the Child and the Perpetrator

Notes: See Figure 2. Reported are the estimates of the overall ATT, obtained by aggregating the cohort-time ATTs across time and cohorts (explained in the paper), and the confidence intervals (CI).

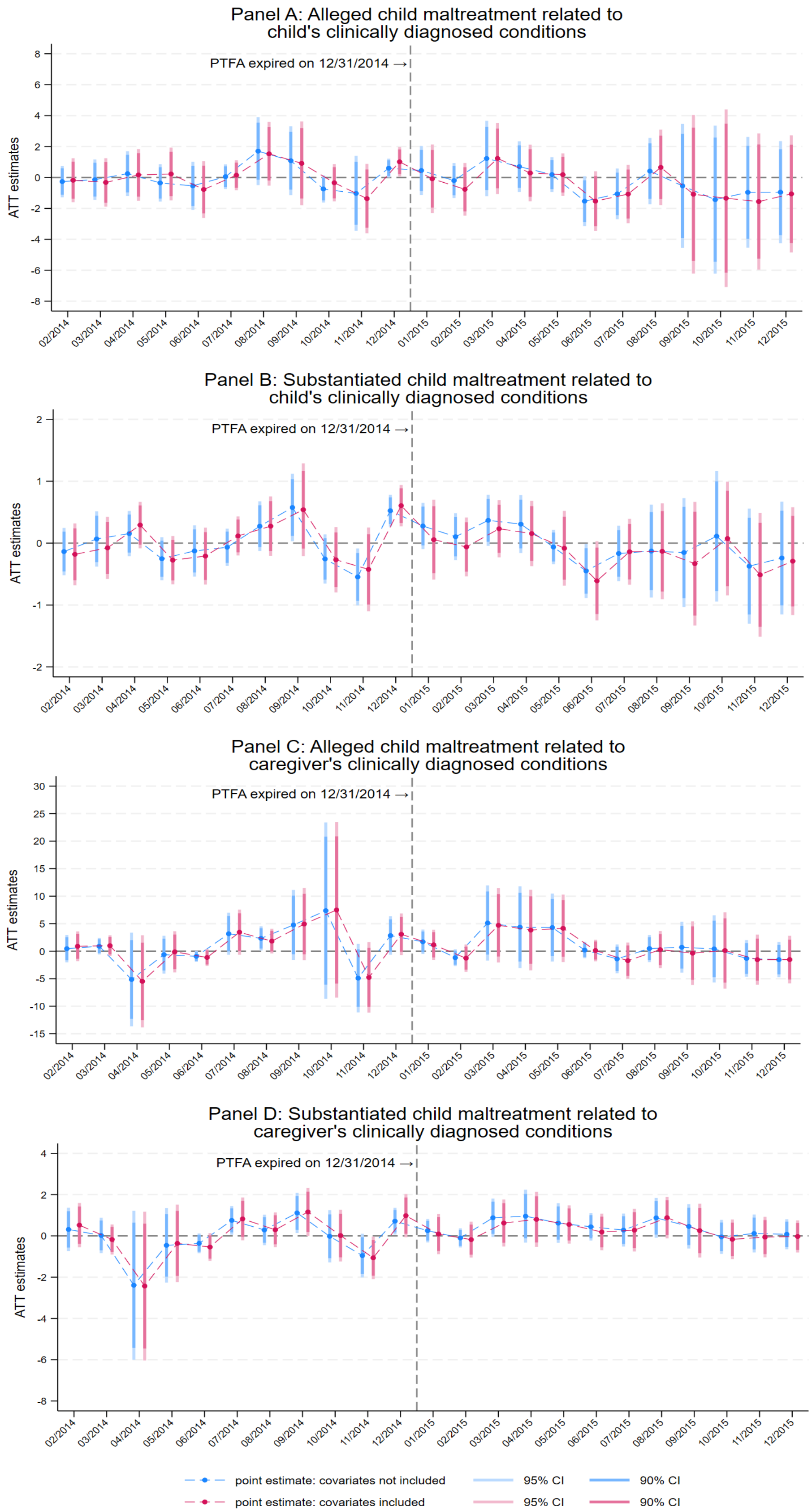


Figure 5: DID Estimates for Child Maltreatment Related to Clinically Diagnosed Conditions of the Child and the Caregiver: Intellectual Disability or Chronic Emotional Disorder

Notes: See Figure 2.

Table 1: Comparisons in the Pre-Treatment Period between the Treated and Control Groups

	Control	Treated
Panel A: Variables used for child maltreatment analysis		
Number of observations	5,061 (39.5%)	7,762 (60.5%)
Alleged cases of child maltreatment related to inadequate housing per 100,000 children aged 0–17 in a county	14.528 (29.312)	20.157 (41.549)
Alleged cases of child maltreatment related to inadequate housing or financial problems of the child's family per 100,000 children aged 0–17 in a county	34.289 (57.023)	55.700 (113.292)
Alleged cases of child maltreatment related to the child's intellectual disability or chronic emotional disorder per 100,000 children aged 0–17 in a county	7.318 (15.966)	15.305 (28.399)
Alleged cases of child maltreatment related to the caregiver's intellectual disability or chronic emotional disorder per 100,000 children aged 0–17 in a county	10.448 (23.063)	26.764 (73.329)
Substantiated cases of child maltreatment related to inadequate housing per 100,000 children aged 0–17 in a county	5.708 (13.115)	10.146 (27.694)
Substantiated cases of child maltreatment related to inadequate housing or financial problems of the child's family per 100,000 children aged 0–17 in a county	10.513 (19.674)	18.333 (45.009)
Substantiated cases of child maltreatment related to the child's intellectual disability or chronic emotional disorder per 100,000 children aged 0–17 in a county	1.224 (3.589)	3.701 (10.521)
Substantiated cases of child maltreatment related to the caregiver's intellectual disability or chronic emotional disorder per 100,000 children aged 0–17 in a county	2.752 (7.698)	7.831 (20.460)
Proportions of individuals aged 0–19 (measured in 2010 from SEER) in a county	0.265 (0.030)	0.268 (0.029)
Proportions of individuals aged 20–64 (measured in 2010 from SEER) in a county	0.596 (0.024)	0.590 (0.026)
Proportions of individuals whose race is white (measured in 2010 from SEER) in a county	0.853 (0.126)	0.844 (0.151)
Proportions of individuals whose race is black (measured in 2010 from SEER) in a county	0.098 (0.110)	0.114 (0.136)
Proportions of individuals who completed at least some college (2010–2014 ACS five-year estimates) in a county	0.081 (0.046)	0.066 (0.037)
Proportions of families below poverty level (2010–2014 ACS five-year estimates) in a county	0.115 (0.044)	0.126 (0.046)
Proportions of owner-occupied housing units with a mortgage (2010–2014 ACS five-year estimates) in a county	0.625 (0.098)	0.612 (0.096)
Proportions of occupied units paying rent with GRAPI (gross rent as a percentage of household income) ≥ 25% (2010–2014 ACS five-year estimates) in a county	0.625 (0.053)	0.614 (0.057)
Proportions of individuals (of the civilian noninstitutionalized population) without health insurance coverage (2010–2014 ACS five-year estimates) in a county	0.135 (0.051)	0.143 (0.042)
Proportions of county population living in rural areas as of the 2010 census	0.309 (0.233)	0.371 (0.245)
Indicator (1/0) for county population (measured in 2010 from SEER) < 25,000	0.009 (0.095)	0.029 (0.167)
Indicator (1/0) for 25,000 ≤ county population (measured in 2010 from SEER) < 50,000	0.123 (0.329)	0.238 (0.426)
Indicator (1/0) for 50,000 ≤ county population (measured in 2010 from SEER) < 100,000	0.257 (0.437)	0.243 (0.429)
Indicator (1/0) for 100,000 ≤ county population (measured in 2010 from SEER) < 250,000	0.261 (0.439)	0.264 (0.441)
Panel B: Variables used for mental health analysis		
Number of observations	79,872 (39.1%)	124,582 (60.9%)
Having mental health problems in the past 30 days (1/0)	0.363 (0.481)	0.366 (0.482)
Age (in 2014)	44.829 (10.984)	44.996 (11.002)
Male (1/0)	0.436 (0.496)	0.426 (0.495)
White (1/0)	0.714 (0.452)	0.777 (0.416)
Black (1/0)	0.095 (0.294)	0.095 (0.293)
hispanic	0.119 (0.324)	0.071 (0.258)
High school education (1/0)	0.225 (0.418)	0.269 (0.444)
Some college (1/0)	0.256 (0.437)	0.284 (0.451)
Married (1/0)	0.572 (0.495)	0.598 (0.490)
Rent a house (1/0)	0.307 (0.461)	0.252 (0.434)

Notes: Data are from the National Child Abuse and Neglect Data System (NCANDS) Child Files, the Surveillance, Epidemiology, and End Results (SEER) program, the American Community Survey (ACS) 2010–2014 five-year county-level estimates, the 2010 census, and the Behavioral Risk Factor Surveillance System (BRFSS). The mean and standard deviation (in parenthesis) are reported for each variable and for the pre-treatment period (i.e., January 2014 through December 2014). For summary statistics of variables related to child maltreatment incidence, the numeric value of the denominator (i.e., for the per 100,000 children aged 0–17 calculation) comes from SEER of year 2010. For summary statistics of variables used for mental health analysis, data are from BRFSS and the sample is restricted to respondents aged between 22 and 60.

Table 2: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family

	(1)	(2)
Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	8.269 ***	7.819 ***
(Standard error)	(2.995)	(2.589)
[P-value]	[0.006]	[0.003]
Mean of the dep. var. in the treated group and in the pre-period	55.700	55.700
(Estimate/mean) × 100%	14.8%	14.0%
Number of observations	26,003	26,003
Covariates included	No	Yes
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	3.141 **	2.831 ***
(Standard error)	(1.282)	(0.943)
[P-value]	[0.014]	[0.003]
Mean of the dep. var. in the treated group and in the pre-period	18.333	18.333
(Estimate/mean) × 100%	17.1%	15.4%
Number of observations	26,003	26,003
Covariates included	No	Yes
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	3.229 *	3.076 ***
(Standard error)	(1.684)	(1.029)
[P-value]	[0.055]	[0.003]
Mean of the dep. var. in the treated group and in the pre-period	20.157	20.157
(Estimate/mean) × 100%	16.0%	15.3%
Number of observations	26,003	26,003
Covariates included	No	Yes
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	1.913 *	1.857 ***
(Standard error)	(1.038)	(0.590)
[P-value]	[0.065]	[0.002]
Mean of the dep. var. in the treated group and in the pre-period	10.146	10.146
(Estimate/mean) × 100%	18.9%	18.3%
Number of observations	26,003	26,003
Covariates included	No	Yes

Notes: The sample period is January 2014 through December 2015. All estimations use the augmented inverse probability weighting (AIPW) version of the Callaway and Sant'Anna (2021) difference-in-differences (DID) estimator. Covariates used in the estimations are: proportions of individuals aged 0–19 (measured in 2010 from SEER) in a county, proportions of individuals aged 20–64 (measured in 2010 from SEER) in a county, proportions of individuals whose race is white (measured in 2010 from SEER) in a county, proportions of individuals whose race is black (measured in 2010 from SEER) in a county, proportions of individuals who completed at least some college (2010–2014 ACS five-year estimates) in a county, proportions of families below poverty level (2010–2014 ACS five-year estimates) in a county, proportions of owner-occupied housing units with a mortgage (2010–2014 ACS five-year estimates) in a county, proportions of occupied units paying rent with GRAPI (gross rent as a percentage of household income) ≥ 25% (2010–2014 ACS five-year estimates) in a county, proportions of individuals (of the civilian noninstitutionalized population) without health insurance coverage (2010–2014 ACS five-year estimates) in a county, proportions of county population living in rural areas as of the 2010 census, an indicator (1/0) for county population (measured in 2010 from SEER) < 25,000, an indicator (1/0) for 25,000 ≤ county population (measured in 2010 from SEER) < 50,000, an indicator (1/0) for 50,000 ≤ county population (measured in 2010 from SEER) < 100,000, and an indicator (1/0) for 100,000 ≤ county population (measured in 2010 from SEER) < 250,000. Standard errors (reported in parentheses) are clustered at the state level. * p -value < 0.1; ** p -value < 0.05; *** p -value < 0.01.

Table 3: DID Estimates for Having Mental Health Problems by Homeownership

	Age 22–50	Age 22–60
Panel A: Likelihood of having mental health problems in the past 30 days among Renters		
ATT (overall aggregation) estimate	0.071 **	0.068 **
(Standard error)	(0.030)	(0.027)
[P-value]	[0.019]	[0.013]
Mean of the dep. var. in the treated group and in the pre-period	0.448	0.452
(Estimate/mean) × 100%	15.7%	14.9%
Number of observations	85,547	113,285
Covariates included	Yes	Yes
Panel B: Likelihood of having mental health problems in the past 30 days among Homeowners		
ATT (overall aggregation) estimate	0.003	-0.003
(Standard error)	(0.023)	(0.019)
[P-value]	[0.899]	[0.879]
Mean of the dep. var. in the treated group and in the pre-period	0.355	0.336
(Estimate/mean) × 100%	0.8%	-0.8%
Number of observations	158,261	286,093
Covariates included	Yes	Yes

Notes: The sample period is January 2014 through December 2015. All estimations use the augmented inverse probability weighting (AIPW) version of the Callaway and Sant'Anna (2021) difference-in-differences (DID) estimator. Having mental health problems is defined as experiencing at least one day of such problems in the past 30 days. Covariates used in the estimations are: age (measured in 2014), male (1/0), White (1/0), Black (1/0), less than high school education (1/0), high school education (1/0), some college (1/0), and married (1/0). For subsample estimation by BRFSS respondent's age, the age is a time-varying variable measured at the time of the BRFSS interview. Standard errors (reported in parentheses) are clustered at the state level. * p -value < 0.1; ** p -value < 0.05; *** p -value < 0.01.

Table 4: DID Estimates for Child Maltreatment Related to Clinically Diagnosed Conditions of the Child and the Caregiver

	Caregiver's intellectual disability or chronic emotional disorder	Child's intellectual disability or chronic emotional disorder
Panel A: Alleged child maltreatment		
ATT (overall aggregation) estimate	0.663	-0.512
(Standard error)	(1.556)	(1.028)
[P-value]	[0.670]	[0.618]
Number of observations	26,003	26,003
Covariates included	Yes	Yes
Panel B: Substantiated child maltreatment		
ATT (overall aggregation) estimate	0.271	-0.137
(Standard error)	(0.401)	(0.251)
[P-value]	[0.499]	[0.584]
Number of observations	26,003	26,003
Covariates included	Yes	Yes

Notes: See Table 2.

Appendices for “Homes in Limbo, Children at Risk: Exploring the Link between Housing Instability and Child Maltreatment Using the Discontinuity of the Protecting Tenants at Foreclosure Act” by Wei Fu[1] and Muzhe Yang[2]

November 2024

Appendix A: Construction of Composite Counties and Determination of the Numeric Values of Their Characteristics

For confidentiality protections, the National Child Abuse and Neglect Data System (NCANDS) Child Files mask a county’s identifier for two cases: (a) the county to which the report of alleged child maltreatment was assigned for a child protective services response has fewer than 700 child maltreatment incidents,[3] and in this case the county code is XX000, where XX represents the state identifier; (b) the child died as a result of maltreatment, and in this case the county code is 00000, where the state identifier is also masked. In addition, there are counties whose identifiers are just missing, and in this case the county code is XX999, where XX represents the state identifier. Because our identification strategy requires state identifiers used for defining the treatment and control groups, we omit case (b) in our empirical analysis. Child maltreatment incidents in case (b) account for only 0.05 percent of the total number reported between 2011 and 2019. For non-fatal child maltreatment incidents reported during the same period, 0.26 percent of them have missing

[1] Department of Health Management and Systems Sciences, University of Louisville, Louisville, KY 40202, wei.fu@louisville.edu. Declarations of relevant or material financial interests that relate to the research described in this paper: none. Declarations of conflicts of interest: none.
[2] Corresponding Author, Department of Economics, Lehigh University, Bethlehem, PA 18015, muzheyang@lehigh.edu. Declarations of relevant or material financial interests that relate to the research described in this paper: none. Declarations of conflicts of interest: none.
[3] For details, please see the section titled “State/County FIPS Code” of the NCANDS Child File Codebook (available at https://www.ndacan.acf.hhs.gov/datasets/pdfs_user_guides/ncands-child-file-codebook.pdf, accessed in September 2024).

county identifiers, and 12.09 percent of them are from counties with fewer than 700 incidents.

To incorporate counties that do not have county identifiers but have state identifiers (i.e., counties coded by XX000 or XX999) into our empirical analysis, we use their state identifiers (which are represented by XX) to form a "composite county" for each state; that is, a composite county of one state includes all counties coded by XX000 or XX999 within that state. For simplicity, hereafter, we refer to these counties as masked counties. The makeup of a composite county (i.e., the list of masked counties) is determined by the Child File data.

To determine the child maltreatment outcomes for each composite county in a year, we count the child maltreatment cases of the masked counties constituting that composite county within the corresponding state for that year.

To construct the pre-treatment and time-invariant covariates for composite counties used for the difference-in-differences (DID) estimation, we obtain variables on demographics from the Surveillance, Epidemiology, and End Results (SEER) program data of year 2010, rural population from the 2010 census, and socioeconomic characteristics from the American Community Survey (ACS) 2010–2014 five-year county-level estimates (e.g., education, income, and housing).

Due to the censoring criteria used in the Child File data (i.e., cases a and b aforementioned), the makeup of a composite county varies by year. For instance, one county may have more than 700 child maltreatment incidents in one year but fewer than 700 in another year. Thus, the characteristics of a composite county are specific to the year when its composition is determined, meaning that these characteristics are time-varying. For our DID estimation, we need to use time-invariant covariates. To make the characteristics of a composite county time-invariant, we need to fix the makeup of that composite county. To do so, we first create a reference list including only identified counties from the Child File data in which child maltreatments were reported in 2014. To determine the characteristics of a composite county within a state, we exclude the counties on that reference list and obtain the characteristics of the remaining counties within that state. Essentially, we fix the makeup of a composite county by using one cross-section of the Child File data. This approach ensures that the composition of a composite county within a state stays the same across different years.

Specifically, to determine the numeric values of county-level variables for a composite county—such as the proportions of population with bachelor's degree or above, families in poverty, housing units with a mortgage, rental burden, and individuals without health insurance—using the ACS five-year (2010–2014) estimates data, we calculate a population-weighted average across all counties not on the reference list. The weight is the county population provided by the ACS five-year (2010–2014) estimates data. To determine the numeric values of a composite county's demographic variables, including total population and population by age and racial groups from the 2010 SEER, as well as the population living in rural areas from the 2010 census, we sum the values for all counties within a state that are not on the reference list. For each composite county, we then calculate the proportion of each specific population segment using its total population as the denominator.

Appendix B: Treatment-Control Group Determination for ID, NE and VT

In the June 2015 factsheet (NLIHC, 2015), which we explained in the identification strategy section of the paper, there is no information regarding the treatment or control group determination for these three states: Idaho, Nebraska and Vermont. As a result, these three states are excluded from the sample used for our DID estimations. In this section we explain how we added these three states back to our estimation sample, using information from NLCHP (2012, specifically "Section Two" regarding state laws) to approximate the treatment-control status of each of the three states.

In Idaho, "[a]fter foreclosure, Idaho law provides that the new owner is entitled to possession on the tenth day following the sale, and a tenant remaining on the property may be subject to expedited eviction proceedings (occurring 5 to 12 days after the tenant receives notice of the eviction proceedings)." We assigned Idaho to the control group.

In Nebraska, "[u]nder Nebraska law, after a foreclosure, the new owner may proceed against the tenant either as a trespasser or a tenant. *Schrunk v. Andres*, 221 Minn. 465, 22 N.W.2d 548 (1946). If proceeding as a trespasser, the tenant must give a notice to leave the premises at least 3 days before the commencement of the action." We assigned Nebraska to the treatment group.

In Vermont, "[a]s under pre-existing law a tenant must be joined as a defendant in a foreclosure action and be provided 60 days' notice of a foreclosure sale. (12 V.S.A. §§ 4932, 4965). One change is the new provisions is that a tenant must be provided notice to vacate the property the longer of 30 days prior to the date the new owner shall take possession or such other time as is required by federal law." We assigned Vermont to the control group.

Our estimation results are similar whether or not we include these three states, as shown by Appendix Table A1.

Appendix C: The Callaway and Sant'Anna (2021) Estimator and Its Alternative Versions

AIPW. The default version of the Callaway and Sant'Anna (2021) estimator (hereafter, the CS estimator) uses the augmented inverse probability weighting (AIPW) to achieve the double-robustness property. Specifically, the estimand of this estimator, which is ATT varying by g and t, implements the following transformation (Callaway and Sant'Anna, 2021, p. 228) of the outcome variable (y), the cohort indicator (G_g) where a cohort is defined by the time when a group is first treated (g), and covariates ($\boldsymbol{x}$), for each combination of cohort (g) and time (t):

$$ATT(g,t) = E\big(y_t(g) - y_t(0)|G_g = 1\big) =$$

$$E\left[\frac{G_g}{E(G_g)}\left(\left(m_t^{treat}(\boldsymbol{x}) - m_{g-1}^{treat}(\boldsymbol{x})\right) - \left(m_t^{control}(\boldsymbol{x}) - m_{g-1}^{control}(\boldsymbol{x})\right)\right)\right]$$

$$+E\left[w_{g,t}^{treat}\big(y - m_t^{treat}(\boldsymbol{x})\big) - w_{g,g-1}^{treat}\left(y - m_{g-1}^{treat}(\boldsymbol{x})\right)\right]$$

$$-E\left[w_{g,t}^{control}(\boldsymbol{x})\left(y - m_t^{control}(\boldsymbol{x})\right) - w_{g,g-1}^{control}(\boldsymbol{x})\left(y - m_{g-1}^{control}(\boldsymbol{x})\right)\right], (1)$$

where $m_s^{treat}(\boldsymbol{x}) = E\big(y\big|\boldsymbol{x}, G_g = 1, T_s = 1\big)$,

$$m_s^{control}(\boldsymbol{x}) = E(y|\boldsymbol{x}, G_0 = 1, T_s = 1),$$

$$w_{g,s}^{treat} = \frac{T_s G_g}{E(T_s G_g)}, w_{g,s}^{control}(\boldsymbol{x}) = \frac{\dfrac{T_s p_g(\boldsymbol{x}) G_0}{1 - p_g(\boldsymbol{x})}}{E\left(\dfrac{T_s p_g(\boldsymbol{x}) G_0}{1 - p_g(\boldsymbol{x})}\right)},$$

and $p_g(\boldsymbol{x}) = Prob\big(G_g = 1\big|\boldsymbol{x}, G_g + G_0 = 1\big)$.

In equation (1), t denotes a year-month pair ($t = 1, 2, ..., T$); T_t equals one if the unit is observed at time t, and zero otherwise. In our study, g takes two values: 01/2015 (the month immediately after the PTFA's expiration on December 31, 2014) and zero, for the treatment and control groups, respectively; that is, $G_{01/2015} = 1$ for the treatment group, and $G_0 = 1$ for the control group. The baseline period used by the CS estimator is $g - 1$, which in our study is December 2014. We use a set of variables, denoted by $\boldsymbol{x}$, measured in the pre-treatment period and at the county level as the conditioning variables (i.e., covariates) for the conditional parallel trend (PT) assumption discussed in Section 3.3 of the paper.

When estimating $ATT(g,t)$, the CS estimator restricts the sample to time t and t_0, and keeps only the units in the treatment group ($G_g = 1$) or in the control group ($G_0 = 1$), where $t_0 = g - 1$ if $t \geq g$ or $t_0 = t - 1$ if $t < g$. The CS estimator uses a linear regression model to estimate $m_t^{treat}(\boldsymbol{x})$, $m_{t_0}^{treat}(\boldsymbol{x})$, $m_t^{control}(\boldsymbol{x})$, and $m_{t_0}^{control}(\boldsymbol{x})$; it uses a logit regression to estimate $p_g(\boldsymbol{x})$; and it replaces the expectation operator (shown in equation 1) by the sample average.

RA and IPW. Here, we discuss two alternative versions of the CS estimator, which use the regression adjustment (RA) and the inverse probability weighting (IPW), respectively. Note that without using any covariates, estimations of $ATT(g,t)$ by AIPW, RA and IPW are all simplified to the standard two-group two-period differencing of two differences. Thus, differences in the estimates obtained by AIPW, RA and IPW come from the different ways in which AIPW, RA and IPW incorporate covariates. The way in which AIPW incorporates covariates has the double-robustness property, whereas the ways in which RA and IPW incorporate covariates only have the single-robustness property.

Specifically, the $ATT(g,t)$ obtained by RA implements the following transformation (Callaway and Sant'Anna, 2021, p. 228) of the outcome variable (y), the cohort indicator (G_g) where a cohort is defined by the time when a group is first treated (g), and covariates ($\boldsymbol{x}$), for each combination of cohort (g) and time (t):

$$ATT(g,t) = E\left(y_t(g) - y_t(0) | G_g = 1\right) =$$

$$E\left[\frac{G_g}{E(G_g)}\left(\left(m_t^{treat}(\boldsymbol{x}) - m_{g-1}^{treat}(\boldsymbol{x})\right) - \left(m_t^{control}(\boldsymbol{x}) - m_{g-1}^{control}(\boldsymbol{x})\right)\right)\right], \quad (2)$$

and the $ATT(g,t)$ obtained by IPW implements the following transformation (Callaway and Sant'Anna, 2021, p. 228):

$$ATT(g,t) = E\left(y_t(g) - y_t(0) | G_g = 1\right) =$$

$$E\left[\left(w_{g,t}^{treat} - w_{g,g-1}^{treat}\right)y\right] - E\left[\left(w_{g,t}^{control}(\boldsymbol{x}) - w_{g,g-1}^{control}(\boldsymbol{x})\right)y\right], \quad (3)$$

where $m_s^{treat}(\boldsymbol{x}) = E\left(y \middle| \boldsymbol{x}, G_g = 1, T_s = 1\right)$,

$$m_s^{control}(\boldsymbol{x}) = E(y|\boldsymbol{x}, G_0 = 1, T_s = 1),$$

$$w_{g,s}^{treat} = \frac{T_s G_g}{E(T_s G_g)}, w_{g,s}^{control}(\boldsymbol{x}) = \frac{\frac{T_s p_g(\boldsymbol{x}) G_0}{1 - p_g(\boldsymbol{x})}}{E\left(\frac{T_s p_g(\boldsymbol{x}) G_0}{1 - p_g(\boldsymbol{x})}\right)},$$

and $p_g(\boldsymbol{x}) = Prob\left(G_g = 1 \middle| \boldsymbol{x}, G_g + G_0 = 1\right)$.

If the unconditional PT assumption is true, we would expect that covariates play little role in the average treatment effect on the treated (ATT) estimation, which implies that estimates obtained by AIPW, RA and IPW that incorporate covariates should be similar. This is exactly what we found. Appendix Table A2 shows that the overall ATT estimates obtained by RA and IPW are very similar to each other (column 1 compared with column 2), and both sets of estimates are very similar to the estimates obtained by AIPW that incorporates covariates (i.e., estimates reported in column 2 of Table 2 in the paper). Furthermore, Appendix Figure A1 presents more detailed evidence of the similarities between the RA, IPW and AIPW estimates, especially in the point estimates: all three versions of the CS estimator have produced similar results, especially in the point estimates, throughout the sample period and except a few confidence intervals obtained by IPW for a few months in the pre-period.

Appendix D: Simultaneous Confidence Intervals

In Appendix Figure A2 we reported the simultaneous confidence intervals (SCIs, a.k.a. uniform confidence bands) for ATTs estimated for each month from the pre-period through the post-period. Note that SCIs are wider than the pointwise (i.e., conventional) confidence intervals (CIs). This is because SCIs simultaneously cover the true values of all ATTs from the pre-period to the post-period with a predefined probability, such as 95%.[4] Results in this figure indicate that the entire path of the ATTs over our sample period is trending above zero in the post-period, when the overall estimation uncertainty is taken into account. This finding is consistent with what we found in Figure 2 of the paper, regarding the persistence of the treatment effect we previously discussed.

[4] Covering the true values of all ATTs is more difficult than covering the true value of a single ATT. As a result, when the coverage probability is fixed at 95% for both types of intervals, SCIs must be wider than the pointwise CIs.

Appendix E: DID Estimates for Child Maltreatment Outcomes with Extended Pre-Treatment and Extended Post-Treatment Periods

In this section we conduct robustness checks with the extended pre-period that goes from January 2011 to December 2014, as well as with the extended post-period that goes from January 2015 to December 2019.[5]

Appendix Figure A3 reports the ATTs estimated for each month of the extended pre-period.[6] Overall, the results indicate that there are no pre-trends between the treatment and control groups in almost every month of the entire four-year period prior to the expiration of the Protecting Tenants at Foreclosure Act (PTFA), regardless of the inclusion of covariates, which lends support to both the unconditional and conditional PT assumptions. In the extended pre-period (and only in the extended pre-period) we dropped the state of Florida from the sample used for estimations. We did so because we learned from the Child Maltreatment Reports (Children's Bureau, 2013, 2014) that there had been a change in Florida during the pre-period regarding whether threatened harm should be considered as a separate category in addition to specific injury and harm in the state's investigation of reports of alleged child maltreatment. When including Florida in the pre-period, the ATT estimates are much noisier, but the pattern remains the same, suggesting no pre-trends between the treatment and control groups for nearly the entire four-year period prior to the expiration of PTFA.

Appendix Table A3 and Appendix Figure A4 present the estimates of the overall ATTs and the monthly ATTs obtained using the extended post-period, respectively.

---

[5] We ended the post-period prior to the year of 2020, the year of the COVID-19 outbreak.

[6] Note that there is no ATT estimated for January 2011. This is because, as we explained in the paper, the base period of the CS estimator for ATT estimated for time t in the pre-period is $t-1$, that is, the previous month. The earliest month in which ATT can be estimated in the pre-period is February 2011. In contrast, ATT can be estimated for every month in the post-period. This is because the base period for the post-period is December 2014 (i.e., $g-1$, where $g=01/2015$).

Here, we assume that our treatment and control group designations are correct (in the sense of capturing differences in housing instability arising from the PTFA's expiration between the two groups) for the extended post-period. In this robustness check we obtained qualitatively the same results previously discussed in the main results section.

Specifically, results in Appendix Table A3 show that there are statistically significant increases in the incidents of child maltreatment due to housing instability in the treatment group in the five-year period following the PTFA's expiration. Appendix Figure A4 indicates the persistence of the treatment effect over an extended period. As we discussed in the main text of the paper, this persistence could result from the cycle of housing instability triggered by the PTFA's expiration in 2014. Furthermore, this figure provides suggestive evidence of a decline in the treatment effect, which could be attributed to the reinstatement of the PTFA in 2018.

Appendix F: "Leave-One-Out" Estimations

To check whether our estimates reported in the main results section, specifically the overall ATT estimates (reported in Table 2 of the paper), are disproportionately influenced by any single state—potentially due to unique post-PTFA policy environments—we conducted a series of "leave-one-out" estimations. The results of these robustness checks are reported in Appendix Figures A5 and A6. Specifically, Appendix Figure A5 presents the results when one state from the treatment group is excluded in each estimation, and Appendix Figure A6 shows the results when one state from the control group is excluded. Overall, results reported in these two figures suggest that no particular state appears to significantly affect the overall ATT estimates reported in the main results section of the paper.

Appendix G: Estimations That Use Only Counties Identified in the Child File Data

In Appendix Table A4 and Appendix Figure A7, we repeated the estimations done in Table 2 (for the overall ATT estimates) and Figure 2 (for the monthly ATT estimates) of the paper, but using only counties identified in the Child File data; that is, we dropped those masked counties explained in the data section of the paper from the estimation sample. We conducted this robustness check to assess whether our estimates reported in the main results section are affected by the way we constructed the composite counties and determined the numeric values of their characteristics (explained in Appendix A). The results reported in Appendix Table A4 show that the overall ATT estimates are very similar whether the sample includes only identified counties or all counties (i.e., both identified and masked counties). Furthermore, Appendix Figure A7 shows that the estimates of the monthly ATTs throughout our sample period are similar to those reported in Figure 2 of the paper, in which the estimation sample includes all counties.

Appendix H: Estimations That Use Alternative Definitions of the Treatment and Control Groups

In Appendix Table A5 and Appendix Figures A8a and A8b, we conducted robustness checks using alternative definitions of the treatment and control groups. Specifically, in case (a) we excluded the “3 days” and “5 days” states (shown in Figure 1 of the paper) from the treatment group, while keeping the same control group. In case (b) we removed the “10 days”, “30 days”, and “60 days” states (shown in Figure 1 of the paper) from the control group, while keeping the treatment group unchanged. The results reported in Appendix Table A5 are similar to those reported in Table 2 of the paper. Furthermore, Appendix Figures A8a (using the alternative definition for the treatment group) and A8b (using the alternative definition for the control group) presents the monthly ATT estimates throughout our sample period, confirming the same pattern observed in Figure 2 of the paper.

Appendix I: Dynamic Two-Way Fixed-Effect (TWFE) Model

In this section we discuss our estimation results using a dynamic TWFE model, which is often referred to as the (conventional) event-study model (Roth et al., 2023). In our empirical setting, where there is no staggered treatment timing, the dynamic TWFE model, estimated by the ordinary least squares (OLS), is adequate for capturing treatment effect heterogeneity solely in time since treatment (Roth et al., 2023). In this regard, the reason for us to choose the CS estimator instead of the OLS is the double-robustness property provided by the AIPW version of the CS estimator. However, as we explained in the paper, this double-robustness comes at a cost: it does not allow us to separately estimate the treatment effect dynamics and the effects of time-varying covariates. In contrast, the conventional event-study model allows us to do so, as long as those covariates are correctly specified in the model. However, this advantage of the event-study model requires the assumption that the treatment is exogenous, conditional on all covariates (both time-invariant and time-varying) used in the model—a stronger assumption than the conditional PT assumption.

We specify the dynamic TWFE model as follows:

$$y_{it} = \alpha_i + \alpha_t + \sum_{k=-K}^{-2} \beta_k^{lead} d_{it}^k + \sum_{k=0}^{L} \beta_k^{lag} d_{it}^k + \boldsymbol{x}_{is}'\gamma + u_{it}$$

with the event-study dummy variables $d_{it}^k = 1\{t - G_i = k\}$. In this regression model, G_i is defined to be January 2015; t varies monthly from January 2014 (denoted by $-K$ in the regression model) to December 2015 (denoted by L in the regression model); the base period (for which $k = -1$) is December 2014; α_i denotes the fixed effect for each county i; α_t denotes the fixed effect for each t; $\boldsymbol{x}_{is}$

is a vector of time-varying covariates including two cases. In case 1, the time-varying covariates are constructed by interacting the time-invariant covariates with time. Here, the time-invariant covariates are the same variables used in our DID estimations by the CS estimator, which are described in the econometric specification section of the paper. These variables were measured in the pre-treatment period and at the county level: proportions of individuals of different age groups (ages 0–19 and 20–64) and of different racial groups (White and Black), indicators of different county population sizes, educational attainment, family poverty rate, housing units with a mortgage, rental burden (i.e., rent divided by household income), health insurance coverage, and the percentage of county population living in rural areas. We multiply these time-invariant covariates aforementioned with the dummy variables for the year (indexed by s) when an alleged child maltreatment was reported. In case 2, the time-varying covariate is the variable on the year when a state passed the state fair housing law protecting some groups from eviction.[7]

In this event-study model the error term, denoted by u_{it}, is assumed to be exogenous conditional on all covariates, fixed effects, and event-study dummies in the model, that is, $E(u_{it}|\boldsymbol{x}_{is}, \alpha_i, \alpha_t, d_{it}^k) = 0$. We estimate the model by OLS and standard errors are clustered at the state level. Zero values of β_k^{lead}'s indicate the absence of pre-trends between the treatment and control groups; estimates of β_k^{lag}'s suggest the treatment effect dynamics in the post-period.

Appendix Figure A9 presents the estimation results, which are very similar to those discussed in the main results section of the paper.[8] We find no significant pre-

[7] We obtained this variable from the Appendix Table 1 in Bradford and Bradford (2021).

[8] We report in this footnote, not in the figure, the overall ATT estimate, calculated by averaging the 12 ATT estimates obtained from the most inclusive model and over the post-period, for each outcome listed in Appendix Figure A9. For alleged (substantiate) child maltreatment related to inadequate housing or financial problems of the child's family: 9.380 with *p*-value = 0.001 (3.163

trends between the treatment and control groups, whether we control for covariates ($\boldsymbol{x}_{is}$) or not. Throughout the sample period, the point estimates of β_k^{lead}'s and β_k^{lag}'s are extremely similar regardless of the inclusion of covariates ($\boldsymbol{x}_{is}$) in almost every month, suggesting that the event-study dummy variables are exogenous to the covariates ($\boldsymbol{x}_{is}$) once we control for the fixed effects denoted by α_i and α_t. We also find that the treatment effect appears to be persistent in the post-period. The similarity in the results between the event-study estimates and the CS estimates, to some extent, mitigates concerns that the treatment effect dynamics indicated by the CS estimates could be driven entirely by post-treatment responses, such as state policy changes following the expiration of the PTFA.

Appendix J: Estimations by Reporting Source

We consider the possibility that families experiencing housing instability have increased interactions with public agencies and professionals who are mandated reporters of suspected child maltreatment. If this is true, then the heightened visibility may lead to increased likelihood of maltreatment being officially reported. Furthermore, professionals can be better trained to recognize signs of child maltreatment, which could result in more accurate reporting. Consequently, we would expect the effect of housing instability on child maltreatment to be more pronounced in reports done by professionals compared with reports done by non-professionals.

To test this hypothesis, we use information provided by the Child File data on the report source, which is defined by the category of the person who makes a report of alleged maltreatment, and we repeat the estimations done in Table 2 of the paper but for cases reported by professionals and non-professionals separately. The results,

with p-value = 0.002). For alleged (substantiated) child maltreatment related to inadequate housing: 3.235 with p-value = 0.004 (1.859 with p-value = 0.008).

reported in Appendix Table A6, confirm the statistically significant effect of housing instability on child maltreatment. The findings generally support our hypothesis: the effect appears to be stronger for cases reported by professionals, particularly in substantiated cases where professional reporting is likely more accurate than that of non-professionals.

Appendix K: Subsample Estimation by Delinquency Rates

We are unable to access foreclosure data. Instead, we use delinquency as a leading indicator of foreclosure. We hypothesize that areas with higher delinquency rates measured prior to the PTFA's expiration are more adversely affected compared with those with lower rates. To test this hypothesis, we obtained the delinquency data from the National Mortgage Database (NMDB), a collaborative effort between the Consumer Financial Protection Bureau and the Federal Housing Finance Agency. "The NMDB has a nationally representative, 5 percent sample of all outstanding, closed-end, first-lien, 1–4 family residential mortgages."[9]

The NMDB provides two key delinquency variables: the 30–89-day delinquency rate and the 90-day delinquency rate, which are available at the state, county and metropolitan area levels. The 30–89-day delinquency rate reflects early-stage delinquencies, calculated as the number of mortgages where borrowers' payments are 30 to 89 days past due divided by the total number of outstanding mortgages. The 90-day delinquency rate captures more severe delinquencies and reflects greater economic distress. It is calculated as the number of mortgages where borrowers' payments are 90 or more days past due (but the properties are not yet in foreclosure) divided by the total number of outstanding mortgages. In our study, we use the 90-day delinquency rate as our preferred measure for assessing local foreclosure risks.

[9] For detailed descriptions, please see https://www.consumerfinance.gov/data-research/mortgage-performance-trends/about-the-data/ (accessed in October 2024).

It is worth noting that most counties covered by NMDB are urban and densely populated, meaning that matching NMDB counties with counties identified in the NCANDS Child File data may result in significant attrition. To address this issue, we use the following four-step procedure.

Step 1: Calculating pre-PTFA delinquency rate at the state, county and metropolitan/non-metropolitan area levels. Using the NMDB data, we calculated the 90-day delinquency rate prior to the PTFA's expiration by averaging the corresponding delinquency rates from 2010 and 2014. This calculation was done at the state, county and metro/non-metro area levels, respectively.

Step 2: Matching by county identifiers. We matched the Child File data with the NMDB county-level pre-PTA delinquency data, using county identifiers. There are 451 matched counties, out of the 1,327 counties identified in the Child File data.

Step 3: Matching with CBSA codes. Focusing on the unmatched counties in the Child File data found in Step 2, we assigned each of them a Core-Based Statistical Area (CBSA) code. Since counties identified in the Child File data do not have CBSA codes but county codes, we used the 2011 CBSA-county crosswalk from the National Bureau of Economic Research[10] to assign each of those counties a CBSA code, if it is available. In this step, 201 counties (among those unmatched in Step 2) were assigned CBSA codes, based on which we assigned each of them a CBSA-level (i.e., a metro-area level) pre-PTFA delinquency rate calculated from the NMDB data.

Step 4: Using state-level average delinquency rates among metropolitan and non-metropolitan areas. According to the Rural-Urban Continuum Codes (RUCC) of version 2013 provided by the United States Department of Agriculture,[11] most of the unmatched counties in Step 3 are suburban or rural counties (with RUCC code

[10] Source: https://data.nber.org/cbsa-msa-fips-ssa-county-crosswalk/ (accessed in October 2024).
[11] For details, see: https://www.ers.usda.gov/data-products/rural-urban-continuum-codes/ (accessed in August 2024).

being 5, 6, 7, 8 or 9); only a few of them are urban counties (with RUCC code being 1, 2 or 3). For those urban counties, each of them was assigned an average delinquency rate calculated from all metropolitan areas of each state those counties belong to. Specifically, the delinquency rate was averaged across all counties included in the NMDB data with RUCC code being 1, 2 or 3 for each state those counties belong to. For those suburban/rural counties, each of them was assigned an average delinquency rate calculated from all non-metropolitan areas of each state those counties belong to. Specifically, the delinquency rate was averaged across all counties included in the NMDB data with RUCC code being 5, 6, 7, 8 or 9 for each state those counties belong to. In this step we assigned a pre-PTFA delinquency rate to 663 counties identified in the Child File data.

Using the four-step procedure described above, we were able to assign a pre-PTFA delinquency rate to 1,315 (or 99%) of the 1,327 counties identified in the Child File data. In our analysis, the cutoff value for the high and low delinquency rates is the median value of delinquency rates.

Appendix Table A7 reports the overall ATT estimates, and Appendix Figure A10 presents the monthly ATT estimates from the pre-period to the post-period. Overall, the results indicate that the PTFA's expiration mainly affects child maltreatment in counties with high delinquency rates, rather than in those with low delinquency rates. This finding is reasonable since the expiration of PTFA directly affects tenants living in foreclosed properties and delinquency can be viewed as a leading indicator of foreclosure.

Appendix L: Subsample Estimation by Pro-Renter and Pro-Business States

In this section we conduct subsample estimations, using the classification proposed by Hatch (2017), which distinguishes between pro-renter and pro-business (i.e., pro-landlord) states based on 22 of the most common state-level landlord-tenant laws. According to Hatch's classification, 13 states are identified as pro-renter:

California, Connecticut, Maine, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Oregon, and Vermont. In contrast, 17 states are categorized as pro-business: Arkansas, Colorado, Florida, Georgia, Idaho, Illinois, Indiana, Louisiana, Michigan, Mississippi, Missouri, North Carolina, Ohio, Pennsylvania, Texas, West Virginia, and Wyoming.

Appendix Tables A8a and A8b report the overall ATT estimates, and Appendix Figures A11a and A11b present the monthly ATT estimates from the pre-period to the post-period. For both cases, we computed two types of standard errors: one clustered by county using the CS estimator (results reported in Appendix Table A8a and Appendix Figure A11a), and the other clustered by state using the event-study model discussed in Appendix I. In the latter case, we used the wild cluster bootstrap method to address the issue of having a small number of clusters (i.e., 13 clusters in the pro-renter subsample and 17 clusters in the pro-business subsample) in computing cluster-robust standard errors (results reported in Appendix Table A8b and Appendix Figure A11b). One limitation of the wild cluster bootstrap method is that it is only applicable to hypothesis testing of parameters in linear regression models. For this reason, we used the event-study model, which is a linear regression model, when computing the wild cluster bootstrapped standard errors.

Overall, the results indicate that the impact of the PTFA's expiration on child maltreatment is primarily concentrated in pro-business states, as opposed to pro-renter states. This finding suggests that a state's policy environment may play an important role in mediating the effect of housing instability on child maltreatment.

Appendix M: Mental Health Analysis

In our mental health analysis, having mental health problems is defined as experiencing at least one day of such problems in the past 30 days. To assess the sensitivity of our results to this specific cutoff value, in Appendix Figure A12 we repeated the estimations done in Table 3 of the paper (for the age 22–60 group) regarding the overall ATT estimates, but varying the cutoff values from one day to 30 days. We find that the results remain qualitative the same across different cutoff values.

Appendix N: Migration Analysis

Using DID estimations with the CS estimator, we assess whether our estimated effects of housing instability on child maltreatment could be explained by self-selection by children and their families into the treatment and control groups based on unobserved factors influencing child well-being. If self-selection invalidates the PT assumption, the estimated ATTs will reflect selection bias rather than the effects of housing instability. In our Child File data, we do not have information on the residential choices of children and their families. To address this, we use data from SEER, which provides information on each U.S. county's population by age group. However, the SEER data are yearly, not monthly, data. As a result, we are unable to estimate the monthly ATTs, as in our main analysis. Instead, we estimate yearly ATTs using the extended pre-period and the extended post-period explained in Appendix E. For the outcome variable, we use the county-level child proportion, which is the ratio of the child population to the total county population.[12] Overall, results presented in Appendix Table 9 (estimates of the overall ATT for the post-period) and Appendix Figure A13 (yearly ATT estimates from the pre-period to the post-period) suggest little evidence of children (of various age groups) moving between the treatment and control groups, given that all ATTs are statistically insignificant and all estimates are close to zero.

[12] We use the proportion instead of the child population as a way of standardizing the dependent variable to facilitate numerical optimization used by the CS estimator. If there are children moving from County A to County B, the child proportion in County B will increase, since both the numerator and the denominator of the ratio increase by the same amount (i.e., the number of children moving).

References

Bradford, A. C. and W. D. Bradford (2021). "The Effect of State and Local Housing Policies on County-Level Rent and Evictions in the United States, 2004–2016." Available at SSRN: https://dx.doi.org/10.2139/ssrn.3623318 (accessed in May 2024).

Callaway, B. and P. H. C. Sant'Anna (2021). "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics* 225 (2): 200–230.

Children's Bureau (2013). "Child Maltreatment 2013." U.S. Department of Health & Human Services. Retrieved from https://www.acf.hhs.gov/cb/report/child-maltreatment-2013 in August 2024.

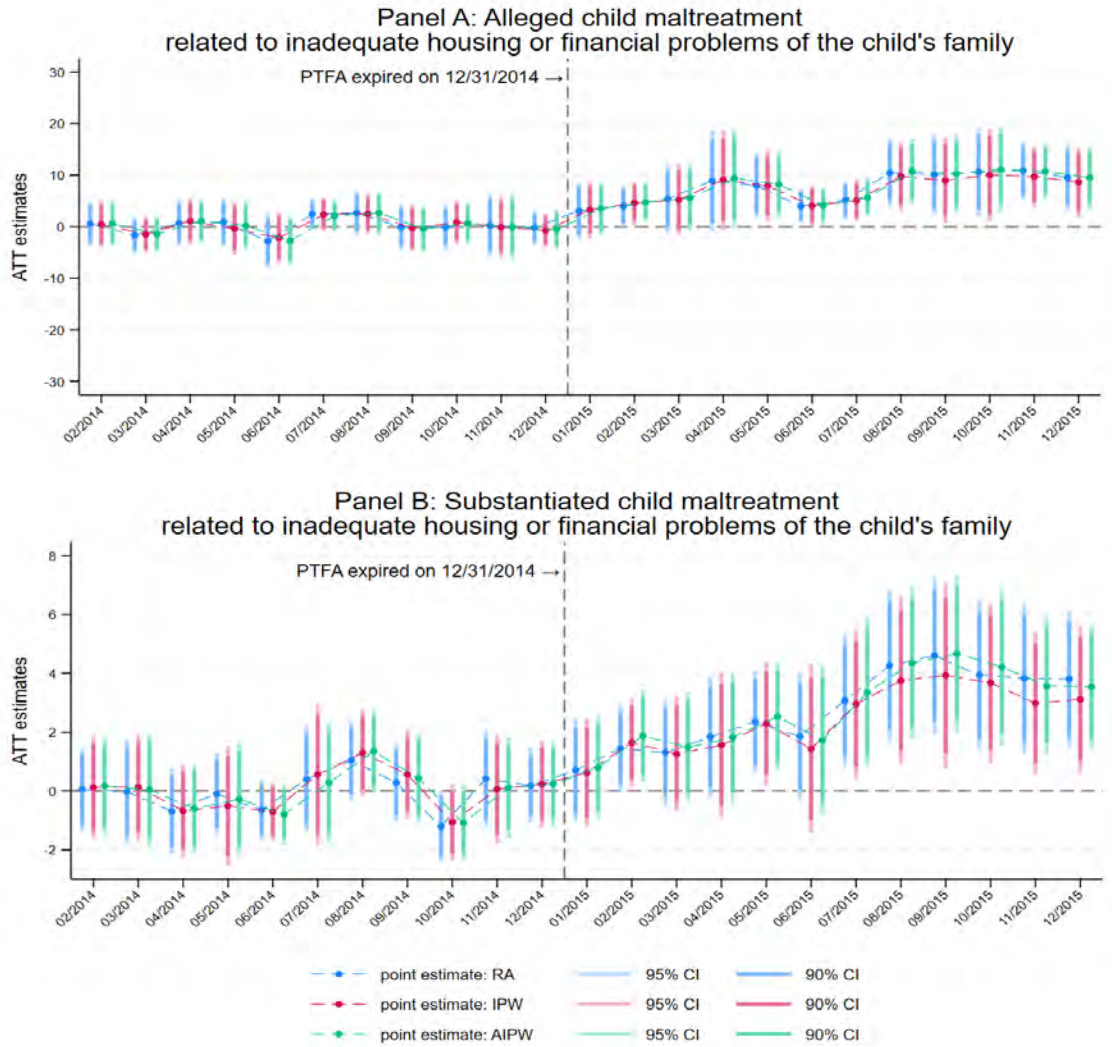
Children's Bureau (2014). "Child Maltreatment 2014." U.S. Department of Health & Human Services. Retrieved from https://www.acf.hhs.gov/cb/report/child-maltreatment-2014 in August 2024.

Hatch, M. E. (2017). "Statutory Protection for Renters: Classification of State Landlord-Tenant Policy Approaches." *Housing Policy Debate* 27 (1): 98–119.

National Law Center on Homelessness & Poverty (NLCHP) (2012). "Eviction (Without) Notice: Renters and the Foreclosure Crisis." Retrieved from https://homelesslaw.org/wp-content/uploads/2018/10/Eviction_Without_Notice.pdf in July 2024.

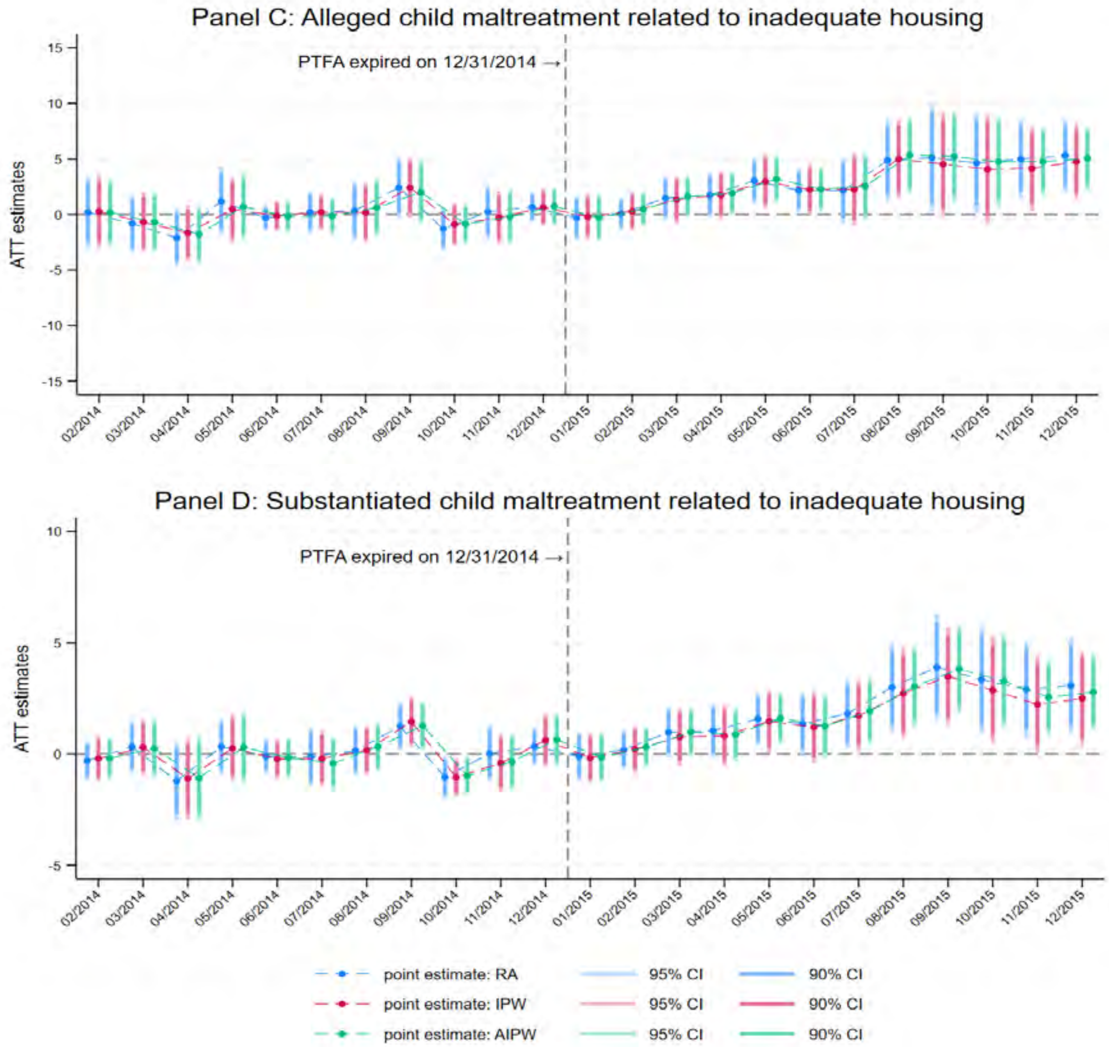
National Low Income Housing Coalition (NLIHC) (2015). "Protecting Tenants at Foreclosure Act." Retrieved from https://nlihc.org/sites/default/files/FactSheet_PTFA_2015.pdf in July 2024.

Roth, J., P. H. C. Sant'Anna, A. Bilinski and J. Poe (2023). "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature." *Journal of Econometrics* 235 (2): 2218–2244.



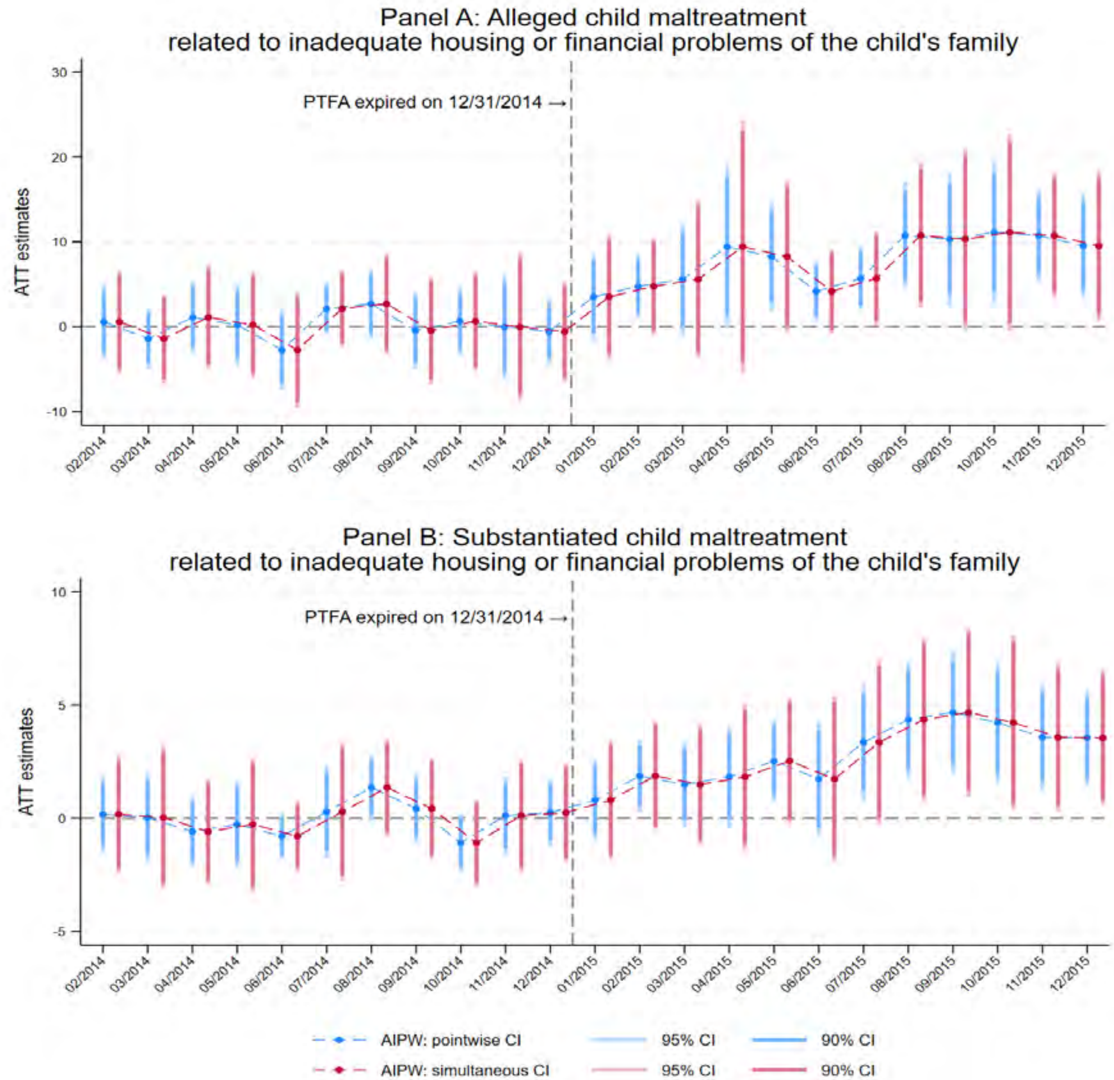
Appendix Figure A1: DID Estimates for Child Maltreatment Using Alternative Versions of the Callaway and Sant'Anna (2021) Estimator

Notes: See Figure 2. All estimations include the covariates explained in Figure 2's notes. In the labels used for the versions of the Callaway and Sant'Anna (2021) estimator, RA stands for regression adjustment; IPW stands for inverse probability weighting; and AIPW stands for augmented inverse probability weighting.



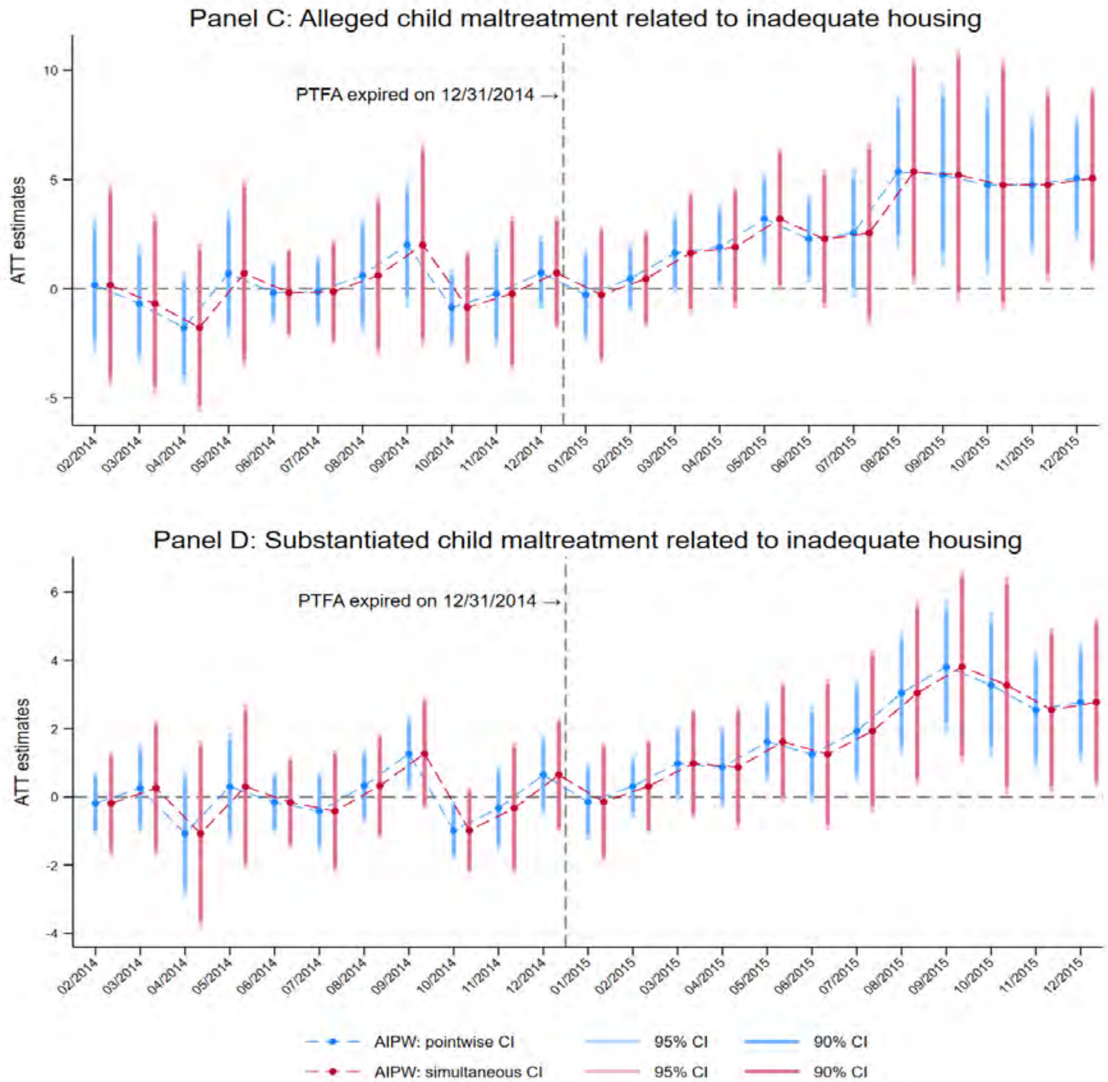
Appendix Figure A1 (cont.): DID Estimates for Child Maltreatment Using Alternative Versions of the Callaway and Sant'Anna (2021) Estimator

Notes: See Figure 2. All estimations include the covariates explained in Figure 2's notes. In the labels used for the versions of the Callaway and Sant'Anna (2021) estimator, RA stands for regression adjustment; IPW stands for inverse probability weighting; and AIPW stands for augmented inverse probability weighting.



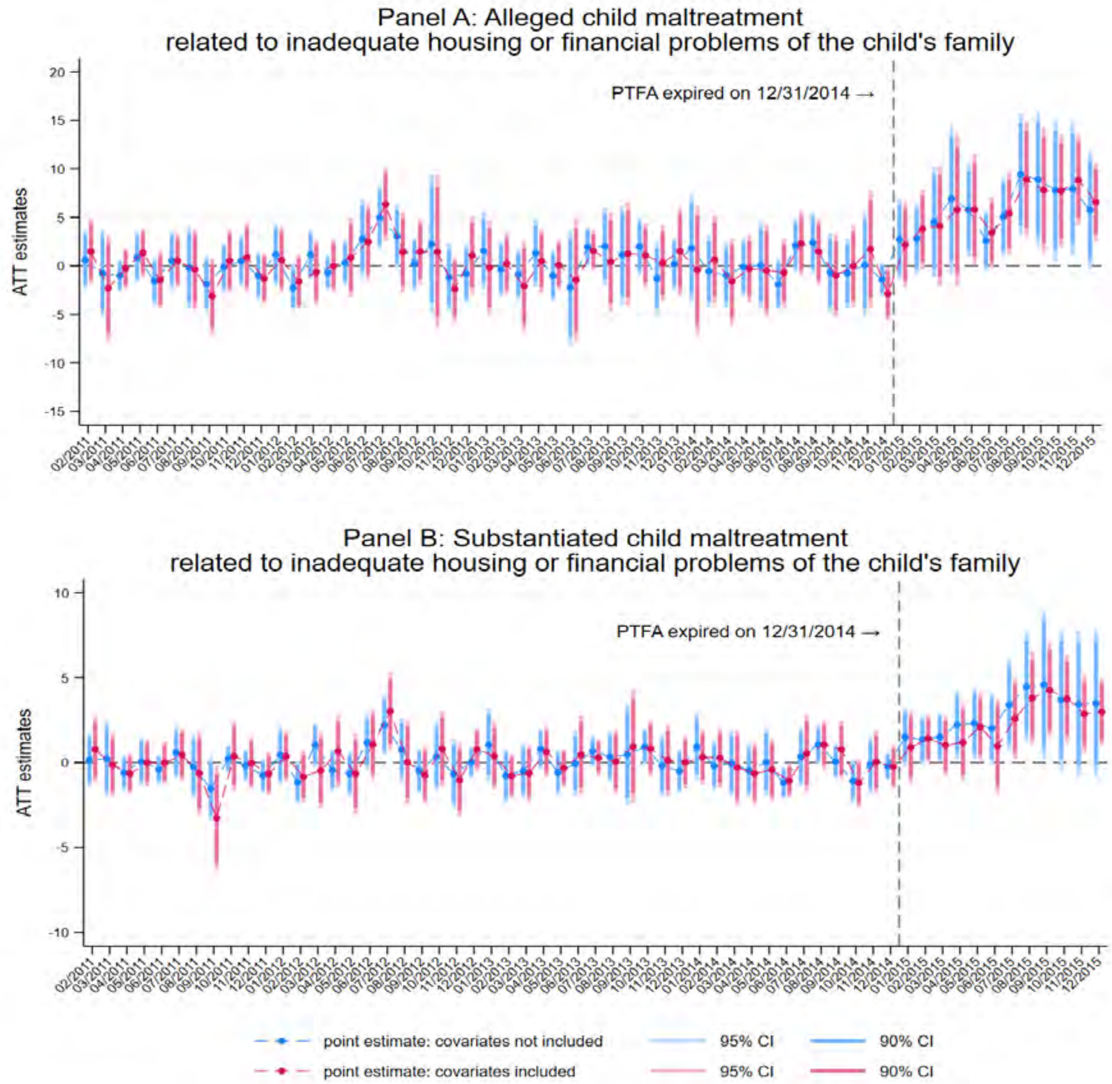
Appendix Figure A2: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family with Simultaneous Confidence Intervals (CIs)

Notes: See Figure 2. All estimations include the covariates explained in Figure 2's notes. All estimations use the augmented inverse probability weighting (AIPW) version of the Callaway and Sant'Anna (2021) Estimator.



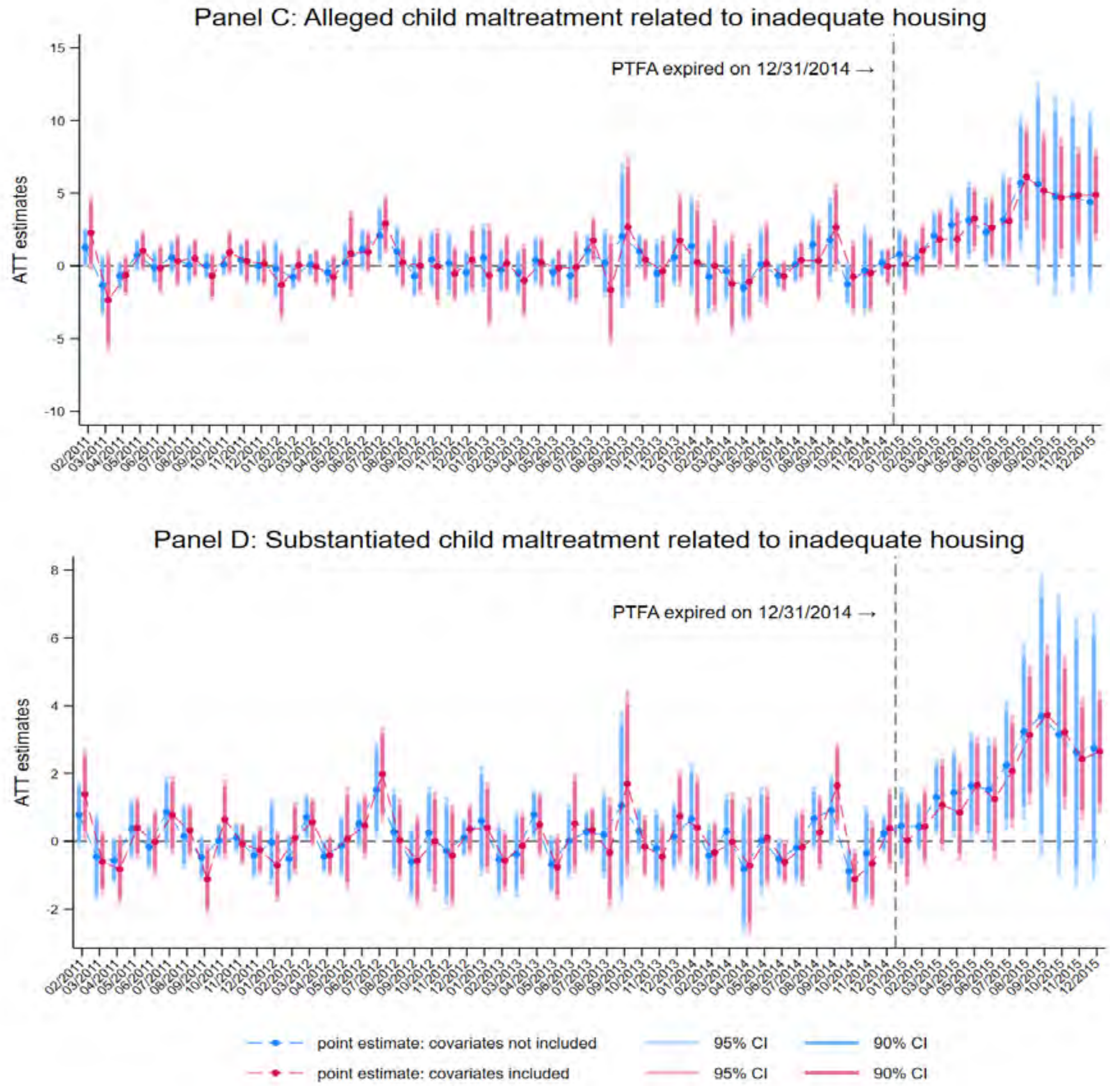
Appendix Figure A2 (cont.): DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family with Simultaneous Confidence Intervals (CIs)

Notes: See Figure 2. All estimations include the covariates explained in Figure 2's notes. All estimations use the augmented inverse probability weighting (AIPW) version of the Callaway and Sant'Anna (2021) Estimator.



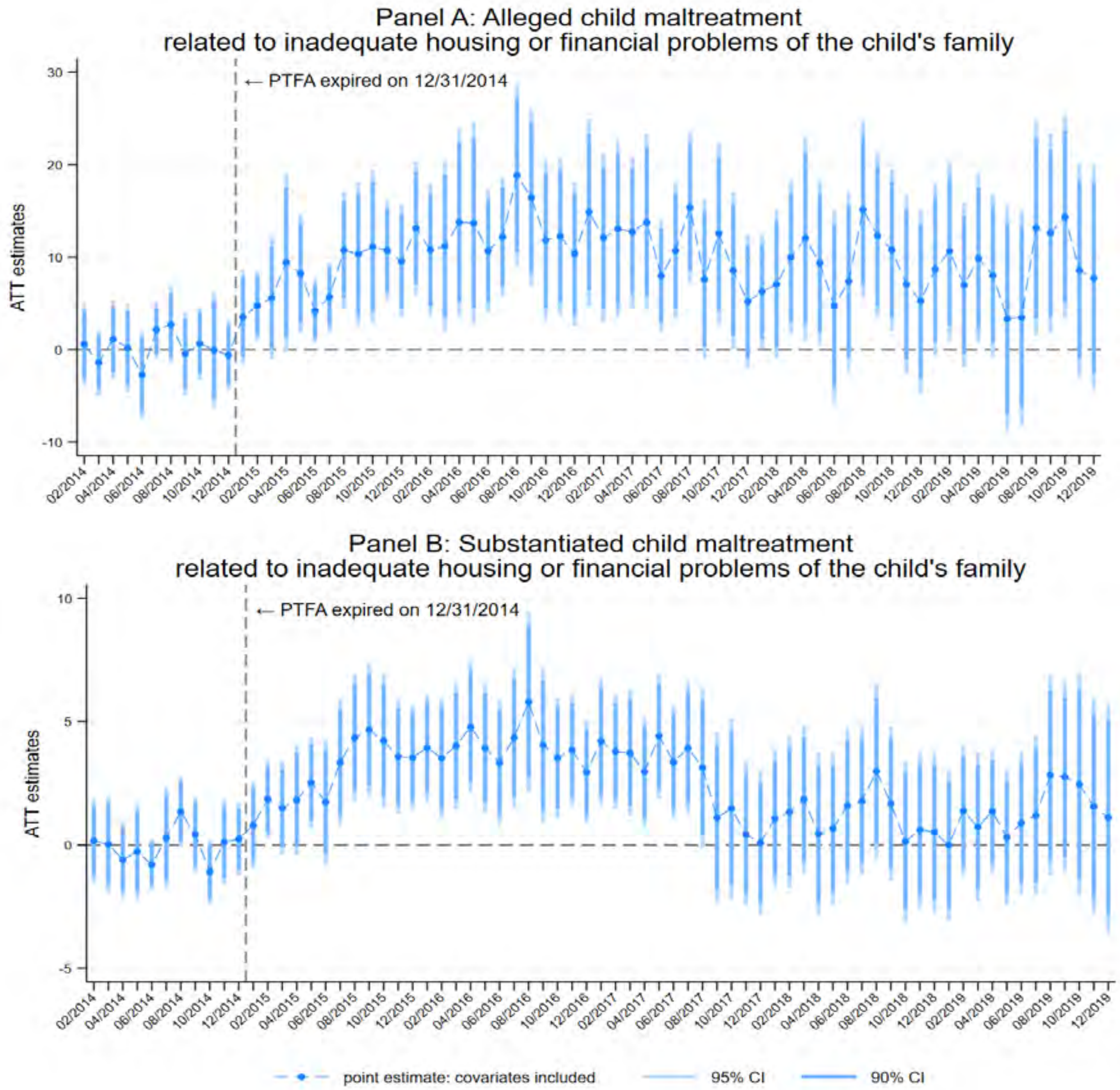
Appendix Figure A3: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family with Extended Pre-Treatment Period

Notes: See Figure 2 except that: 1) the sample period is January 2011 through December 2015; 2) the state of Florida was dropped from the sample in (and only in) the pre-period, which goes from January 2011 to December 2014. Detailed explanations for this exclusion were provided in Appendix D.



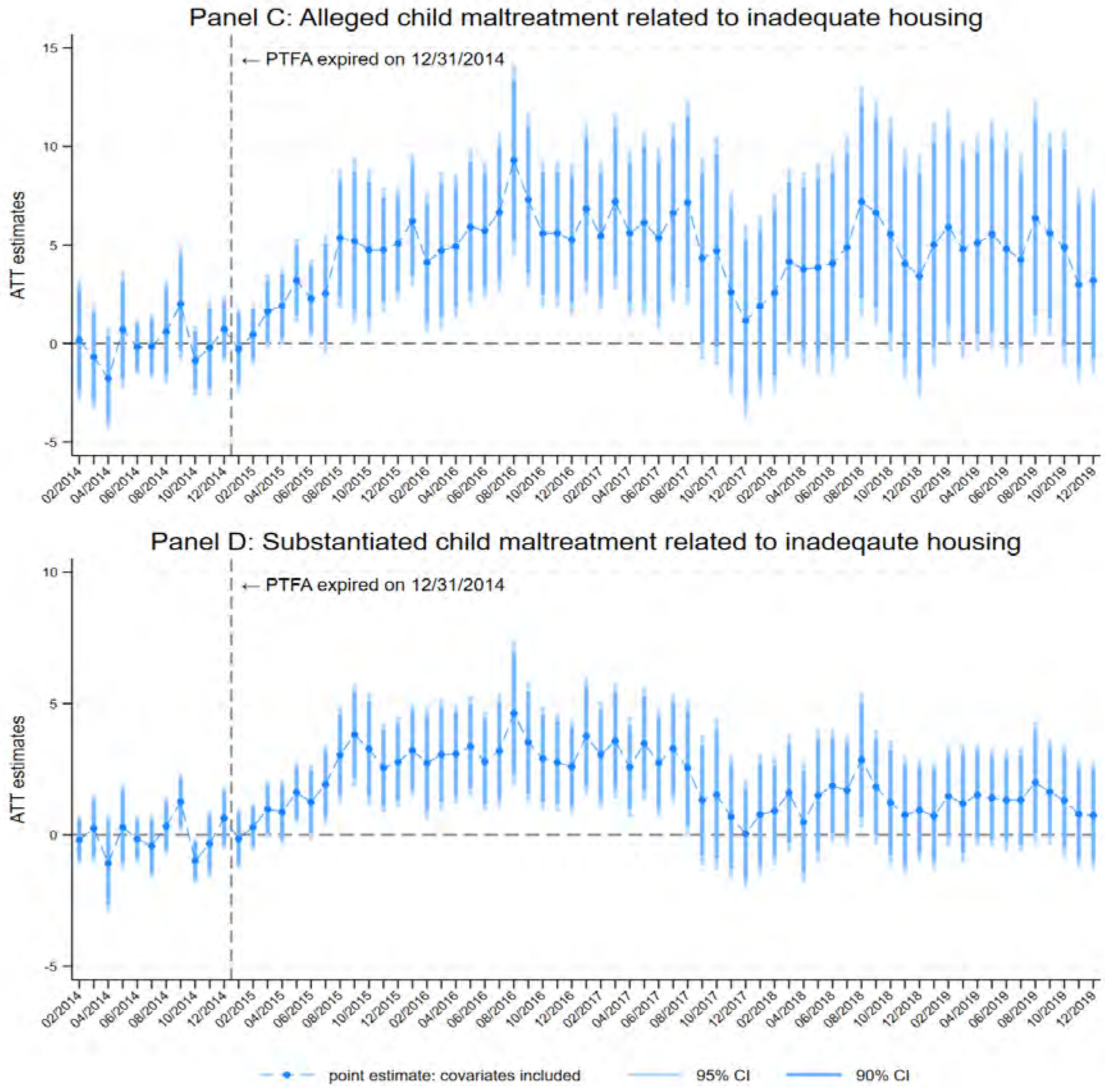
Appendix Figure A3 (cont.): DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family with Extended Pre-Treatment Period

Notes: See Figure 2 except that: 1) the sample period is January 2011 through December 2015; 2) the state of Florida was dropped from the sample in (and only in) the pre-period, which goes from January 2011 to December 2014. Detailed explanations for this exclusion were provided in Appendix D.



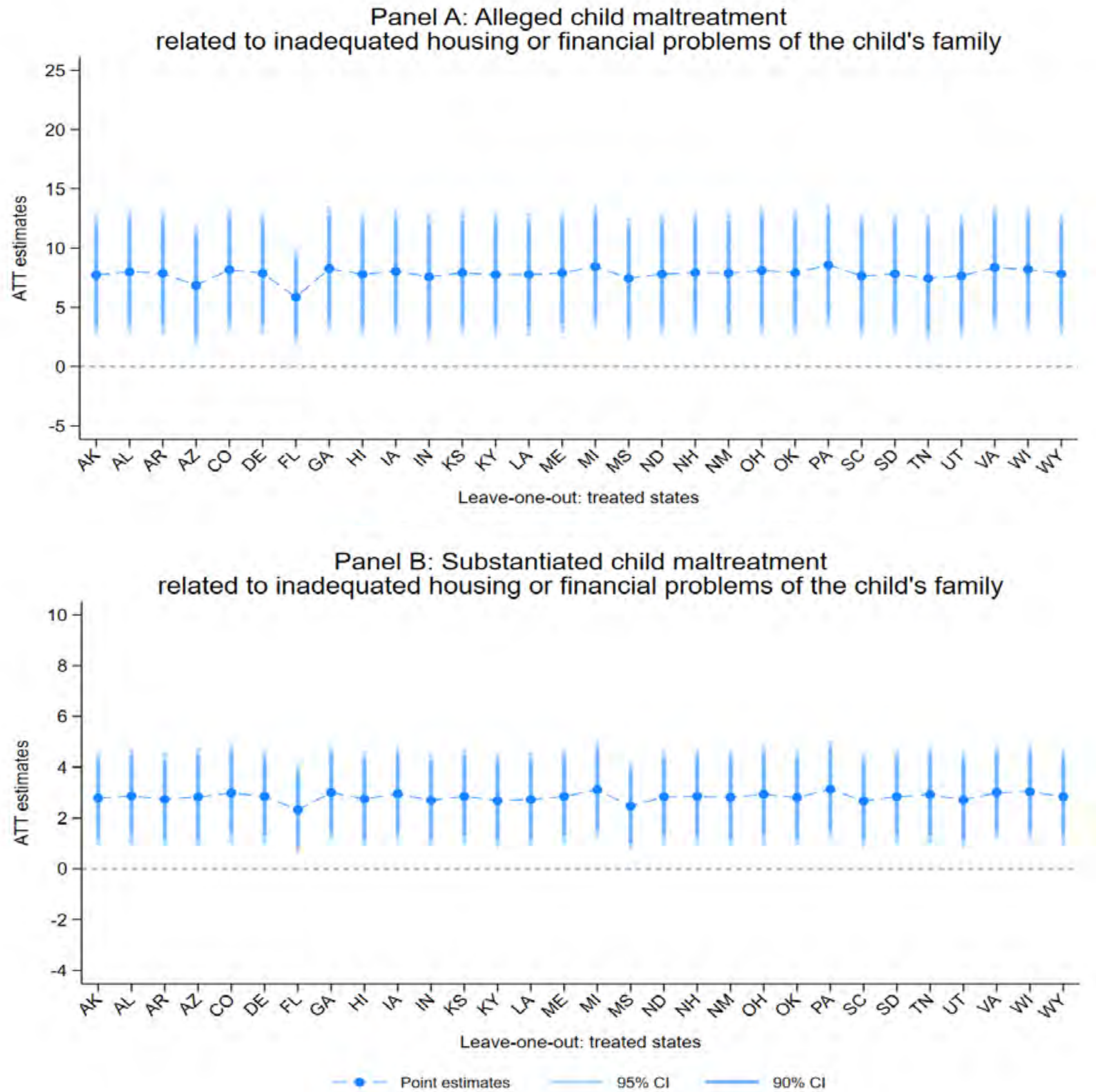
Appendix Figure A4: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family with Extended Post-Treatment Period

Notes: See Figure 2 except that the sample period is January 2014 through December 2019. All estimations include the covariates explained in Figure 2's notes.



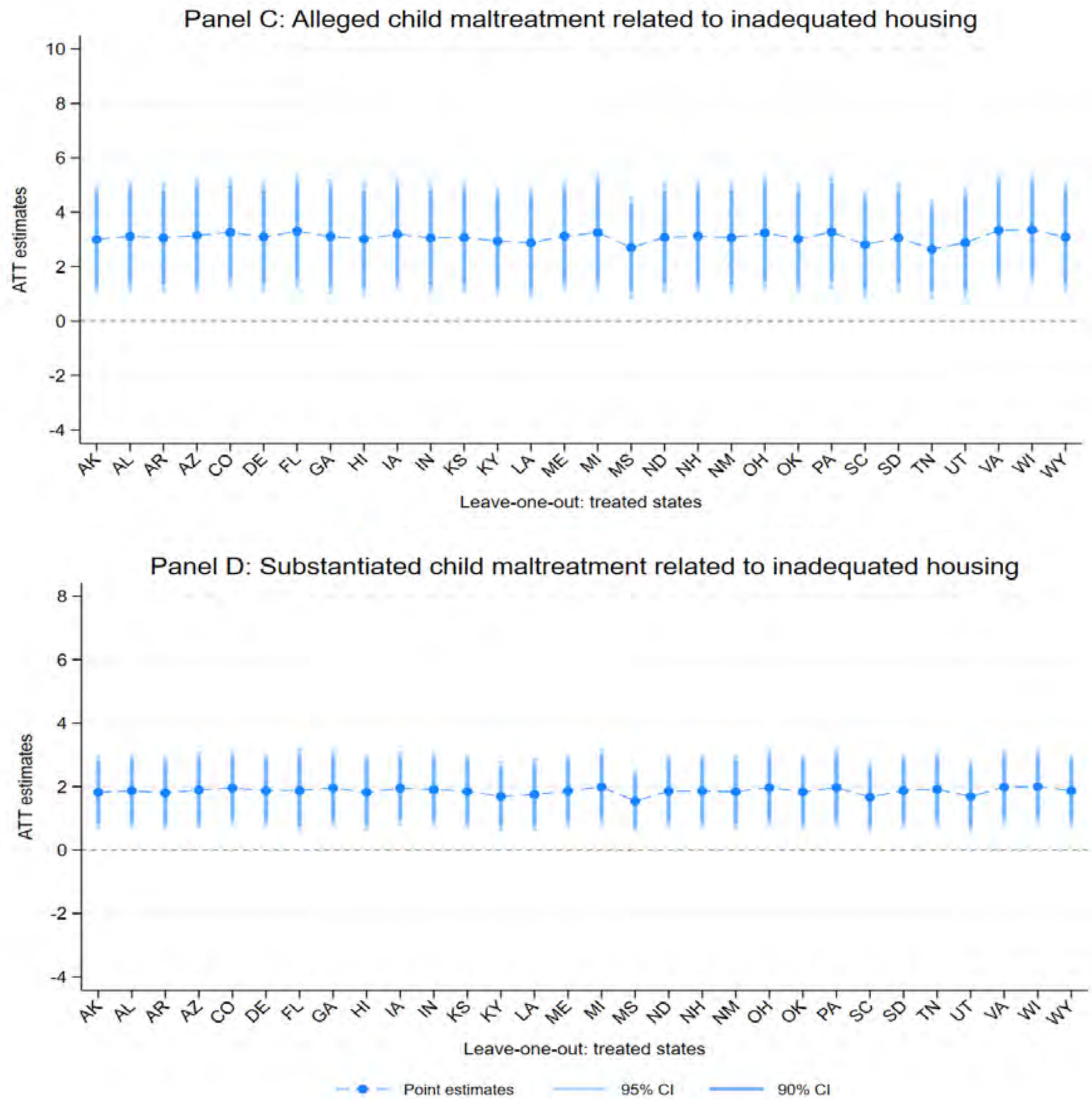
Appendix Figure A4 (cont.): DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family with Extended Post-Treatment Period

Notes: See Figure 2 except that the sample period is January 2014 through December 2019. All estimations include the covariates explained in Figure 2's notes.



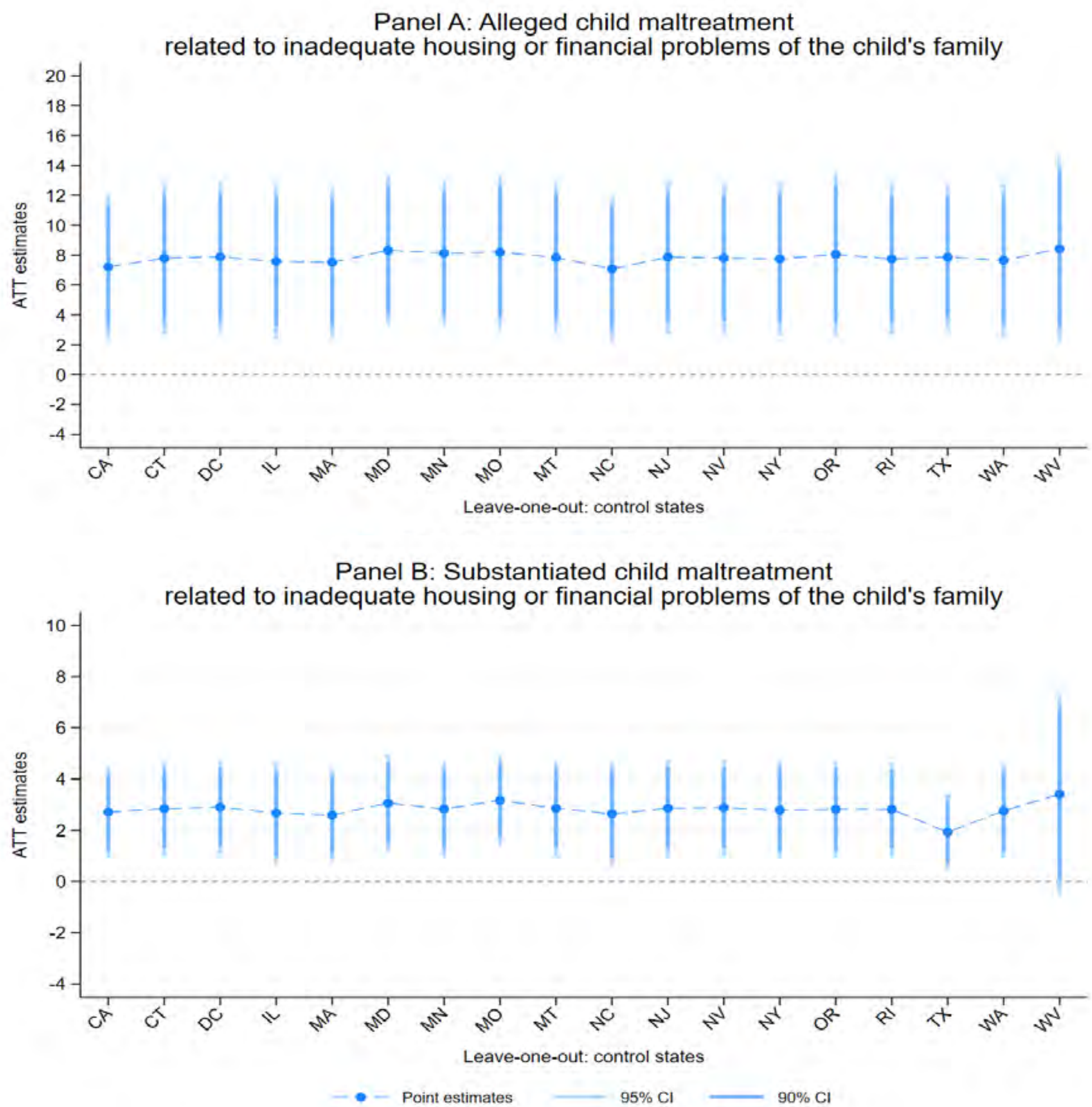
Appendix Figure A5: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family, Leaving Out One State in the Treatment Group

Notes: See Figure 2. All estimations include the covariates explained in Figure 2's notes.



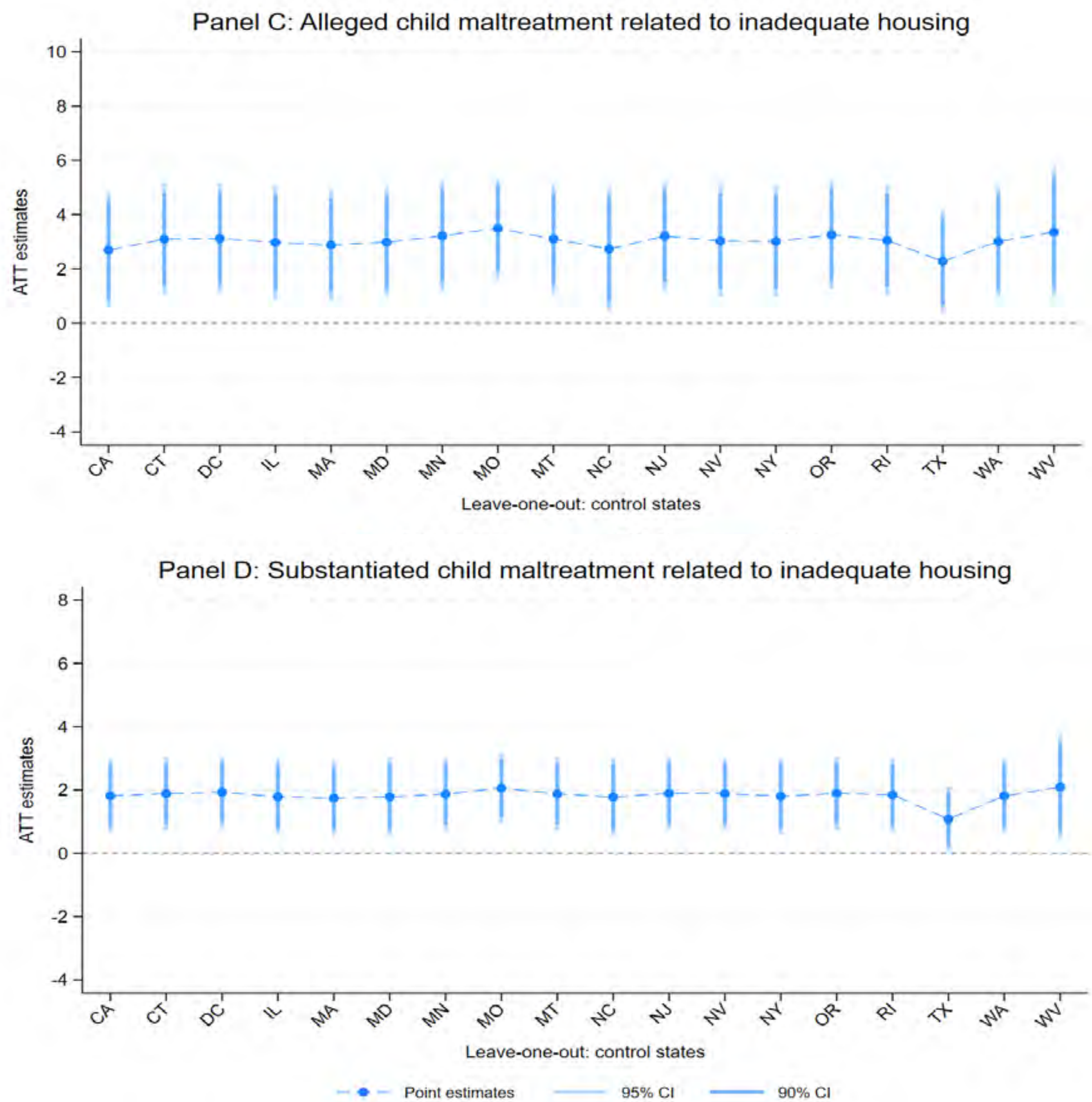
Appendix Figure A5 (cont.): DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family, Leaving Out One State in the Treatment Group

Notes: See Figure 2. All estimations include the covariates explained in Figure 2's notes.



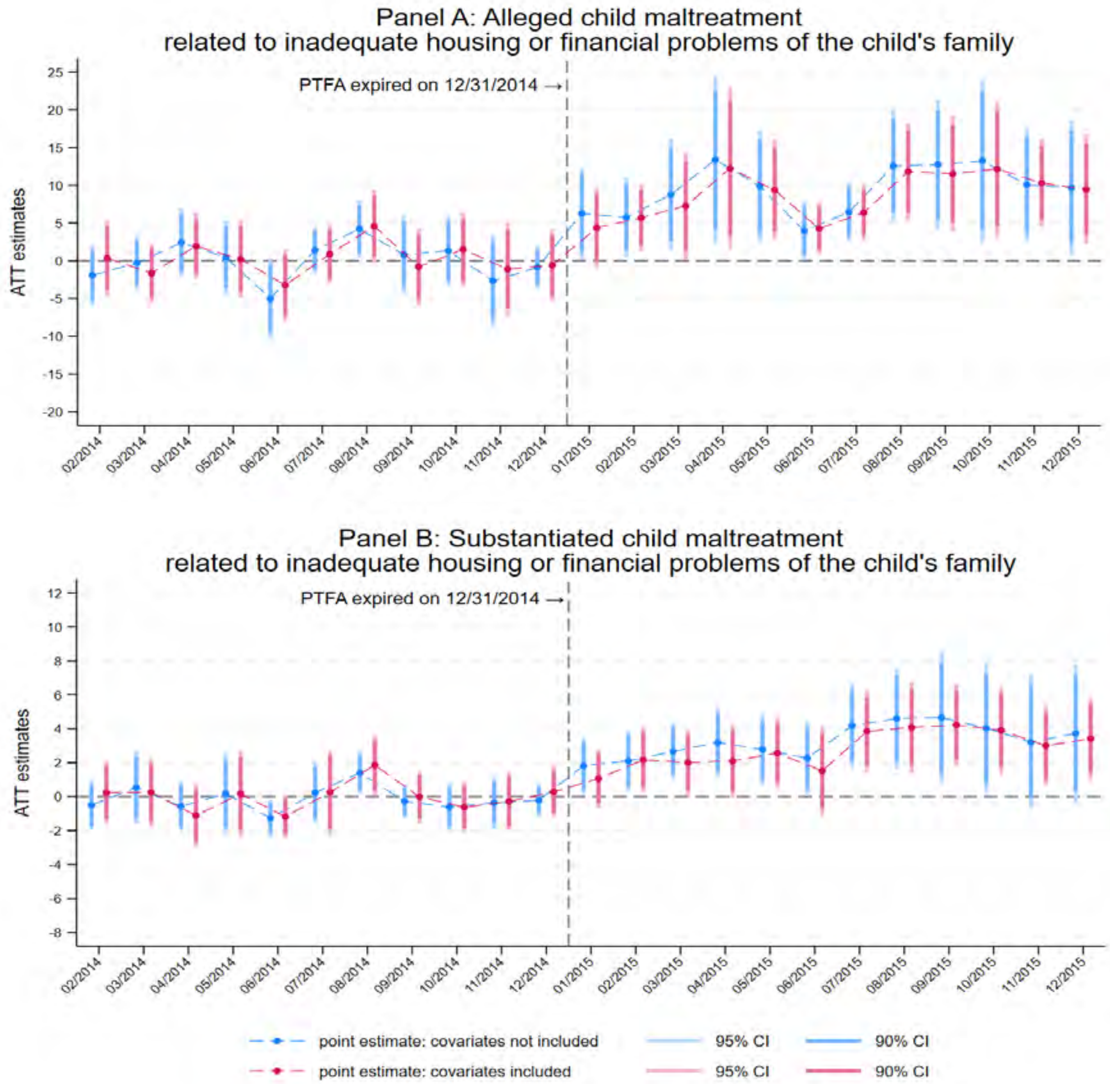
Appendix Figure A6: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family, Leaving Out One State in the Control Group

Notes: See Figure 2. All estimations include the covariates explained in Figure 2's notes.



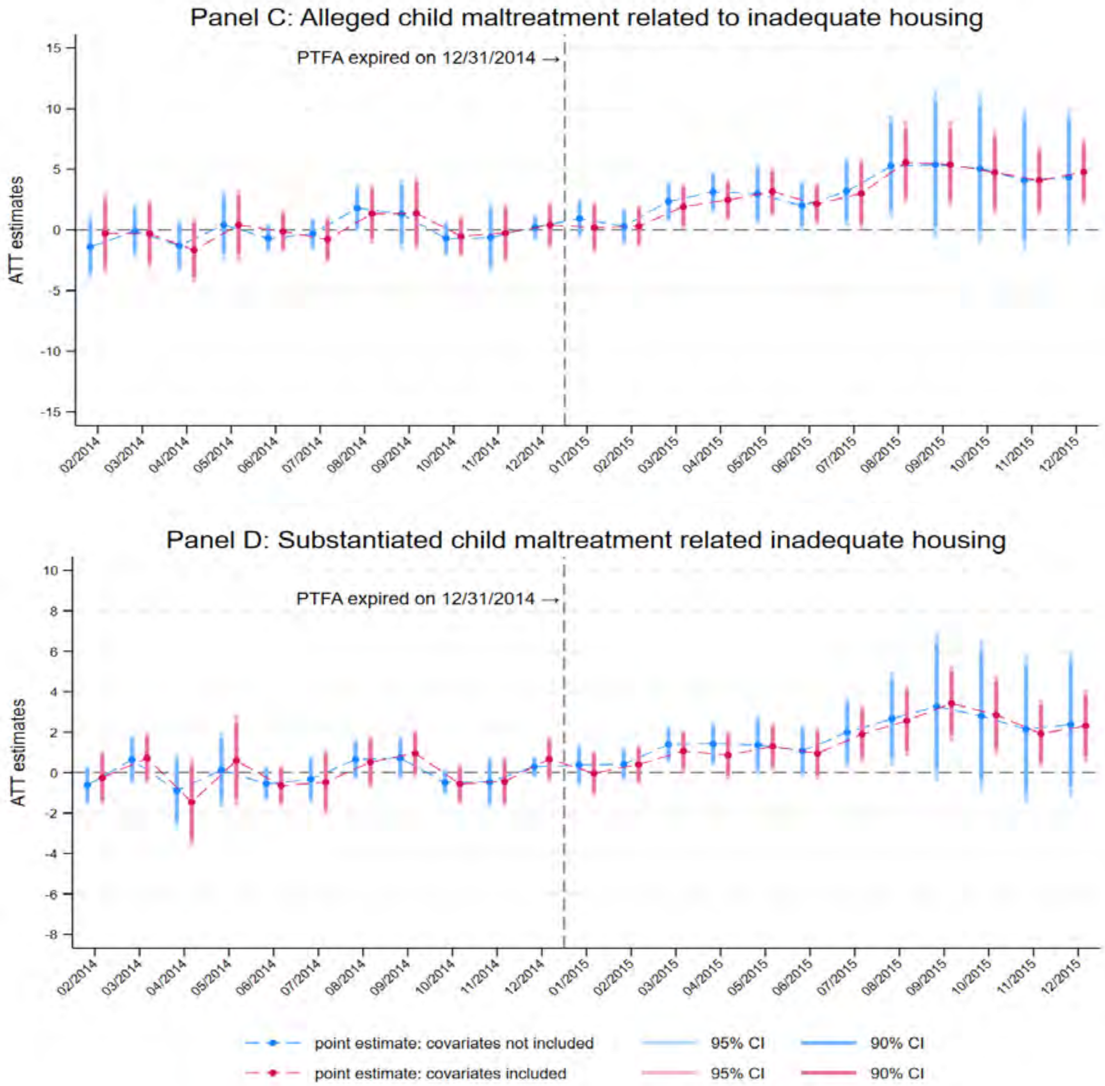
Appendix Figure A6 (cont.): DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family, Leaving Out One State in the Control Group

Notes: See Figure 2. All estimations include the covariates explained in Figure 2's notes.



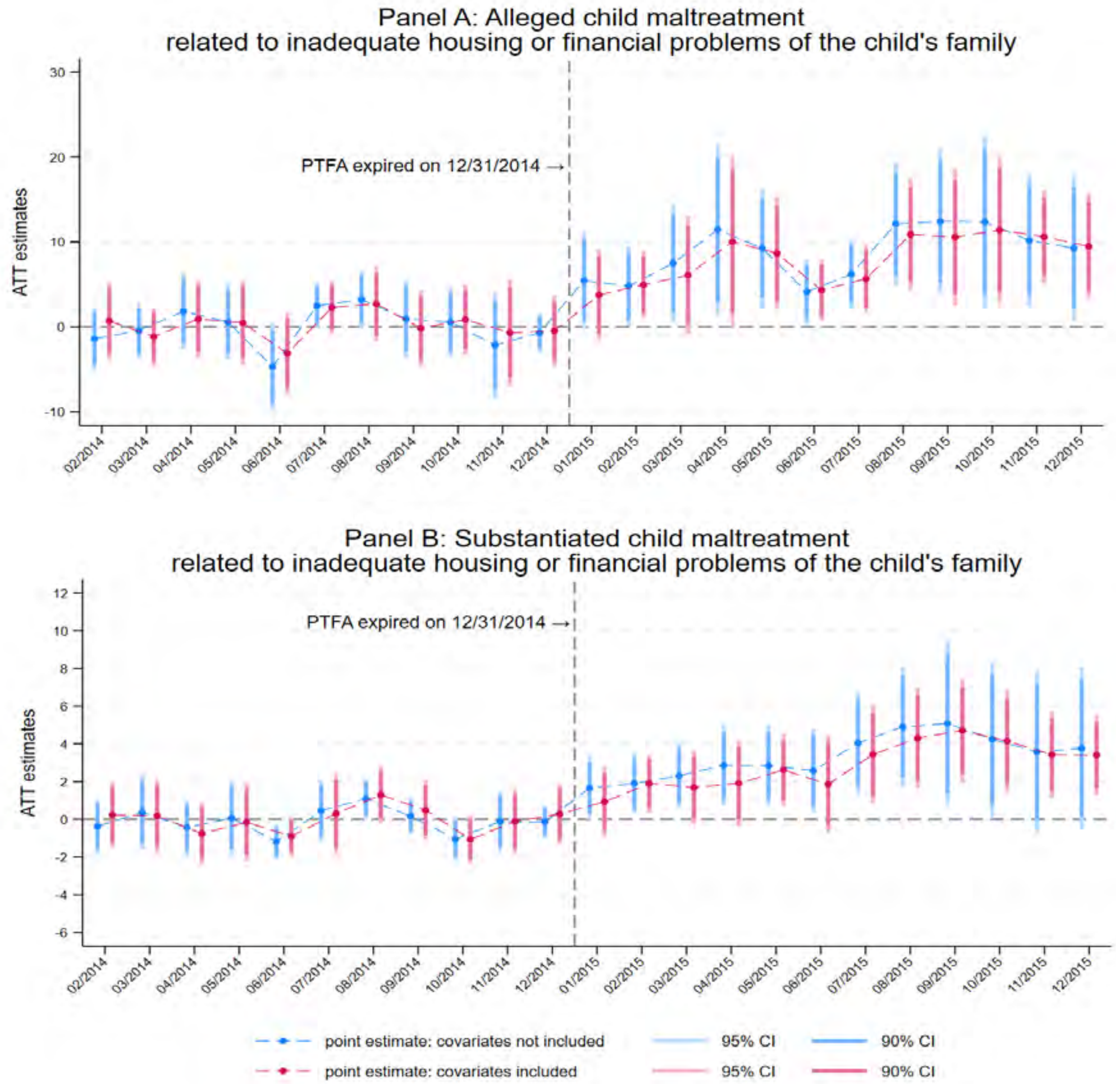
Appendix Figure A7: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family Based Only on Identified Counties in the NCANDS Child File Data

Notes: See Figure 2.



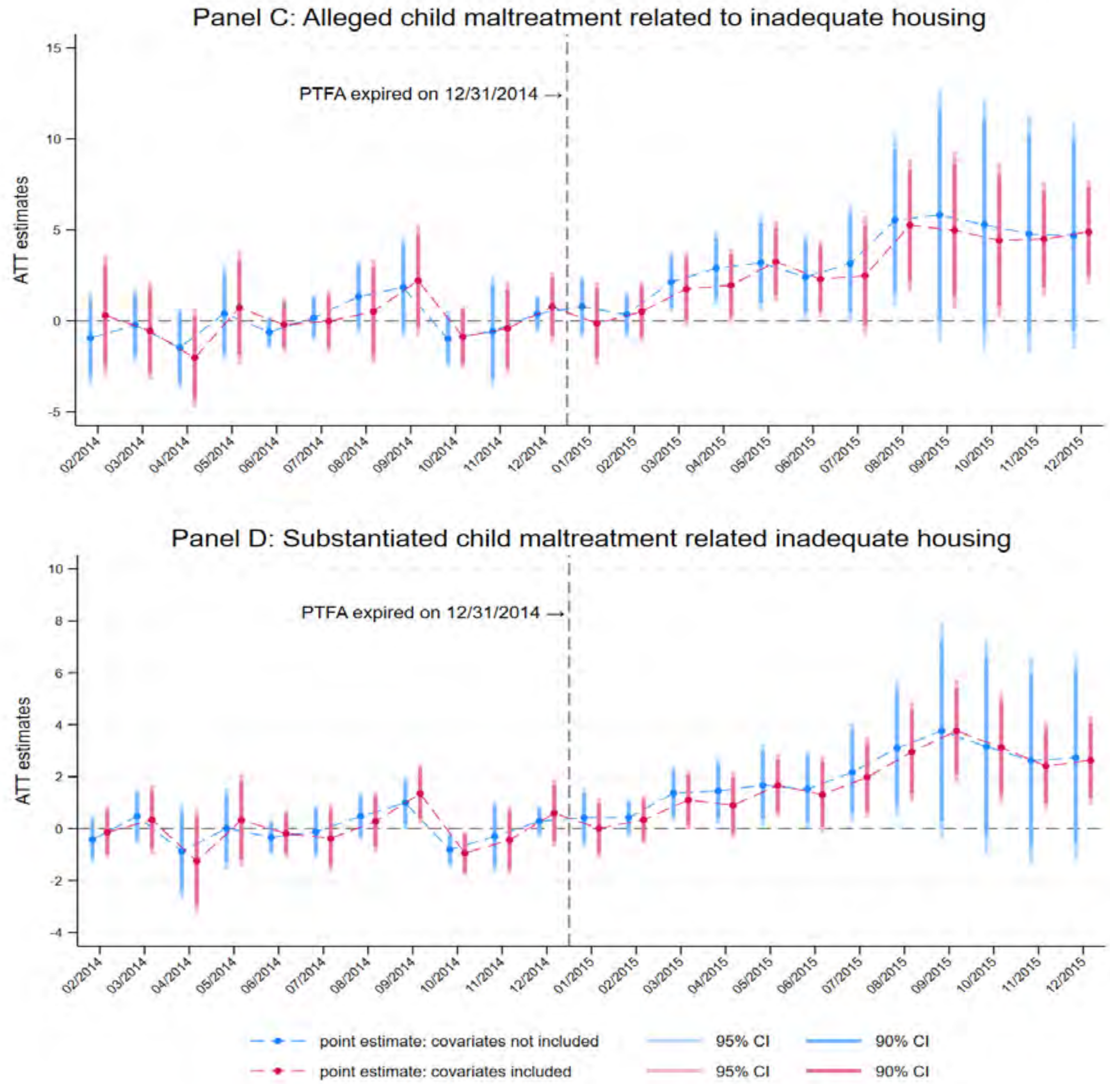
Appendix Figure A7 (cont.): DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family Based Only on Identified Counties in the NCANDS Child File Data

Notes: See Figure 2.



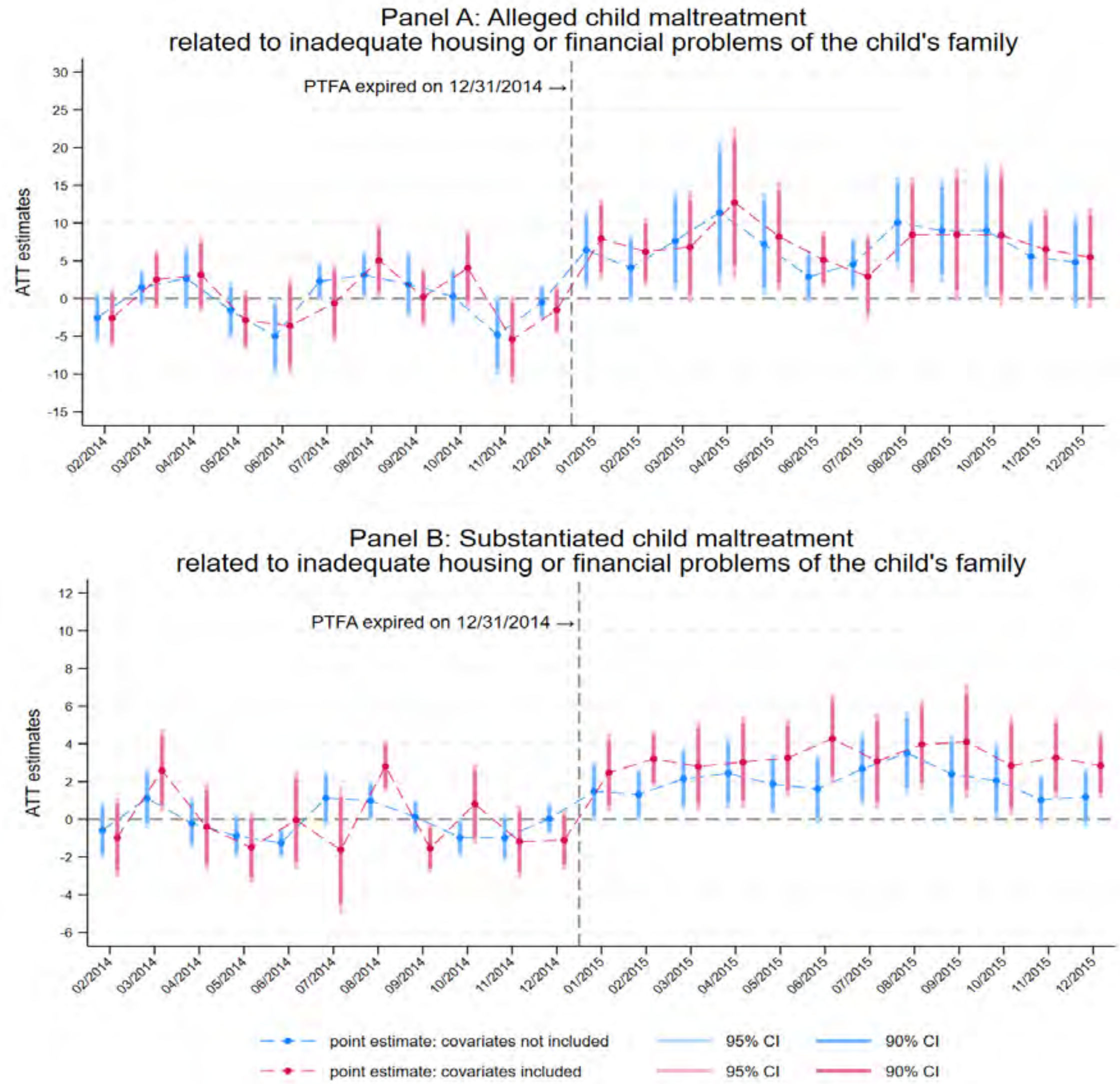
Appendix Figure A8a: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family Using the Alternative Definition for the Treatment Group

Notes: See Figure 2 except that we excluded the "3 days" and "5 days" states (shown in Figure 1) from the treatment group, while keeping the same control group.



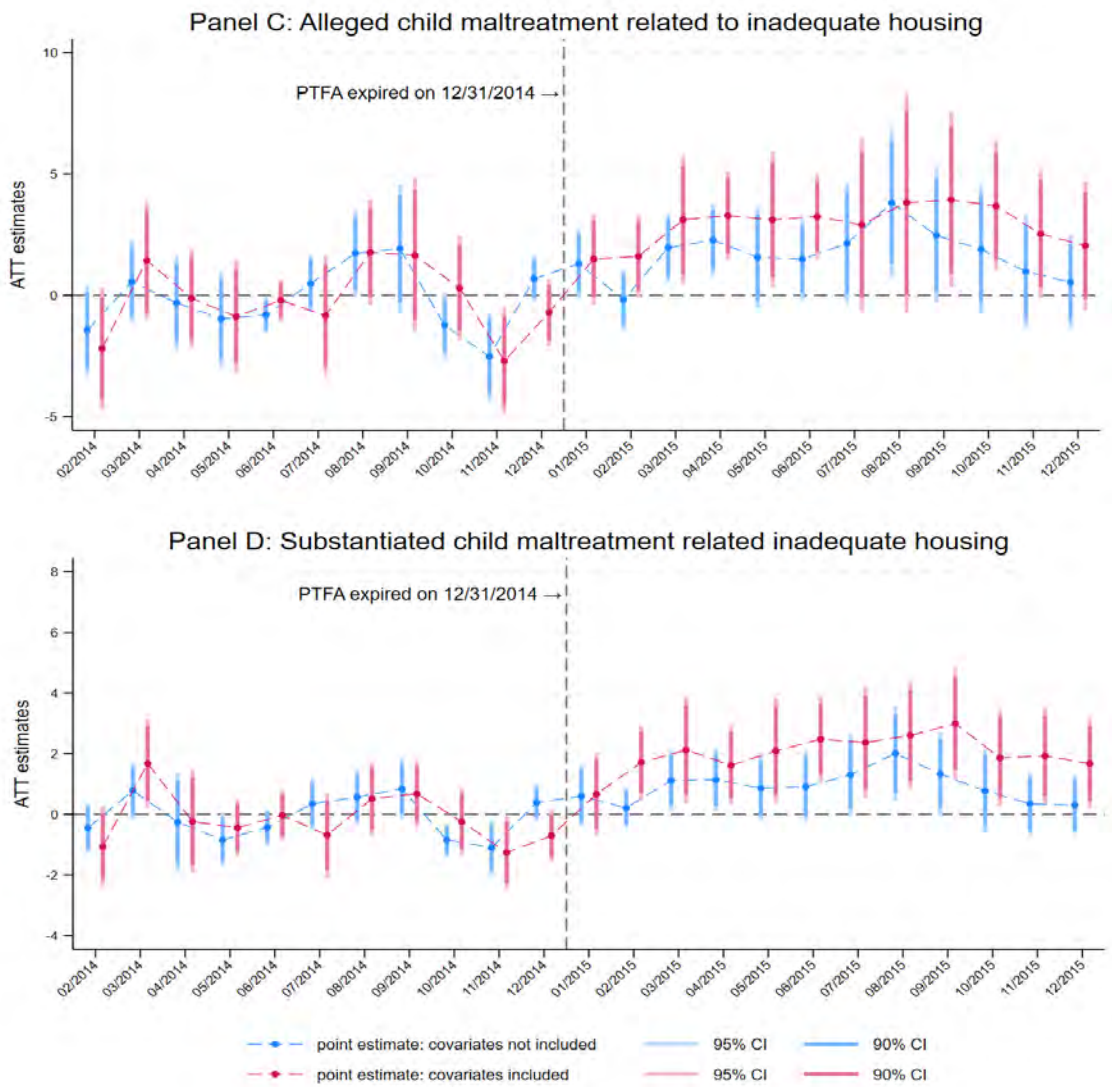
Appendix Figure A8a (cont.): DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family Using the Alternative Definition for the Treatment Group

Notes: See Figure 2 except that we excluded the "3 days" and "5 days" states (shown in Figure 1) from the treatment group, while keeping the same control group.



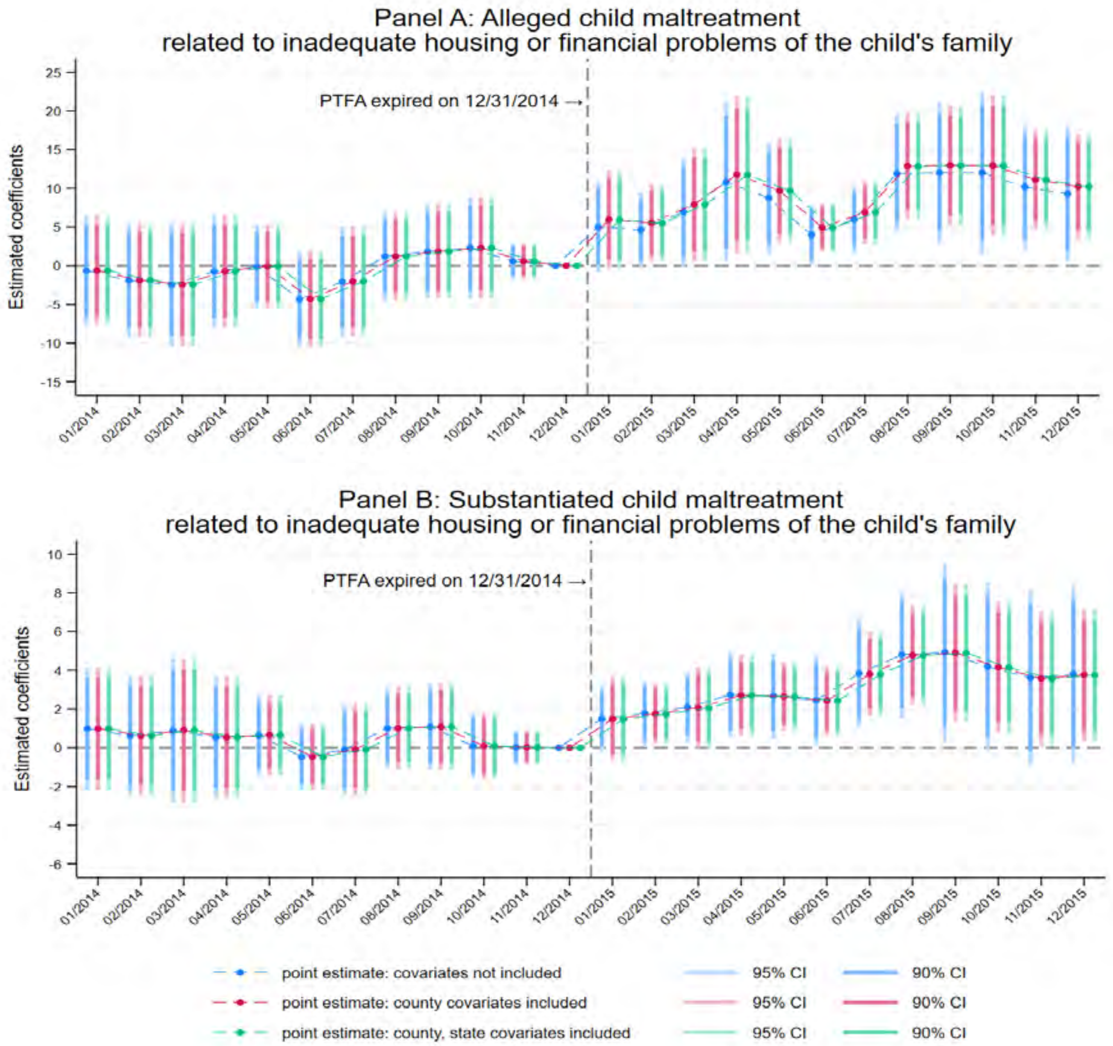
Appendix Figure A8b: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family Using the Alternative Definition for the Control Group

Notes: See Figure 2 except that we excluded the "10 days", "30 days", and "60 days" states (shown in Figure 1) from the control group, while keeping the same treatment group.



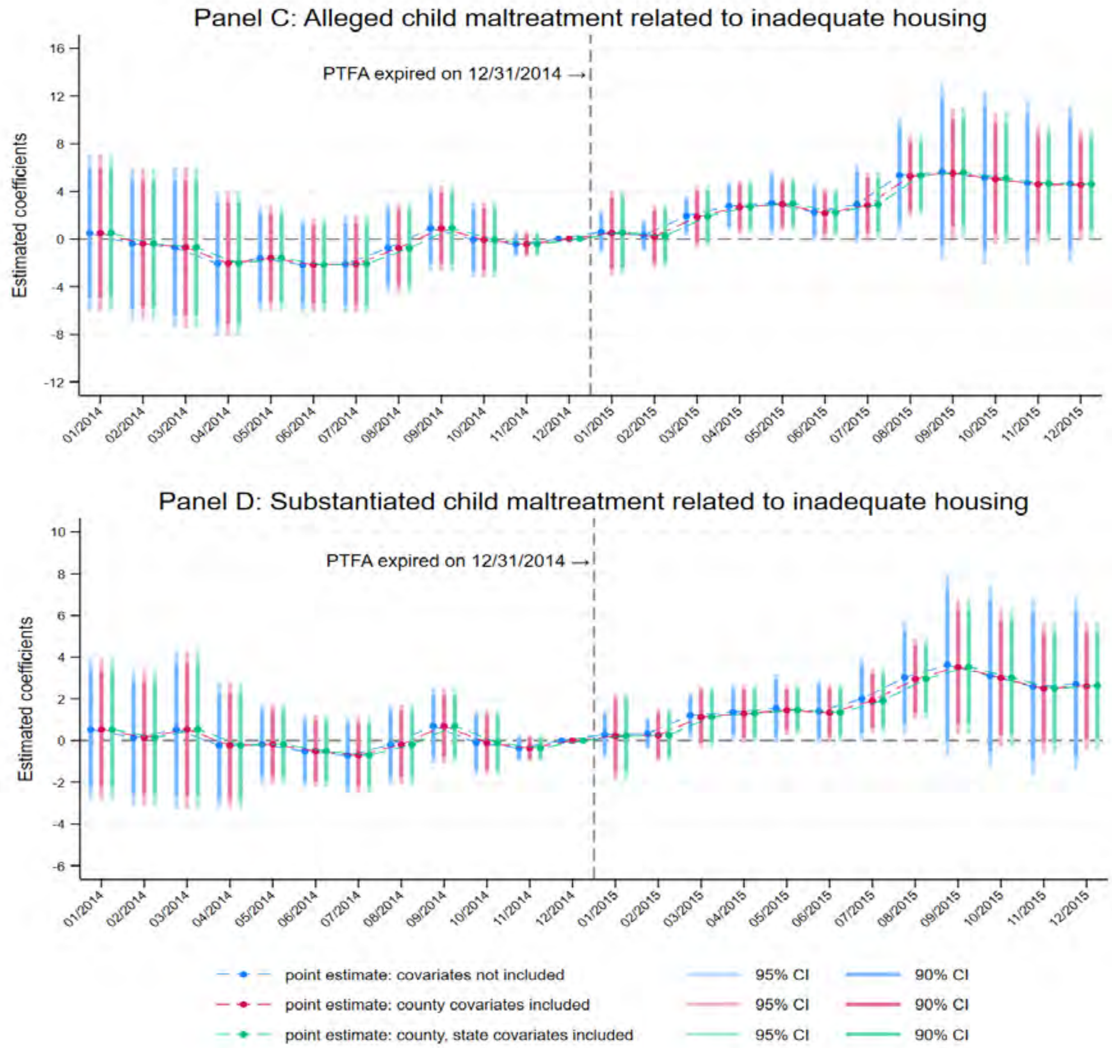
Appendix Figure A8b (cont.): DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family Using the Alternative Definition for the Control Group

Notes: See Figure 2 except that we excluded the "10 days", "30 days", and "60 days" states (shown in Figure 1) from the control group, while keeping the same treatment group.



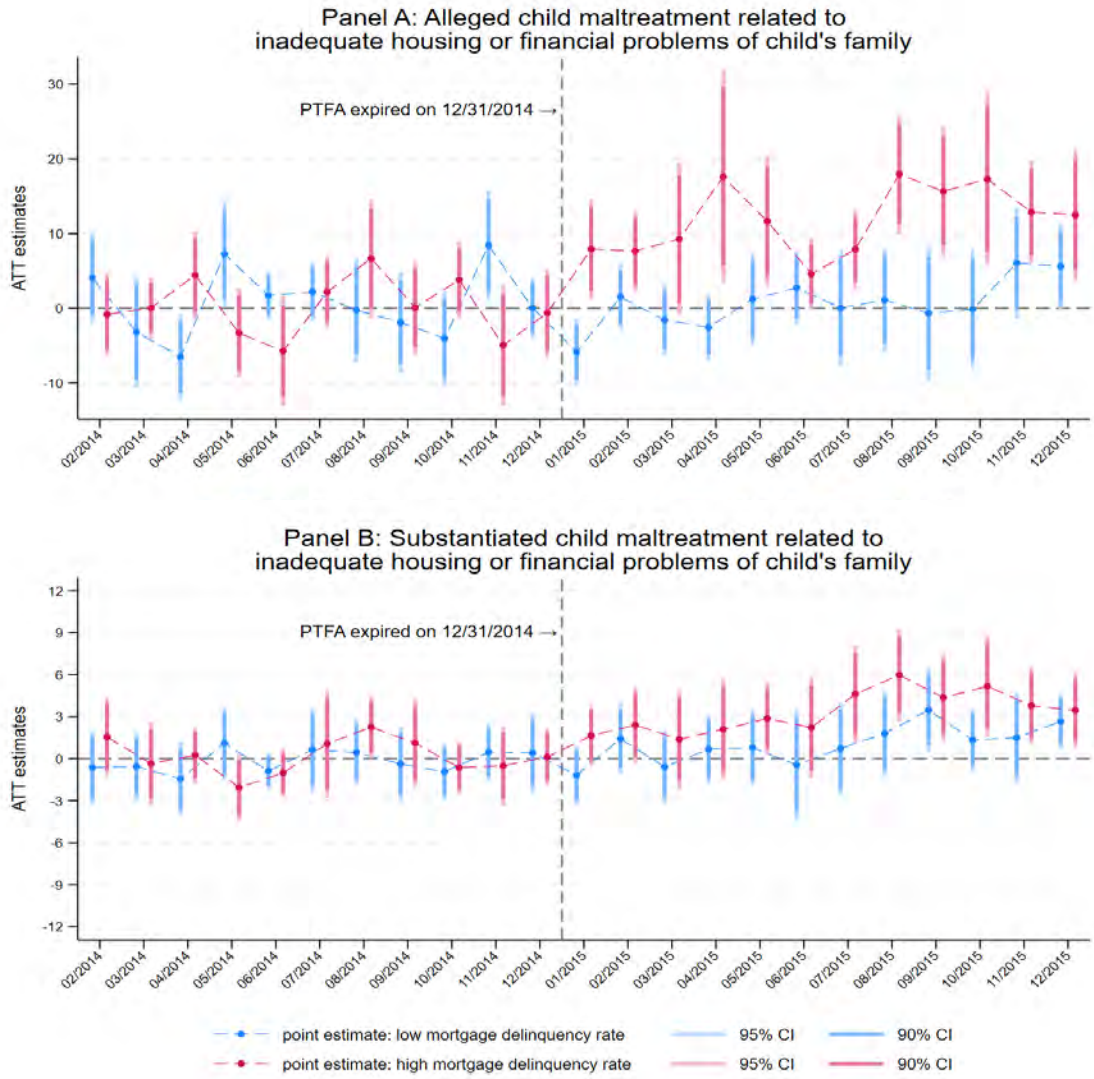
Appendix Figure A9: Event-Study Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family

Notes: The sample period is January 2014 through December 2015. All estimations use the ordinary least squares based on the dynamic two-way fixed-effect model described in Appendix E. The estimations include three cases: (a) covariates not included; (b) county covariates included; (c) county and state covariates included. In case (b), the covariates are the following variables interacted with the dummy variables for year: proportions of individuals aged 0–19 (measured in 2010 from SEER) in a county, proportions of individuals aged 20–64 (measured in 2010 from SEER) in a county, proportions of individuals whose race is white (measured in 2010 from SEER) in a county, proportions of individuals whose race is black (measured in 2010 from SEER) in a county, proportions of individuals who completed at least some college (2010–2014 ACS five-year estimates) in a county, proportions of families below poverty level (2010–2014 ACS five-year estimates) in a county, proportions of owner-occupied housing units with a mortgage (2010–2014 ACS five-year estimates) in a county, proportions of occupied units paying rent with GRAPI (gross rent as a percentage of household income) ≥ 25% (2010–2014 ACS five-year estimates) in a county, proportions of individuals (of the civilian noninstitutionalized population) without health insurance coverage (2010–2014 ACS five-year estimates) in a county, proportions of county population living in rural areas as of the 2010 census, an indicator (1/0) for county population (measured in 2010 from SEER) < 25,000, an indicator (1/0) for 25,000 ≤ county population (measured in 2010 from SEER) < 50,000, an indicator (1/0) for 50,000 ≤ county population (measured in 2010 from SEER) < 100,000, and an indicator (1/0) for 100,000 ≤ county population (measured in 2010 from SEER) < 250,000. In case (c), the covariates are those used in case (b) plus the variable on the year when a state passed the state fair housing law protecting some groups from eviction. Standard errors (reported in parentheses) are clustered at the state level.



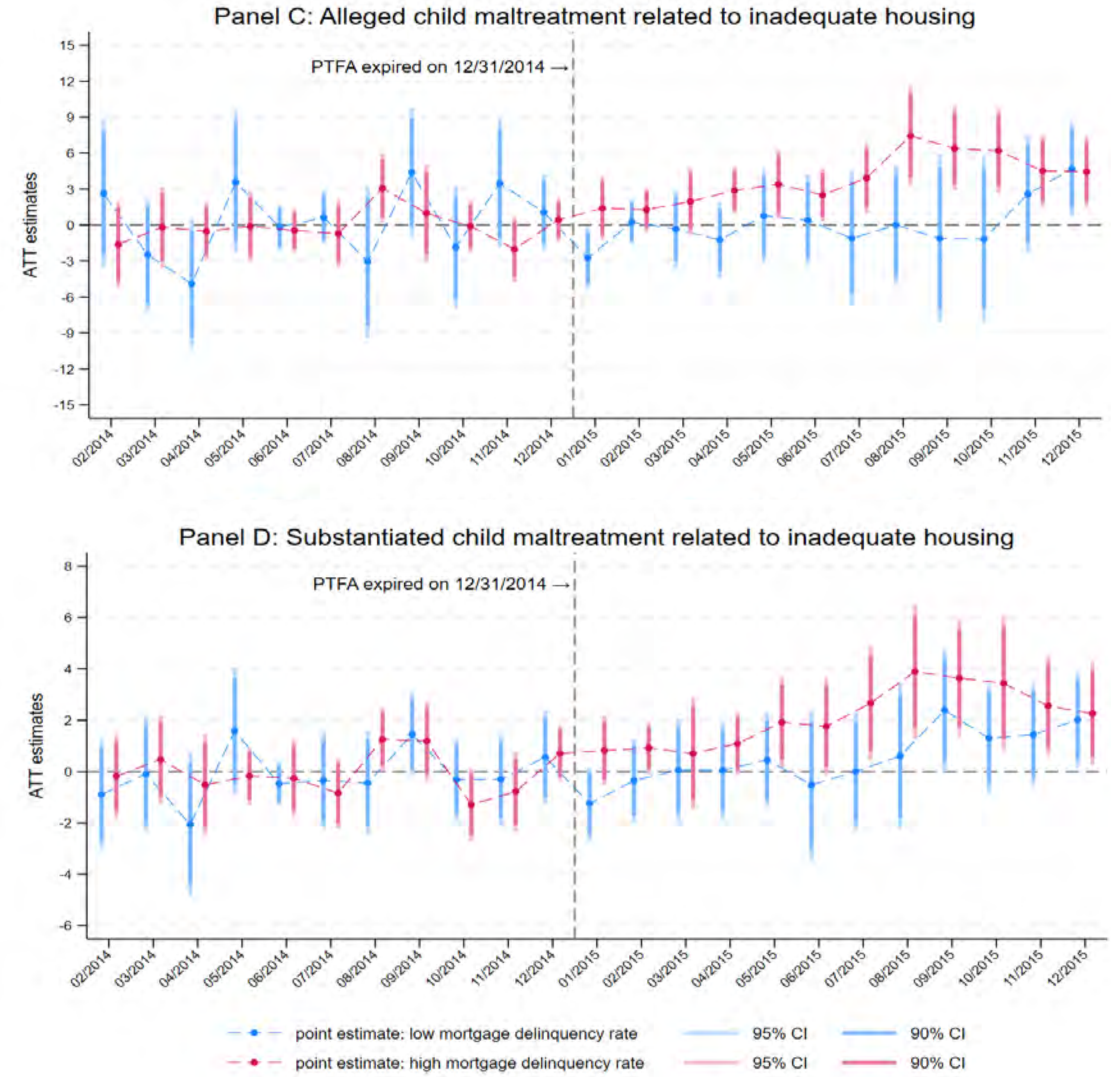
Appendix Figure A9 (cont.): Event-Study Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family

Notes: The sample period is January 2014 through December 2015. All estimations use the ordinary least squares based on the dynamic two-way fixed-effect model described in Appendix E. The estimations include three cases: (a) covariates not included; (b) county covariates included; (c) county and state covariates included. In case (b), the covariates are the following variables interacted with the dummy variables for year: proportions of individuals aged 0–19 (measured in 2010 from SEER) in a county, proportions of individuals aged 20–64 (measured in 2010 from SEER) in a county, proportions of individuals whose race is white (measured in 2010 from SEER) in a county, proportions of individuals whose race is black (measured in 2010 from SEER) in a county, proportions of individuals who completed at least some college (2010–2014 ACS five-year estimates) in a county, proportions of families below poverty level (2010–2014 ACS five-year estimates) in a county, proportions of owner-occupied housing units with a mortgage (2010–2014 ACS five-year estimates) in a county, proportions of occupied units paying rent with GRAPI (gross rent as a percentage of household income) ≥ 25% (2010–2014 ACS five-year estimates) in a county, proportions of individuals (of the civilian noninstitutionalized population) without health insurance coverage (2010–2014 ACS five-year estimates) in a county, proportions of county population living in rural areas as of the 2010 census, an indicator (1/0) for county population (measured in 2010 from SEER) < 25,000, an indicator (1/0) for 25,000 ≤ county population (measured in 2010 from SEER) < 50,000, an indicator (1/0) for 50,000 ≤ county population (measured in 2010 from SEER) < 100,000, and an indicator (1/0) for 100,000 ≤ county population (measured in 2010 from SEER) < 250,000. In case (c), the covariates are those used in case (b) plus the variable on the year when a state passed the state fair housing law protecting some groups from eviction. Standard errors (reported in parentheses) are clustered at the state level.



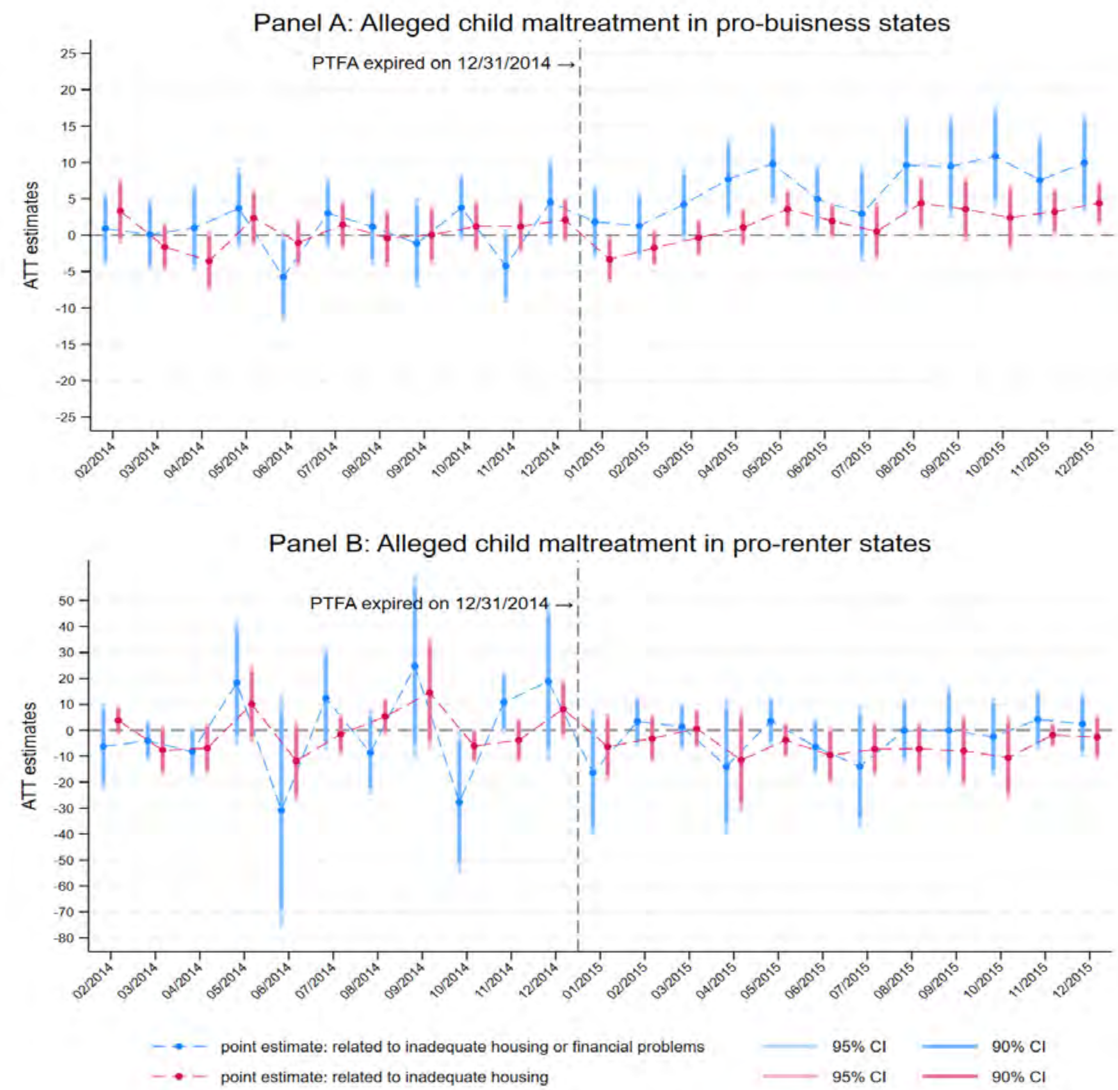
Appendix Figure A10: Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Counties with High and Low Delinquency Rates Separately

Notes: See Figure 2. The cutoff value for the high and low delinquency rates is the median value of delinquency rates.



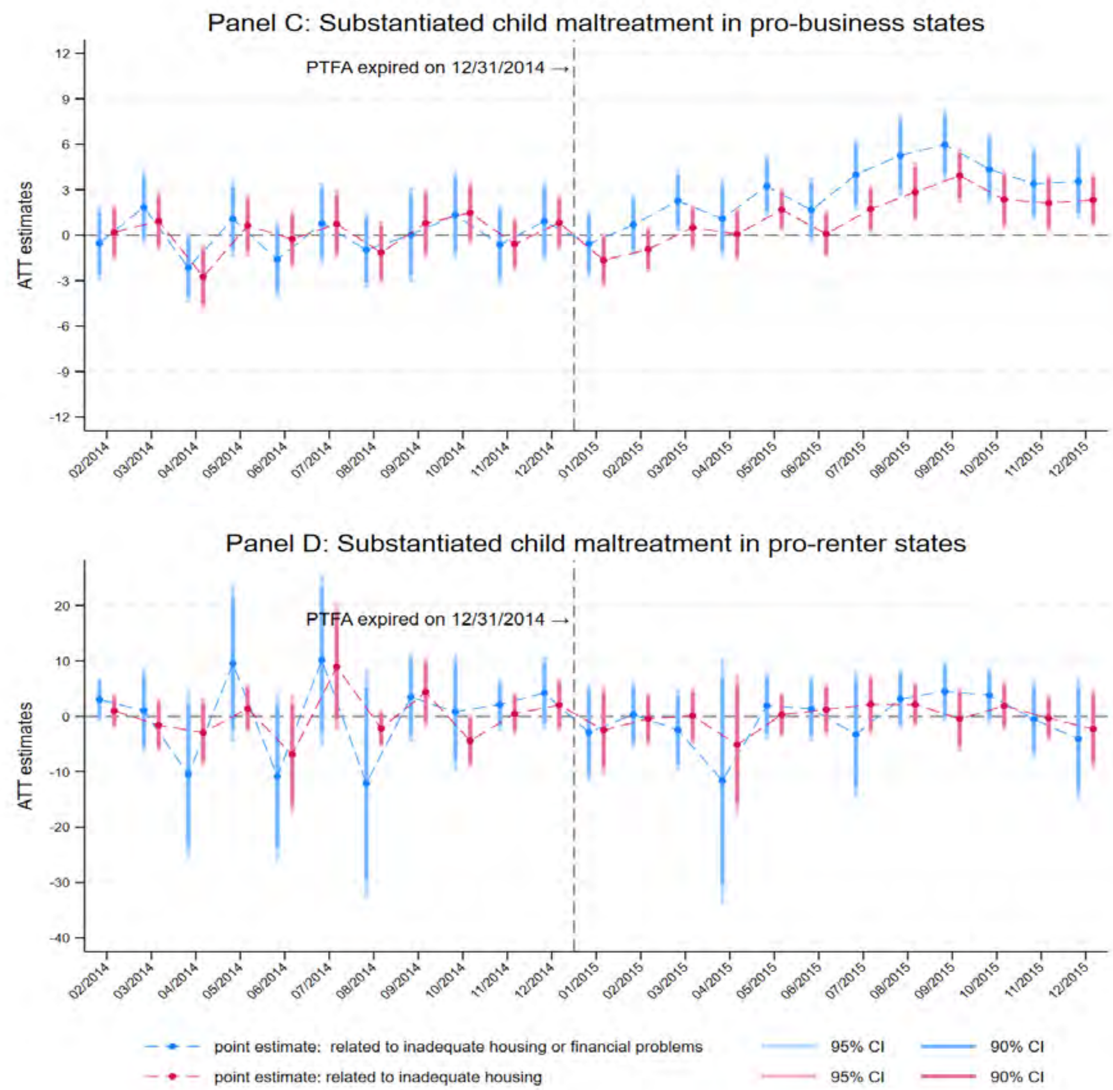
Appendix Figure A10 (cont.): Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Counties with High and Low Delinquency Rates Separately

Notes: See Figure 2. The cutoff value for the high and low delinquency rates is the median value of delinquency rates.



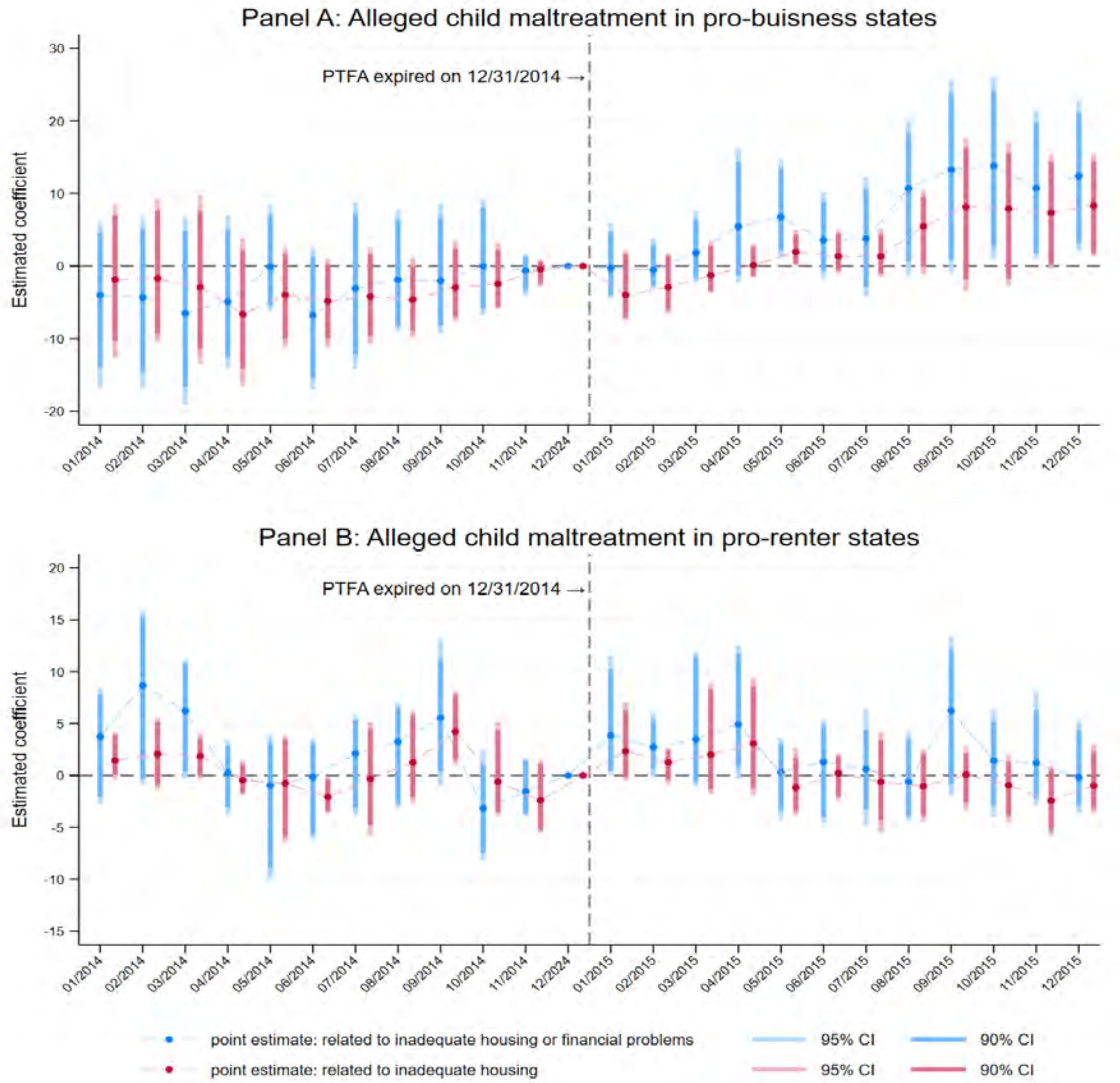
Appendix Figure A11a: Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Pro-Renter and Pro-Business States Separately—Standard Errors Clustered by County

Notes: See Figure 2 except that standard errors are clustered at the county level. All estimations include the covariates explained in Figure 2's notes. Pro-renter states (13 in total) are: California, Connecticut, Maine, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Oregon, and Vermont. Pro-business states (17 in total) are: Arkansas, Colorado, Florida, Georgia, Idaho, Illinois, Indiana, Louisiana, Michigan, Mississippi, Missouri, North Carolina, Ohio, Pennsylvania, Texas, West Virginia, and Wyoming.



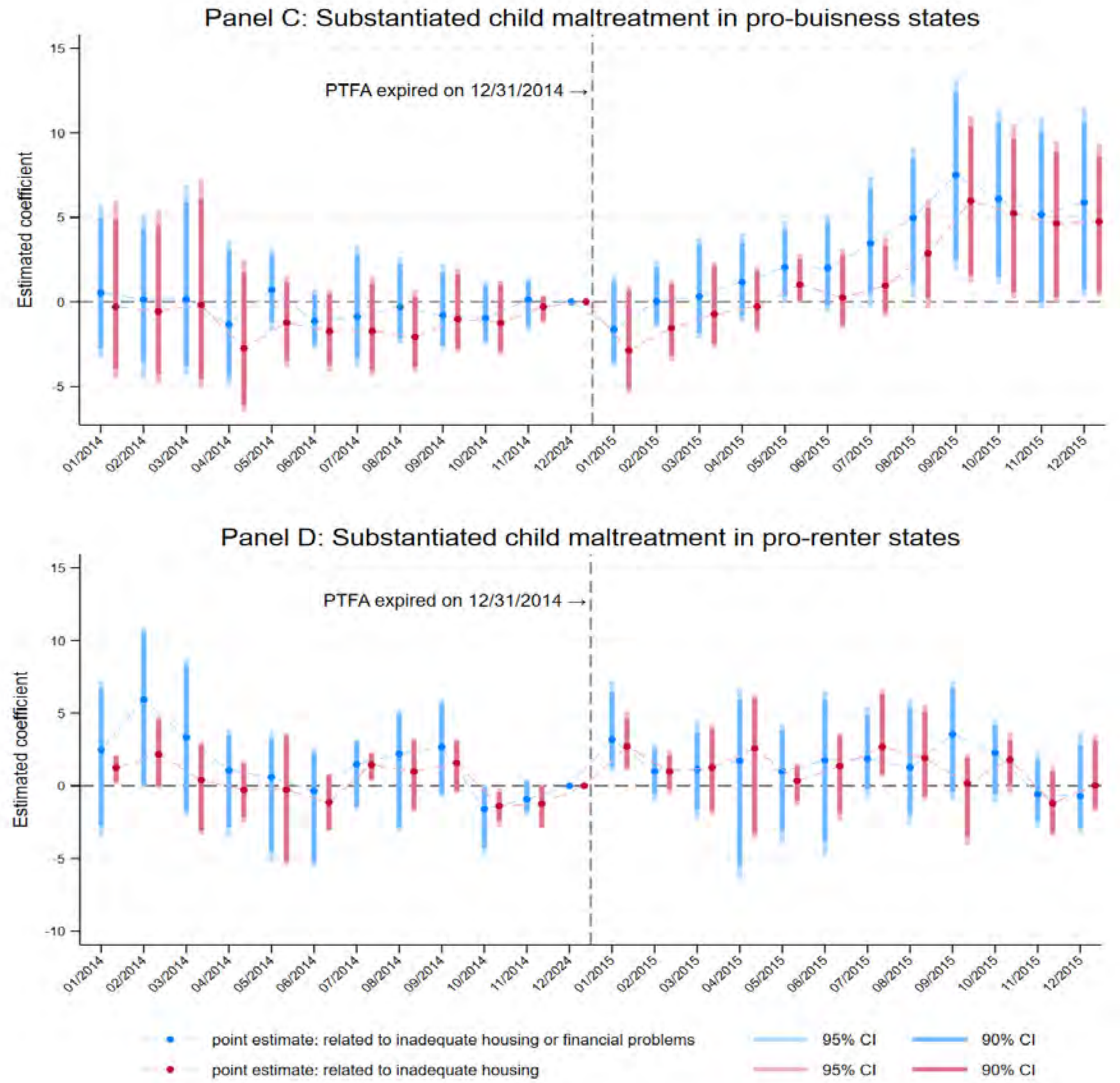
Appendix Figure A11a (cont.): Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Pro-Renter and Pro-Business States Separately—Standard Errors Clustered by County

Notes: See Figure 2 except that standard errors are clustered at the county level. All estimations include the covariates explained in Figure 2's notes. Pro-renter states (13 in total) are: California, Connecticut, Maine, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Oregon, and Vermont. Pro-business states (17 in total) are: Arkansas, Colorado, Florida, Georgia, Idaho, Illinois, Indiana, Louisiana, Michigan, Mississippi, Missouri, North Carolina, Ohio, Pennsylvania, Texas, West Virginia, and Wyoming.



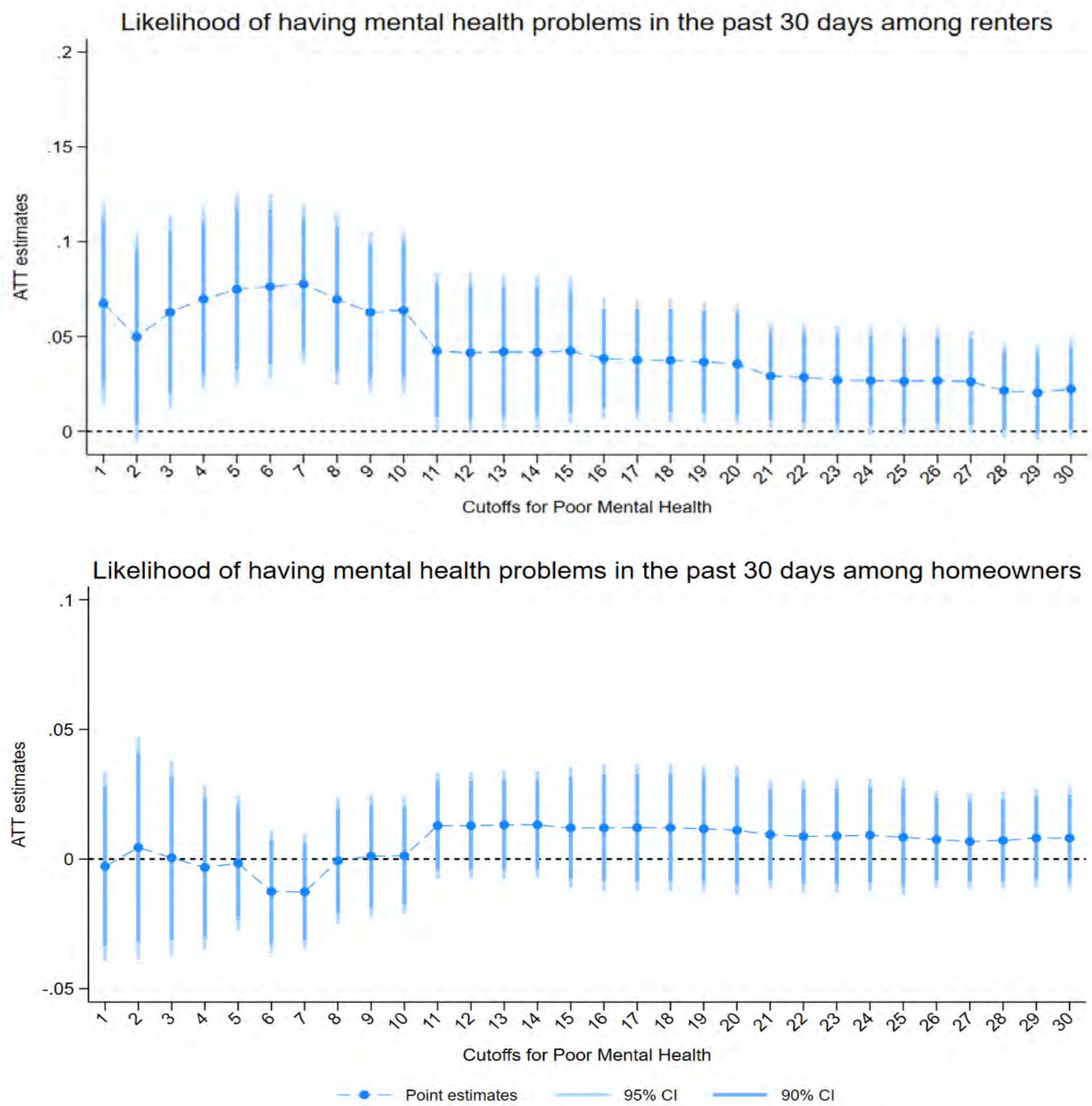
Appendix Figure A11b: Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Pro-Renter and Pro-Business States Separately—Standard Errors Obtained by Wild Cluster Bootstrap with Each Cluster Being a State

Notes: See Figure 2 except that: 1) the estimation is based on the event-study model estimated by the ordinary least squares, which is explained in Appendix E; 2) all estimations include the covariates listed in Figure 2's notes which are interacted with the dummy variables for year, in order to estimate their coefficients in the event-study model that uses fixed effects; 3) standard errors are clustered by state obtained by wild cluster bootstrap. Pro-renter states (13 in total) include California, Connecticut, Maine, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Oregon, and Vermont. Pro-business states (17 in total) include Arkansas, Colorado, Florida, Georgia, Idaho, Illinois, Indiana, Louisiana, Michigan, Mississippi, Missouri, North Carolina, Ohio, Pennsylvania, Texas, West Virginia, and Wyoming.



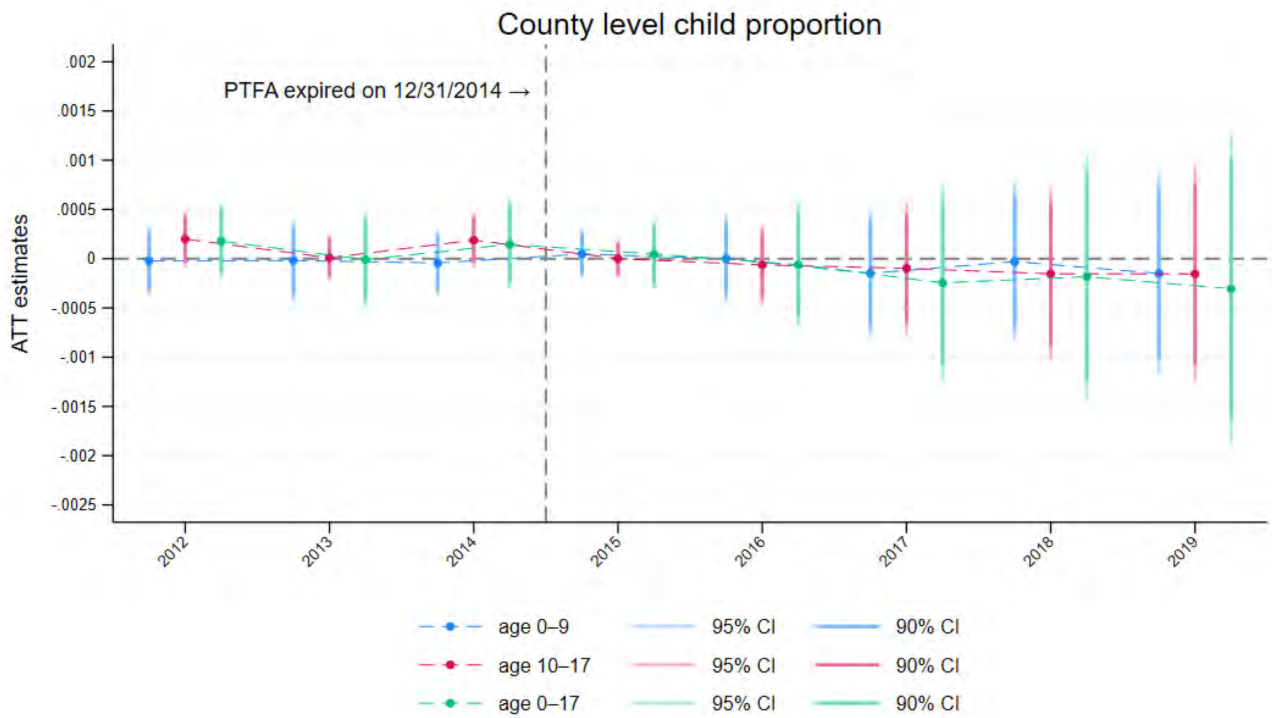
Appendix Figure A11b (cont.): Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Pro-Renter and Pro-Business States Separately—Standard Errors Obtained by Wild Cluster Bootstrap with Each Cluster Being a State

Notes: See Figure 2 except that: 1) the estimation is based on the event-study model estimated by the ordinary least squares, which is explained in Appendix E; 2) all estimations include the covariates listed in Figure 2's notes which are interacted with the dummy variables for year, in order to estimate their coefficients in the event-study model that uses fixed effects; 3) standard errors are clustered by state obtained by wild cluster bootstrap. Pro-renter states (13 in total) include California, Connecticut, Maine, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Oregon, and Vermont. Pro-business states (17 in total) include Arkansas, Colorado, Florida, Georgia, Idaho, Illinois, Indiana, Louisiana, Michigan, Mississippi, Missouri, North Carolina, Ohio, Pennsylvania, Texas, West Virginia, and Wyoming.



Appendix Figure A12: DID Estimates for Having Mental Health Problems by Homeownership with Alternative Definitions of the Dependent Variable

Notes: See Figure 3 except that having mental health problems is defined as experiencing at least X days of such problems in the past 30 days, where X takes values from 1 to 30.



Appendix Figure A13: DID Estimates for County Level Child Proportion (Couty Child Count/County Population Count)

Notes: See Figure 2 except that: 1) the sample period is 2011 through 2019; 2) the data are yearly; 3) the dependent variables are from SEER.

Appendix Table A1: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family, Adding Idaho, Nebraska and Vermont to the Estimation Sample

Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	7.745	***
(Standard error)	(2.555)	
[P-value]	[0.002]	
Mean of the dep. var. in the treated group and in the pre-period	55.700	
(Estimate/mean) × 100%	13.9%	
Number of observations	26,539	
Covariates included	Yes	
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	2.801	***
(Standard error)	(0.935)	
[P-value]	[0.003]	
Mean of the dep. var. in the treated group and in the pre-period	18.333	
(Estimate/mean) × 100%	15.3%	
Number of observations	26,539	
Covariates included	Yes	
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	3.102	***
(Standard error)	(1.012)	
[P-value]	[0.002]	
Mean of the dep. var. in the treated group and in the pre-period	20.157	
(Estimate/mean) × 100%	15.4%	
Number of observations	26,539	
Covariates included	Yes	
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	1.836	***
(Standard error)	(0.587)	
[P-value]	[0.002]	
Mean of the dep. var. in the treated group and in the pre-period	10.146	
(Estimate/mean) × 100%	18.1%	
Number of observations	26,539	
Covariates included	Yes	

Notes: See Table 2.

Appendix Table A2: DID Estimates for Child Maltreatment Using Alternative Versions of the Callaway and Sant'Anna (2021) Estimator

	(1)	(2)
Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	7.512 ***	7.205 **
(Standard error)	(2.670)	(2.828)
[P-value]	[0.005]	[0.011]
Mean of the dep. var. in the treated group and in the pre-period	55.700	55.700
(Estimate/mean) × 100%	13.5%	12.9%
Number of observations	26,003	26,003
Estimation method	RA	IPW
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	2.752 ***	2.438 **
(Standard error)	(0.906)	(1.049)
[P-value]	[0.002]	[0.020]
Mean of the dep. var. in the treated group and in the pre-period	18.333	18.333
(Estimate/mean) × 100%	15.0%	13.3%
Number of observations	26,003	26,003
Estimation method	RA	IPW
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	2.941 ***	2.765 **
(Standard error)	(1.118)	(1.266)
[P-value]	[0.009]	[0.029]
Mean of the dep. var. in the treated group and in the pre-period	20.157	20.157
(Estimate/mean) × 100%	14.6%	13.7%
Number of observations	26,003	26,003
Estimation method	RA	IPW
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	1.925 ***	1.653 **
(Standard error)	(0.660)	(0.740)
[P-value]	[0.004]	[0.025]
Mean of the dep. var. in the treated group and in the pre-period	10.146	10.146
(Estimate/mean) × 100%	19.0%	16.3%
Number of observations	26,003	26,003
Estimation method	RA	IPW

Notes: See Table 2. In the row describing estimation methods, RA stands for regression adjustment, and IPW stands for inverse probability weighting.

Appendix Table A3: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family with Extended Post-Treatment Period

	(1) Alleged child maltreatment related to inadequate housing	(2) Substantiated child maltreatment related to inadequate housing	(3) Alleged child maltreatment related to inadequate housing or financial problems of the child's family	(4) Substantiated child maltreatment related to inadequate housing or financial problems of the child's family
ATT (overall aggregation) estimate	4.700 **	2.012 **	9.967 ***	2.431 **
(Standard error)	(1.832)	(0.858)	(2.901)	(1.056)
[P-value]	[0.010]	[0.019]	[0.001]	[0.021]
Mean of the dep. var. in the treated group and in the pre-period	20.157	10.146	55.700	18.333
(Estimate/mean) × 100%	23.3%	19.8%	17.9%	13.3%
Number of observations	80,298	80,298	80,298	80,298
Covariates included	Yes	Yes	Yes	Yes

Notes: See Table 2 except that the sample period is January 2014 through December 2019.

Appendix Table A4: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family Based Only on Identified Counties in the NCANDS Child File Data

	(1) Sample includes identifed counties and masked counties	(2) Sample includes only identified counties
Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	7.819 ***	8.737 ***
(Standard error)	(2.589)	(2.807)
[P-value]	[0.003]	[0.002]
Mean of the dep. var. in the treated group and in the pre-period	55.700	55.859
(Estimate/mean) × 100%	14.0%	15.6%
Number of observations	26,003	24,987
Covariates included	Yes	Yes
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	2.831 ***	2.828 ***
(Standard error)	(0.943)	(0.973)
[P-value]	[0.003]	[0.004]
Mean of the dep. var. in the treated group and in the pre-period	18.333	18.414
(Estimate/mean) × 100%	15.4%	15.4%
Number of observations	26,003	24,987
Covariates included	Yes	Yes
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	3.076 ***	3.145 ***
(Standard error)	(1.029)	(0.922)
[P-value]	[0.003]	[0.001]
Mean of the dep. var. in the treated group and in the pre-period	20.157	20.288
(Estimate/mean) × 100%	15.3%	15.5%
Number of observations	26,003	24,987
Covariates included	Yes	Yes
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	1.857 ***	1.629 ***
(Standard error)	(0.590)	(0.578)
[P-value]	[0.002]	[0.005]
Mean of the dep. var. in the treated group and in the pre-period	10.146	10.190
(Estimate/mean) × 100%	18.3%	16.0%
Number of observations	26,003	24,987
Covariates included	Yes	Yes

Notes: See Table 2.

Appendix Table A5: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family Using Alternative Definitions of the Treatment and Control Groups

	(1) Drop 3, 5 days in treated	(2) Drop 10, 30 , 60 days in control
Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	8.034 ***	7.266 **
(Standard error)	(2.709)	(2.970)
[P-value]	[0.003]	[0.014]
Mean of the dep. var. in the treated group and in the pre-period	58.607	55.700
(Estimate/mean) × 100%	13.7%	13.0%
Number of observations	25,203	20,573
Covariates included	Yes	Yes
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	2.859 ***	3.264 ***
(Standard error)	(0.970)	(0.909)
[P-value]	[0.003]	[0.000]
Mean of the dep. var. in the treated group and in the pre-period	19.210	18.333
(Estimate/mean) × 100%	14.9%	17.8%
Number of observations	25,203	20,573
Covariates included	Yes	Yes
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	3.017 ***	2.899 ***
(Standard error)	(1.075)	(1.113)
[P-value]	[0.005]	[0.009]
Mean of the dep. var. in the treated group and in the pre-period	21.166	20.157
(Estimate/mean) × 100%	14.3%	14.4%
Number of observations	25,203	20,573
Covariates included	Yes	Yes
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	1.850 ***	2.008 ***
(Standard error)	(0.607)	(0.641)
[P-value]	[0.002]	[0.002]
Mean of the dep. var. in the treated group and in the pre-period	10.617	10.146
(Estimate/mean) × 100%	17.4%	19.8%
Number of observations	25,203	20,573
Covariates included	Yes	Yes

Notes: See Table 2, except that: in column (1) we excluded the "3 days" and "5 days" states (shown in Figure 1) from the treatment group, while keeping the same control group; in column (2) we excluded the "10 days", "30 days", and "60 days" states (shown in Figure 1) from the control group, while keeping the same treatment group.

Appendix Table A6: DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family by Report Source

	(1) Reported by professionals	(2) Reported by non-professionals
Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	4.147 **	3.673 ***
(Standard error)	(1.842)	(1.054)
[P-value]	[0.024]	[0.000]
Mean of the dep. var. in the treated group and in the pre-period	31.930	23.770
(Estimate/mean) × 100%	13.0%	15.5%
Number of observations	26,003	26,003
Covariates included	Yes	Yes
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	2.050 ***	0.782 *
(Standard error)	(0.585)	(0.432)
[P-value]	[0.000]	[0.070]
Mean of the dep. var. in the treated group and in the pre-period	12.200	6.133
(Estimate/mean) × 100%	16.8%	12.7%
Number of observations	26,003	26,003
Covariates included	Yes	Yes
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	1.336 **	1.740 ***
(Standard error)	(0.657)	(0.658)
[P-value]	[0.042]	[0.008]
Mean of the dep. var. in the treated group and in the pre-period	11.569	8.588
(Estimate/mean) × 100%	11.6%	20.3%
Number of observations	26,003	26,003
Covariates included	Yes	Yes
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	1.352 ***	0.504 *
(Standard error)	(0.380)	(0.257)
[P-value]	[0.000]	[0.050]
Mean of the dep. var. in the treated group and in the pre-period	6.448	3.698
(Estimate/mean) × 100%	21.0%	13.6%
Number of observations	26,003	26,003
Covariates included	Yes	Yes

Notes: See Table 2.

Appendix Table A7: Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Counties with High and Low Delinquency Rates Separately

	(1) Sample includes counties with low delinquency rates	(2) Sample includes counties with high delinquency rates
Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	0.624	11.911 ***
(Standard error)	(2.384)	(3.518)
[P-value]	[0.794]	[0.001]
Mean of the dep. var. in the treated group and in the pre-period	45.297	67.125
(Estimate/mean) × 100%	1.4%	17.7%
Number of observations	12,190	13,589
Covariates included	Yes	Yes
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	1.019	3.336 ***
(Standard error)	(1.096)	(1.239)
[P-value]	[0.352]	[0.007]
Mean of the dep. var. in the treated group and in the pre-period	19.480	17.136
(Estimate/mean) × 100%	5.2%	19.5%
Number of observations	12,190	13,589
Covariates included	Yes	Yes
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	0.086	3.867 ***
(Standard error)	(1.723)	(1.006)
[P-value]	[0.960]	[0.000]
Mean of the dep. var. in the treated group and in the pre-period	22.030	18.223
(Estimate/mean) × 100%	0.4%	21.2%
Number of observations	12,190	13,589
Covariates included	Yes	Yes
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	0.517	2.139 ***
(Standard error)	(0.844)	(0.757)
[P-value]	[0.540]	[0.005]
Mean of the dep. var. in the treated group and in the pre-period	11.869	8.313
(Estimate/mean) × 100%	4.4%	25.7%
Number of observations	12,190	13,589
Covariates included	Yes	Yes

Notes: See Table 2. The cutoff value for the high and low delinquency rates is the median value of delinquency rates.

Appendix Table A8a: Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Pro-Renter and Pro-Business States Separately—Standard Errors Clustered by County

	(1) Sample includes pro-renter states	(2) Sample includes pro-business states
Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	-3.192	6.691 ***
(Standard error)	(6.079)	(2.254)
[P-value]	[0.600]	[0.003]
Mean of the dep. var. in the treated group and in the pre-period	24.872	74.171
(Estimate/mean) × 100%	-12.8%	9.0%
Number of observations	5,196	13,700
Covariates included	Yes	Yes
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	-0.811	2.911 ***
(Standard error)	(3.213)	(0.874)
[P-value]	[0.801]	[0.001]
Mean of the dep. var. in the treated group and in the pre-period	13.972	20.304
(Estimate/mean) × 100%	-5.8%	14.3%
Number of observations	5,196	13,700
Covariates included	Yes	Yes
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	-5.950	1.650
(Standard error)	(4.963)	(1.196)
[P-value]	[0.231]	[0.168]
Mean of the dep. var. in the treated group and in the pre-period	16.526	19.071
(Estimate/mean) × 100%	-36.0%	8.7%
Number of observations	5,196	13,700
Covariates included	Yes	Yes
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	-0.276	1.254 *
(Standard error)	(2.293)	(0.646)
[P-value]	[0.904]	[0.052]
Mean of the dep. var. in the treated group and in the pre-period	8.962	8.669
(Estimate/mean) × 100%	-3.1%	14.5%
Number of observations	5,196	13,700
Covariates included	Yes	Yes

Notes: See Table 2 except that standard errors (reported in parentheses) are clustered at the county level. Pro-renter states (13 in total) are: California, Connecticut, Maine, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Oregon, and Vermont. Pro-business states (17 in total) are: Arkansas, Colorado, Florida, Georgia, Idaho, Illinois, Indiana, Louisiana, Michigan, Mississippi, Missouri, North Carolina, Ohio, Pennsylvania, Texas, West Virginia, and Wyoming.

Appendix Table A8b: Subsample DID Estimates for Child Maltreatment Related to Inadequate Housing or Financial Problems of the Child's Family for Pro-Renter and Pro-Business States Separately—Standard Errors Obtained by Wild Cluster Bootstrap with Each Cluster Being a State

	(1) Sample includes pro-renter states	(2) Sample includes pro-business states
Panel A: Alleged child maltreatment related to inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	0.113	9.618 **
(Standard error)	(0.059)	(1.938)
[P-value]	[0.969]	[0.026]
Mean of the dep. var. in the treated group and in the pre-period	24.872	74.171
(Estimate/mean) × 100%	0.5%	13.0%
Number of observations	5,196	13,700
Covariates included	Yes	Yes
Panel B: Substantiated child maltreatment related inadequate housing or financial problems of the child's family		
ATT (overall aggregation) estimate	0.053	3.392 *
(Standard error)	(0.042)	(1.848)
[P-value]	[0.990]	[0.058]
Mean of the dep. var. in the treated group and in the pre-period	13.972	20.304
(Estimate/mean) × 100%	0.4%	16.7%
Number of observations	5,196	13,700
Covariates included	Yes	Yes
Panel C: Alleged child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	-0.207	5.860 **
(Standard error)	(-0.273)	(2.062)
[P-value]	[0.767]	[0.037]
Mean of the dep. var. in the treated group and in the pre-period	16.526	19.071
(Estimate/mean) × 100%	-1.3%	30.7%
Number of observations	5,196	13,700
Covariates included	Yes	Yes
Panel D: Substantiated child maltreatment related to inadequate housing		
ATT (overall aggregation) estimate	0.929	2.780 *
(Standard error)	(1.852)	(1.777)
[P-value]	[0.104]	[0.071]
Mean of the dep. var. in the treated group and in the pre-period	8.962	8.669
(Estimate/mean) × 100%	10.4%	32.1%
Number of observations	5,196	13,700
Covariates included	Yes	Yes

Notes: See Table 2 except that: 1) the estimation is based on the event-study model estimated by the ordinary least squares, which is explained in Appendix E; 2) the covariates listed in Table 2's notes are interacted with the dummy variables for year, in order to estimate their coefficients in the event-study model that uses fixed effects; 3) standard errors are clustered by state obtained by wild cluster bootstrap. Pro-renter states (13 in total) include California, Connecticut, Maine, Maryland, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Oregon, and Vermont. Pro-business states (17 in total) include Arkansas, Colorado, Florida, Georgia, Idaho, Illinois, Indiana, Louisiana, Michigan, Mississippi, Missouri, North Carolina, Ohio, Pennsylvania, Texas, West Virginia, and Wyoming.

Appendix Table A9: DID Estimates for County Level Child Proportion (Couty Child Count/County Population Count)

	Age 0–9	Age 10–17	Age 0–17
ATT (overall aggregation) estimate	-0.00006	-0.00009	-0.00015
(Standard error)	(0.0003)	(0.0003)	(0.0005)
[P-value]	[0.856]	[0.771]	[0.755]
Mean of the dep. var. in the treated group and in the pre-period	0.124	0.105	0.229
(Estimate/mean) × 100%	-0.04%	-0.09%	-0.07%
Number of observations	26,811	26,811	26,811
Covariates included	Yes	Yes	Yes

Notes: See Table 2 except that: 1) the sample period is 2011 through 2019; 2) the data are yearly; 3) the dependent variables are from SEER.