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Dedication

To my parents and grandparents. None of this would have been possible without your love and support.

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Abstract

This dissertation examines the impact of social policies related to the well-being of at-risk children. It consists of three chapters. The first chapter estimates the effect of public housing receipt in the US during the 1940s on childrens' short-term educational attainment. The second chapter measures the extent to which the opioid crisis has affected child out-of-home placements through the US child welfare system. The third chapter analyzes whether the legalization of same-sex marriage in the US has affected same-sex couples' decision to raise children with particular focus on adopted or foster children.

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Introduction

This dissertation examines the impact of social policies related to the well-being of at-risk children. It consists of three chapters. The first chapter estimates the effect of public housing receipt in the US during the 1940s on childrens' short-term educational attainment. The second chapter measures the extent to which the opioid crisis has affected child out-of-home placements through the US child welfare system. The third chapter analyzes whether the legalization of same-sex marriage in the US has affected same-sex couples' decision to raise children with particular focus on adopted or foster children.

Initially constructed during the Great Depression, public housing developments provided decent and affordable housing for many US families affected by the economic downturn. In the first chapter, I use the 1940 US Census to study the effect of public housing on child recipients' short-term educational attainment as measured by school attendance, grade attainment and employment. Compared to a matched group of eligible children who did not receive public housing assistance as of the 1940 Census, I find that receipt of public housing significantly improved short-term educational attainment. These effects are largely driven by additional grades completed among non-white children and children older than the compulsory age required to attend school. Among children living in public housing as of the 1940 Census, greater exposure to public housing increased the likelihood that children older than the compulsory schooling age continued to attend school and increased rates of employment among female and non-white children who completed mandatory schooling requirements.

Opioid abuse has become a major public health issue in the US. While opioid misuse has direct consequences for addicts, children may also be affected. To prevent misuse of the prescription opiate OxyContin, Purdue Pharma released an abuse-deterrent version in 2010. Unintentionally, this reformulation caused many addicted to OxyContin to substitute more harmful opioids such as heroin

and fentanyl in its place.

The second chapter estimates the effect of opioid abuse on child out-of-home placements, the removal of a child from home due to maltreatment, using data on opioid abuse and OxyContin's reformulation. Using the reformulation as an instrumental variable for opioid abuse rates, I estimate that a 10% increase in the opioid abuse treatment admission rate caused an additional 2.4 foster care entries per 100,000 children due to drug abuse – equivalent to nearly 1,800 child foster care out-of-home placements nationally. An average 27.2% increase in the opioid abuse treatment admission rate over the study period caused an estimated 4,900 additional drug-related child out-of-home placements or $26M in additional costs to the US foster care system from 2006-2016.

The legal right to marry for same-sex couples in the US has dramatically changed in recent years, but has same-sex marriage legalization increased the likelihood that same-sex couples parent a child? The third chapter examines the effect of same-sex marriage equality on same-sex couples' decision to raise a child using the American Community Survey from 2008-2019. Using a difference-in-differences strategy for staggered treatment timing (Callaway & Sant'Anna, 2021), I find an increasing trend in the overall likelihood same-sex couples are parenting any child or adopting a child. However, I only find a lagged significant effect of same-sex marriage legalization on the likelihood of same-sex households fostering a child. This is suggestive evidence that while overall rates of parenthood among same-sex couples have not increased, following legal marriage equality more same-sex couples have chosen to adopt or foster a child due to greater access.

Chapter 1

The Effect of Public Housing Assistance on Short-Run Educational Attainment During the US Great Depression

1.1 Introduction

Public housing assistance affords low-income households the opportunity to re-allocate rental expenses towards other activities which improve upon household welfare and economic productivity. For families, receiving public housing assistance allows for additional expenditures on goods and services to improve their children's health and development. This study assesses the short-term educational benefits of public housing receipt for school-age child residents during the Great Depression.[1] Initially constructed by the federal government during the 1930s as a public works program to clear urban slums, public housing developments were built and leased across 46 US cities as of the 1940 Census.

While a large literature has investigated the effect on child development of contemporary US housing assistance programs such as Housing Choice Voucher (HCV) programs (Chetty et al., 2016; Chyn, 2018; Jacob, 2004; Jacob et al., 2015; Katz et al., 2001), to the author's knowledge this is the first study which examines the effect of public housing receipt on child residents during the Great Depression. Although contemporary studies on public housing developments have found significant benefits of moving away from public housing for children, the demographic composition of public housing tenants differed significantly during the 1930's and 1940's. Initially, public housing was targeted towards the "submerged middle class", families who were temporally affected by the economic downturn of the Great Depression. Application for newly constructed public housing units was highly competitive and the selection process was extensive – including in-person interviews and financial/rental background checks – to ensure tenants' "desirability". Therefore although the earliest public housing residents were statutorily qualified as low-income, they were not necessarily the most economically disadvantaged among the urban poor.

Despite these differences from contemporary US public housing assistance programs, it is still important to study the 'historical' effect of public housing assistance on child educational attainment for several reasons. Since the application process historically was more stringent relative to contemporary programs, it is possible that public housing assistance had little effect on historic residents due to their higher socio-economic status. On the other hand, historic child residents may have

[1] Federal housing assistance for low-income families was provided solely through means-tested rental subsidies to live in public housing developments prior to the US Housing Act of 1974 which enacted the Housing Choice Voucher (HCV) Program as Section 8.

benefited from receiving public housing assistance via greater investment in child schooling. Hence, this study provides historical evidence of how the socio-economic composition of public housing development residents may influence the efficacy of providing public housing assistance on children's educational attainment.

To examine the effect of public housing assistance on children's short-term educational attainment, I use data which identifies the population of public housing residents in the 1940 US Census and estimate the effect of receiving public housing assistance on the likelihood of children's school attendance, on-time school completion, labor force participation, and years of schooling as of 1940. To account for potential selection bias into public housing due to the tenant application process, I use two methods to examine the effect of public housing receipt on child educational outcomes. First, I compare children living in public housing to a constructed sample of children who were eligible to receive but did not live in public housing as of the 1940 Census using coarsened exact matching (CEM). I find that school-age children who lived in public housing were more likely to be attending school and on-time to complete compulsory schooling requirements than the comparison group of eligible children. I also find that children living in public housing who were older than the compulsory schooling age were less likely to be employed in the labor force and had more overall years of schooling. Among child demographics, those with the lowest marginal benefits from additional years of schooling or the highest opportunity costs historically in terms of loss labor income benefited most from public housing receipt such as nonwhite children and children who had already completed their compulsory schooling requirements.

Second, to control for potential selection bias into public housing, I compare educational outcomes among only children living in public housing using differential rates of exposure approximated by the initial occupancy date of each housing development. On average, I find that children who had lived in public housing longer had significantly higher school attendance rates and more years of schooling among children who had already completed their compulsory education. While I find no effects on current school attendance or on-time completion rates, I do find higher rates of employment in the labor force among female and non-white public housing children age 14 and older which may proxy for greater ability to find employment due to compulsory schooling completion since these demographic populations were less likely to continue post-secondary education beyond the mandatory schooling age.

This study makes several contributions to the economic literature related to public housing assistance and child development. First, little empirical research has been done on historic public housing residents other than recent work by Allen and Van Riper (2020) and archival studies (Bloom, 2008; Radford, 1996, 2000). Although there is a large economic literature on other New Deal and early 20th century public welfare program receipt, no study has examined the effect of public housing assistance during the Great Depression on child residents (Aizer et al., 2016; Fishback et al., 2001, 2007). Second, while studies on public housing assistance have found that moving away from contemporary public housing developments to be beneficial for children, public housing developments were highly desirable at their inception and assisted a less socio-economically disadvantaged population than contemporary public housing policy. Contrary to those findings, I find that children who lived in public housing during the Great Depression had significantly better educational outcomes than children who received no housing assistance measured in terms of years of schooling completed. This result is consistent and more contextually related to contemporary findings from Currie and Yelowitz (2000) which found that children living in public housing projects were less likely to suffer from overcrowding and to have been held back in school than eligible children who received no housing assistance.

Receipt of public housing in 1940 could have benefited children's educational development through several channels. Public housing assistance subsidizes rental expenditures thereby decreasing households demand for children's labor earnings and allowing children to complete more years of schooling. Social networks within public housing developments may have also potentially served to enforce implicit social contracts among neighbors ensuring higher school achievement (i.e. "Keeping up with the Joneses"). Public housing communities were also targeted for other social welfare and health programs such as parental education and child care which may have contributed to greater child welfare (Radford, 1996). Hence, this study provides evidence that public assistance via living in a public housing development can be beneficial for children. Given that public housing developments are currently targeted towards the most economically disadvantaged populations and children been found to be negatively affected by this experience , this study provides evidence that the socio-economic conditions created within public housing due to the tenant selection process may have a significantly effect on child residents' educational achievement.

1.2 Public Housing in the US

Prior to the Great Depression, the US federal government remained uninvolved in the provision of affordable public housing. In 1933, 14 million Americans were unemployed, 273,000 families lost their homes to mortgage foreclosure, and construction of new housing had fallen 90% from 1925 (Fish, 1979; Rowe, 1993). As a response to rising unemployment, expanding slums, and limited affordable housing, the National Industrial Recovery Act of 1933 established the Public Works Administration (PWA) Housing Division to construct low-cost housing and clear urban slums (US Department of the Interior, 2004). First only providing limited-dividend loans for public housing projects to be constructed independently by local developers, the PWA later directly oversaw construction done by local architects and builders after 1934.

Federal income requirements to receive public housing assistance were first enacted with the George-Healey Act in 1936. Although the PWA's intention was to target families most in need of housing assistance as tenants, it was also necessary that occupants were able to afford rental rates high enough to repay federal construction loans. To accommodate these requirements, it was officially stated that public housing would be targeted towards families in the "middle third" of the US income distribution affected by the Great Depression popularly referred to as the "submerged middle class" (Radford, 1996). As a result, the only federal eligibility requirement for public housing was that the net income of the family could not exceed five times the cost of one month's rent and utilities.

In 1937, the Wagner-Steagall Act established the US Housing Authority (USHA) to replace the PWA (Radford, 1996). The Wagner-Steagall Act allowed local housing authorities to charge lower rental rates and increasingly target low-income families through more generous federal subsidies which covered up to 90% of housing development costs and more restrictions on construction costs (Hunt, 2005; Plunz, 1990; Radford, 2000; US Housing Act, 1937). Similar to the George-Healey Act, the Wagner-Steagall Act of 1937 maintained that the net income for households with two or less children receiving public housing not exceed five times the cost of one month's rent and utilities. However, the Act relaxed the income restriction to six times one month's rent and utilities for families with three or more children (US Housing Act, 1937). No additional federal legislation provided further income-based tenant selection criteria. At the time of the 1940 Census (April 1,

1940), the US government had built and leased over 30,000 public housing units across 46 cities in 22 different states and outlying territories (US Department of the Interior, 2004).

Obtaining a newly-constructed public housing unit could be highly competitive. For example, New York City's first two public housing projects – First Houses and Harlem River Houses – received approximately 25 tenant applications per available unit (Radford, 1996). To assist local housing authorities, in 1937 the PWA published a tenant selection manual which outlined standardized application procedures and selection criteria (PWA, 1937). Tenant selection was often based on three criteria: need for housing, desirability as tenants, and financial security. To evaluate each applicant's need for housing, inspectors would create an "eligibility score" based on these three criteria. To examine potential tenants' need for housing, interviewers would conduct in-home visits with applicants' to inspect the dwelling conditions, its amenities, and the presence of undesirable neighborhood influences on children. To assess tenant desirability, interviewers scored the cleanliness and maintenance of the current dwelling and conducted interviews with landlords and neighbors regarding the applicant's character. Finally, applicants were asked to provide financial records of rent paid and sources of income to assess credit worthiness.[2]

A large body of research has documented the political figures and institutional process behind early US public housing construction through biographical accounts and aggregate federal statistics, yet little empirical research has examined individual-level records to assess how public housing assistance affected its earliest recipients due in part to data scarcity (Bloom, 2008; Radford, 1996, 2000). Since the initial federal legislation allowed local authorities to independently operate and manage their public housing developments, records on individual public housing tenants were not federally managed or collected. Therefore limited historical sources are available to examine a nationally representative sample of early public housing residents.

To the author's knowledge, a recent study by Allen and Van Riper (2020) is the only statistical analysis of early US public housing residents. Using the 1940 Census to identify the population of public housing project residents in New York City, they compared the demographic composition of households who received public housing to households who would have been eligible for public housing but did not receive a unit. They found that across New York City public housing devel-

[2] An excellent review of US public housing around the period of the 1940 Census has been conducted by Allen and Van Riper (2020)

opments public housing residents on average were of higher socio-economic status and paid 15-35% less rent per month than the comparison group (Allen & Van Riper, 2020). Demographically, the majority of households living in public housing were nuclear families, employed in the labor force, and were significantly more likely to be US citizens than the eligible population. They also found evidence that the socio-economic status of residents in early PWA-era public housing projects was relatively higher than that of residents in later USHA-era constructed developments. Public housing authorities were particularly selective in choosing black public housing tenants.[3] Relative to other public housing tenants, black public housing residents were more educated and had higher household income.

Although there is limited evidence of whether early 20th century public housing assistance affected child development, several studies have shown beneficial effects of childhood exposure to other early US public welfare and New Deal-era policies on human capital development and mortality. Receipt of welfare assistance through programs such as mother's pensions (Aizer et al., 2016), participation in youth training programs (Aizer et al., 2020), and living in cities which improved urban water purification and sewage infrastructure (Beach et al., 2016), have been shown to improve children's long-term educational attainment and labor productivity. There has also been extensive work on the benefits of increased childhood schooling on educational attainment using state-variation in US compulsory schooling laws (Acemoglu & Angrist, 2000; Angrist & Keueger, 1991; Clay et al., 2021; Goldin & Katz, 2011; Lleras-Muney, 2002).

Initially targeted towards working and lower-middle class households, public housing assistance has increasingly served lower income populations in recent decades.[4] Greater rates of poverty among tenants consequentially has limited economic opportunities for social mobility within public housing developments and their surrounding neighborhoods. In 1974, the Housing Choice Voucher (HCV) Program was enacted as Section 8 of the United States Housing Act to improve economic opportunities for low-income families and children receiving housing assistance by allowing public housing programs to rely more heavily on the private housing market through tenant-based subsidy pro-

[3]The majority of public housing developments were racially segregated during this time period. Although there were racially-integrated housing projects, a third of public housing was designated for black tenants (Radford, 1996).

[4]Currently to qualify for housing assistance in the US, household income must be less than the Department of Housing and Urban Development's (HUD) income thresholds of 80% (low-income) or 50% (very low-income) of the median income for the county or metropolitan area of residence and household size. Households pay rent equal to 30% of their income while the remaining costs are subsidized by the federal government.

grams.[5] Research assessing HCV Programs like the Moving to Opportunity study have found conflicting effects on the long-term benefits for child development from moving out of public housing developments (Chetty et al., 2016; Chyn, 2018; Jacob, 2004; Katz et al., 2001). Work by Chetty et al. (2016) and Chyn (2018) have found a number of economic benefits for children, particularly younger children, from moving out of public housing developments due improved neighborhood environments. However, other studies work has found no effect of housing voucher receipt on young adult education and employment outcomes relative to public housing (Andersson et al., 2016; Jacob, 2004; Jacob et al., 2015).[6]

Given that HCV programs were not available to historical child residents of public housing, results from this study are akin to the effect of public housing receipt for children who otherwise would be considered homeless or underhoused today. Although living in a public housing development is commonly considered less desirable than receipt of a HCV, research has still found children benefit from living in subsidized public housing developments relative to receiving no assistance (Currie & Yelowitz, 2000; S. Newman & Harkness, 2000; S. J. Newman & Harkness, 2002).

1.3 Data

Identifying Public Housing Residents

To identify public housing residents in the 1940 US Census, I use data from the IPUMS-USA 1940 full count Census file constructed by Allen and Van Riper (2020) (Ruggles et al., 2021). After 72 years, the National Archives and Records Administration (NARA) releases complete US census records to the public, including names and addresses. Using a digitized version of the complete 1940 Census, Allen and Van Riper (2020) identified the population of public housing residents by cross-referencing street addresses with Sanborn and Census enumeration district maps. The identification strategy is

[5] Rental assistance is currently provided by HUD through either housing-based or tenant-based subsidy programs. Housing-based programs provide households a subsidy to live in either a contracted private housing unit or public housing development. Tenant-based programs such as the Housing Choice Voucher (HCV) Program allow participants to rent private housing subsidized by local public housing agencies and are transferable to a new location if the tenant moves. Tenant-based programs are currently the most prevalent form of public housing assistance provided in the US.

[6] A potential reason for these differential conclusions is that families often do not relocate to higher quality neighborhoods with greater economic opportunity; not due to preferences, but barriers in the housing search process (Bergman et al., 2019)

detailed in the Appendix.

In total, 66 public housing developments were built and occupied across 46 cities as of the 1940 Census. Tables 1.1 and 1.2 compare the count of housing units identified in the 1940 Census to administrative records maintained by the Federal Works Agency (Federal Works Agency, 1940). Using this strategy, the count of households identified as living in public housing units in the 1940 Census was largely consistent with administrative records. Undercounts of public housing resident households may reflect housing unit vacancies at the time of the Census since many developments were initially occupied only months prior to the Census enumeration. Overcounts are potentially due to incorrect identification or double-counts of households due to enumerator revisitations.

Identifying Eligible Households for Public Housing

To construct a comparison group of households eligible for public housing, I use a methodology developed by Allen and Van Riper (2020) which constructs specific household income eligibility criteria separately for each public housing development by household composition. Although public housing residents are accurately identified in the 1940 Census, little historic information is available to identify the set of households which applied for and did not receive public housing assistance (i.e. the set of eligible households). Since the only federal eligibility requirement for public housing stated in the Housing Act of 1937 was household income, local housing authorities had a large amount of autonomy selecting tenants:

> *The term "low-rent housing" means decent, safe, and sanitary dwellings within the financial reach of families of low income, and developed and administered to, promote serviceability, efficiency, economy, and stability, and embraces all necessary appurtenances thereto. The dwellings in low-rent housing as defined in this Act shall be available solely for families whose net income at the time of admission does not exceed five times the rental (including the value or cost to them of heat, light, water, and cooking fuel) of the dwellings to be furnished such families, except that in the case of families with three or more minor dependents, such ratio shall not exceed six to one.* (US Housing Act, 1937)

Along with the Housing Act's minimum income requirement for eligibility, Allen and Van Riper

(2020) consider several additional demographic factors they believe were influential in local authorities' determination of household eligibility and rental rates to construct a comparison group. First, local public housing authorities often designated race-specific public housing projects. Second, the population of 1940 public housing residents contained a high proportion of traditionally-defined nuclear families, hence the success rate of receiving public housing for large multi-family household was low. Third, household income and rent qualifications were tied to the construction cost for PWA public housing projects. Fourth, many public housing residents' income began to exceed eligibility limits as of the 1940 Census as the economy was beginning to recover from the Great Depression. To prevent eviction of public housing residents due to rising income, local authorities often disregarded the federal income restriction for current tenants. Fifth, rental rates varied by household size since they were based on the size of the housing unit. Finally, public housing units were highly desirable among qualified households creating a large incentive to apply.

To recover household income eligibility ranges for each public housing development and household type, I follow Allen and Van Riper (2020) and construct eligibility thresholds which relies on monthly rent paid by current public housing tenants to determine the set of households who qualified for but did not live in public housing. For each public housing development, $i \in I$, define household types $t \in T$ using the three criteria: number of family members, whether there are three or more children in the household, and the race of the household. Then for each public housing development, i, for each household type, t, the income eligibility criteria range, $[min\_hhincome_{it}, max\_hhincome_{it}]$, is defined as:

$$min\_hhincome_{it} = rent\_p90_{it} * 12 \tag{1.1}$$

$$max\_hhincome_{it} = rent\_p90_{it} * 12 * wage2rent\_ratio_{it} \tag{1.2}$$

Where $rent\_p90_{it}$ is the 90^{th} percentile of average self-reported monthly rent paid and $wage2rent\_ratio_{it}$ is the federally determined wage-to-rent ratio (5 or 6) based on the 1937 Housing Act. Households with zero value monthly rent were not included in the estimation. Rather than the mean or median reported monthly rents, the 90^{th} percentile is preferred in order that household income for the comparison group is greater on average than public housing residents who were subjected to income restrictions. Due to this sample restriction, the estimated effects of receiving public housing assistance will likely be more conservative than if the constructed sample of eligible households included lower income households. Hence, low-income should not be a significant factor in differential

child schooling outcomes between public housing residents and the comparison groups. While the Wagner-Steagall Act defined income eligibility based upon rental value and utility costs, information regarding other costs of living (heat, light, water and cooking fuel) is not available.

Although 1940 Census enumerators were instructed to enter monthly rental rates as whole numbers some enumerators entered non-integer values. To account for potential transcription errors, if rent was greater than $750 and would place a rental burden greater than 75% of annual income, the observed value was divided by a factor of 100 – the rationale being that households likely did not pay rent equivalent to either 75% or more of total household since that would leave few resources for other essential goods, and $750 in monthly rental expenses would be too high for the average household in 1940 (equivalent to roughly $14,000 in US$2020). For public housing residents, monthly rent recorded as greater than $100 was divided by either a factor of 10 or 100 if greater than $99 or $999. Once the income eligibility criteria were estimated using information on current public housing resident's monthly rent, the comparison group was drawn from the set of households which lived in US cities with an inhabited public housing project as of the 1940 Census, were not currently living in public housing, and were renters. Since income was the only federally mandated eligibility criteria, I constrain the sample used to construct the eligibility criteria to only households without missing household income information. If misreported or missing income values are randomly distributed, this should not introduce additional estimation bias.

While legislation did not target public housing assistance towards single family households with children, a high proportion of public housing units were constructed as single family units and subjected to occupancy limits to prevent overcrowding (Allen & Van Riper, 2020). Therefore, constructing an eligible comparison group of households based directly on Census enumeration can potentially cause multi-family or multi-generational households not residing in public housing to be misclassified as ineligible although each nuclear family may have separately applied and potentially been eligible for public housing. To decompose multi-family households into the set of households who could have potentially applied for public housing as a single family unit, I split non-public housing households based on the household subfamily unit.[7] Once split, I estimate the household income for each subfamily and assign eligibility for each subfamily household if they satisfy the in-

[7]For example consider a household which contains two parents, their two sons, one single and one married with children. This would be defined as a single household however it contains two sub-families: two parents and their un-married son, and their son, his wife and child.

come criteria for public housing. The final columns of Tables 1.1 and 1.2 show the overall number of households in public housing and eligible households identified using this method by public housing development as of the 1940 Census.

Constructing the Coarsened Exact Matched Child Sample

The federal government allows each state to establish its own compulsory schooling and child labor laws (Acemoglu & Angrist, 2000; Goldin & Katz, 2011; Stephens Jr & Yang, 2014). During the 1940s, most state compulsory schooling laws required children to begin primary school at age 7 and attend until age 16. In order to estimate the effect of receiving public housing on children's short-term educational attainment, for each state I subset the population to children old enough to be required to attend school due to state compulsory schooling minimum age laws up to age 18. To identify the population of children required to attend school and construct measures of schooling attendance and on-time educational attainment as of 1940, I use state-year cohort measures of the required years of schooling from Stephens Jr and Yang (2014) which revised measures of overall childhood exposure to compulsory schooling and child labor laws previously compiled by Acemoglu and Angrist (2000), Goldin and Katz (2011), and Lleras-Muney (2002) by interating through ages 6 to 17 to determine whether the child is required to attend school at that age based on the law that in place that same year. For each child, i, of age, a, in state, s, I merge information on both the age requirement to start attending school at age 6, $ReqAgeStart_i^{Age6}$, and total required years of schooling, $TotalReqYrSchool_i^{Age6}$.[8] Using this information, I define the years of schooling required as of 1940, $ReqYrSchool_{as}^{1940}$, as either the total required years of school legislated as of age 6 defined by Stephens Jr and Yang (2014), or the total state mandated years of schooling for a child as of 1940:

$$ReqYrSchool_{i(a,s)}^{1940} = \min\{TotalReqYrSchool_{as}^{Age6},\ Age_{as}^{1940} - ReqAgeStart_{as}^{Age6} + 1\} \tag{1.3}$$

For the analysis, I include any child with a positive value for $ReqYrSchool_{as}^{1940}$.

In order to estimate the effect of receiving public housing on children's short-term educational attainment, I use coarsened exact matching (CEM) to construct sample weights and compare out-

[8] Age requirements as of age 6 are used since this was the earliest age required to attend school in 1940.

comes between children living in public housing to a demographically similar comparison group of eligible children which did not receive housing assistance (Blackwell et al., 2009; Iacus et al., 2012). For each public housing development, I construct the matched sample of eligible children not living in public housing using the following demographic characteristics which may affect a child's educational attainment: family household size, whether three or more minors are present in the household, child's age, whether the child was female, non-white, or a US citizen, and total household income.[9]

1.4 Descriptive Statistics

Table 1.3 presents summary statistics for the child populations living in public housing or eligible to receive housing assistance. Nearly 19,200 children were identified as living in public housing and over 1.8 million children were eligible for housing assistance using income criteria and compulsory schooling requirements. Relative to the comparison group, children living in public housing were on average younger, more likely to be nonwhite, married, a US citizen, and attending school. Households in public housing with children were smaller on average, had an estimated monthly household income approximately $1,300 more than the comparison group ($2,300), and 20% of the sample population received nonwage, nonsalary income of more than $50 the previously year.

Tables 1.4 and 1.5 show the count of children by public housing development for children living in public housing, the set of eligible children based on Allen and Van Riper (2020), and the CEM matched sets of eligible children. Tables 1.6-1.9 present balance tables for child demographic used as the matching variables. I present descriptive statistics for both CEM and CEM k-to-k weighted samples as robustness.

Tables 1.6 to 1.8 compare the set of children living in public housing to children in households which were eligible to apply but were not living in public housing. Compared to the unweighted set of eligible households, a larger proportion of the child population in public housing were nonwhite, younger, lived in smaller household units, and were more likely to be attending school as shown in Table 1.6. Children in public housing were also significantly more likely to currently be attending school (4.6%), less likely to be employed (-8.9%), and had 0.5 years of schooling less than the

[9] I also include an indicator for whether total household income was zero value (i.e. wage income for the previous year was missing for all working persons age 14+ in the household)

comparison group. After applying CEM weights, the covariate balance between children in public housing and the comparison group shown in Table 1.7 improves significantly. I find that differences in child outcomes are smaller and become statistically insignificant except for school attendance (1.5%) and employment in the labor force (-7.0%). However, the proportion of the CEM weighted group of eligible children that were nonwhite or in larger households still significantly differs from the matched sample of children in public housing (See Table 1.7). When CEM weights for an exact matched k-to-k set eligible children are applied in Table 1.8, there is no significant difference in matched demographic covariates between groups except for total annual household income ($1.5). Table 1.8 shows that on average children who received public housing assistance were 2.9% more likely to currently be enrolled in school, 5.8% less likely to be in the labor force, were 4.3% more likely to be on-time to complete schooling, and had 0.2 more years of schooling than children in the eligible group. If receipt of public housing was conditional on only these observed demographics used to match public housing tenants and eligible households, then using CEM weights with exact k-to-k matching would estimate the least biased causal effect of public housing receipt on children's short-term educational outcomes. Table 1.9 summarizes the child outcomes of interest and matched demographics for the sample populations using each weighting method.

1.5 Methodology

Measuring Short-term Educational Attainment

Public housing assistance could improve a child's short-term educational attainment through multiple channels. The provision of stable and safe living conditions in public housing may improve the quality of children's home learning environment increasing the likelihood a child is on time to complete school or currently attending school. More affordable housing may also relax public housing households' budget constraint allowing them to allocate more of their children's time towards human capital investment in lieu of labor productivity (i.e. more schooling). Receipt of public housing may have particularly strong effects among subgroups such as females or nonwhite minorities who had lower marginal economic returns to additional non-compulsory education given historic labor market discrimination and social norms.

To examine short-term educational attainment, I focus on four child outcome measures as of 1940: whether a child is attending school, whether a child is on-time to complete the required years of schooling, whether a child age 14 or older is employed in the labor force, and the total years of school completed by a child. To determine the effect of public housing assistance on children's on-time completion of school, for each child, I define them as being on-time to complete school, $OnTime_i$, if the number of school years completed or currently in progress by the child is greater than or equal to the required years of school defined above in equation (1.3):

$$OnTime_{i(a,s)} = \{1 \text{ if } YrsSchoolCompleted^{1940}_{i(a,s)} \geq ReqYrSchool^{1940}_{as} \ ; \ 0 \text{ otherwise}\} \tag{1.4}$$

Where $OnTime_i$ is a binary indicator for whether the level of education completed or currently enrolled by a child of age a in state s is greater than or equal to the state-mandated years of education determined using state compulsory schooling, child labor laws, and the mandated age to start enrollment (Stephens Jr & Yang, 2014).

Estimating the Effect of Public Housing Receipt

CEM is only a technique to preprocess data in order to reduce the model-dependency of sample average treatment on the treated (SATT) estimates by constructed a matched comparison sample (Blackwell et al., 2009). The effect of receiving public housing on child short-term educational attainment outcomes is estimated using both logit and linear probability models:

$$Y_i = \delta PublicHousing_i + \beta X_i + \alpha_a + \gamma_s + \epsilon_i \tag{1.5}$$

where Y_i are the child outcomes of interest (whether a child is currently attending school, whether a child is on-time to complete the required years of schooling, whether a child age 14 or older is employed in the labor force, and the total years of school completed by a child as of 1940), $PublicHousing_i$ is an indicator for whether the child lived in public housing as of the 1940 census, X_i is the set of demographic covariates, α_a and γ_s are age and state fixed-effects, and ϵ_i is the error term. In other specifications, I include interaction terms between $PublicHousing_i$ and indicators for child sex (female), race (nonwhite), and whether the child had already completed the required years of schooling as of 1940 to assess the effect of public housing receipt for these subgroups. Standard

errors are clustered at the state-level. For the main results, I interpret coefficients using a linear probability model. Estimates using logistic regression models are included in the Appendix.

Estimating the Effect of Differential Exposure to Public Housing

Estimated effects using CEM are unbiased causal estimates of public housing receipt if controlling for observable variation across public housing child and eligible child populations also controls for any unobservable variation which may affect the likelihood of public housing receipt or the child outcomes of interest. However since the tenant application process for public housing was highly competitive, there is potential selection bias due to unobservable economic or behavioral tenant characteristics such as creditworthiness or household cleanliness which would likely lead to overstated effects of public housing receipt using CEM. An alternative approach which controls for any potential unobservable selection bias related to public housing assistance is to estimate the effect of differential rates of public housing exposure for only children currently living in public housing as of the 1940 Census. Since all public housing residents were subject to similar tenant selection criteria, estimates using only the subsample of children currently living in public housing should not suffer from bias due to tenant selection.

To estimate the effect of differential rates of public housing exposure on child outcomes, I construct a measure of child exposure to public housing using information on the public housing development's published occupancy date (US Department of the Interior, 2004) and the 1940 Census date, April 1st, 1940:

$$Exposure_i = OccupancyDate_d - 1940CensusDate \tag{1.6}$$

Where $Exposure_i$ is the length of time lapsed between the public housing's occupancy date, $OccupancyDate_d$, and the 1940 Census, $1940CensusDate$ measured in years for all children living in public housing development d. For this measure to accurately represent a child's exposure to public housing, the underlying assumption is that households residing in public housing developments as of the 1940 Census were the initial tenants. Therefore, due to limited information on households' true duration of occupancy in public housing, $Exposure_i$ can be interpreted as the intent to treat effect of receiving public housing. However, since receipt of a public housing unit was highly competitive and contemporary policies allowed tenants to remain in public housing as the economy improved

following the Great Depression, the initial public housing tenants had few economic incentives to relocate.

The distribution of children in public housing by age and differential lengths of public housing exposure is shown in Table 1.10. The sample contains 19,190 children age 6 to 18 living in public housing who were required to have attended school.[10] Using only children residing in public housing, I estimate the effect of receiving public housing assistance on educational outcomes using the following model:

$$Y_i = \delta Exposure_i + \beta X_i + \alpha_a + \gamma_s + \epsilon_i \tag{1.7}$$

Similar to equation (1.5), Y_i are outcomes of interest, α_a and γ_s are age and state fixed-effects and X_i is the set of demographic controls. Here $Exposure_i$ is the length of public housing exposure measured in years defined by equation (1.6). Again, equation (1.7) is estimated in other models with additional interaction terms. Standard errors are clustered at the state-level. I use a linear probability model for the main results. Estimates using logistic regression models are included in the Appendix.

1.6 Results

The Effect of Receiving Public Housing

Results using CEM to estimate the effect of receiving public housing using equation (1.5) are presented by child outcome of interest (child currently attends school, on-time to complete compulsory schooling requirement, employed in the labor force, and total years of school completed). First, the effect of public housing receipt on the likelihood a child was currently attending school are shown in Table 1.11. For each comparison group (unweighted, CEM, CEM k-to-k) I use two model specifications. The first model only controls for whether a child lived in public housing and fixed effects for child age and state of residence. The preferred specification is the full model which controls for additional demographics which may impact child educational attainment (female, non-white, older than the compulsory school age, citizenship, two parent household, total household income, whether three

[10] The distribution of children which have non-zero values for household income used to construct the income criteria for the set of eligible households is shown in Table 5.1 in the Appendix.

or more children lived in the household, and total household size) and includes interaction terms with public housing receipt for whether the child was female, non-white, and older than compulsory school age in order to assess differential effects by demographic.[11]

Across model specifications, I find that children in public housing were significantly more likely to currently be attending school than the comparison group. Interpreting the coefficient on Public Housing in column (6) of Table 1.11 which includes the full set of demographic covariates and interaction terms, children living in public housing as of the 1940 Census were 1.1 percentage points (pp) more likely to currently be attending school than the CEM k-to-k matched comparison group at the 5% confidence level. The overall effect is significantly larger for non-white (4.2pp), and older children (6.1pp) relative to their peers who did not live in public housing.

Second, the effect of public housing receipt on the likelihood a child was on-time to complete the compulsory schooling requirements are shown in Table 1.12. Compared to the full and CEM weighted comparison group of children, I find that non-white and older children living in public housing were significantly more likely to be on-time to complete compulsory schooling requirements. Compared to the CEM k-to-k matched sample of eligible children in column (6) of Table 1.12, I find that on average children living in public housing were 3.2pp more likely to be on-time to complete schooling at the 5% confidence level and the magnitude of the total effect was significantly larger for non-white (6.1pp), and older children (6.1pp) relative to children who did not live in public housing.

Third, the effect of public housing receipt on the likelihood a child age 14 or over was employed in the labor force are shown in Table 1.13. Consistent with finding that children living in public housing were more likely to currently be attending school, column (6) of Table 1.13 shows that the effect is largely driven by significantly less non-white (-8.9pp) and older (-6.1pp) children in public housing employed in the labor force. Finally, the effect of public housing receipt on the total years of schooling completed or currently enrolled are shown in Table 1.14. Compared to the full and CEM weighted comparison group of children, I find no significant difference on average in years of school completed for children in public housing. However using the k-to-k matched CEM sample, I do find that children in public housing had more schooling completed, largely due to differences among non-white and older children. Compared to the CEM k-to-k matched sample of eligible children in column (6) of Table 1.14, on average white, male children required to attend school who lived in

[11]The full model also includes an indicator for whether household income information was missing

public housing did not have significantly more years of schooling completed than the comparison group at the 10% confidence level. However, I find that nonwhite (0.3 years) and older children not required to attend school (0.4 years) had significantly more schooling relative to their comparison group.

The Effect of Greater Public Housing Exposure

As previously stated, CEM estimates the causal effect of receiving public housing on children if matching on observable variation across child populations living in or eligible for public housing accounts for any unobservable variation which may be correlated to either the likelihood of public housing receipt or the child outcomes. Alternatively, I also examine whether children with greater exposure to public housing (i.e. longer total time elapsed since initial occupancy) were more likely to have greater short-term educational attainment using variation in the total time elapsed from the initial occupancy date to the 1940 Census. Results by child outcome of interest are shown in Table 1.15. Similar to prior result tables, I use two model specifications for each comparison group. The first model only controls for whether the child lived in public housing and fixed effects for child age and state of residence. The preferred full specification controls for the additional demographics stated previously.

Examining the effect of greater public housing exposure on the likelihood the child was currently attending school, I find that an additional year of public housing exposure significantly increased the likelihood a children was currently enrolled in school by 1.4pp at the 1% confidence level under the base model in Table 1.15. Controlling for child demographics and subpopulation-specific effects in the full model, the effect is largely driven by an increased likelihood of attending school for older children (4.9pp). I find no significant effect of additional public housing exposure on the likelihood a child was on-time to complete compulsory schooling requirements, and only a significant effect on total years of schooling completed by older children (0.1 years) at the 10% confidence level. I also find that children who lived in public housing longer and were no longer required to attend schooling were less likely to be employed (-2.1pp), however I do find that female (1.5pp) and non-white (2.3pp) children with greater exposure to public housing were significantly more likely to be employed in the labor force at the 5% confidence level.

1.7 Conclusion

In this study, I examine the effect of receiving public housing on children's short-term educational attainment during the Great Depression using two estimation methods. First, I construct a comparison group of children which may have been eligible and applied for public housing in order to estimate credible effects of public housing receipt for child educational attainment on the extensive margin. Using coarsened exact matching to compare the short-term educational outcomes of children in public housing to similar children not living in public housing as of the 1940 Census, I find that children living in public housing were more likely to currently be attending school and on-time to complete schooling requirements. Children who lived in public housing were also less likely to be employed in the labor force and had completed more years of schooling than the comparison groups. These effects were driven by female, non-white, and older child subpopulations.

Second, the effect of greater exposure is estimated on the intensive margin using only children living in public housing to remove any estimation bias due to unobservable tenant selection criteria. Comparing short-term educational attainment outcomes among only children living in public housing, I find that greater exposure to public housing increased school attendance among female and older children, as well as reduced the likelihood of labor force participation and increased overall schooling among older children. On average, I find no significant effect of public housing exposure for white male children required to attend school. Therefore effects of public housing receipt on both the extensive and intensive margin are driven largely by improved educational outcomes among non-white, female, or older child subpopulations.

These results are consistent with the theory that public housing assistance reduced the financial rent burden on household's budget constraint allowing them to allocate more of their children's time towards human capital production activities instead of labor. Given that compulsory schooling laws were likely the binding constraint in households' child labor decision, receipt of public housing had little effect on the short-term educational attainment outcomes for children still required to attend school. Children who experienced the greatest benefit from public housing receipt are those from subgroups who had the lowest marginal benefit economically from more schooling in terms of expected future labor wages due to labor market discrimination and societal norms (non-white and female children) or children which had the highest opportunity costs of additional schooling in terms

of loss current labor wages (older children who completed schooling requirements). The ability of families receiving public housing assistance to allow their children to remain in school longer may provide an important channel of inter-generational mobility.

There are a few data limitations which affect the accuracy of measuring the effect of public housing receipt. Although several strategies were taken to construct the group of households who may have applied for but not received public housing assistance, estimated effects of public housing on the intensive margin may suffer from unobservable selection bias not controlled for either through CEM matching or in the regression model. Further, I do not observe the true duration of public housing residency for tenants. Therefore estimated effects from the duration analysis likely overestimate public housing exposure effects on child educational attainment if residency churn rates within public housing developments were non-zero and endogenous to other characteristics which may affect households' child investment. However, if tenant turnover was low and the churn rate was similar across public housing developments, estimates still represent the relative effect of receiving additional public housing exposure. Although prior studies have used information on child's birth quarter to more precisely estimate the effect of compulsory school laws, this variable was not transcribed for the IPUMS-USA complete count 1940 Census file used in the analysis. Income values enumerated in the 1940 Census also have known transcription issues which limits its accuracy. However, if transcription error is randomly distributed this should not introduce bias but decrease precision. Finally, while this study finds that children who received public housing assistance had higher educational attainment, it does not definitively identify the underlying mechanisms for greater educational outcomes.

This study examines the effect of public housing receipt during the Great Depression for the earliest public housing tenants over 80 years ago, however the economic context of this study can still inform current debates regarding affordable housing policy in the US. As well as providing affordable housing, early public housing developments were well-built, designed to include public spaces, provided residents communal and social services, and established interpersonal networks between households from various socio-economic backgrounds which may have improved children's short-term education particularly among historically marginalized demographics. Taking new approaches towards public housing construction which integrate mixed-use, mixed-income, and mixed-ownership housing could reduce the concentration of poverty and create economic opportunities within public housing developments.

Future research could improve upon the identification of households which applied for but did not receive public housing by linking information on waitlists or tenant applications and evaluation forms from the National Archives and other historical resources. Similarly, additional research can be undertaken to better identify public housing residents' true duration of stay. Finally, additional work should be done to further understand the specific mechanisms which improved public housing children's educational outcomes (housing quality, income, enrollment in extra-curriculars, social networks, neighborhood effects), and the long-term effect of public housing assistance for these children's overall educational attainment, labor productivity, and inter-generational mobility.

1.8 Tables

Table 1.1: Public housing developments at the time of the 1940 Census (1/2)

Public Housing Development	City	Program	Date Occupied	Housing Units	1940 Census Households	Eligible Households
Techwood Homes	Atlanta	PWA	8/1/1936	604	570	16295
University Homes	Atlanta	PWA	4/1/1937	675	674	15709
Stanley S Holmes Village	Atlantic City	PWA	4/1/1937	277	276	1792
Cherry Tree Crossing	Augusta	USHA	3/1/1940	168	71	3356
Olmstead Homes	Augusta	USHA	3/1/1940	167	87	1978
Chalmers St	Austin	USHA	1/1/1940	87	88	2292
Rosewood Courts	Austin	USHA	9/1/1939	60	53	1095
Santa Rita	Austin	USHA	7/1/1939	40	40	1940
Smithfield Court	Birmingham	PWA	2/1/1938	512	503	11603
Mary Ellen McCormack	Boston	PWA	5/1/1938	1016	988	54259
AD Price Courts	Buffalo	USHA	1/1/1940	173	171	1595
Kenfield	Buffalo	PWA	10/1/1937	658	651	38159
Lakeview Homes	Buffalo	USHA	12/1/1939	668	564	23229
Newtown Homes	Cambridge	PWA	1/1/1938	294	273	7864
Westfield Acres	Camden	PWA	5/1/1938	514	512	5307
Meeting Street Manor	Charleston	PWA	8/1/1937	212	210	7565
Robert Mills Manor	Charleston	USHA	11/1/1939	140	140	1408
Jane Addams Houses	Chicago	PWA	12/1/1938	1027	936	191139
Julia Lathrop Homes	Chicago	PWA	2/1/1938	925	921	156847
Trumball Park Homes	Chicago	PWA	2/1/1938	462	457	225776
Laurel Homes	Cincinnati	PWA	8/1/1938	1039	978	33368
Lakeview Terrace	Cleveland	PWA	10/1/1937	620	558	69181
Outhwaite Homes	Cleveland	PWA	8/1/1937	579	572	11959
University Terrace	Columbia	PWA	8/1/1937	122	122	5778
Cedar Springs Place	Dallas	PWA	10/1/1937	181	181	14839
Brewster Homes	Detroit	PWA	10/1/1938	701	779	16202
Parkside	Detroit	PWA	10/1/1938	775	767	98258
Cherokee Terrace	Enid	PWA	6/1/1938	80	80	1103
Lincoln Gardens	Evansville	PWA	7/1/1938	191	190	657
Lockefield Garden Apts	Indianapolis	PWA	8/1/1938	748	744	6056
Brentwood Park	Jacksonville	USHA	11/1/1939	234	229	1919
Durkeeville	Jacksonville	PWA	6/1/1937	215	207	5110
Totals				17177	15824	2080511

Data from Public Housing in the United States, 1933-1949 (US Department of the Interior, 2004) and the 1940 Census (Ruggles et al., 2021). Eligible households are defined as family-unit households which would have met the federal eligibility criteria to live in a public housing development.

Table 1.2: Public housing developments at the time of the 1940 Census (1/2)

Public Housing Development	City	Program	Date Occupied	Housing Units	1940 Census Households	Eligible Households
Baker Homes	Lackawanna	PWA	7/1/1938	271	270	909
Bluegrass and Aspendale	Lexington	PWA	1/1/1938	287	283	4717
College Court	Louisville	PWA	1/1/1938	125	123	6848
LaSalle Place	Louisville	PWA	1/1/1938	210	210	19470
Dixie Homes	Memphis	PWA	2/1/1938	636	653	17249
Lauderdale Courts	Memphis	PWA	2/1/1938	449	445	9648
Edison Courts	Miami	USHA	3/1/1940	345	321	5034
Liberty Square	Miami	PWA	2/1/1937	242	242	3844
Parklawn	Milwaukee	PWA	6/1/1937	518	510	48571
Sumner Field Homes	Minneapolis	PWA	12/1/1938	464	461	29341
Paterson Court	Montgomery	PWA	2/1/1937	156	154	3453
Riverside Heights	Montgomery	PWA	6/1/1937	100	94	1619
Andrew Jackson Courts	Nashville	PWA	6/1/1938	398	374	5959
Cheatham Place	Nashville	PWA	2/1/1938	314	312	8419
First Houses	New York	PWA	5/1/1936	123	122	299396
Harlem River Houses	New York	PWA	10/1/1937	576	576	54743
Queensbridge	Long Island City	USHA	10/1/1939	3148	3148	365791
Red Hook	Brooklyn	USHA	7/1/1939	2545	2527	398324
Williamsburg	Brooklyn	PWA	4/1/1938	1622	1607	522197
Will Rogers Court	Oklahoma City	PWA	12/1/1937	354	346	6873
Logan Fontenelle	Omaha	PWA	8/1/1936	284	284	13694
Southside Terrace	Omaha	USHA	4/1/1940	522	155	12399
Hill Creek	Philadelphia	PWA	3/1/1938	258	257	99825
Schonowee Village	Schenectady	PWA	7/1/1938	219	219	4769
Fairfield Court	Stamford	PWA	9/1/1937	146	146	3837
Pioneer Homes	Syracuse	USHA	2/1/1940	678	324	11021
Brand Whitlock Homes	Toledo	PWA	3/1/1938	264	259	1975
Charles Weiler Homes	Toledo	USHA	3/1/1940	384	271	13214
Major Bowman	Vincennes	USHA	4/1/1940	83	51	638
Langston Terrace	Washington	PWA	5/1/1938	274	271	21071
Highland Homes	Wayne	PWA	3/1/1938	50	49	1828
Olde Cedar Apartments	Cleveland	PWA	8/1/1937	654	560	65449
Pennington Court	Newark	USHA	3/1/1940	236	187	16998
Jordan Park	St. Petersburg	USHA	4/1/1940	242	13	1388
Totals				17177	15824	2080511

Data from Public Housing in the United States, 1933-1949 (US Department of the Interior, 2004) and the 1940 Census (Ruggles et al., 2021)

Table 1.3: Child Characteristics for Public Housing Residents and Eligible Households in 1940

	Public Housing Residents	Eligible Households	$Difference$	$p-value$
Child Characteristics				
Age: 6 to 8	22.06	17.10	$4.96 * **$	(0.00)
9 to 11	28.97	25.98	$2.99 * **$	(0.00)
12 to 14	23.89	25.08	$-1.20 * **$	(0.01)
15 to 18	25.08	31.83	$-6.75 * **$	(0.00)
Female	50.85	50.03	0.82	(0.18)
White	78.32	93.98	$-15.66 * **$	(0.00)
Non-white	21.68	6.02	$15.66 * **$	(0.00)
Married	1.54	0.95	$0.59 * *$	(0.02)
Native Born	98.21	96.73	$1.49 * **$	(0.00)
Naturalized Citizen	1.14	1.36	-0.23	(0.20)
Foreign Born	0.65	1.91	$-1.26 * **$	(0.00)
No school	2.32	1.78	$0.54*$	(0.09)
Kindergarten-7th grade	63.48	59.84	3.64	(0.22)
8th grade	7.38	9.50	$-2.12 * **$	(0.00)
High school, no degree	17.98	22.22	$-4.24 * **$	(0.00)
High school	3.03	4.13	$-1.10 * **$	(0.00)
More than high school	0.89	0.86	0.03	(0.91)
Missing	4.92	1.67	3.25	(0.37)
Employed	9.69	13.70	$-4.01 * **$	(0.00)
Unemployed	5.84	10.73	$-4.89 * **$	(0.00)
Not in labor force	84.47	75.56	$8.90 * **$	(0.00)
Household Characteristics				
1-2 people	3.18	4.02	$-0.84 * *$	(0.01)
3 people	17.51	14.13	$3.38 * **$	(0.00)
4 people	29.10	25.52	$3.59 * **$	(0.00)
5 people	24.66	26.60	-1.94	(0.19)
6 or more people	25.53	29.73	$-4.20 * *$	(0.03)
Total Household Income ($)	2,335.26	1,032.78	$1,302.48 * **$	(0.00)
Missing Household Income (%)	7.51	0.00	$7.51 * **$	(0.00)
Monthly Rent ($)	23.20	28.74	$-5.54 * *$	(0.02)
Missing Monthly Rent (%)	0.82	1.37	$-0.55*$	(0.05)
More than $50 nonwage, nonsalary income (%)	21.46	24.56	$-3.10 * *$	(0.03)
Observations	19,190	1,875,551		

Table 1.4: Child Sample by Public Housing Development

Public Housing Development	City	Program	Resident Children	Eligible Children	CEM Matched Children	CEM k2k Matched Children
Techwood Homes	Atlanta	PWA	288	10339	1252	474
University Homes	Atlanta	PWA	268	7614	1092	392
Stanley S Holmes Village	Atlantic City	PWA	123	666	184	134
Cherry Tree Crossing	Augusta	USHA	61	2109	133	78
Olmstead Homes	Augusta	USHA	66	1573	167	74
Chalmers St	Austin	USHA	65	1779	136	66
Rosewood Courts	Austin	USHA	27	698	44	26
Santa Rita	Austin	USHA	33	1700	74	26
Smithfield Court	Birmingham	PWA	175	7320	250	106
Mary Ellen McCormack	Boston	PWA	714	37356	6909	728
AD Price Courts	Buffalo	USHA	82	697	88	50
Kenfield	Buffalo	PWA	684	24512	5097	1070
Lakeview Homes	Buffalo	USHA	416	17712	2764	614
Newtown Homes	Cambridge	PWA	322	4939	913	308
Westfield Acres	Camden	PWA	227	2881	572	330
Meeting Street Manor	Charleston	PWA	144	5414	368	166
Robert Mills Manor	Charleston	USHA	139	1102	461	174
Jane Addams Houses	Chicago	PWA	715	113249	13098	816
Julia Lathrop Homes	Chicago	PWA	710	111020	13364	936
Trumball Park Homes	Chicago	PWA	434	133145	120	10
Laurel Homes	Cincinnati	PWA	586	20372	3305	700
Lakeview Terrace	Cleveland	PWA	377	47046	3750	594
Outhwaite Homes	Cleveland	PWA	459	7992	1402	578
University Terrace	Columbia	PWA	86	3820	109	60
Cedar Springs Place	Dallas	PWA	73	9162	328	76
Brewster Homes	Detroit	PWA	479	10039	2397	694
Parkside	Detroit	PWA	583	69201	7712	720
Cherokee Terrace	Enid	PWA	35	664	41	20
Lincoln Gardens	Evansville	PWA	65	282	33	16
Lockefield Garden Apts	Indianapolis	PWA	274	2982	532	336
Brentwood Park	Jacksonville	USHA	205	1485	492	144
Durkeeville	Jacksonville	PWA	105	3186	366	110
Totals			9020	662056	67553	10626

Data from Public Housing in the United States, 1933-1949 (US Department of the Interior, 2004) and the 1940 Census (Ruggles et al., 2021)

Table 1.5: Child Sample by Public Housing Development

Public Housing Development	City	Program	Resident Children	Eligible Children	CEM Matched Children	CEM k2k Matched Children
Baker Homes	Lackawanna	PWA	276	926	383	202
Bluegrass and Aspendale	Lexington	PWA	110	2508	139	96
College Court	Louisville	PWA	42	2885	60	44
LaSalle Place	Louisville	PWA	152	13130	475	164
Dixie Homes	Memphis	PWA	249	7196	662	322
Lauderdale Courts	Memphis	PWA	226	5166	635	306
Edison Courts	Miami	USHA	267	2070	720	208
Liberty Square	Miami	PWA	165	2141	377	144
Parklawn	Milwaukee	PWA	448	36029	4981	754
Sumner Field Homes	Minneapolis	PWA	325	16654	1462	336
Paterson Court	Montgomery	PWA	58	1715	135	22
Riverside Heights	Montgomery	PWA	32	882	45	22
Andrew Jackson Courts	Nashville	PWA	137	2457	497	190
Cheatham Place	Nashville	PWA	107	4930	387	94
First Houses	New York	PWA	73	139682	2150	82
Harlem River Houses	New York	PWA	420	27926	3766	634
Queensbridge	Long Island City	USHA	1982	225541	51755	1948
Red Hook	Brooklyn	USHA	1787	247807	54590	2246
Williamsburg	Brooklyn	PWA	1319	301078	56597	2016
Will Rogers Court	Oklahoma City	PWA	204	4929	665	256
Logan Fontenelle	Omaha	PWA	165	7518	621	196
Southside Terrace	Omaha	USHA	111	7379	435	174
Hill Creek	Philadelphia	PWA	150	62812	51	10
Schonowee Village	Schenectady	PWA	80	2450	124	88
Fairfield Court	Stamford	PWA	94	2918	180	118
Pioneer Homes	Syracuse	USHA	173	6784	709	140
Brand Whitlock Homes	Toledo	PWA	123	1290	176	120
Charles Weiler Homes	Toledo	USHA	197	10439	835	276
Major Bowman	Vincennes	USHA	53	428	71	46
Langston Terrace	Washington	PWA	151	10464	749	252
Highland Homes	Wayne	PWA	22	945	31	12
Olde Cedar Apartments	Cleveland	PWA	335	44697	3053	466
Pennington Court	Newark	USHA	135	9424	405	126
Jordan Park	St. Petersburg	USHA	2	295	2	2
Totals			10170	1213495	187923	12112

Data from Public Housing in the United States, 1933-1949 (US Department of the Interior, 2004) and the 1940 Census (Ruggles et al., 2021)

Table 1.6: Balance Table by Public Housing Receipt: Unweighted

	Public Housing Residents	Eligible Households	$Difference$	$p-value$
Currently attending school (%)	90.70	86.13	$4.57 * **$	(0.00)
Years of schooling	5.57	6.10	$-0.53 * **$	(0.00)
On time for school completion (%)	73.87	74.89	-1.03	(0.68)
Employed in labor force (%)	15.53	24.44	$-8.90 * **$	(0.00)
1-2 people	3.18	4.02	$-0.84 * *$	(0.01)
3 people	17.51	14.13	$3.38 * **$	(0.00)
4 people	29.10	25.52	$3.59 * **$	(0.00)
5 people	24.66	26.60	-1.94	(0.19)
6 or more people	25.53	29.73	$-4.20 * *$	(0.03)
Three or more minors in household (%)	45.04	43.19	1.85	(0.32)
Child's Age	11.74	12.38	$-0.64 * **$	(0.00)
Female	50.85	50.03	0.82	(0.18)
Non-white	21.68	6.02	$15.66 * **$	(0.00)
Total Household Income ($)	2,335.26	1,032.78	$1,302.48 * **$	(0.00)
Missing Household Income (%)	7.51	0.00	$7.51 * **$	(0.00)
Observations	19,190	1,875,551		

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. P-values are reported in parentheses. The unit of observation are children. The observable demographics used to match the comparison sample using Coarsened Exact Match are: number of household members, whether there were three or more minors in the household, child's age, sex, and whether the child was non-white, total annual household income, and an indicator for whether total household income was missing value.

Table 1.7: Balance Table by Public Housing Receipt: CEM Weights

	Public Housing Residents	Eligible Households	$Difference$	$p-value$
Currently attending school (%)	91.26	89.73	1.53*	(0.07)
Years of schooling	5.29	5.35	−0.06	(0.67)
On time for school completion (%)	72.78	77.30	−4.52	(0.25)
Employed in labor force (%)	16.05	23.02	−6.98 * **	(0.00)
1-2 people	2.98	2.12	0.86 * *	(0.04)
3 people	17.50	15.82	1.68*	(0.09)
4 people	28.70	32.78	−4.09 * **	(0.00)
5 people	24.91	26.17	−1.26 * *	(0.04)
6 or more people	25.91	23.09	2.81 * **	(0.01)
Three or more minors in household (%)	45.74	43.66	2.08	(0.26)
Child's Age	11.44	11.41	0.02	(0.83)
Female	51.00	49.86	1.14	(0.12)
Non-white	20.58	4.84	15.74 * **	(0.00)
Total Household Income ($)	1,126.76	1,136.62	−9.86	(0.71)
Missing Household Income (%)	0.00	0.00	0.00	()
Observations	11,433	244,043		

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. P-values are reported in parentheses. The unit of observation are children. The observable demographics used to match the comparison sample using Coarsened Exact Match are: number of household members, whether there were three or more minors in the household, child's age, sex, and whether the child was non-white, total annual household income, and an indicator for whether total household income was missing value.

Table 1.8: Balance Table by Public Housing Receipt: CEM k2k Weights

	Public Housing Residents	Eligible Households	$Difference$	$p-value$
Currently attending school (%)	91.24	88.36	$2.88 * **$	(0.00)
Years of schooling	5.29	5.08	$0.21 * **$	(0.00)
On time for school completion (%)	72.90	68.61	$4.29 * **$	(0.00)
Employed in labor force (%)	16.04	21.80	$-5.76 * **$	(0.00)
1-2 people	2.99	2.99	-0.00	(1.00)
3 people	17.44	17.44	0.00	()
4 people	28.72	28.72	0.00	(1.00)
5 people	24.84	24.84	0.00	()
6 or more people	26.01	26.01	0.00	(1.00)
Three or more minors in household (%)	45.78	45.78	0.00	()
Child's Age	11.44	11.44	0.00	(1.00)
Female	50.94	50.94	0.00	(1.00)
Non-white	20.38	20.38	0.00	()
Total Household Income ($)	1,126.57	1,125.07	$1.50*$	(0.07)
Missing Household Income (%)	0.00	0.00	0.00	()
Observations	11,369	11,369		

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. P-values are reported in parentheses. The unit of observation are children. The observable demographics used to match the comparison sample using Coarsened Exact Match are: number of household members, whether there were three or more minors in the household, child's age, sex, and whether the child was non-white, total annual household income, and an indicator for whether total household income was missing value.

Table 1.9: Coarsened Exact Matching Balance Table by Public Housing Receipt: Weighted

	Unweighted		CEM Weighted		CEM k2k Weighted	
	Public Housing	Eligible	Public Housing	Eligible	Public Housing	Eligible
Child Outcomes of Interest						
Currently attending school (%)	90.70	86.13	91.26	89.73	91.24	88.36
Years of schooling	5.57	6.10	5.29	5.35	5.29	5.08
On time for school completion (%)	73.87	74.89	72.78	77.30	72.90	68.61
Employed in labor force (%)	15.53	24.44	16.05	23.02	16.04	21.80
CEM Matched Demographic Covariates						
Number of household members						
1-2 people	3.18	4.02	2.98	2.12	2.99	2.99
3 people	17.51	14.13	17.50	15.82	17.44	17.44
4 people	29.10	25.52	28.70	32.78	28.72	28.72
5 people	24.66	26.60	24.91	26.17	24.84	24.84
6 or more people	25.53	29.73	25.91	23.09	26.01	26.01
Three or more minors in household (%)	45.04	43.19	45.74	43.66	45.78	45.78
Child's Age	11.74	12.38	11.44	11.41	11.44	11.44
Female	50.85	50.03	51.00	49.86	50.94	50.94
Non-white	21.68	6.02	20.58	4.84	20.38	20.38
Total Household Income ($)	2335.26	1032.78	1126.76	1136.62	1126.57	1125.07
Missing Household Income (%)	7.51	0.00	0.00	0.00	0.00	0.00
Observations	19190	1875551	11433	244043	11369	11369

Authors' calculations of complete-count 1940 Census data (Ruggles et al., 2021)

Table 1.10: Distribution of Children Age 6-18 by Public Housing Exposure

Age (years)	> 1	$1 - 2$	$2 - 3$	$3 - 4$	Total
6	22	52	133	0	207
7	603	494	783	92	1972
8	607	525	825	97	2054
9	580	510	753	101	1944
10	581	469	710	97	1857
11	527	427	715	90	1759
12	533	409	665	115	1722
13	486	336	565	88	1475
14	401	330	559	97	1387
15	405	284	499	98	1286
16	366	282	475	92	1215
17	324	253	459	77	1113
18	366	276	461	96	1199
Total	5801	4647	7602	1140	19190
Observations	19190				

Authors' calculations of complete-count 1940 Census data (Ruggles et al., 2021)

Table 1.11: Linear Probability Model - Child Currently Attends School

	Unweighted		CEM Weighted		CEM k2k Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Public Housing	0.032***	0.001	0.029***	0.019***	0.023***	0.011**
	(0.005)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)
Public Housing × Female		0.003		−0.004		−0.008
		(0.002)		(0.003)		(0.006)
Public Housing × Non-white		0.040***		0.030***		0.032***
		(0.008)		(0.007)		(0.010)
Public Housing × Older than CSL age		0.044***		0.031**		0.050**
		(0.010)		(0.012)		(0.023)
Effect of Public Housing + Female		0.004		0.015***		0.003
		(0.005)		(0.003)		(0.004)
Effect of Public Housing + Nonwhite		0.041***		0.048***		0.042***
		(0.007)		(0.008)		(0.011)
Effect of Public Housing + Older than CSL Age		0.045***		0.050***		0.061***
		(0.009)		(0.013)		(0.021)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1894741	1894741	255476	255476	22738	22738
Mean of Dep. Variable	0.862	0.862	0.898	0.898	0.901	0.901
R^2	0.348	0.354	0.321	0.327	0.345	0.359

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

Table 1.12: Linear Probability Model - Child Ontime for School Completion Requirements

	Unweighted		CEM Weighted		CEM k2k Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Public Housing	0.034	−0.022	0.020	0.003	0.036***	0.032**
	(0.022)	(0.028)	(0.017)	(0.024)	(0.010)	(0.012)
Public Housing × Female		−0.005		−0.008		−0.014
		(0.012)		(0.012)		(0.012)
Public Housing × Non-white		0.068**		0.037		0.029
		(0.031)		(0.035)		(0.035)
Public Housing × Older than CSL age		0.025		0.094***		0.029*
		(0.034)		(0.024)		(0.015)
Effect of Public Housing + Female		−0.027		−0.005		0.017*
		(0.018)		(0.015)		(0.010)
Effect of Public Housing + Nonwhite		0.045		0.040		0.061*
		(0.039)		(0.036)		(0.036)
Effect of Public Housing + Older than CSL Age		0.003		0.097***		0.061***
		(0.013)		(0.019)		(0.012)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1894741	1894741	255476	255476	22738	22738
Mean of Dep. Variable	0.749	0.749	0.771	0.771	0.711	0.711
R^2	0.151	0.166	0.126	0.141	0.208	0.219

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

Table 1.13: Linear Probability Model - Child Employed in Labor Force

	Unweighted		CEM Weighted		CEM k2k Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Public Housing	−0.077***	0.058***	−0.068***	0.027	−0.061***	−0.016
	(0.015)	(0.011)	(0.015)	(0.021)	(0.019)	(0.015)
Public Housing × Female		−0.043***		−0.049**		0.007
		(0.013)		(0.020)		(0.016)
Public Housing × Non-white		−0.064**		−0.045		−0.073*
		(0.027)		(0.028)		(0.035)
Public Housing × Older than CSL age		−0.095***		−0.085***		−0.044*
		(0.012)		(0.014)		(0.023)
Effect of Public Housing + Female		0.015		−0.022***		−0.009
		(0.008)		(0.008)		(0.016)
Effect of Public Housing + Nonwhite		−0.005		−0.018		−0.089**
		(0.024)		(0.032)		(0.036)
Effect of Public Housing + Older than CSL Age		−0.036***		−0.058***		−0.061***
		(0.009)		(0.016)		(0.022)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	742623	742623	59592	59592	6386	6386
Mean of Dep. Variable	0.244	0.244	0.227	0.227	0.191	0.191
R^2	0.289	0.295	0.290	0.296	0.197	0.216

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

Table 1.14: Number of Years Attending School

	Unweighted		CEM Weighted		CEM k2k Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Public Housing	0.175	−0.064	0.105	0.041	0.164**	0.074
	(0.111)	(0.101)	(0.098)	(0.101)	(0.070)	(0.074)
Public Housing × Female		0.010		−0.015		−0.042
		(0.029)		(0.031)		(0.038)
Public Housing × Non-white		0.397**		0.254*		0.201
		(0.142)		(0.137)		(0.155)
Public Housing × Older than CSL age		0.112		0.213***		0.346***
		(0.071)		(0.049)		(0.073)
Effect of Public Housing + Female		−0.054		0.026		0.032
		(0.087)		(0.090)		(0.054)
Effect of Public Housing + Nonwhite		0.333**		0.295**		0.275*
		(0.146)		(0.145)		(0.150)
Effect of Public Housing + Older than CSL Age		0.048		0.255***		0.420***
		(0.059)		(0.076)		(0.074)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1862447	1862447	249723	249723	22062	22062
Mean of Dep. Variable	6.100	6.100	5.351	5.351	5.201	5.201
R^2	0.806	0.809	0.815	0.818	0.812	0.814

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

Table 1.15: Heterogeneous Exposure Summary Results (Linear Probability Model)

	Currently Attends School		Ontime to Complete Schooling		Employed in Labor Force		Years of Schooling	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Duration in Public Housing	0.014***	−0.004	0.005	0.002	−0.005	0.009	0.045	−0.004
	(0.004)	(0.007)	(0.011)	(0.014)	(0.008)	(0.007)	(0.047)	(0.047)
Female × Duration in Public Housing		0.009**		−0.003		0.006		0.014
		(0.003)		(0.005)		(0.008)		(0.029)
Non-white × Duration in Public Housing		−0.006		0.002		0.014		0.108
		(0.008)		(0.022)		(0.012)		(0.070)
Older than CSL age × Duration in Public Housing		0.053***		0.033		−0.030***		0.145*
		(0.011)		(0.030)		(0.009)		(0.078)
Effect of Exposure + Female		0.005		−0.001		0.015***		0.010
		(0.005)		(0.010)		(0.004)		(0.054)
Effect of Exposure + Nonwhite		−0.010		0.004		0.023**		0.104
		(0.008)		(0.025)		(0.009)		(0.066)
Effect of Exposure + Older than CSL Age		0.049***		0.036		−0.021**		0.141*
		(0.011)		(0.025)		(0.009)		(0.080)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	19190	19190	19190	19190	6200	6200	18246	18246
Mean of Dep. Variable	0.907	0.907	0.739	0.739	0.155	0.155	5.575	5.575
R^2	0.317	0.331	0.214	0.224	0.188	0.215	0.816	0.818

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

Chapter 2

The Effect of Opioid Abuse on Child Out-of-Home Placements

2.1 Introduction

Opioid over-prescription and misuse have created a public health epidemic in the US. While many adults suffer from opioid dependency, their addiction also affects their children. More than half a million children are estimated to live with parents who have prescription opioid dependency, and 1.5 million children live away from a parent due to the opioid crisis (K. Buckles et al., 2020; Bullinger & Wing, 2019). Although the proportion of foster care cases due to parental drug abuse has grown by a factor of 2.5 since 2000, there is little evidence of opioid abuse's causal effect on foster care caseloads (K. Buckles et al., 2020; Meinhofer & Angleró-Díaz, 2019).

This study uses an exogeneous policy change to estimate the effect of opioid abuse on child out-of-home placements – the removal of a child from home due to maltreatment and placement in formal foster care or informal family living arrangements. A catalyst of the current opioid crisis was the introduction of the prescription opiate OxyContin in 1996. Widely-prescribed, individuals misused the original version of OxyContin by crushing it into a powder and ingesting the entire dose instantly to experience an euphoric high (Powell et al., 2020; Quinones, 2015). In response, an abuse-deterrent reformulation of OxyContin was released in August 2010. While the reformulation deterred misuse of OxyContin, prior studies have shown that heroin and synthetic opioid-related overdoses increased due to addicts' substitution of these opiates for OxyContin (A. Alpert et al., 2018; Evans et al., 2019).

Constructing a nationally representative panel using administrative data on child out-of-home placements merged with adult opioid abuse treatment entry rates, the supply of OxyContin, and demographics, I estimate the effect on child out-of-home placements using an instrumental variable for adult opioid abuse by modeling the reformulation of OxyContin with a trend-break specification as done by A. Alpert et al. (2018).[1] The measure of pre-intervention exposure to prescription opioids is variation in the 2010 state-level supply of oxycodone, the main ingredient in OxyContin. The identification assumption is that higher rates of opioid abuse increase child maltreatment and out-of-home placements. The exclusion restriction is that only opioid abuse, not child maltreatment and out-of-home placements, is affected by variation in exposure to the supply of OxyContin and its reformulation.

[1] I will refer to this study as APP for the remainder of the paper

I find that a 10% percent increase in opioid abuse treatment entries caused an additional 2.4 parental drug abuse related foster care entries per 100,000 children in the US over the study period 2006-2016. This is equivalent to a 10% percent increase in the national opioid abuse treatment admission rate on average causing nearly 1,800 child foster care entries due to parental drug abuse. Due to under-identification of substance abuse as a reason for a child's removal from home, this is likely a lower bound estimate (Correia, 2013; Seay, 2015; Wiltz, 2016). For robustness, I replicate APP (A. Alpert et al., 2018) to show that the reformulation significantly increased opioid-related substance abuse treatment entry rates. I also find a lagged effect of the reformulation on child formal foster care out-of-home placements. Together, these findings suggest that higher rates of opioid abuse have significantly increased rates of drug-related out-of-home foster care placements.

However, I find that the overall rate of child out-of-home placements into foster care has remained unaffected by the opioid crisis indicating a decline in non-drug related entries. While the overall foster care entry rate has not been affected by the opioid crisis, this may be due to the substitution of more out-of-home placements for drug-related cases than less severe non-drug related cases and limited state resources to handle an increase in caseloads. A growing number of child out-of-home placements due to parental drug abuse is a concern if the cost of care is higher for children of parents who abuse drugs. Constructing a child-level longitudinal panel of foster care histories over the study period 2006-2016, I show that children who enter foster care due to parental drug abuse on average are more likely to recidivate to foster care and remain in foster care for an additional 2 months over their childhood. Given an average 27.2% increase in opioid abuse treatment admission rates from 2006-2010 to 2011-2016, this is equivalent to approximately $26M in additional costs to the US foster care system over the study period.

This study makes two significant contributions. First, I present new findings on the effect of opioid abuse on children. Along with more children living in households affected by opioid addiction, more children are entering foster care due to parental drug abuse (K. Buckles et al., 2020; Bullinger & Wing, 2019; Meinhofer et al., 2020). Several studies have found increased rates of child out-of-home placements to be associated with greater rates of opioid prescription and overdose hospitalizations (Ghertner et al., 2018; Quast, 2018; Quast et al., 2019), but recent work by Gihleb et al. (2019) estimate that greater opioid prescription oversight by mandatory prescription drug monitoring programs (PDMPs) decreased child removals by 10%. K. Buckles et al. (2020) using the Current Population

Survey (CPS) also found no statistically significant effect of the opioid crisis on the population share of children in foster care. However, under-reporting of foster children in national population surveys has been well documented (O'Hare, 2008). This study uses administrative data on the census of children who entered foster care to estimate the effect opioid abuse on the flow of child out-of-home placements into foster care.

Second, this study contributes to the economic literature on children by providing new evidence on the effect of drug abuse on child out-of-home placements using a national opioid supply intervention. Several economic studies have established a relation between child maltreatment and welfare program receipt (Paxson & Waldfogel, 2003), labor market conditions (Lindo et al., 2018; Paxson & Waldfogel, 1999, 2002), and access to abortion (M. Bitler & Zavodny, 2002; M. P. Bitler & Zavodny, 2004). However the effect of drug abuse on child out-of-home placements in foster care has been less studied. In addition to recent work by K. Buckles et al. (2020) and Gihleb et al. (2019), Cunningham and Finlay (2013) examined the impact of federal restrictions for methamphetamine precursors on drug abuse related foster care entries.

2.2 The US Opioid Crisis and Child Maltreatment

In 1898, Bayer Pharmaceutical began marketing heroin as a non-addictive opiate medication to relieve respiratory disease. Heroin was later banned in 1924 in the US when discovered to be highly additive and susceptible to abuse (Sneader, 1998). Since the early 20th century, opiate-based medications such as morphine and codeine have been used restrictively for only severe cases of pain management. The recent proliferation of opioid prescription can be traced to a widely-cited study by Portenoy and Foley (1986) which found oxycodone, an opioid derivative, to be a non-addictive pain medication (Kolodny et al., 2015). In 1996, Purdue Pharma began promoting OxyContin, an extended-release oxycodone containing a large opiate dose per pill designed to be slowly released throughout the day. Aggressively marketed, OxyContin become one of the highest selling prescription drugs in the US (A. Alpert et al., 2018; A. E. Alpert et al., 2019; GAO, 2003).

Consequentially, widespread misuse of OxyContin led to increased rates of opioid addiction and deaths (Compton et al., 2016; Kolodny et al., 2015; Powell et al., 2020). Since 2000, the number

of opioid-related deaths in the US has increased by a factor of 5 (See Figure 2.1). As well as over-consumption, individuals could misuse the original version of OxyContin by crushing it into a powder to ingest the entire dose instantly for an euphoric high (GAO, 2003). In response, Purdue Pharma released an abuse-deterrent reformulation of OxyContin which made the pill less soluable or crushable in August 2010. The year following this intervention, prescription opioid overdose deaths decreased for the first time since 1990 (Dart et al., 2015).

Although the 2010 reformulation of OxyContin reduced prescription opioid-related deaths, prior studies have shown that it unintentionally caused OxyContin abusers to substitute heroin and synthetic opioids. APP estimate that nearly 80% of heroin-related deaths from 2010-2013 were due to the reformulation of OxyContin, and the increase in heroin-related deaths due to the reformulation has offset reductions in prescription opioid-related deaths (Evans et al., 2019). Following OxyContin's reformulation in 2010, heroin and synthetic opioid-related deaths have increased by a factor of 5.8 and account for the majority of US opioid-related deaths (See Figure 2.1).

Foster care is a state-provided temporary service for children who cannot live with their families due to maltreatment and are placed out-of-home most often with relatives or unrelated foster parents. More than 250,000 children enter the foster care system annually and by age 18 nearly 6% of all US children experience placement in foster care (Wildeman & Emanuel, 2014). While the opioid crisis may have been more likely to affect child in-home living arrangements than foster care placements, children removed from home due to opioid abuse and placed in foster care represent the most severe cases of child maltreatment (K. Buckles et al., 2020). Among foster care placements, cases involving parental drug abuse, particularly opioid abuse, represent the most complex cases to resolve (Radel et al., 2018; Wiltz, 2016). Work by Meinhofer and others using administrative data has shown a national increase in the number of foster care entries related to parental drug abuse (Meinhofer & Angleró-Díaz, 2019; Meinhofer et al., 2020). Related work has found a strong association between greater rates of opioid prescription abuse and child foster care entries (Ghertner et al., 2018; Quast et al., 2019).

Foster care experiences and household instability can have long-term consequences for children. Studies by Doyle and co-authors as well as Bald et al. (2019) have found suggestive evidence that foster care experiences negatively impact children on the margin of foster care out-of-home placement (Doyle Jr, 2007b, 2008, 2013). Child maltreatment and foster care out-of-home placements also incur

significant costs. Peterson et al. (2018) estimate that the lifetime costs of child maltreatment per child not including productivity loss is $842,337 ($2016). In 2016, the estimated average cost to provide out-of-home foster care placement services per child was $33,210 and the average length of stay in foster care was 20.1 months (Crowley & Jones, 2017; US Department of Health and Human Services, 2017). The annual budget for the US foster care system was $5 billion.[2]

2.3 Data on Opioid Abuse and Foster Care

Directly estimating the effect of opioid abuse on child out-of-home placement rates using regression methods likely suffers from omitted variable bias (Swann & Sylvester, 2006). To correct for this, I first estimate opioid abuse rates using information on the supply of oxycodone and OxyContin's reformulation. Rates of opioid abuse should be highly correlated with the supply of OxyContin (oxycodone) and its reformulation in 2010. Then using the first-stage predicted opioid abuse rates, I estimate the effect on child out-of-home placements using instrumental variables. Greater rates of opioid abuse are predicted to cause higher rates of child out-of-home placements.

For the analysis, I construct a nationally representative panel at the age-state-year level of observation for children age 0-17 from 2006-2016. Information on child out-of-home placements and demographics are combined with data on the legal supply of oxycodone, opioid abuse rates, and demographics for age groups 25-34, 35-44, and 45-54 based on the mean age of parents. Overall, the dataset contains 9,882 observations.[3]

To quantify the effect of opioid misuse on out-of-home placements into foster care, I construct foster care entry rates per 10,000 children in the US using case-level information from the Adoption and Foster Care Analysis and Reporting System (AFCARS). Since 1995, the US Department of Health and Human Services has mandated states to annually submit information on all children served by public agencies to AFCARS. The sample used includes all children less than 18 years old who entered the foster care system available from the National Data Archive on Child Abuse and

[2]HHS FY2016 Budget in Brief: ACF Budget Overview

[3]The total number of adult (25-34, 35-44, and 45-54) and child (0-17) observations from 2006-2016 are 1,647, and 10,098. Due to missing TEDS data, the final dataset contains only 9,882 observations.

Neglect (NDACAN).[4] AFCARS data is reported on a fiscal year basis and merged to other data sources for the same calendar year i.e. fiscal year 2010 foster care entries are merged to other data sources reported for the 2010 calendar year.[5]

I construct three child out-of-home placement outcomes related to foster care. The first outcome is the overall foster care entry rate. AFCARS provides information on 15 non-mutually exclusive reasons for the child's removal from home including parental neglect, drug abuse, alcohol abuse, physical or sexual abuse, incarceration, and death. The second outcome is foster care entries where parental drug abuse was indicated as a reason for removal. Parental opioid abuse represents an unknown proportion of overall drug abuse related foster care entries since AFCARS does not provide detailed information on the specific drug abused. The third outcome is foster care entries where drug abuse, neglect, incarceration, or parental death were indicated as the reason for child out-of-home placement into foster care.

Although estimating the effect on drug-related foster care entries is most relevant to identifying the impact of opioid abuse on the foster care system, I examine multiple foster care entry outcomes due to widely-acknowledged under-reporting of parental drug abuse as the reason why a child entered the foster care system. There is no standardized practice for how states report substance abuse (Wiltz, 2016). Case workers often only indicate child neglect as the reason for removal if neglect was the only substantiated reason for child out-of-home placement. This practice emphasizes reporting the reasons for removal on the basis of behavior towards the child rather than reporting the underlying issues within the household (Correia, 2013; Dore et al., 1995). Foster care entries due to parental neglect, incarceration, and death are included since parental drug abuse is likely correlated with these reasons for child out-of-home placement.

Lack of formal reporting guidelines likely accounts for large variation across counties in the number of drug-abuse related entries seen in the AFCARS data (Seay, 2015). For instance, of more than 3,600 foster care entry cases in Orange County, California from 2006-2007, none were attributed to parental drug abuse. However, in Bexlar county, Texas, approximately 68% of all foster care entries were attributed to parental drug abuse during the same period. Although instrumental variables

[4] A child was **served** by the foster care system if they passed through the foster care system at some point during the fiscal year. A child **enters** foster care if the child's date of most recent removal is between the beginning and the end of the fiscal year.

[5] The federal fiscal year begins Oct 1 and ends Sep 30 of the nominal fiscal year.

may improve results by mitigating extreme cases, The estimated effect of opioid abuse on parental drug abuse related child out-of-home placements likely represents a lower bound estimate, and the effect on out-of-home placements due to parental drug abuse, neglect, incarceration, or parental death represents an upper bound.

Along with formal out-of-home foster care placements, I also estimate the effect of opioid abuse on the share of children cared for by a grandparent to measure informal out-of-home placements. A large increase in child maltreatment cases due to opioid addiction may create a shortage of foster homes for out-of-home care (Radel et al., 2018). One alternative to providing foster homes is to increase the use of informal kinship care. Child protective service caseworkers often prefer children to remain in the care of family members without social service interventions (Child Welfare Information Gateway, 2016). As a proxy for informal out-of-home care with a grandparent, I construct the share of children living in households where a grandparent is the primary caregiver for a grandchild using the American Community Survey (ACS).

Greater rates of opioid abuse are strongly correlated with more foster care out-of-home placements (Quast et al., 2019; Radel et al., 2018). To measure parental opioid abuse, I collect drug treatment admission rates per 100,000 people for age cohorts 25-34, 35-44, and 45-54 from the Substance Abuse and Mental Health Services Administration's Treatment Episode Data Set Admissions (TEDS-A) dataset. TEDS-A reports all treatment admissions to facilities which receive public funding. For the main analysis, I use the treatment admission rate for any opioid – heroin or Oxycontin and other synthetic opioid – as the endogenous independent variable for estimating the effect on child out-of-home placements.[6] As robustness checks, I also construct drug treatment admission rates for heroin, Oxycontin, and non-opiate related entries separately. Non-opioid related drug treatment admission rates should be unaffected by OxyContin's 2010 reformulation.

Adults with greater exposure to OxyContin were more likely to abuse prescription opioids, and later substitute heroin and other synthetic opiates for OxyContin after its reformulation in 2010 (A. Alpert et al., 2018). To measure variation in the exposure to OxyContin across the US, I use the annual state-level supply of oxycodone in kilograms per 100,000 persons collected by the Drug Enforcement Agency's (DEA) Automation of Reports and Consolidated Orders System (ARCOS).

[6]Other opiates and synthetics includes buprenorphine, codeine, hydrocodone, hydromorphone, meperidine, morphine, opium, oxycodone, pentazocine, propoxyphene, tramadol, and any other drug with morphine-like effects.

ARCOS is the federal system which records the supply distribution of Schedule II-IV controlled substances by active ingredient as required by the Controlled Substance Act of 1970.[7]

Demographics are controlled for using the ACS available from IPUMS-USA. For both children and adults, I collect information on the total population, population shares by race and sex, and poverty rates. Additionally, for adults, I construct controls for level of education, marital status, per capita income, veteran status, unemployment rates. For children, I also include controls for household income, and whether the child lived in a single female-headed households. I also collect information on state policies which may affect either the supply of oxycodone or child out-of-home placements. PDMPs may reduce the supply of oxycodone and prescription opioid abuse if they provide greater oversight of opioid prescriptions.[8] For opioid abuse rates, I collect information on the introduction of PDMPs by states using data collected by the PEW Charitable Trust. Variation in the amount of welfare assistance received by low-income families likely affects their ability to adequately provide care for a child (Paxson & Waldfogel, 2003). For child out-of-home placements, I control for state-level Supplemental Nutrition Assistance Program (SNAP) and Temporary Assistance for Needy Families (TANF) benefits for a family of three people obtained from the University of Kentucky Center for Poverty Research.

Summary statistics are presented in Tables 2.1 and 2.2 for children and adults by time period. The first column presents mean values for the entire study period, 2006-2016. The second and third columns decompose mean values by pre (2006-10) and post (2011-16) OxyContin reformulation (treatment) periods. The fourth column shows the difference and column five shows the percentage change across periods. Although not controlled for in the model, I present demographic statistics for the population of children in foster care and adults in substance abuse treatment as a comparison to the general US population.

Comparing the pre-treatment period (2006-2010) to the post-treatment period (2011-2016), the overall foster care entry rate per 10,000 children has decreased by 6.5% per annum. Since 2006, the foster care out-of-home placement rate related to parental drug abuse has steadily increased

[7]Prior work examining the effect of drug abuse on children has relied upon illicit drug price data collected through the Drug Enforcement Agency's System to Retrieve Information from Drug Evidence (STRIDE) (Cunningham & Finlay, 2013). However, STRIDE is a convenience sample and cannot address the overall stock of illicit substance available (Arkes et al., 2008; Horowitz, 2001).

[8]Prescription Drug Monitoring Programs: https://www.pewtrusts.org/-/media/assets/2016/12/prescription_drug_monitoring_programs.pdf

and largely accounts for an overall increase in the foster care entry rate beginning around 2010 (See Figure 2.2). Over the sample period, foster care entries due to parental drug abuse as well as the combined drug abuse, neglect, incarceration, and death entry rate have increased by 27.5% and 8.9%. The share of children living in households where a grandparent is the primary caregiver has also increased by 3.5%. Within the public foster care system, there are disproportionately more non-white and Hispanic children relative to the US population.

Although the entry rate for non-opioid related treatment has decreased since 2006, entries for prescription opiates (other opioids) and heroin-related addiction have remained constant or increased (See Figure 2.3). Drug abuse treatment entry rates related to opioids have increased by 27% while non-opioid related entry rates have decreased by 24%. Non-white, male, single, unemployed, and people with less education are disproportionately more represented among substance abuse treatment patients than in the US population. Across periods, the oxycodone supply increased on average by 7.7% or 1.3 kg per 100,000 persons. This is equivalent to an additional 0.7 20mg OxyContin pills per person in the US. The geographic dispersion of the supply of oxycodone in 2010 is shown in Figure 2.4. The North Pacific, Southwest and East Atlantic areas of the US experienced higher exposure to OxyContin prior to its reformulation.

2.4 The Effect of Opioid Abuse on Children

To examine whether greater rates of opioid abuse increased child out-of-home placements, I first estimate the linear model:

$$Y_{ast} = \delta ln(Opioid\_Abuse_{ast}) + \beta X_{ast} + \eta_a + \alpha_s + \tau_t + \epsilon_{ast} \tag{2.1}$$

Here, Y_{ast} are child out-of-home placements expressed as the rate per 10,000 children. $ln(Opioid\_Abuse_{ast})$ is the natural log of the substance abuse treatment admission rate for any opiate (heroin or Oxy-Contin and other synthetic opioids) per 100,000 persons. δ is the estimated effect of opioid abuse on child out-of home placements. X_{ast} are child demographic and state policy controls. η_a, α_s and τ_t are age cohort, state, and year fixed effects. All standard errors are clustered at the state-level.

Panel A of Table 2.3 shows the estimated effect of parental opioid abuse treatment entries on child out-of-home placements using OLS (2.1). I find a significant increase in the rate of parental drug-related child foster care entries (1.3) at the 10% significance level. Interpreting the coefficient δ for drug-related entries, a 1% percent increase in the parental substance abuse treatment entry rate is associated with an additional .013 child foster care entries per 10,000 children on average over the sample period. This is equivalent to a 10% percent increase in the parental substance abuse treatment entry rate associated with an additional 1.3 child foster care entries per 100,000 children. I find no significant effect of parental opioid abuse treatment admissions on total foster care entries, drug and other related entries, or the share of children cared for by a grandparent.

Opioid abuse may be endogeneous to unobserved variation in the error term. Although less likely, greater rates of child out-of-home placements may also cause an increase in adult opioid abuse. Studies have found states with greater rates of OxyContin abuse and access to oxycodone prior to the 2010 reformulation experienced higher rates of opioid addiction and overdose deaths (A. Alpert et al., 2018; Evans et al., 2019). While variation in the supply of OxyContin and its 2010 reformulation directly affects the behavior of parents addicted to opioids, neither should affect child out-of-home placement rates other than through increased rates of parental opioid abuse. To correct for endogeneity bias, I estimate the effect of opioid abuse on child out-of-home placements using two-stage least squares. In the first-stage, I model the 2010 OxyContin reformulation as done by APP to estimate opioid abuse rates:

$$\begin{aligned} ln(Opioid\_Abuse_{ast}) = \; & \delta_1[Post_t \times ln(Oxy\_Supply_s^{2010})] + \delta_2[t \times ln(Oxy\_Supply_s^{2010})] \\ & + \delta_3[Post_t \times (t-2011) \times ln(Oxy\_Supply_s^{2010})] \\ & + \beta X_{ast} + \eta_a + \alpha_s + \tau_t + \epsilon_{ast} \end{aligned} \tag{2.2}$$

Here, $Opioid\_Abuse_{ast}$ are adult drug abuse treatment entry rates per 100,000 person in the population. $Post_t$ is an indicator for post-2010, the year OxyContin was reformulated, and $ln(Oxy\_Supply_s^{2010})$ is the 2010 retail supply of oxycodone in 2010.[9] t controls for pre-existing trends and a linear trend break beginning in 2012 (i.e. $t-2011=0$ in 2011). δ_1 represents the initial effect of the reformulation

[9]The 2010 retail supply of oxycodone is preferred since it allows for a more contemporaneous measure of exposure to OxyContin immediately prior to the 2010 reformulation of OxyContin. Among three measures of oxycodone exposure: the 2006 oxycodone supply, the 2010 oxycodone supply and the mean pre-2010 oxycodone supply, the lowest coefficient of correlation is 0.9 suggesting little variation in estimated effects using any measure of OxyContin exposure. Correlation plots of the three different measures of pre-2010 exposure are shown in the Appendix.

in 2011. A common linear time trend is controlled for by δ_2, and the additive effect of the reformulation after 2011 is restricted to be linear by δ_3. X_{ast} are demographic and state policy controls. η_a, α_s and τ_t are age cohort, state and fiscal year fixed effects. All standard errors are clustered at the state-level. Then, opioid-related treatment admission rates predicted in the first-stage (2.2) are used to estimate the effect on child out-of-home placements in (2.1):

$$Y_{ast} = \delta ln(\widehat{Opioid\_Abuse}_{ast}) + \beta X_{ast} + \eta_a + \alpha_s + \tau_t + \epsilon_{ast} \tag{2.3}$$

If the 2010 OxyContin reformulation is a relevant and valid instrument for opioid abuse conditional on other covariates, then δ identifies the causal effect of opioid abuse on child out-of-home placements. Estimates using IV (2.3) are shown in Panel B of Table 2.3.

Using the reformulation of OxyContin as an instrumental variable for parental opioid abuse treatment admissions in equation (2.3), I find a statistically significant increase in the foster care entry rate (per 10,000 children) for both parental drug abuse (2.4), as well as drug abuse and other related reasons (5.6), but no statistical effect on the overall foster care entry rate or share of children cared for by a grandparent. Interpreting the coefficient of $ln(\widehat{Opioid\_Abuse}_{ast})$ for drug abuse related foster care entries, a 10% percent increase in the treatment admission rate for opioid abuse caused an additional 2.4 foster care entries due to parental drug abuse per 100,000 children. This is statistically significant at the 1% level.

Alternative Parental Abuse Measures

For the main results, I use the supply of oxycodone as an instrument to estimate adult substance abuse treatment admission rates related to heroin or OxyContin and other synthetic opioids. Alternatively, I could use treatment admission rates for heroin, or OxyContin and other synthetic opioids separately. Results are shown in Tables 2.4 and 2.5. Using heroin-related adult treatment admissions, I estimate an increase in drug-related (2.6), and neglect-related child out-of-home placements (5.8) due to opioid abuse similar to the main results. I also find a significant effect on the overall foster care entry rate (3.8). However, I find no significant effect on child out-of-home placement outcomes using only OxyContin and other synthetic opioid-related adult drug treatment admissions. A potential reason for this finding is that heroin-related drug treatment admissions are significantly correlated

with child out-of-home placement rates since they represent severe opioid abuse and account for a proportionately large number of adult substance abuse treatment admissions relative to those for OxyContin and synthetic opioids.

Instrumenting with Contemporaneous Oxycodone Sales

As a further robustness check for the first-stage of the instrumental variable regression, I replace equation (2.2) with the natural log of the contemporary oxycodone supply for opioid abuse treatment admission rates as an instrument along with demographic and fixed-effects,

$$ln(Opioid\_Abuse_{ast}) = \delta\ ln(Oxy\_Supply_{st}) + \beta X_{ast} + \eta_a + \alpha_s + \tau_t + \epsilon_{ast} \tag{2.4}$$

Results are shown in Table 2.6. Similar to results using equation (2.3) I find no effect on either the overall foster care entry rate (3.0) or share of children cared for by a grandparent (7.2) at 10% significant level. I find significant effects on both parental drug abuse related (2.3) and the combined drug and related reasons (5.4) child foster care entry rates similar to the main results at the 1% level.

The Effect of OxyContin's Reformulation

I have shown that opioid abuse has increased drug-related child foster care out-of-home placements over the period 2006-2016. As done by APP, I also examine the causal effect of OxyContin's reformulation on adult opioid abuse as well as child out-of-home placement rates. Relaxing the linearity assumptions in equation (2.2), I first estimate the following generalized model as a baseline comparison to APP's model (2.2):

$$ln(Y_{ast}) = \delta[Post_t \times ln(Oxy\_Supply_s^{2010})] + \beta X_{ast} + \eta_a + \alpha_s + \tau_t + \epsilon_{ast} \tag{2.5}$$

To compare models, the joint significance test of the common linear time trend and post-2011 break coefficients in equation (2.2), $\delta_2 = \delta_3 = 0$, are also estimated. Rather than construct rates of opioid abuse and out-of-home placements, I measure outcomes as counts and estimate a quasi-Poisson

regression model.[10] I also assess the timing of the treatment effect with the generalized fixed-effects model used by APP:

$$ln(Y_{ast}) = \sum_{t=2006}^{2008} \delta_t[\mathbb{1}_t \times ln(Oxy\_Supply_s^{2010})] + \sum_{t=2010}^{2016} \delta_t[\mathbb{1}_t \times ln(Oxy\_Supply_s^{2010})] + \beta X_{ast} + \eta_a + \alpha_s + \tau_t + \epsilon_{ast} \tag{2.6}$$

$\mathbb{1}_t$ are a set of indicators for each year. Therefore the set of coefficients δ_t estimate the effect of treatment exposure relative to the year prior to OxyContin's reformulation, 2009. If OxyContin's reformulation in 2010 was an exogeneous shock to the menu of opiates available to addicts, then equation (2.6) should estimate a significant effect for δ_t for $t \geq 2011$ and no significant effect prior to 2010. A significant effect for δ_t found prior to 2010 indicates a threat to the internal validity of causal estimates using equations (2.5) and (2.2) due to spurious correlation. Table 2.7 and 2.8 show the estimated effect of OxyContin's 2010 reformulation on adult substance abuse treatment entries and child out-of-home placements using the baseline (2.5) and APP's (2.2) model. Results using the fixed-effects model (2.6) are shown in Tables 2.9 and 2.10, as well as graphically presented in Figure 2.5 for adult (Panel A) and child (Panel B) outcomes.

Under the baseline model (2.5), Table 2.7 shows a statistically significant effect of the reformulation on adult substance abuse treatment entries for heroin (0.3) at the 10% level. Interpreting the coefficient for heroin entries in column (1), a one percent increase in the average 2010 supply of oxycodone increased heroin-related adult substance abuse treatment entries by 0.3%. I find no significant effect on OxyContin related, or overall opioid treatment entries.

Controlling for a pre-existing common trend using APP's model (2.2), there is a statistically significant initial effects on heroin (0.2), OxyContin (0.09), and overall opiate (0.1) related treatment entry rates in 2011. However, there is only a significant additive effect for heroin (0.08) in the years following 2011. Interpreting the coefficients for heroin entries under (2.2), a one percent increase in the average 2010 supply of oxycodone had an initial effect of increasing heroin-related adult treatment entries by 0.2% in 2011 and an additive effect of 0.08% per annum. For the period 2011-2016, this is equivalent to a one percent increase in the 2010 oxycodone supply causing a 0.6% increase in the 2016 heroin-related treatment entry rate ($\delta_1 + 5 \times \delta_3$).

[10] Estimates shown do not impose the population offset condition which constrains the population coefficient to be equal to 1. Estimated effects are similar when the offset constraint is imposed.

Panel A of Figure 2.5 graphical shows the estimated coefficient value for adult substance abuse treatment admissions using equation (2.6). Relative to 2009, there was a significant increase in heroin, OxyContin, and overall opiate related substance abuse treatment admissions after OxyContin's reformulation and no significant effect prior to 2009. As found by APP, OxyContin's reformulation increased opioid abuse rates largely through a significant increase in heroin-related substance abuse treatment entries. Interpreting the coefficient δ_{2016} from Table 2.9, a one percent increase in the mean amount of 2010 oxycodone supplied increased heroin-related treatment admissions in 2016 by 0.5% relative to 2009 at the 5% significance level. For non-opioid related treatment admissions, I find a significant negative effect (0.07) in the reformulation year.

For child out-of-home placements, I find no statistically significant effect of OxyContin's reformulation using equation (2.5) shown in Panel A of Table 2.8. Using equation (2.2), while I find no initial effect of OxyContin's reformulation in 2011 on child out-of-home placements, there was a significant increase in total foster care out-of-home placements (0.03), and out-of-home placements for parental drug abuse or other related reasons for entry (0.04) following 2011. Interpreting the coefficient for total foster care out-of-home placements using linear trends in Panel B, a one percent increase in the average 2010 supply of oxycodone increased overall foster care entries by an additional 0.03% annually after 2011 at the 5% significance level. I find no effect on the share of children cared for by a grandparent, entries due to drug abuse, or due to drug abuse and related reasons at the 10% significance level.

Panel B of Figure 2.5 shows no strong relationship between child out-of-home placements and pre-2010 oxcodone misuse using equation (2.6). I find no significant increase in child out-of-home placements relative to 2009 due to OxyContin's reformulation except for a significant decrease in parental drug abuse and other related reasons for entry in 2008. Interpreting the coefficient δ_{2008} for column (3) of Table 2.10, a one percent increase in the mean 2010 supply of oxycodone significantly increased drug-related entries in 2008 by 0.06% relative to 2009 at the 5% confidence level.

Effect of Drug Abuse on Child Lifetime Foster Care Experiences

Although the overall rate of child out-of-home placements may have been unaffected by opioid abuse, a greater proportion of children entering foster care due to parental drug abuse is more costly if these

children are more likely to stay in care longer or to recidivate to foster care. To assess the effect of drug abuse on the children's total lifetime length of stay in foster care, I estimate the following model by constructing child-level observations for all children who entered foster care from 2006-2016 using child age, state, birthdate, and case identification number,

$$Y_i = \delta Drug\_Abuse_i + \beta X_{ist} + \alpha_s + \tau_t + \epsilon_{ist} \tag{2.7}$$

Where Y_i, is the total lifetime length of stay for each child from 2006-2016 measured in months, or an indicator for whether the child ever recidivated to the foster care system. I only include children who entered the foster care system in 2006 or after. $Drug\_Abuse_i$ is an indicator for whether parental drug abuse was ever cited as a reason for child foster care out-of-home during child's lifetime. X_{ist}, is a set of controls for child age, gender, and race. $\alpha_s,\ and\ \tau_t$ are state and year of first foster care out-of-home placement fixed effects. In total, more than 2.6 million children have entered the foster care system from 2006-2016. Results are shown in Table 2.11.

Using OLS, Panel A shows that children who experienced any out-of-home placement in foster care due to parental drug abuse were associated with a longer average length of stay in the foster care system of an additional 2 months after controlling for state and year fixed effects. This is potentially due to child of parents who abuse drugs being more likely to recidivate to foster care. Using a logit model, Panel B of 2.11 shows that the log odds of foster care recidivism by children of parents who abuse drugs increases by 1.4.

2.5 Additional Robustness Checks

Assessing Linearly Additive Dynamic Effects

In model (2.2), linearly additive dynamic effects of the reformulation over the post-2010 period are assumed. I assess this assumption using the following:

$$\begin{aligned} ln(Y_{ast}) = &\sum_{T=2011}^{2016} \delta_T[Post_t \times ln(Oxy\_Supply_s^{2010}) \times \mathbb{1}_{t \leq T}] + \delta_2[t \times ln(Oxy\_Supply_s^{2010})] \\ &+ \beta X_{ast} + \eta_a + \alpha_s + \tau_t + \epsilon_{ast} \end{aligned} \tag{2.8}$$

Here, Y_{ast} are outcomes expressed as counts. The set of δ_T estimate the effect of the reformulation for years following the 2010 reformulation where $\mathbb{1}_{t \leq T}$ are indicators for observations up to year T. Relative to equation (2.2), equation (2.8) is a generalized specification which estimates the additive effect of the reformulation for each year separately. I can then test the assumption of linear effects:

$$\delta_{2012} = \delta_{2013} = \delta_{2014} = \delta_{2015} = \delta_{2016} \tag{2.9}$$

Results are shown in Tables 2.12 and 2.13. For adult drug treatment admission outcomes, the F-test reject linear effects for all drug abuse outcomes except overall opioid-related treatment admissions at the 10% significance level. For child outcomes, the F-test rejects the linearity assumption for all out-of-home placement outcomes except the share of children cared for by grandparents at the 10% significance level. Together with the F-test for joint significance of $\delta_2 = \delta_3 = 0$ presented in Tables 2.7 and 2.8, these results suggest mis-specification of APP's model (2.2) since the estimated effect of the reformulation was not linear following 2011. However, the F-test for statistically negligible coefficients on the common linear trend, δ_2, and the linear trend break post-2011, δ_3, fails to rejects that both are significant to estimating the effect of the reformulation on opioid abuse and out-of-home placement outcomes in equation (2.2). Therefore, although the preferred specification (2.2) accurately models overall trend results, interpretation of individual coefficients from this model are likely bias.

Measuring Opioid Exposure using 2010 Prescription Rates

For the main results, I use the rate of adult opioid treatment admissions to identify pre-2010 exposure to OxyContin abuse. For robustness, I replicate the analysis using the natural log of the 2010 opioid prscription rate, $ln(Prescribe_s^{2010})$, as another measure of pre-2010 OxyContin exposure. Opioid prescription rates (per 100 persons) were collected from the Center for Diease Control's U.S. County Prescribing Rates Maps. This sample covers nearly 90% of all retail prescriptions in the US.[11] One prescription is defined as an initial or refill prescription dispensed at a retail pharmacy and does not include mail-order prescriptions.

[11] Estimates are based on a sample of approximately 50,000 retail (non-hospital) pharmacies collected by IQVIA Xponent.

For child out-of-home placements, I find a significant effect of opioid abuse on parental drug abuse related foster care entry rate (1.3) under OLS shown Panel A of Table 2.14. Using opioid prescription rates as an instrument for opioid abuse treatment admissions, Panel B shows estimated significant effects on drug (1.4) or neglect (3.7) related foster care entries consistent with the main analysis. Interpreting the coefficient for on drug-related child out-of-home foster placements in Panel B, a 10% increase in the average opioid prescription rate increased the drug-related out-of-home placement rate by 1.3 additional entries per 100,000 children. Slightly larger effects are estimated using only contemporaneous prescription rates as an instrument for opioid abuse on drug-related (2.5) or neglect (5.6) as well as significant effect on the overall foster care out-of-home placement rate (3.1) shown in Table 2.15.

Estimating the effect of OxyContin's reformulation directly on opioid abuse rates, I find an increase in heroin-related treatment admissions (0.4) and reductions in OxyContin and other synthetic (-0.3) and non-opioid (-0.3) related treatment admissions using equation (2.5) shown in Panel A of Table 2.16. Assuming linear trends under equation (2.2), only the effect on heroin-related drug treatment admissions (0.13) is estimated to be significant at the 10% significance level in Panel B. Relative to 2009, I find no significant correlation between adult opioid or non-opioid related treatment admission rates with opioid prescription rates prior to 2009 using the fixed effects model (2.6) shown in Table 2.17, but a highly significant association post 2010.

For child out-of-home placements, I find a significant effect of the 2010 reformulation on drug abuse related foster care entry rate (0.2) using the baseline model shown in Panel A of Table 2.18. However, I find no significant effect on any child out-of-home placement outcome after controlling for a linear trend shown in Panel B. Using the generalized fixed effects model (2.6), relative to 2009 I find a significant positive association between the overall child out-of-home foster care placement rate and pre-2010 opioid exposure using opioid prescription rates but find no significant association on drug-related child out-of-home placements (See Table 2.19 and Figure 2.7)

State-level Analysis

As a final validity check, I replicate the main analysis at the state-year level of observation. For child outcomes, the sample consists of 561 state-year observations. Due to missing data, for adult

substance abuse, there are 549 state-year observations. Relative to the main results, estimates at the state-level are not robust or statistically significant due to a decrease in statistical power.

Tables 2.20 and 2.21 show the state-level analysis estimates. Using ordinary least squares, I find no significant effect of opioid abuse on any child foster care out-of-home placement outcome. Under instrumental variables, I do find a significant decrease in the rate of children being cared for by a grandparent using either the reformulation (-47.9) or contemporary oxycodone retail distribution rates (-70.4). However, I find no significant effect on any measure of child foster care out-of-home placements.

Examining the effect of OxyContin's reformulation separately on opioid abuse and out-of-home placement rates, for adult drug treatment admissions, I find an increase in heroin (0.3) and overall opioid-related (0.2) drug treatment admissions due to the reformulation using the baseline model shown in Panel A of Table 2.22. Controlling for linear trends, shown in Panel B, I find significant initial effects on OxyContin (0.1) and overall opioid-related (0.130) treatment admission rates, and only a significant effect after 2011 for heroin-related treatment admissions (0.09) at the 10% significance level. Looking at the estimated effect of the reformulation on treatment admission rates relative to 2009 in Table 2.23 (shown graphically in Figure 2.8), I find a strong association between the 2010 opioid prescription rate and opioid-related drug abuse treatment admissions after 2010.

For child outcomes, I find no significant effect of OxyContin's reformulation using the baseline model in Panel A of Table 2.24. After controlling for a linear time trend, Panel B shows a significant effect of the reformulation on overall foster care entries (0.02) and drug or neglect related entries (0.03) after 2011. However, looking at the relative effect of OxyContin's reformulation on child out-of-home placements compared to 2009 in Table 2.25, I find no significant effect after 2010.

2.6 Conclusion

This study estimated the effect of the recent US opioid abuse crisis on rates of child out-of-home placements. Using data collected on the national supply of oxycodone, adult drug abuse treatment admissions, and child out-of home placements from 2006-2016, I constructed an instrumental variable for opioid abuse rates by modeling the 2010 reformulation of OxyContin as done by APP. I find

that a 10% percent increase in the national opioid abuse rate increased the rate of foster care entries due to parental drug abuse by 2.4 entries per 100,000 children. Including child foster care entries due to parental neglect, incarceration, death, or abandonment, a 10% increase in the national opioid abuse rate caused an estimated 5.6 additional entries per 100,000 children. However, I find no significant effect on the overall rate of children entering foster care or share of children cared for by a grandparent.

These effects are largely driven by a significant increase in heroin-related substance abuse rather than OxyContin abuse over the sample period. Replicating APP, I show that the 2010 reformulation significantly increased overall opioid abuse treatment admissions though an increase in heroin-related abuse. I also find evidence of a lagged effect on greater overall and neglect related foster care out-of-home placements beginning in 2012. These results suggest that a national increase in opioid abuse has significantly increased the proportion of children entering foster care due to drug-related reasons for entry. However, I estimate no significant effect on the overall rate of child foster care out-of-home placements indicating that non-drug related foster care entries have decreased over the study period.

Using 2010 opioid prescription rates as an alternative measure of OxyContin exposure, I find slightly larger effects on child out-of-home placements. Estimated effects at the state-level of observation were largely consistent with the main results regarding the effect of OxyContin's reformulation on adult and child outcomes. However, I find no significant effect of opioid abuse on child out-of-home placements using instrumental variables at the state-level of analysis. One reason is that the 2010 oxycodone retail supply measure is a much weaker instrument for adult opioid abuse at the state-level. For the state-level first-stage instrumental variable regression, the F-statistic is only 4.7. Another reason for conflicting estimates is loss of statistical power due to a large reduction in sample size. Although I find an increase in the flow of child out-of-home placements due to drug abuse, the analysis does not account for foster care re-entries by a child within a given fiscal year and does not consider cases of foster care entry due to child drug abuse. Parental opioid abuse also represents an unknown proportion of overall drug abuse related foster care entries measured in this study since no detailed information on the specific drug abused is available.

While work by K. Buckles et al. (2020) estimate that more children live in a household headed by a grandparent due to the opioid crisis, I find no significant effect of opioid abuse on the share of children cared for by grandparents over the period 2006-2016. One interpretation is that the opioid

crisis has not affected the behavior of very risky households where both parents have severe drug abuse problems over a relative short-time period – children from households with severe drug abuse problems are being cared for by grandparents at a consistent rate regardless of the substance. On the other hand, no significant effect on the share of children cared for by grandparents also indicates that grandparents are a potentially under-utilized alternative care arrangement for children of severely opioid addicted adults.

A large influx of children who need foster care out-of-home placement due to opioid abuse poses significant additional costs for the child welfare system since cases related to opioid abuse are more complex and take a longer time to resolve (Radel et al., 2018). The average length of stay in foster care was 20.1 months in fiscal year 2016 (US Department of Health and Human Services, 2017). However I find that children who have been placed out-of-home in the foster care system due to parental drug abuse stay in the system 2 months longer on average.

In 2016 estimated average cost to provide out-of-home foster care placement services per child was $33,200. As of the 2010 Census, 74.1 million children lived in the U.S.[12] I estimate that a 10% percent increase in the the opioid abuse treatment admission rate on average caused nearly 1,800 child foster care entries due to opioid abuse nationally over the study period 2006-2016.[13] This is equivalent to almost an additional 300 child-years of out-of-home foster care, or approximately $9.6 million ($2016).[14] Given a 27.2% increase in the opioid abuse treatment admission rate from the period 2006-2010 to 2011-2016, this equals $26M in additional costs to the US foster care system over the study period. For comparision, the annual budget for the US foster care system was $5 billion in 2016.[15]

Policies which allow children of drug-addicted parents to live with other family members such as grandparents should receive greater consideration in order to reduce public foster caregiving costs. Providing higher financial assistance to adopt or provide foster caregiving has been shown to increase child out-of-home placement rates among kin (Argys & Duncan, 2013; Brehm, 2018; K. S. Buckles, 2013; Doyle Jr, 2007a). While potentially more costly, greater reimbursement rates for foster care assistance could also increase the supply of foster parents since state foster care payments cover only

[12] U.S. Census Bureau - Age and Sex Composition: 2010 https://www.census.gov/prod/cen2010/briefs/c2010br-03.pdf
[13] $\frac{2.4}{10,000} \times 74,100,000 = 1,800$ additional drug-related foster placements
[14] $1,800 \times \frac{1.95}{12} = 300$ ($\times 33,210 = \$9.6$ million)
[15] HHS FY2016 Budget in Brief: ACF Budget Overview

35% to 45% of the estimated minimum adequate cost of child care (Ahn et al., 2018). For elderly or retired individuals, additional assistance should be provided to reduce the cost of care burden for a grandchild.

Interventions aimed at reducing parental drug abuse to improve household conditions may also be effective. One policy to reduce the number of children cared for by the foster system is to target in-home services towards children living in high risk households. The Family First Prevention Act of 2018 gave states increased controlled over how Title IV-E funding can be spent. Under the Act, states, territories, and tribes with an approved Title IV-E plan can use funding for in-home prevention services that would allow children who would otherwise enter the foster care system to stay with their parents or relatives.[16] Given the recent passage of the Family First Prevention Services Act, further evaluation of effective family support services is also necessary to ensure that those children who remain at home with parents are provided better care relative to the foster care system.

In the case of pharmaceutical drug abuse, opiate-based and other addictive medications should be more closely monitored and restrictively prescribed as a "last resort" after alternative pain medications and treatment methods have been exhausted (Powell et al., 2018). Greater regulation on how pharmaceutical companies are allowed to market their products should also be enforced. Future drug abuse interventions should also better coordinate efforts to reduce the supply of addictive substances with efforts to provide substance abuse recovery treatment – particularly for adults caring for children. Providing greater resources towards substance abuse recovery and prevention programs may be a cost-effective solution to reducing the underlying demand for illicit substances in comparison to the long-term consequences of parental drug abuse on children.

Children in the foster care system whose parents have substance abuse problems are a well-defined, high-risk population. Additional research on the benefits of narrowly-targeted interventions to assist them and their families should be a paramount concern to reduce the social costs associated with child maltreatment and foster care. Future work should further examine the long-term causal effects of parental opioid and other types of substance abuse on children using more detailed information on specific-types of substance abuse, accurate costs of care or prevention services, and later child

[16] NCSL: Family First Prevention Services Act https://www.ncsl.org/research/human-services/family-first-prevention-services-act-ffpsa.aspx

outcomes which are currently unavailable in nationally representative child foster care out-of-home placement data.

2.7 Figures and Tables

Figure 2.1: National Overdose Deaths - National Institute of Drug Abuse

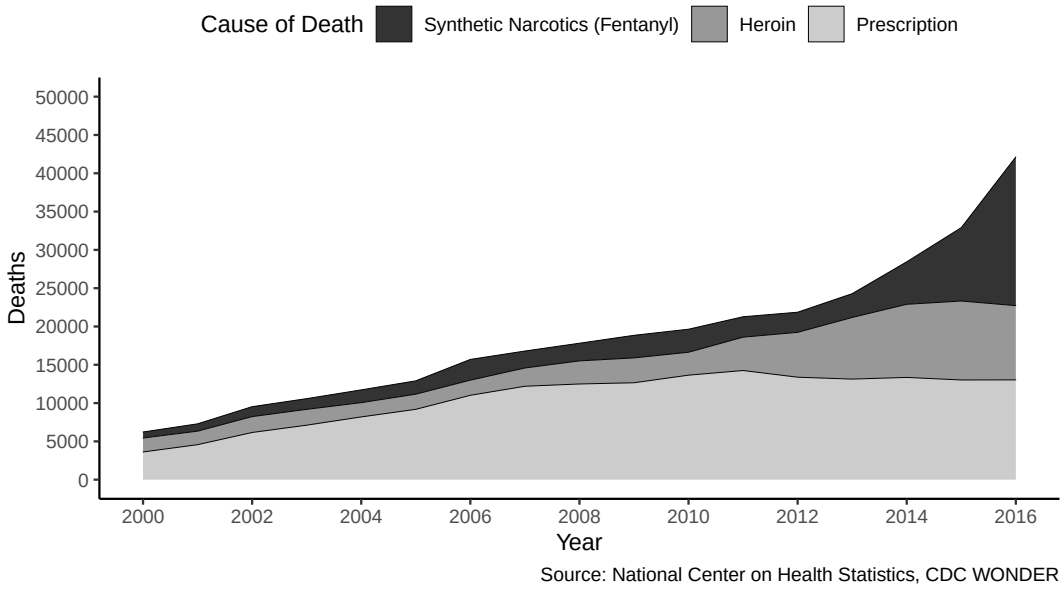


Figure 2.2: National Child Foster Care Entry Rates, 2006-2016

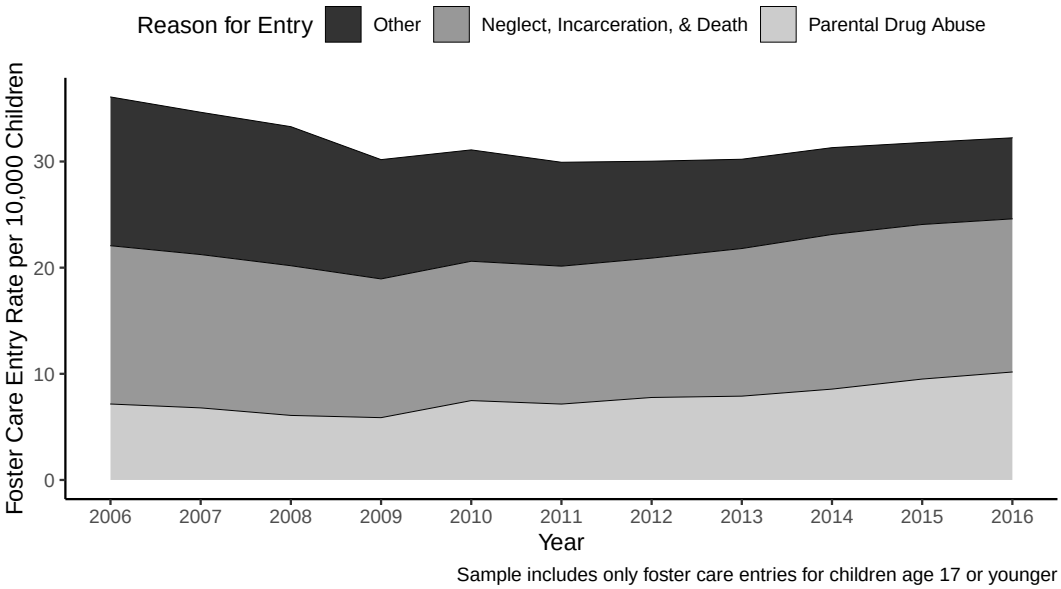


Figure 2.3: National Adult Drug Abuse Treatment Entry Rates, 2006-2016

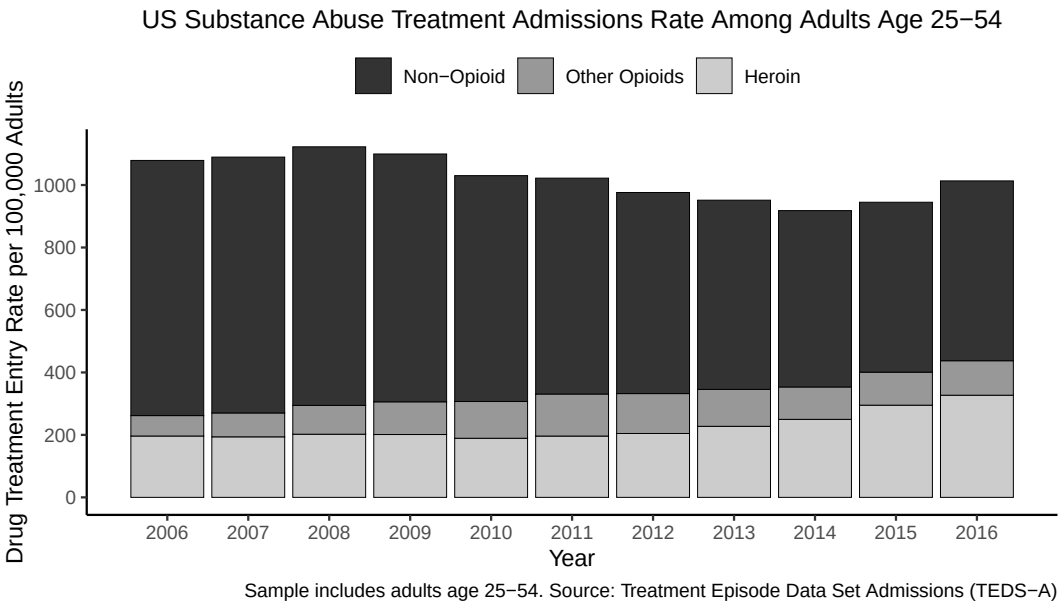


Figure 2.4: National Mean Oxycodone Supply Rates, 2010

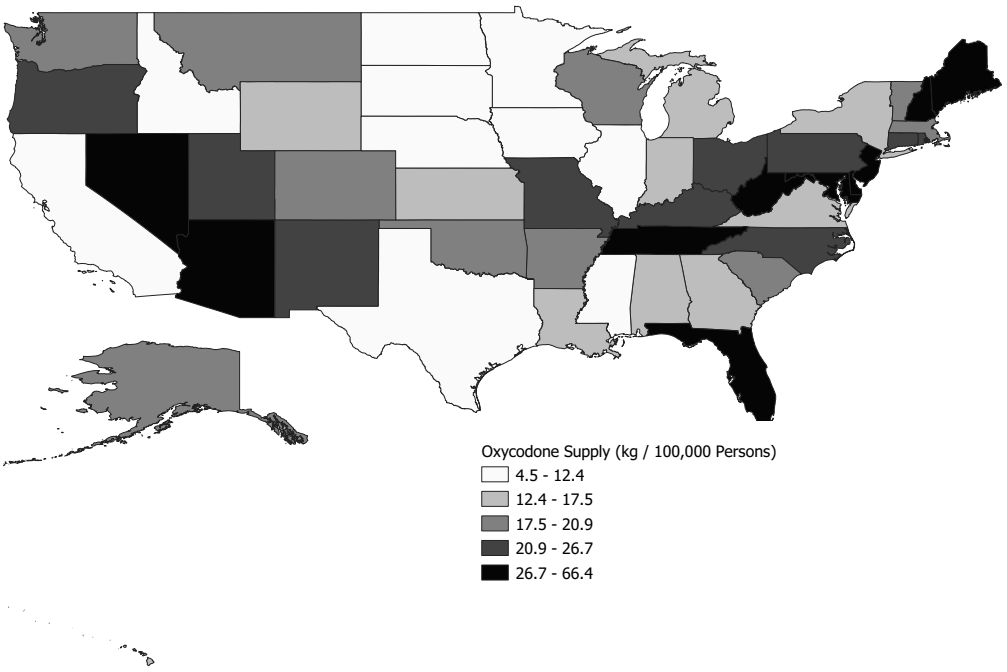


Table 2.1: Mean Annual Summary Statistics: Children Age 0-17

	All Years 2006-16	2006-10	2011-16	Difference	% Change
Foster Children					
Total Entry Rate Per 10,000	31.89	33.05	30.91	-2.13	-6.45
Drug-Related Entry Rate Per 10,000	7.67	6.67	8.51	1.83	27.48
Drugs, Neglect, etc. Entry Rate Per 10,000	21.60	20.60	22.44	1.84	8.91
% Cared for by Grandparent	4.97	4.88	5.06	.17	3.51
% White	43.05	42.99	43.11	0.11	0.27
% Black	26.31	28.20	24.72	-3.48	-12.35
% Hispanic	20.44	19.73	21.03	1.30	6.59
% Other Race	10.20	9.07	11.14	2.07	22.79
% Female	49.08	49.22	48.97	-0.25	-0.51
Population Statistics					
% White	53.75	55.78	52.05	-3.72	-6.68
% Black	13.81	14.03	13.62	-0.41	-2.95
% Hispanic	23.05	21.66	24.22	2.55	11.79
% Other Race	9.39	8.53	10.11	1.58	18.56
% Female	48.85	48.84	48.86	0.02	0.04
Unemployment Rate	5.84	5.89	5.80	-0.10	-1.65
Household Income (000's $2010)	76.82	77.36	76.37	-0.99	-1.28
Mean Parent's Age	38.54	38.31	38.73	0.42	1.10
% Less than H.S. Educ.	12.67	13.25	12.19	-1.06	-8.02
% H.S. Educ.	21.41	22.93	20.15	-2.78	-12.14
% Some College	30.03	29.88	30.16	0.29	0.96
% Bachelor or More	35.88	33.94	37.50	3.56	10.50
% Single Female Head of Household	21.45	21.27	21.60	0.34	1.58
Poverty Rate	21.10	19.88	22.13	2.25	11.30
Oxycodone Supply (kg/100,000 persons)	17.95	17.13	18.63	1.50	8.75
Total Population (10,000's)	80.14	80.40	79.93	-0.46	-0.57

Notes: Sample includes children less than 18. Foster care and adoption statistics were obtained from the Adoption and Foster Care Analysis and Report System (AFCARS). Population demographics were obtained from the 1-year American Community Survey (ACS). Unemployment rates were obtained from the Bureau of Labor Statistics. Oxycodone supply data was obtained from the DEA's Automated Reports and Consolidated Ordering System (ARCOS).

Table 2.2: Mean Annual Summary Statistics: Adults Age 25-54

	All Years 2006-16	2006-10	2011-16	Difference	% Change
Drug Treatment Patients					
Heroin Entry Rate Per 100,000	225.30	196.58	249.34	52.76	26.84
OxyContin/Synthetic Entry Rate Per 100,000	132.14	109.98	150.69	40.71	37.02
Any Opiate Entry Rate Per 100,000	330.52	287.89	366.21	78.32	27.21
Non-Opiate Entry Rate Per 100,000	691.89	796.09	604.68	-191.41	-24.04
% White	61.47	60.09	62.63	2.54	4.22
% Black	20.91	22.88	19.26	-3.62	-15.82
% Hispanic	15.56	15.67	15.46	-0.21	-1.36
% Other Race	4.52	4.18	4.80	0.62	14.73
% Female	35.66	35.04	36.18	1.14	3.26
% Military Veteran	4.30	5.20	3.57	-1.63	-31.40
% Married	18.04	19.38	16.92	-2.45	-12.66
% Less than H.S. Educ.	31.42	32.12	30.83	-1.28	-3.99
% H.S. Educ.	44.93	44.49	45.30	0.82	1.83
% Some College	20.25	19.69	20.71	1.03	5.22
% Bachelor or More	5.75	5.46	5.99	0.53	9.71
Unemployment Rate	62.11	58.69	64.98	6.29	10.72
Population Statistics					
% White	62.43	64.54	60.67	-3.88	-6.01
% Black	12.28	12.05	12.47	0.42	3.51
% Hispanic	17.09	15.92	18.08	2.16	13.56
% Other Race	8.19	7.49	8.78	1.29	17.29
% Female	50.13	50.04	50.20	0.17	0.33
% Military Veteran	5.29	5.79	4.86	-0.93	-16.05
% Married	56.44	58.36	54.83	-3.53	-6.04
% Less than H.S. Educ.	12.26	12.77	11.83	-0.94	-7.37
% H.S. Educ.	26.55	27.67	25.61	-2.06	-7.43
% Some College	30.29	30.03	30.51	0.48	1.60
% Bachelor or More	30.90	29.53	32.05	2.52	8.52
Unemployment Rate	5.87	5.85	5.88	0.04	0.61
Per Capita Income (000's $2010)	37.31	37.79	36.91	-0.88	-2.34
Poverty Rate (Population)	12.43	11.38	13.30	1.92	16.83
Oxycodone Supply (kg/100,000 persons)	18.19	17.46	18.80	1.34	7.67
Total Population (100,000's)	8.40	8.48	8.34	-0.13	-1.56

Notes: Sample includes adults age 25-54. Adult Substance Abuse Treatment Admissions were obtained from the Substance Abuse and Mental Health Services Administration's Treatment Episode Data Set Admissions (TEDS-A) dataset. Population demographics were obtained from the 1-year American Community Survey (ACS). Unemployment rates were obtained from the Bureau of Labor Statistics. Oxycodone supply data was obtained from the DEA's Automated Reports and Consolidated Ordering System (ARCOS).

Table 2.3: Effect of Opioid Abuse on Out-of-Home Placements

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
Panel A: Ordinary Least Squares				
$ln(Opioid\_Abuse_{ast})$	−0.116	1.266*	1.538	0.321
	(1.231)	(0.694)	(1.061)	(6.934)
R^2	0.621	0.664	0.753	0.544
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Instrumental Variables				
$ln(\widehat{Opioid\_Abuse}_{ast})$	3.268	2.401***	5.631***	9.294
	(2.277)	(0.784)	(1.430)	(11.319)
First-Stage F-Statistic	244.751	244.751	244.751	244.751
R^2	0.618	0.662	0.746	0.544
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Observations	9,882	9,882	9,882	9,882

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares and instrumental variable regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from 2006 to 2016. All estimates control for age, year, and state fixed-effects. For instrumental variables (Panel B), the first stage regression estimate the effect of OxyContin's reformulation on opioid abuse rates controlling for a common linear trend, post-2010 trend break, and the following adult demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education. The second stage and ordinary least squares regressions estimate the effect of opioid abuse on child out-of-home placements controlling for the following child demographics: share of the population non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.4: Effect of Heroin Abuse on Out-of-Home Placements

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
Panel A: Ordinary Least Squares				
$ln(Heroin\_Abuse_{ast})$	1.464	2.081***	2.755***	1.802
	(0.908)	(0.551)	(0.839)	(5.356)
R^2	0.623	0.672	0.757	0.543
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Instrumental Variables				
$ln(\widehat{Heroin\_Abuse}_{ast})$	3.770*	2.613***	5.831***	8.141
	(2.148)	(0.732)	(1.341)	(10.901)
First-Stage F-Statistic	223.126	223.126	223.126	223.126
R^2	0.621	0.672	0.751	0.543
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Observations	9,809	9,809	9,809	9,809

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares and instrumental variable regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from 2006 to 2016. All estimates control for age, year, and state fixed-effects. For instrumental variables (Panel B), the first stage regression estimate the effect of OxyContin's reformulation on heroin abuse rates controlling for a common linear trend, post-2010 trend break, and the following adult demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education. The second stage and ordinary least squares regressions estimate the effect of heroin abuse on child out-of-home placements controlling for the following child demographics: share of the population non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.5: Effect of OxyContin Abuse on Out-of-Home Placements

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
Panel A: Ordinary Least Squares				
$ln(OxyContin\_Abuse_{ast})$	−1.785	0.060	−0.917	3.642
	(1.660)	(0.655)	(1.218)	(7.328)
R^2	0.621	0.662	0.753	0.545
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Instrumental Variables				
$ln(\widehat{OxyContin\_Abuse}_{ast})$	−0.504	0.929	2.016	0.265
	(2.002)	(0.824)	(1.253)	(10.047)
First-Stage F-Statistic	202.431	202.431	202.431	202.431
R^2	0.621	0.661	0.749	0.545
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Observations	9,864	9,864	9,864	9,864

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares and instrumental variable regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from 2006 to 2016. All estimates control for age, year, and state fixed-effects. For instrumental variables (Panel B), the first stage regression estimate the effect of OxyContin's reformulation on OxyContin abuse rates controlling for a common linear trend, post-2010 trend break, and the following adult demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education. The second stage and ordinary least squares regressions estimate the effect of opioid abuse on child out-of-home placements controlling for the following child demographics: share of the population non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.6: Effect of Opioid Abuse on Out-of-Home Placements

using Only Contemporaneous Oxycodone Sales

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
$ln(\widehat{Opioid\_Abuse}_{ast})$	2.985	2.281***	5.378***	7.193
	(2.277)	(0.784)	(1.430)	(11.319)
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
First-Stage F-Statistic	277.583	277.583	277.583	277.583
R^2	0.618	0.663	0.747	0.544
Observations	9,882	9,882	9,882	9,882

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares and instrumental variable regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from 2006 to 2016. All estimates control for age, year, and state fixed-effects. The first stage regression estimate the effect of OxyContin's reformulation on opioid abuse rates using only contemporaneous oxycodone supply rates, and the following adult demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education. The second stage and ordinary least squares regressions estimate the effect of opioid abuse on child out-of-home placements controlling for the following child demographics: share of the population non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.7: Effect of Oxycontin's Reformulation on Adults

	Heroin (1)	OxyContin Synthetic (2)	Any Opiate (3)	Not Opiate (4)
Panel A: Baseline Model				
$Post \times ln(Oxy\_Supply_s^{2010})$	0.291*	0.061	0.215	0.008
	(0.174)	(0.063)	(0.135)	(0.135)
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Controlling for Linear Trends				
$Post \times ln(Oxy\_Supply_s^{2010})$	0.158*	0.086**	0.133**	0.074
	(0.087)	(0.042)	(0.062)	(0.047)
$Post \times (t-2011) \times ln(Oxy\_Supply_s^{2010})$	0.078*	−0.115	−0.021	−0.041
	(0.047)	(0.074)	(0.072)	(0.046)
F-statistic: $\delta_2 = \delta_3 = 0$	1.399	1.627	0.892	0.428
Pr(>F)	0.247	0.197	0.410	0.652
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes	Yes
Mean of Dependent Variable	225.298	132.138	330.524	691.891
Observations	1,647	1,647	1,647	1,647

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are adult age-state-year cells from fiscal year 2006 to 2016. Panel A estimates the effect of OxyContin's reformulation on adult drug abuse rates as an averaged effect over the post-reformulation period. Panel B shows estimates when also controlling for a common linear trend, post-2010 trend break. All estimates control for age, year, and state fixed-effects and the following adult demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education.

Table 2.8: Effect of Oxycontin's Reformulation on Children

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
Panel A: Baseline Model				
$Post \times ln(Oxy\_Supply_s^{2010})$	−0.017	−0.075	−0.021	0.007
	(0.047)	(0.075)	(0.030)	(0.030)
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Controlling for Linear Trends				
$Post \times ln(Oxy\_Supply_s^{2010})$	0.043	−0.085	0.028	−0.009
	(0.069)	(0.099)	(0.070)	(0.016)
$Post \times (t-2011) \times ln(Oxy\_Supply_s^{2010})$	0.027**	−0.014	0.042*	0.005
	(0.013)	(0.039)	(0.024)	(0.005)
F-statistic: $\delta_2 = \delta_3 = 0$	2.142	0.078	2.743	0.463
Pr(>F)	0.118	0.925	0.064	0.630
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes	Yes
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Observations	10,098	10,098	10,098	10,098

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from fiscal year 2006 to 2016. Panel A estimates the effect of OxyContin's reformulation on child out-of-home placement rates as an averaged effect over the post-reformulation period. Panel B shows estimates when also controlling for a common linear trend, post-2010 trend break. All estimates control for age, year, and state fixed-effects and the following child demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.9: Effect of Oxycontin's Reformulation on Adults using Fixed Effects

	Heroin (1)	OxyContin Synthetic (2)	Any Opiate (3)	Not Opiate (4)
$\mathbb{1}_{2006} \times ln(Oxy\_Supply_s^{2010})$	0.036	−0.137	−0.068	−0.072
	(0.062)	(0.103)	(0.085)	(0.085)
$\mathbb{1}_{2007} \times ln(Oxy\_Supply_s^{2010})$	0.075	−0.138	−0.016	−0.032
	(0.061)	(0.096)	(0.077)	(0.077)
$\mathbb{1}_{2008} \times ln(Oxy\_Supply_s^{2010})$	0.034	−0.036	−0.007	0.018
	(0.054)	(0.046)	(0.048)	(0.048)
$\mathbb{1}_{2010} \times ln(Oxy\_Supply_s^{2010})$	0.017	0.034	0.050	−0.067*
	(0.045)	(0.042)	(0.035)	(0.035)
$\mathbb{1}_{2011} \times ln(Oxy\_Supply_s^{2010})$	0.123**	0.268***	0.245**	0.112
	(0.058)	(0.088)	(0.102)	(0.102)
$\mathbb{1}_{2012} \times ln(Oxy\_Supply_s^{2010})$	0.273**	0.056	0.204*	0.008
	(0.135)	(0.053)	(0.112)	(0.112)
$\mathbb{1}_{2013} \times ln(Oxy\_Supply_s^{2010})$	0.339**	−0.000	0.205	−0.028
	(0.164)	(0.087)	(0.143)	(0.143)
$\mathbb{1}_{2014} \times ln(Oxy\_Supply_s^{2010})$	0.235	−0.139	0.080	−0.128
	(0.223)	(0.135)	(0.189)	(0.189)
$\mathbb{1}_{2015} \times ln(Oxy\_Supply_s^{2010})$	0.497**	−0.096	0.258	−0.082
	(0.231)	(0.185)	(0.220)	(0.220)
$\mathbb{1}_{2016} \times ln(Oxy\_Supply_s^{2010})$	0.477*	−0.093	0.250	−0.061
	(0.254)	(0.205)	(0.237)	(0.237)
Mean of Dependent Variable	225.298	132.138	330.524	691.891
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Observations	1,647	1,647	1,647	1,647

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are adult age-state-year cells from fiscal year 2006 to 2016. All estimates control for age, year, and state fixed-effects and the following demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education.

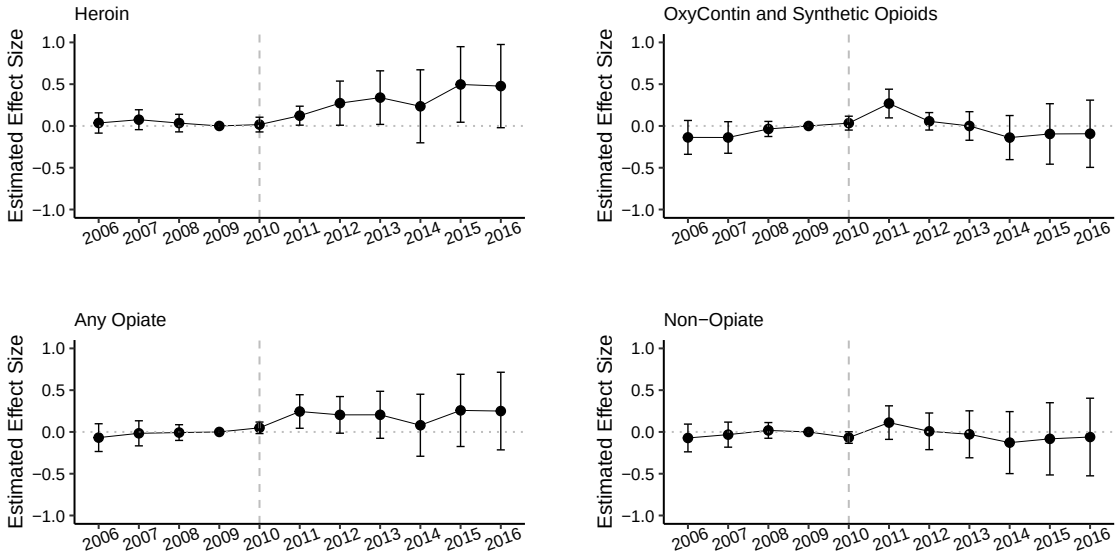
Table 2.10: Effect of Oxycontin's Reformulation on Children using Fixed Effects

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
$\mathbb{1}_{2006} \times ln(Oxy\_Supply_s^{2010})$	0.073	−0.037	0.085	−0.007
	(0.064)	(0.119)	(0.092)	(0.092)
$\mathbb{1}_{2007} \times ln(Oxy\_Supply_s^{2010})$	0.043	−0.056	0.043	0.037
	(0.039)	(0.064)	(0.055)	(0.055)
$\mathbb{1}_{2008} \times ln(Oxy\_Supply_s^{2010})$	0.056	0.036	0.064**	0.026
	(0.038)	(0.034)	(0.027)	(0.027)
$\mathbb{1}_{2010} \times ln(Oxy\_Supply_s^{2010})$	−0.021	−0.021	−0.034	0.019
	(0.050)	(0.076)	(0.050)	(0.050)
$\mathbb{1}_{2011} \times ln(Oxy\_Supply_s^{2010})$	0.008	−0.080	−0.030	−0.003
	(0.075)	(0.052)	(0.067)	(0.067)
$\mathbb{1}_{2012} \times ln(Oxy\_Supply_s^{2010})$	0.023	−0.033	0.012	0.027
	(0.071)	(0.048)	(0.061)	(0.061)
$\mathbb{1}_{2013} \times ln(Oxy\_Supply_s^{2010})$	−0.014	−0.139	−0.028	0.023
	(0.068)	(0.085)	(0.045)	(0.045)
$\mathbb{1}_{2014} \times ln(Oxy\_Supply_s^{2010})$	0.004	−0.133	0.014	0.041
	(0.068)	(0.096)	(0.051)	(0.051)
$\mathbb{1}_{2015} \times ln(Oxy\_Supply_s^{2010})$	0.039	−0.056	0.059	0.011
	(0.073)	(0.095)	(0.055)	(0.055)
$\mathbb{1}_{2016} \times ln(Oxy\_Supply_s^{2010})$	0.022	−0.110	0.031	0.044
	(0.081)	(0.094)	(0.064)	(0.064)
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Observations	10,098	10,098	10,098	10,098

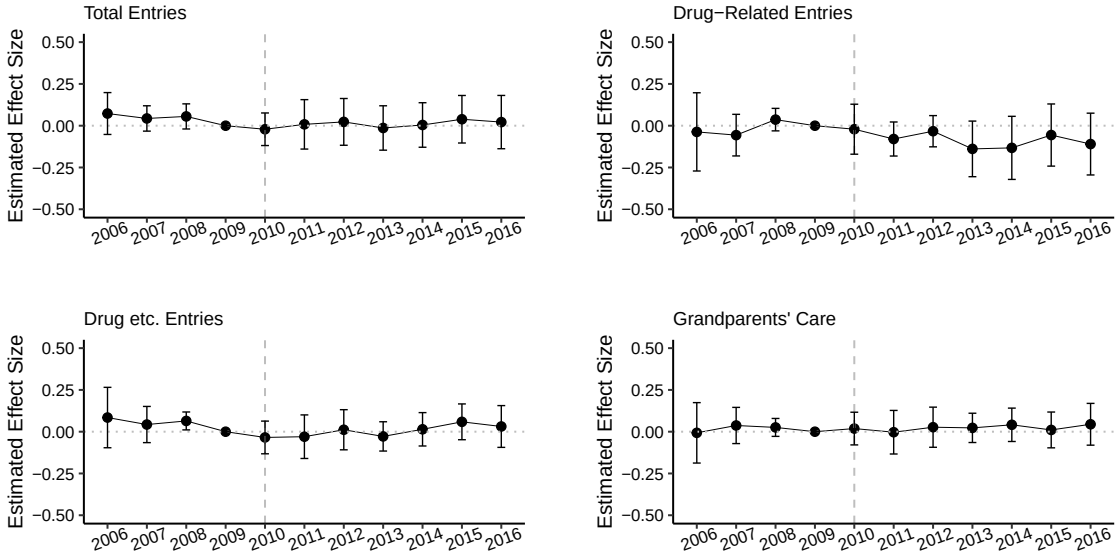
Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from fiscal year 2006 to 2016. All estimates control for age, year, and state fixed-effects and the following demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Figure 2.5: Fixed Effects Model Estimates of δ_t

Panel A: Adult Substance Abuse Treatment Admissions



Panel B: Child Out-of-Home Placements



Note: Each graph includes 95 percent confidence intervals using standard errors clustered at the state-level.

Table 2.11: Effect of Drug Abuse on Child Lifetime Foster Care Experiences

	(1)	(2)	(3)
Panel A: Effect on Total Length of Stay (OLS)			
Drug Abuse	0.662	1.358**	1.953***
	(0.737)	(0.412)	(0.373)
Demographic Controls	Yes	Yes	Yes
State Fixed-Effects		Yes	Yes
Year Fixed-Effects			Yes
Observations	2,604,836	2,604,836	2604,836
Panel B: Effect on Likelihood of Foster Care Recidivism (Logit)			
Drug Abuse	1.229**	1.311***	1.427***
	(0.0913)	(0.0386)	(0.0422)
Demographic Controls	Yes	Yes	Yes
State Fixed-Effects		Yes	Yes
Year Fixed-Effects			Yes
Observations	2,662,332	2,662,332	2,662,332

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are children who entered foster care from fiscal year 2006 to 2016. All estimates control for age of first entry, year, and state fixed-effects, the following demographics: sex, race. Omitted categorical variables were non-Hispanic white, and males.

Table 2.12: Linear Additive Effect of the Reformulation on Adults

	Heroin (1)	OxyContin Synthetic (2)	Any Opiate (3)	Not Opiate (4)
$\mathbb{1}_{2011} \times ln(Oxy\_Supply_s^{2010})$	0.125^{*}	0.180^{***}	0.178^{**}	0.126^{**}
	(0.071)	(0.069)	(0.081)	(0.062)
$\mathbb{1}_{2012} \times ln(Oxy\_Supply_s^{2010})$	0.162	-0.260^{*}	-0.065	-0.110
	(0.104)	(0.136)	(0.172)	(0.122)
$\mathbb{1}_{2013} \times ln(Oxy\_Supply_s^{2010})$	0.078^{*}	-0.103	-0.024	-0.041
	(0.042)	(0.064)	(0.065)	(0.034)
$\mathbb{1}_{2014} \times ln(Oxy\_Supply_s^{2010})$	-0.092	-0.187^{**}	-0.150^{*}	-0.105^{**}
	(0.091)	(0.077)	(0.078)	(0.048)
$\mathbb{1}_{2015} \times ln(Oxy\_Supply_s^{2010})$	0.273	-0.004	0.153	0.040
	(0.185)	(0.119)	(0.162)	(0.057)
$\mathbb{1}_{2016} \times ln(Oxy\_Supply_s^{2010})$	-0.008	-0.045	-0.033	0.016
	(0.088)	(0.055)	(0.064)	(0.051)
Mean of Dependent Variable	225.298	132.138	330.524	691.891
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes	Yes
F-statistic	2.094	2.232	1.419	2.535
Pr(>F)	0.079	0.063	0.225	0.039
Observations	1,647	1,647	1,647	1,647

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are adult age-state-year cells from fiscal year 2006 to 2016. All estimates control for age, year, and state fixed-effects, a common linear trend, post-2010 trend break, and the following demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education.

Table 2.13: Linear Additive Effects of the Reformulation on Children

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
$\mathbb{1}_{2011} \times ln(Oxy\_Supply_s^{2010})$	0.047	−0.087	0.023	−0.021
	(0.077)	(0.103)	(0.083)	(0.018)
$\mathbb{1}_{2012} \times ln(Oxy\_Supply_s^{2010})$	0.038**	0.038	0.070***	0.029
	(0.019)	(0.044)	(0.024)	(0.019)
$\mathbb{1}_{2013} \times ln(Oxy\_Supply_s^{2010})$	−0.013	−0.114	−0.012	−0.005
	(0.013)	(0.073)	(0.020)	(0.020)
$\mathbb{1}_{2014} \times ln(Oxy\_Supply_s^{2010})$	0.041*	−0.002	0.071	0.017
	(0.024)	(0.030)	(0.043)	(0.024)
$\mathbb{1}_{2015} \times ln(Oxy\_Supply_s^{2010})$	0.057**	0.069*	0.073**	−0.032*
	(0.027)	(0.039)	(0.030)	(0.017)
$\mathbb{1}_{2016} \times ln(Oxy\_Supply_s^{2010})$	0.006	−0.062	0.000	0.033*
	(0.023)	(0.068)	(0.036)	(0.017)
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes	Yes
F-statistic	4.169	3.492	4.505	1.817
Pr(>F)	0.002	0.007	0.001	0.123
Observations	10,098	10,098	10,098	10,098

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are adult age-state-year cells from fiscal year 2006 to 2016. All estimates control for age, year, and state fixed-effects, a common linear trend, post-2010 trend break, and the following demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Figure 2.6: National Opioid Prescription Rates, 2010

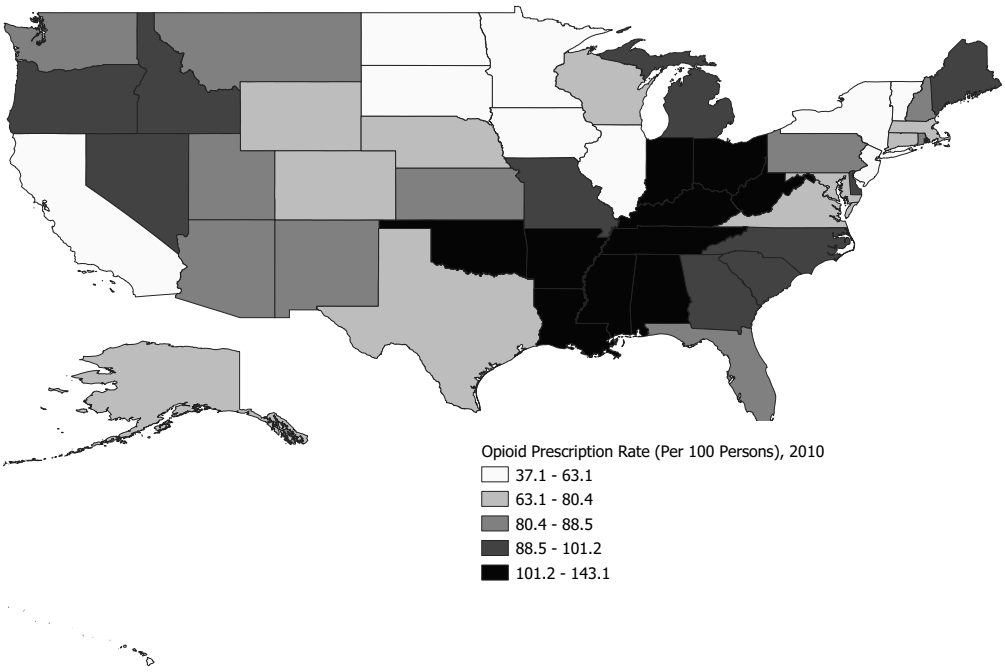


Table 2.14: Effect of Opioid Abuse on Out-of-Home Placements using Prescription Rates

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
Panel A: Ordinary Least Squares				
$ln(Opioid\_Abuse_{ast})$	−0.116	1.266*	1.538	0.321
	(1.231)	(0.694)	(1.061)	(6.934)
R^2	0.621	0.664	0.753	0.544
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Instrumental Variables				
$ln(\widehat{Opioid\_Abuse}_{ast})$	0.494	1.362**	3.710***	3.951
	(1.753)	(0.672)	(1.027)	(9.723)
First-Stage F-Statistic	273.707	273.707	273.707	273.707
R^2	0.620	0.664	0.751	0.544
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Observations	9,882	9,882	9,882	9,882

Notes: *** Denotes significance at .1%, ** at 1%, and * at 5%. Results from ordinary least squares and instrumental variable regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from 2006 to 2016. All estimates control for age, year, and state fixed-effects. For instrumental variables (Panel B), the first stage regression estimate the effect of OxyContin's reformulation on opioid abuse rates controlling for a common linear trend, post-2010 trend break, and the following adult demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education. The second stage and ordinary least squares regressions estimate the effect of opioid abuse on child out-of-home placements controlling for the following child demographics: share of the population non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.15: Effect of Opioid Abuse on Out-of-Home Placements

using Only Contemporary Opioid Prescription Rates

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
$ln(\widehat{Opioid\_Abuse}_{ast})$	3.143*	2.461***	5.572***	8.570
	(1.753)	(0.672)	(1.027)	(9.723)
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
First-Stage F-Statistic	275.673	275.673	275.673	275.673
R^2	0.618	0.662	0.746	0.544
Observations	9,882	9,882	9,882	9,882

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares and instrumental variable regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from 2006 to 2016. All estimates control for age, year, and state fixed-effects. The first stage regression estimate the effect of OxyContin's reformulation on opioid abuse rates using only contemporaneous opioid prescription rates, and the following adult demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education. The second stage and ordinary least squares regressions estimate the effect of opioid abuse on child out-of-home placements controlling for the following child demographics: share of the population non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.16: Effect of Oxycontin's Reformulation on Adults (Opioid Prescription Rates)

	Heroin (1)	OxyContin Synthetic (2)	Any Opiate (3)	Not Opiate (4)
Panel A: Baseline Model				
$Post \times ln(Prescribe_s^{2010})$	0.352^{*}	-0.344^{**}	0.035	-0.299^{**}
	(0.182)	(0.163)	(0.122)	(0.122)
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Controlling for Linear Trends				
$Post \times ln(Prescribe_s^{2010})$	0.135	-0.093	-0.006	-0.155
	(0.148)	(0.104)	(0.133)	(0.117)
$Post \times (t-2011) \times ln(Prescribe_s^{2010})$	0.128^{*}	-0.002	-0.078	0.006
	(0.073)	(0.061)	(0.075)	(0.065)
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes	Yes
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Observations	1,647	1,647	1,647	1,647

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are adult age-state-year cells from fiscal year 2006 to 2016. Panel A estimates the effect of OxyContin's reformulation on adult drug abuse rates as an averaged effect over the post-reformulation period. Panel B shows estimates when also controlling for a common linear trend, post-2010 trend break. All estimates control for age, year, and state fixed-effects and the following adult demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education.

Table 2.17: Effect of Oxycontin's Reformulation on Adults

using Fixed Effects (Opioid Prescription Rates)

	Heroin (1)	OxyContin Synthetic (2)	Any Opiate (3)	Not Opiate (4)
$\mathbb{1}_{2006} \times ln(Prescribe_s^{2010})$	0.033	0.156	−0.152	0.018
	(0.214)	(0.163)	(0.193)	(0.193)
$\mathbb{1}_{2007} \times ln(Prescribe_s^{2010})$	0.031	0.001	−0.114	−0.036
	(0.190)	(0.107)	(0.152)	(0.152)
$\mathbb{1}_{2008} \times ln(Prescribe_s^{2010})$	0.004	0.082	−0.022	−0.015
	(0.133)	(0.090)	(0.102)	(0.102)
$\mathbb{1}_{2010} \times ln(Prescribe_s^{2010})$	−0.065	−0.084	0.019	−0.156**
	(0.077)	(0.102)	(0.070)	(0.070)
$\mathbb{1}_{2011} \times ln(Prescribe_s^{2010})$	0.117	−0.148	0.124	−0.223*
	(0.118)	(0.127)	(0.117)	(0.117)
$\mathbb{1}_{2012} \times ln(Prescribe_s^{2010})$	0.156	−0.299**	0.038	−0.300***
	(0.116)	(0.146)	(0.111)	(0.111)
$\mathbb{1}_{2013} \times ln(Prescribe_s^{2010})$	0.280**	−0.345**	0.002	−0.357***
	(0.142)	(0.169)	(0.119)	(0.119)
$\mathbb{1}_{2014} \times ln(Prescribe_s^{2010})$	0.311	−0.377	−0.105	−0.510***
	(0.213)	(0.238)	(0.171)	(0.171)
$\mathbb{1}_{2015} \times ln(Prescribe_s^{2010})$	0.453**	−0.477**	−0.131	−0.453**
	(0.223)	(0.243)	(0.177)	(0.177)
$\mathbb{1}_{2016} \times ln(Prescribe_s^{2010})$	0.647**	−0.390	0.001	−0.254
	(0.316)	(0.250)	(0.233)	(0.233)
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Observations	1,647	1,647	1,647	1,647

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are adult age-state-year cells from fiscal year 2006 to 2016. All estimates control for age, year, and state fixed-effects and the following demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education.

Table 2.18: Effect of Oxycontin's Reformulation on Children (Opioid Prescription Rates)

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
Panel A: Baseline Model				
$Post \times ln(Prescribe_s^{2010})$	0.230***	0.091	−0.036	0.039
	(0.048)	(0.212)	(0.097)	(0.097)
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Controlling for Linear Trends				
$Post \times ln(Prescribe_s^{2010})$	0.102	−0.331	−0.023	0.005
	(0.090)	(0.229)	(0.121)	(0.046)
$Post \times (t-2011) \times ln(Prescribe_s^{2010})$	0.055	0.007	0.056	0.019
	(0.034)	(0.091)	(0.043)	(0.022)
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes	Yes
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Observations	10,098	10,098	10,098	10,098

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from fiscal year 2006 to 2016. Panel A estimates the effect of OxyContin's reformulation on child out-of-home placement rates as an averaged effect over the post-reformulation period. Panel B shows estimates when also controlling for a common linear trend, post-2010 trend break. All estimates control for age, year, and state fixed-effects and the following child demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.19: Effect of Oxycontin's Reformulation on Children

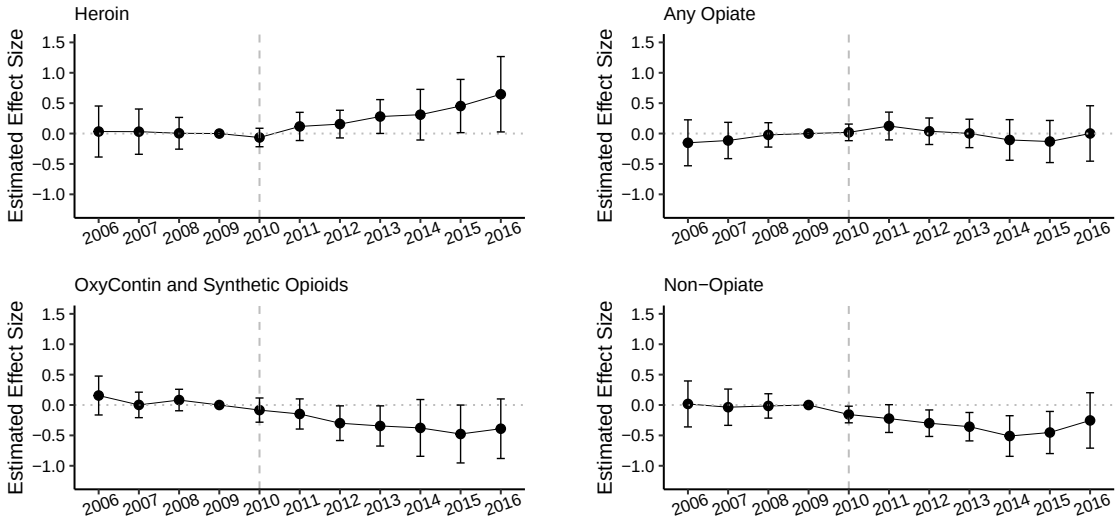
using Fixed Effects (Opioid Prescription Rates)

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
$\mathbb{1}_{2006} \times ln(Prescribe_s^{2010})$	0.025	−0.338	0.135	−0.012
	(0.101)	(0.285)	(0.179)	(0.179)
$\mathbb{1}_{2007} \times ln(Prescribe_s^{2010})$	−0.002	−0.153	0.121	−0.003
	(0.076)	(0.216)	(0.170)	(0.170)
$\mathbb{1}_{2008} \times ln(Prescribe_s^{2010})$	0.014	−0.073	0.095	0.019
	(0.051)	(0.142)	(0.081)	(0.081)
$\mathbb{1}_{2010} \times ln(Prescribe_s^{2010})$	0.022	−0.040	0.057	−0.019
	(0.053)	(0.169)	(0.098)	(0.098)
$\mathbb{1}_{2011} \times ln(Prescribe_s^{2010})$	0.135**	−0.229	0.016	−0.029
	(0.062)	(0.208)	(0.104)	(0.104)
$\mathbb{1}_{2012} \times ln(Prescribe_s^{2010})$	0.177***	−0.085	0.076	0.093
	(0.064)	(0.223)	(0.102)	(0.102)
$\mathbb{1}_{2013} \times ln(Prescribe_s^{2010})$	0.217***	−0.081	−0.034	−0.041
	(0.081)	(0.225)	(0.110)	(0.110)
$\mathbb{1}_{2014} \times ln(Prescribe_s^{2010})$	0.221**	−0.116	−0.031	0.064
	(0.092)	(0.207)	(0.090)	(0.090)
$\mathbb{1}_{2015} \times ln(Prescribe_s^{2010})$	0.323***	0.069	0.092	0.076
	(0.099)	(0.200)	(0.108)	(0.108)
$\mathbb{1}_{2016} \times ln(Prescribe_s^{2010})$	0.427***	0.235	0.198	0.082
	(0.115)	(0.241)	(0.135)	(0.135)
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Observations	10,098	10,098	10,098	10,098

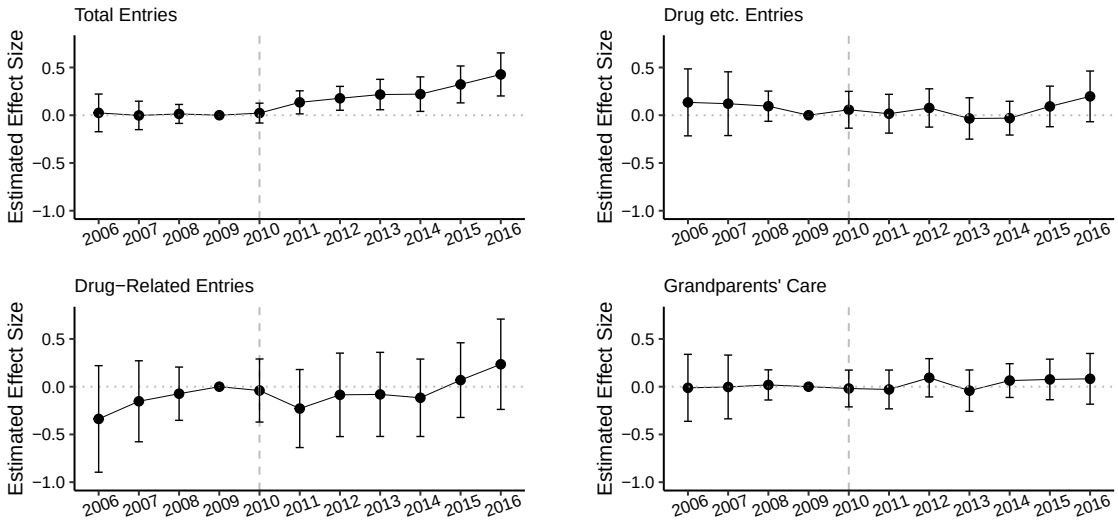
Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are child age-state-year cells from fiscal year 2006 to 2016. All estimates control for age, year, and state fixed-effects and the following demographic population shares: non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Figure 2.7: Fixed Effects Model Estimates of δ_t using Prescription Rates

Panel A: Adult Substance Abuse Treatment Admissions



Panel B: Child Out-of-Home Placements



Note: Each graph includes 95 percent confidence intervals using standard errors clustered at the state-level.

Table 2.20: State Analysis: Effect of Opioid Abuse on Out-of-Home Placements

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
Panel A: Ordinary Least Squares				
$ln(Opioid\_Abuse_{st})$	−1.064	0.731	−0.007	−5.201
	(2.035)	(0.964)	(1.687)	(12.938)
R^2	0.896	0.768	0.848	0.907
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Instrumental Variables				
$ln(\widehat{Opioid\_Abuse}_{st})$	−1.196	0.742	0.157	−47.871*
	(3.313)	(1.937)	(2.613)	(27.711)
First-Stage F-Statistic	4.687	4.687	4.687	4.687
R^2	0.896	0.768	0.848	0.903
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Observations	549	549	549	549

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares and instrumental variable regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are state-year cells from 2006 to 2016. All estimates control for year and state fixed-effects. For instrumental variables (Panel B), the first stage regression estimate the effect of OxyContin's reformulation on opioid abuse rates controlling for a common linear trend, post-2010 trend break, and the following adult demographic population shares: share of the population age 25-34, 35-44, and 44-54 years old, non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education. The second stage and ordinary least squares regressions estimate the effect of opioid abuse on child out-of-home placements controlling for the following child demographics: share of the population age less than 2, 2-5, 6-11, and 12-17 years old, non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education.

Table 2.21: State Analysis: Effect of Opioid Abuse on Out-of-Home Placements using Only Contemporaneous Oxycodone Sales

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
$ln(\widehat{Opioid\_Abuse}_{st})$	−3.593	0.277	−1.830	−70.360**
	(3.313)	(1.937)	(2.613)	(27.711)
Mean of Dependent Variable	31.887	7.672	21.600	497.760
Age Fixed–Effects	Yes	Yes	Yes	Yes
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
First-Stage F-Statistic	4.698	4.698	4.698	4.698
R^2	0.894	0.767	0.847	0.898
Observations	549	549	549	549

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares and instrumental variable regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are state-year cells from 2006 to 2016. All estimates control for year, and state fixed-effects. The first stage regression estimate the effect of OxyContin's reformulation on opioid abuse rates using only contemporaneous oxycodone supply rates, and the following adult demographic population shares: 25-34, 35-44, and 44-54 years old, non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education. The second stage regression contains the following child demographic controls: share of the population age less than 2, 2-5, 6-11, and 12-17 years old, non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education.

Table 2.22: State Analysis: Effect of Oxycontin's Reformulation on Adults

	Heroin (1)	OxyContin Synthetic (2)	Any Opiate (3)	Not Opiate (4)
Panel A: Baseline Model				
$Post \times ln(Oxy\_Supply_s^{2010})$	0.322**	0.116	0.234**	0.029
	(0.145)	(0.076)	(0.117)	(0.117)
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Controlling for Linear Trends				
$Post \times ln(Oxy\_Supply_s^{2010})$	0.151	0.116*	0.130*	0.078
	(0.098)	(0.069)	(0.075)	(0.053)
$Post \times (t-2011) \times ln(Oxy\_Supply_s^{2010})$	0.088**	−0.108	−0.015	−0.044
	(0.043)	(0.076)	(0.076)	(0.047)
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes	Yes
Observations	549	549	549	549

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are state-year cells from fiscal year 2006 to 2016. Panel A estimates the effect of OxyContin's reformulation on child out-of-home placement rates as an averaged effect over the post-reformulation period. Panel B shows estimates when also controlling for a common linear trend, post-2010 trend break. All estimates control for year and state fixed-effects, a common linear trend, post-2010 trend break, and the following child demographic population shares: share of the population age 25-34, 35-44, and 44-54 years old, non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Table 2.23: State Analysis: Effect of Oxycontin's Reformulation on Adults using Fixed Effects

	Heroin (1)	OxyContin Synthetic (2)	Any Opiate (3)	Not Opiate (4)
$\mathbb{1}_{2006} \times ln(Oxy\_Supply_s^{2010})$	0.027	−0.140	−0.084	−0.074
	(0.058)	(0.114)	(0.101)	(0.101)
$\mathbb{1}_{2007} \times ln(Oxy\_Supply_s^{2010})$	0.044	−0.152	−0.055	−0.055
	(0.069)	(0.095)	(0.094)	(0.094)
$\mathbb{1}_{2008} \times ln(Oxy\_Supply_s^{2010})$	0.012	−0.053	−0.040	0.001
	(0.067)	(0.052)	(0.067)	(0.067)
$\mathbb{1}_{2010} \times ln(Oxy\_Supply_s^{2010})$	0.025	0.018	0.024	−0.063
	(0.065)	(0.047)	(0.043)	(0.043)
$\mathbb{1}_{2011} \times ln(Oxy\_Supply_s^{2010})$	0.130*	0.281**	0.219*	0.112
	(0.068)	(0.124)	(0.127)	(0.127)
$\mathbb{1}_{2012} \times ln(Oxy\_Supply_s^{2010})$	0.268*	0.080	0.195*	0.018
	(0.149)	(0.057)	(0.116)	(0.116)
$\mathbb{1}_{2013} \times ln(Oxy\_Supply_s^{2010})$	0.386**	0.025	0.208	−0.024
	(0.179)	(0.079)	(0.145)	(0.145)
$\mathbb{1}_{2014} \times ln(Oxy\_Supply_s^{2010})$	0.300	−0.095	0.099	−0.113
	(0.228)	(0.124)	(0.186)	(0.186)
$\mathbb{1}_{2015} \times ln(Oxy\_Supply_s^{2010})$	0.553***	−0.047	0.278	−0.079
	(0.214)	(0.166)	(0.204)	(0.204)
$\mathbb{1}_{2016} \times ln(Oxy\_Supply_s^{2010})$	0.561**	−0.054	0.270	−0.053
	(0.258)	(0.196)	(0.226)	(0.226)
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Observations	549	549	549	549

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are state-year cells from fiscal year 2006 to 2016. All estimates control for year and state fixed-effects and the following demographic population shares: share of the population age 25-34, 35-44, and 44-54 years old, non-Hispanic black, non-Hispanic other race and Hispanic, female, with a high school degree, some college or associates degree, bachelors degree or more, military veteran, married, and living below 100% of the poverty line. Also included are the average per capita income, unemployment rates, whether a state PDMP program was active, and the natural log of the total population. Other race non-Hispanic includes Asian, Pacific Islander, American Indian, and other racial groups as well as multi-racial individuals. Omitted categorical variables were the share of the non-Hispanic white, males, and less than high school education.

Table 2.24: State Analysis: Effect of Oxycontin's Reformulation on Children

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
Panel A: Baseline Model				
$Post \times ln(Oxy\_Supply_s^{2010})$	0.007	−0.044	0.004	0.001
	(0.030)	(0.048)	(0.029)	(0.029)
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Panel B: Controlling for Linear Trends				
$Post \times ln(Oxy\_Supply_s^{2010})$	−0.004	−0.095	−0.014	−0.012
	(0.042)	(0.059)	(0.044)	(0.011)
$Post \times (t-2011) \times ln(Oxy\_Supply_s^{2010})$	0.018*	0.005	0.029**	0.006
	(0.009)	(0.022)	(0.014)	(0.005)
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes	Yes
Observations	561	561	561	561

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are state-year cells from fiscal year 2006 to 2016. Panel A estimates the effect of OxyContin's reformulation on child out-of-home placement rates as an averaged effect over the post-reformulation period. Panel B shows estimates when also controlling for a common linear trend, post-2010 trend break. All estimates control for year and state fixed-effects, a common linear trend, post-2010 trend break, and the following child demographic population shares: share of the population age less than 2, 2-5, 6-11, and 12-17 years old, non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

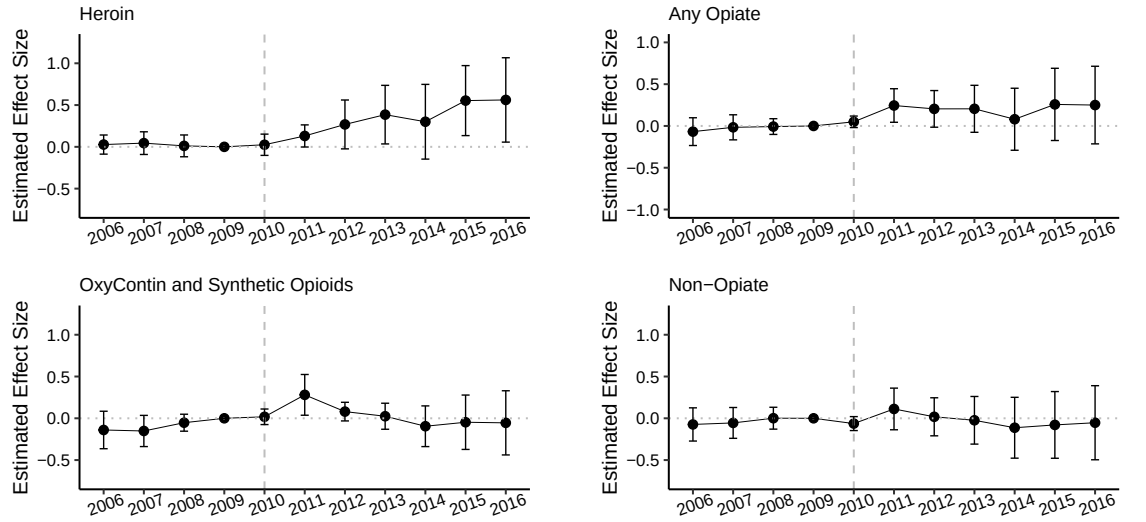
Table 2.25: State Analysis: Effect of Oxycontin's Reformulation on Children using Fixed Effects

	Total Entries (1)	Drug-Related Entries (2)	Drugs, Neglect, etc. (3)	Grandchildren (4)
$\mathbb{1}_{2006} \times ln(Oxy\_Supply_s^{2010})$	0.013	−0.049	0.015	−0.008
	(0.036)	(0.057)	(0.051)	(0.051)
$\mathbb{1}_{2007} \times ln(Oxy\_Supply_s^{2010})$	0.006	−0.048	−0.001	0.019
	(0.025)	(0.035)	(0.030)	(0.030)
$\mathbb{1}_{2008} \times ln(Oxy\_Supply_s^{2010})$	0.038	0.036*	0.038**	0.019
	(0.024)	(0.019)	(0.017)	(0.017)
$\mathbb{1}_{2010} \times ln(Oxy\_Supply_s^{2010})$	−0.014	−0.032	−0.032	0.002
	(0.032)	(0.051)	(0.035)	(0.035)
$\mathbb{1}_{2011} \times ln(Oxy\_Supply_s^{2010})$	−0.006	−0.089	−0.037	−0.013
	(0.048)	(0.056)	(0.048)	(0.048)
$\mathbb{1}_{2012} \times ln(Oxy\_Supply_s^{2010})$	0.009	−0.054	−0.004	0.012
	(0.041)	(0.055)	(0.043)	(0.043)
$\mathbb{1}_{2013} \times ln(Oxy\_Supply_s^{2010})$	−0.007	−0.082	−0.019	0.002
	(0.038)	(0.061)	(0.034)	(0.034)
$\mathbb{1}_{2014} \times ln(Oxy\_Supply_s^{2010})$	0.018	−0.071	0.020	0.022
	(0.037)	(0.065)	(0.041)	(0.041)
$\mathbb{1}_{2015} \times ln(Oxy\_Supply_s^{2010})$	0.046	−0.022	0.057	0.007
	(0.043)	(0.065)	(0.048)	(0.048)
$\mathbb{1}_{2016} \times ln(Oxy\_Supply_s^{2010})$	0.055	−0.014	0.061	0.032
	(0.059)	(0.056)	(0.060)	(0.060)
Year Fixed–Effects	Yes	Yes	Yes	Yes
State Fixed–Effects	Yes	Yes	Yes	Yes
Observations	561	561	561	561

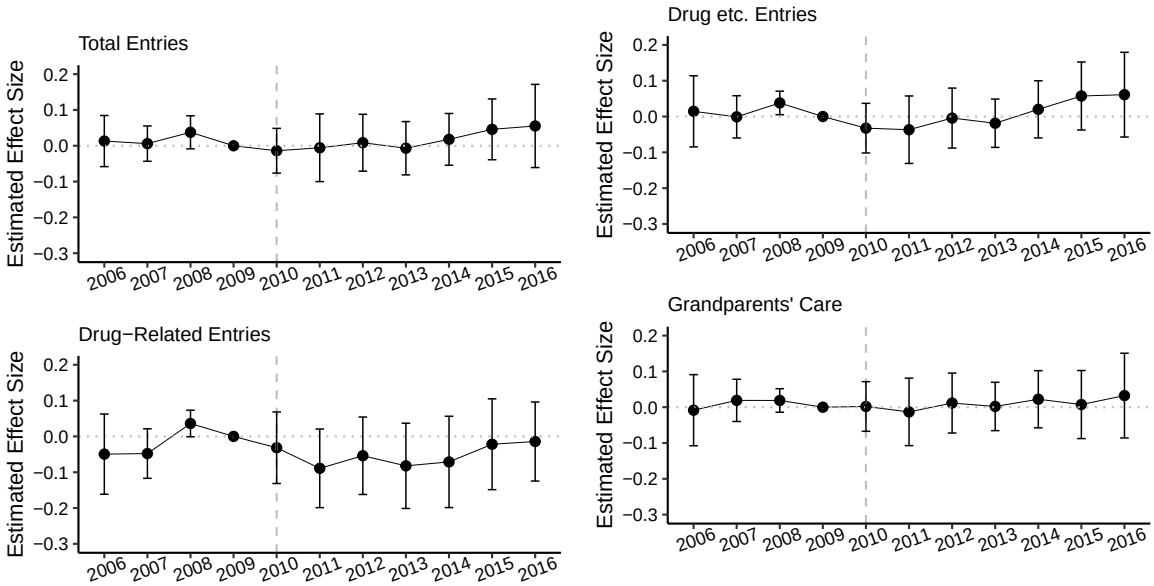
Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from Quasi-Poisson regressions with standard errors clustered at the state-level are reported in parentheses. The unit of observation are state-year cells from fiscal year 2006 to 2016. All estimates control for year and state fixed-effects and the following demographic population shares: share of the population age less than 2, 2-5, 6-11, and 12-17 years old, non-Hispanic black, non-Hispanic other race and Hispanic, female, parent's with a high school degree, parent's with a some college or associates degree, parent's with a bachelors degree or more, living in single female households, and living below 100% of the poverty line. Also included are the average household income, mean parent's age, parent's unemployment rates, whether a state PDMP program was active, average receipt from state SNAP and TANF programs for a family of three, and the natural log of the total population. Omitted categorical variables were the share of the non-Hispanic white, males, and mother's education less than high school education.

Figure 2.8: State Analysis: Fixed Effects Model Estimates of δ_t

Panel A: Adult Substance Abuse Treatment Admissions



Panel B: Child Out-of-Home Placements



Note: Each graph includes 95 percent confidence intervals using standard errors clustered at the state-level.

Chapter 3

Removing Barriers to Parenthood: The Effect of Legalizing Same-Sex Marriage on Childrearing

3.1 Introduction

Marriage provides a legal social contract intended to improve economic stability and childcare within households (Becker, 1973). While marriage in the US has historically pertained solely to the civil union of one man and one woman, the legal right to marry for same-sex couples has dramatically changed within the last decade. In January 2013, the Supreme Court decision in *United States v. Windsor* found restrictions on same-sex marriage under Section 3 of the Defense of Marriage Act (DOMA) unconstitutional by the Due Process Clause of the Fifth Amendment. In 2014, the Internal Revenue Service (IRS) began recognizing all legally married same-sex couples eligible for federal marital tax benefits (IRS, 2013). The subsequent federal ruling in *Obergefell v. Hodges* (2015) required all that US states legally recognize marriage equally for same-sex and heterosexual couples and may not deny marriage licenses to same-sex couples.

This study examines whether marriage legalization has influenced child rearing decisions among same-sex couples. Legal recognition of same-sex marital rights could influence same-sex couples' decision to raise a child for several reasons. Marriage equality allows same-sex couples access to social safety net programs which assist in raising a child and increase household economic security. Married couples and their families receive more than 1,138 benefits such as health and disability insurance, retirement (social security), employment, veterans, and military benefits as well as preferential treatment for food stamps, welfare, housing and credit access, rights to citizenry, Medicare, Medicaid, and federal tax benefits (GAO, 2004).

Constitutional marriage rights also provide greater political stability and reduce uncertainty related to legal guardianship and presumption of parentage for same-sex couples' children (Goldberg & Conron, 2018). Married same-sex couples may also face reduce discrimination in markets for adoption or foster care which makes parenting more accessible. This may be particularly important to same-sex couples since they are more likely than heterosexual couples to adopt or foster a child (Taylor, 2020). In general, legal marriage equality signals greater social acceptance of same-sex relationships which may influence same-sex couples' decision to raise a child.

In this study I use state-level variation in the timing of same-sex marriage legalization across the US to estimate the effect of same-sex marriage legalization on child rearing decisions among same-sex households. Using a naive OLS difference-in-differences strategy I find a significant effect on the

overall likelihood that female same-sex households were raising a child (1.6 percentage points [pp])) following passage of same-sex marriage legalization. I also find significant increases in the likelihood of child adoption (0.06pp) by same-sex households which is largely driven by male same-sex households (0.10pp).

Since same-sex marriage was legalized at different times over the study period, this study's main results correct for staggered treatment timing using the Callaway & Sant'anna method (Callaway & Sant'Anna, 2021). After correcting for staggered treatment (legalization of same-sex marriage) across states, I find an increasing trend in the overall likelihood same-sex are parenting any child or adopting a child. However, I only find a significant effect of same-sex marriage legalization on the likelihood of same-sex households fostering a child. These results are suggestive that although marriage equality laws have not increased overall rates of parenthood among same-sex couples in the US, same-sex couples have increasingly chosen foster a child following same-sex marriage legalization. Increased rates of foster parenting may be due to a combination of reduced discrimination against same-sex couples with children and lower rates of biological children from prior heterosexual relationships among younger same-sex cohorts causing greater access and demand for child welfare placement services.

This study makes two contributions to the economic literature. First, while numerous economic studies have examined the demographics of the LGBT community and the effect of discriminatory practices on their socio-economic well-being within public and private markets (M. Badgett et al., 2021), to the author's knowledge, no empirical study has examined the effect of legal marriage equality in the US on same-sex couples' decision to parent a child with particular focus on same-sex couples' use of alternative options for children (adoption, foster care). Related work have found that foster care agencies are often less receptive to same-sex male couples seeking a child, particularly among religious organizations (Mackenzie-Liu et al., 2020, 2022). These studies provide evidence of discrimination against same-sex couples attempting to foster a child using an email-based audit study. However, they do not examine whether these practices affected same-sex couples' decision to foster or adopt a child and do not address whether same-sex marriage equality has affected these choices over time.

Second, this study provides a general result on how more inclusive market access can increase rates of child fostering and adoption. Prior economic studies have found that same-sex marriage

legalization has increased marriage take-up, health insurance coverage, access to care, and healthcare utilization for adult gay men (Carpenter et al., 2021). This study shows suggestive evidence that more inclusive access to the child welfare placement market by same-sex couples may increase child placement rates with caregivers.

3.2 Background

Although there are well-documented privacy and instrument validity concerns regarding survey response on sexual orientation, its estimated that 5.6% of the US population identify as LGBT (M. Badgett et al., 2021; D. A. Black et al., 2007; Gallup, 2021).[1] While many same-sex couples reside in domestic partnerships, the judicial basis to historically deny same-sex couples legal marital status in the US relied on the interpretation of marriage as 'one man one woman' and the questionable ability of same-sex couples to raise children. In the first US legal challenge to same-sex marriage bans, *Baker v. Nelson 1970 191 N.W.2d 185 (1971)*, a same-sex couple applied for and were denied a marriage license in Minnesota. When appealed to the state Supreme Court, it affirmed the lower court decision citing "The institution of marriage as a union of man and woman, uniquely involving the procreation and rearing of children within a family, is as old as the book of Genesis." (Justia, 1971).

In 1973, Maryland became the first state to explicitly ban same-sex marriage by including within its Family Law Code, "Only a marriage between a man and a woman is valid in this State." (ProCon.org, 2016). In the 1990s, while some states began to legally recognize same-sex domestic partnerships or civil unions, others enacted statures explicitly restricting legal marital status to heterosexual couples. In 1995, Utah became the first state to enact a Defense of Marriage Act (DOMA), which stated it did not have to legally recognize out-of-state same-sex marriages and banned same-sex unions in the state (New York Times, 1995). In 1996, the Clinton Administration enacted the federal DOMA which legally defined marriage as "a legal union between one man and one woman as husband and wife" (PEW, 2015). Under the federal DOMA, same-sex couples were denied 1,049 federal benefits, rights, and privileges contingent on marital status (GAO, 1997).[2]

[1] Work using US Census data has found potential over-counts of same-sex couples due to small sample size and potential survey instrument errors (D. Black et al., 2000, 2006).

[2] As of September 21, 1996, the date DOMA was signed into law. As of December 31, 2003, 1,138 federal statutory provisions used marital status in determining or receiving benefits, rights, and privileges (GAO, 2004).

Following passage of the federal DOMA, legal challenges to same-sex marriage bans argued that DOMA violate state constitutions' equal protection and due process clause. In 2004, Massachusetts became the first state to legalize same-sex marriage following *Goodridge v. Department of Public Health*, in which the Supreme Judicial Court ruled that denying marriage rights to same-sex couples violated the Massachusetts Constitution (Gates, 2015).[3] In response to Massachusetts' ruling, several additional states enacted constitutional amendments prohibiting same-sex marriage.

Since DOMA was enacted, public acceptance of same-sex relationships in the US has dramatically increased. At the time of the federal DOMA's enactment in 1996 only 27% of the US public polled by Gallop responded that gay marriage should be legal, however by 2011 53% of respondents stated it should be legal (Gallup, 2023; Newport, 2012). On February 23, 2011, the Obama Administration declared the federal DOMA unconstitutional and directed the Justice Department to cease defending the DOMA in court (Savage & Stolberg, 2011). In 2013, the federal Supreme Court ruling in *United States v. Windsor* found restrictions for same-sex marriage as defined under Section 3 of the Defense of Marriage Act (DOMA) unconstitutional by the Due Process Clause of the Fifth Amendment (CNN, 2020).[4] As a result, the Internal Revenue Service (IRS) announced that starting in 2014 it would recognize all legally married same-sex couples for federal spousal tax benefits irregardless of whether they lived in a state that still did not recognize same-sex marriage at the time (IRS, 2013; PEW, 2015). In 2015, the Supreme Court ruling in *Obergefell v. Hodges* subsequently required that all states issue marriage licenses to same-sex couples and recognize the legal rights of married same-sex couples equally to those of married heterosexual couples under the Constitution specifically citing the right to safeguard children and families (Gates, 2015).[5]

Apart from additional spousal benefits, legal marital status is particularly important for minor dependents of same-sex couples since one or both parents are often not biologically-related to the child. To avoid discriminatory practices by child welfare service single-parent legal guardianship of adopted or foster children is commonly done by same-sex couples (Bradley, 2007; T. Washington, 2014; T. M. Washington, 2008). However since legal presumption of parentage is not always granted to an unmarried partner potential guardianship issues may occur if the legal caregiver is unable to

[3]Evan Wolfson developed the argument that denial of access to marriage was state-sponsored discrimination against the gay population in his thesis on the freedom to marry (Freedom to Marry, 2016).
[4]United States v. Windsor - Supreme Court
[5]In the majority opinion, Justice Kennedy stated, *"Four principles and traditions demonstrate that the reasons marriage is fundamental under the Constitution apply with equal force to same-sex couples... A third basis for protecting the right to marry is that it safeguards children and families..."*[6]

raise the child. In the most severe case, a child may be removed from the home if the legal guardian is deceased, disabled, or otherwise found unfit to raise the child even if their partner was capable of raising the child. Although two-thirds of children raised by same-sex couples are biologically-related, same-sex couples are seven times more likely than heterosexual couples to adopt or foster a child (Taylor, 2020).[7]

While legalization of same-sex marriage signals a major shift in public opinion regarding same-sex relationships in the US, homosexual individuals still face discrimination due to sexual orientation (M. Badgett et al., 2021; D. A. Black et al., 2007). Prior economic studies have found discriminatory practices against homosexuals in housing access (Ahmed & Hammarstedt, 2009; Levy et al., 2017; Schwegman, 2019), wages and employment (M. L. Badgett, 1995; Burn, 2020; Carpenter & Eppink, 2017), and disparities in household health insurance coverage (M. L. Badgett et al., 2008; Gonzales & Blewett, 2013, 2014). Therefore for same-sex couples unable to have biological children, its unclear whether their decision to raise a child would be significantly affected by marriage legalization due to discriminatory adoption or foster child placement services. For example, same-sex couples can still be denied the right to adopt a child from publicly-funded private adoption agencies if placement with a same-sex couple is against the organization's religious or political view. Recent work has found that although foster care agencies responded at similar rates to same-sex and heterosexual couples, responses sent to same-sex male couples were shorter, included less information about the process to become a foster parent, and took longer to receive particularly among religious organizations (Mackenzie-Liu et al., 2020, 2022). Although prior economic studies have found that same-sex marriage legalization has reduced hate crimes against homosexual individuals (Pettis et al., 2022), and increased marriage take-up, health insurance coverage, access to care, and healthcare utilization for adult gay men (Carpenter et al., 2021), no economic study to the author's knowledge has examined the effect of same-sex marriage legalization on household child rearing decisions.

From a theoretical perspective, marriage signals greater commitment to household stability and creates economies of scale in home production (Becker, 1973). Laws protecting same-sex marriage are intended to reduce disparities in same-sex couples' access to the marital benefits, rights, and privileges which improve household welfare. Legal precedence also signals greater state protection of same-sex couples' civil rights and public acceptance of same-sex relationships, decreasing the likeli-

[7]It is estimated that 14.7% of same-sex couples are raising over 290,000 children in the US (Taylor, 2020)

hood of future judicial overrulings (Aksoy et al., 2020). Therefore if same-sex marriage legalization allows equal access to rights and benefits intended to support child rearing, more same-sex couple households may decide to raise children given reduced social risks and stigma. On the other hand, legalization of same-sex marriage may have little effect on child rearing decisions due to both existing discriminatory practices against same-sex couples as well as the homosexual population's younger demographic who often identify as homosexual at earlier ages and are therefore less likely to have children. Along with child adoption services, demand for substitute markets such as artificial reproductive technology is also growing among same-sex couples (Gates, 2015). Therefore it is ambiguous whether same-sex marriage legalization would significantly influence same-sex couples' decision to raise a biological or adopted/foster child.

3.3 Data and Descriptive Statistics

The main data for the analysis was collected from the annual American Community Survey (IPUMS-ACS) at the household-level of observation for the period 2008 to 2019. I constrain the ACS sample to only married heterosexual and same-sex households and define three household types: heterosexual, male same-sex, and female same-sex households.[8] In order to focus on households who would have completed schooling and were within the age cohort most likely to consider raising a new child, the sample includes only households where both the reference person and their partner are between age 25 and 45.

Since same-sex marriage was only recently legalized at the federal-level, the ACS did not directly collect information on whether households were same-sex couples until 2013 when a question was added to specifically identify married same-sex couples. The ACS did not identify married or unmarried same-sex couples prior to 2013. However, it is possible to indirectly identify same-sex households in the ACS prior to 2013 using information on household members' gender and their relation to the household reference person. To identify same-sex couples in the ACS for all years in this study, I infer whether households are heterosexual or same-sex using only information on the gender of the household reference person and their cohabitating spouse or unmarried partner. To determine

[8]The analytic sample is first conditionally selected to include only [1] Married-couple family household: HHTYPE==1 and [2] Same-sex couple households whose household type could not be determined" (HHTYPE==9) as defined by the Census Bureau.

the presence of a spouse or partner in the household, I use the IPUMS-constructed spousal linkage variable SPLOC. Compared to the original ACS survey responses on household member's relation to the reference person, the spousal linkage identifier uses logical edits to improve the quality of relationship linkages using additional household demographics.

Importantly, this study uses the IPUMS spousal linkage identifier because it was revised in 2017 to improve and harmonize the linkage of household reference persons' to their spouse or cohabitating partner, particularly unmarried couples, in the ACS before and after the addition of the same-sex marriage question in 2013. 4-5% of spousal relationship were revised using this method.[9] I use information on the relationship of persons less than 18 to the household reference person and their spouse/unmarried partner to determine whether either were raising a child (biological, adopted, step-child, child-in-law, or foster) in the household. A timeline of changes to the ACS related to same-sex couples and the spousal linkage variable constructed by IPUMS is shown in Table 3.1.

There are several potential limitations to this classification method. One is survey-based misclassification. Heterosexual households may be incorrectly classified as same-sex (or vice versa) because of survey response error for gender and relationship status information. Due to high rates of accidentally misreporting multiple genders by respondents, in 2008 the ACS's gender question underwent a major survey redesign which allowed respondents to only select one gender and relationship to the household reference person.[10] Using spousal linkages to define intra-household relationships, IPUMS found a dramatic decrease in estimated rates of same-sex couples beginning in 2008.[11] To minimize any estimation bias due to survey design-based differences in the ACS prior to the redesign of gender question in 2008, ACS survey years 2000-2007 are not included in the sample.

A second limitation is that this classifaction strategy does not differentiate same-sex households based on marital status. Although same-sex couples may have been legally married in certain states prior to 2013, the spousal linkage identifier does not differentiate married from unmarried same-sex couples before 2013 because limited information is collected to differentiate civil unions from marriage for the ACS. Therefore estimates from this study should be interpreted as the intent to

[9] https://usa.ipums.org/usa/chapter5/NewfamilyinterrelationshipvariablesinIPUMSUSA.shtml

[10] "Changes to the American Community Survey between 2007 and 2008 and their Potential Effect on the Estimates of Same-Sex Couple Households". https://www2.census.gov/topics/families/same-sex-couples/guidance/changes-to-acs-2007-to-2008.pdf

[11] "Frequently Asked Questions About Same-Sex Couple Households". https://www2.census.gov/topics/families/same-sex-couples/faq/sscplfactsheet-final.pdf

treat effect of same-sex legalization.

Since marital status is not used to differentiate same-sex households, this classification method estimates higher rates of same-sex couples than official Census Bureau tabulations for married same-sex couples. Figure 3.1 compares rates of same-sex household respondents estimated in the ACS from 2008-2019 using the official Census measure (Census) and the IPUMS method using spousal linkage identifiers (IPUMS) described above. Using the IPUMS method, I find 2-3 times higher rates of same-sex households in the ACS relative to official Census estimates. These differences are due to the inclusion of unmarried same-sex households.[12] Although estimated rates of same-sex households using this method are higher than Census estimates from 2013-2019, the trend over time is consistent between Census estimates and the IPUMS method.

Consequently, if unmarried couples are less likely to have children relative to married couples, the IPUMS household classification method would then also likely estimate lower rates of parenthood than official Census estimates which only account for married same-sex couples. Figures 3.2 and 3.3 show rates of same-sex households raising a child in the ACS by gender of same-sex couples using official Census estimates and the IPUMS method. I find lower rates of parenting for both male and female same-sex couples using the IPUMS methods relative to official Census estimates.

State-level information on same-sex marriage legalization enactment dates was collected and harmonized from the Pew Research Center, the National Council of State Legislatures, and the Movement Advancement Project. For the purpose of this study, a state is considered "treated" if that state legalized same-sex marriage or post-2015 when same-sex marriage was federally recognized. Figure 3.4 shows a timeline of state counts which enacted same-sex marriage laws beginning in 2004 with Massachusetts. From 2005-2012, less than 10 states had enacted same-sex marriage laws. Beginning in 2013, a number of states began to enact same-sex marriage legalization laws with the ruling of *United States v. Windsor*. All states were federally mandated to legally recognize same-sex marriage with the 2015 federal Supreme Count ruling in *Obergefell v. Hodges*.[13]

Descriptive statistics for the sample by pre- and post-enactment of same-sex marriage legalization are shown in Table 3.2 by the sexual orientation of the head of household. For the overall sample of same-sex and heterosexual couples there is a decrease in the proportion of households with any child

[12] https://usa.ipums.org/usa/chapter5/NewfamilyinterrelationshipvariablesinIPUMSUSA.shtml
[13] The list of same-sex marriage law enactment dates by state can be found in the Appendix Table 5.6

present comparing the pre- to post-SSML period. Heterosexual households are more than twice as likely to have a child in household compared to same-sex couples. Relative to heterosexual married couples, the proportion of same-sex households with an adopted or foster child is larger and has increased post-SSML. Although the racial composition is similar across same-sex and heterosexual households in the sample, same-sex households on average are younger, more educated, more often live in metro areas, and have lower total household income. Same-sex households were 42 percentage points [pp] more likely to report being married post-enactment of same-sex marriage legalization. Table 3.3 shows descriptive statistics by pre- and post-period for same-sex households split by gender. Comparing same-sex households by gender, male same-sex couples are less likely to have any type of child relative to female same-sex couples. Relative to the pre- period, both male and female same-sex sample populations have a smaller proportion of have any child post-SSML, but both male and female same-sex households have increasingly fostered a child following the enactment of same-sex marriage legalization.[14]

3.4 Empirical Framework and Results

To assess whether the enactment of same-sex marriage equality laws in the US has increased the likelihood of same-sex households raising a child, I use a difference-in-differences approach to examine three outcomes: whether any child resided in the household, whether an adopted child resided in the household, and whether a foster child resided in the household. I define three different treatment populations relative to heterosexual households – the full sample of same-sex households, only male same-sex households, and only female same-sex households. As a first step, I estimate the effect of same-sex marriage legalization on parenting decisions with ordinary least squares (OLS) using the model:

$$Y_{ist} = \beta_0 + \delta_0 SameSex_{ist} + \delta_1 Post_{st} + \delta_2 (Post_{st} \times SameSex_{ist}) + \beta X_{ist} + \alpha_s + \tau_t + \epsilon_{ist} \quad (3.1)$$

where Y_{ist} are the outcomes of interest, α_i and τ_t are geographic and year fixed-effects, respectively, and X_{it} is the vector of demographic controls. I control for household characteristics associated with

[14] Although some demographic characteristics differ by several percentage points between pre- and post-SSML sample periods, these differences are due to states "switching" from pre- to post-SSML status and change sensible when calculated from year to year.

child rearing or gay/lesbian demographics such as the household reference person's age and race as well as total family income and metro status. Here t subscripts the nominal year ($t = 2006, ..., 2018$), $SameSex_{ist}$ is an indicator for whether it was a same-sex household, and $Post_{st}$ is an indicator for whether state s enacted a same-sex marriage law or post-2015.

However, estimation of (3.1) using OLS unconditionally compares all treated and untreated state pairs to estimate the average treatment effect on the treated without accounting for heterogeneity in treatment timing. It cannot account for potential selection bias in legalizing same-sex marriage due to states' shifting social norms or legal interpretations and differential treatment effects on parenting decisions by early versus late treatment states. By comparing outcomes between households in newly treated states and already treated states to estimate the average treatment effect of marriage legalization on same-sex couples' parenting decisions, OLS also does not correctly identify the causal effect of same-sex marriage legalization due to treatment dynamics.

Another potential issue with OLS estimation is that it does not account for potential differences in effect intensity over the post-treatment time period. For example, there may be a behavioral lag between couples' decision to raise a child and completion of the process to become a parent following legalization since the process to foster or adopt a child can take a significant amount of time to complete once initiated. Younger cohorts with more exposure to same-sex marriage legalization who enter the sample in later years may also be more tolerant of changing social norms and same-sex couples parenting children. In both cases, it would be expected that the effect of same-sex marriage legalization would increase the longer same-sex marriage was legal and among younger cohorts in later ACS years.

To identify the causal effect using a difference-in-difference strategy correcting for variation in treatment timing and intensity, I estimate sets of treatment effects across groups of treated and not-yet treated cohorts defined by the year of treatment and weight these estimates to obtain a sample average treatment effect on the treated per Callaway and Sant'Anna (2021). To correct for staggered treatment, states are grouped into cohorts assigned by the year a state legalized same-sex marriage. All persons in states which legalized same-sex marriage in that year are grouped together as a treatment group. All persons in states which have not yet legalization same-sex marriages are defined as the not yet treated group. The average treatment effect on the treated (ATT) is estimated for each group, G_g, by taking the mean difference in outcome between treated, Y^1, and

not-yet treated cohorts in the group, Y^0, for each time period, t, indexed by the group's treatment period:

$$ATT_{g,t} = E[Y_t^1 - Y_t^0 | G_g = 1] \tag{3.2}$$

To obtain the sample average treatment effect on the treated ($SATT$), the weighted average across all group cohorts, g, is estimated using a generalized propensity score based on the probability of an observation being included for each group cohort comparison used to estimate the sample average treatment effect on the treated. If the conditional parallel trends assumption holds for all group cohort treatment estimates between treated and not-yet treated groups, then the causal sample average treatment effect on the treated of same-sex marriage legalization is obtained using the weighted average of group cohort treatment estimates. The main results use a doubly robust difference-in-difference estimator with augmented inverse probability weight to correct estimates from equation (3.1) for staggered treatment (Callaway & Sant'Anna, 2021). Standard errors are wild bootstrapped and clustered at the state-level.

3.5 Results

To compare estimates between the main results using the difference-in-differences strategy for staggered treatment per Callaway and Sant'Anna (2021) and results for the naive difference-in-differences estimation strategy using OLS, I first present results using the naive OLS strategy in Tables 3.4 to 3.6. Table 3.4 shows results for whether same-sex couples were more likely to be raising a child in household following passage of same-sex marriage legalization. For the overall sample (male and female) I find that same-sex marriage legalization had no significant effect on the likelihood of any child residing the household. However, I do find a significant positive effect for the female same-sex sample (0.016) or female same-sex households were 1.6 percentage points [pp] more likely to raise any child in the household at the 10% significance level post-legalization of same-sex marriage.

Tables 3.5 and 3.6 shows the results for whether same-sex couples were more likely to be raising an adopted child or foster child in the household following passage of same-sex marriage legalization using the naive OLS estimator. I find significant effects of same-sex marriage legalization on likelihood of an adopted child resided in the household for the overall sample of same-sex household (0.006) and

male same-sex household (0.010) treatment groups. Interpreting the coefficient for the overall sample of same-sex households, same-sex households were 0.6 pp more likely to raise an adopted child in the household at the 10% significance level following passage of same-sex marriage legalization. I find no significant effect of same-sex marriage legalization on the likelihood of a foster child residing the household relative to adopted children for either female (0.001) or male (0.001) same-sex households as shown in Table 3.6.

Correcting for staggered treatment timing, the main results are estimated per Callaway and Sant'Anna (2021) with inverse augmented probability weights and wild bootstrapped clustered standard errors. Results are presented as group-time averaged dynamic effects for five periods before and after treatment intervention ($I_{t=-5}$ to $I_{t=5}$) in Tables 3.7 to 3.9. The first row of each table, "Pre-treatment Average", shows results for the joint hypothesis test of significant pre-treatment effects averaged across group cohorts by time until treatment. The second row, "Post-treatment Average", is the joint hypothesis test of statistical significant post-treatment effects across group cohorts since treatment. Group-time average treatment effects on the treated are also shown graphically as dynamic effect plots with 95% confidence intervals.

The effect of same-sex marriage legalization on same-sex couples parenting any child in the household are shown in Table 3.7 and Figures 3.5 to 3.7 show estimates with 95% confidence intervals. Table 3.7 shows that pre- and post-treatment effects were found to not be statistically significant using the Callaway & Sant'anna estimator at the 5% significance level. The corrected estimates are smaller relative to the OLS results (Table 3.4) and not statistically significant. Examining the sample-averaged dynamic effects in Figures 3.5 to 3.7, I find an increasing trend in same-sex households parenting a child over post-treatment period but these effects are not statistically significant. Similarly, estimates for any adopted child in the household are shown in Table 3.8 and Figures 3.8 to 3.10 show dynamic effect estimates with 95% confidence intervals. Estimated effects using the Callaway & Sant'anna correction are again smaller than OLS results (Table 3.5) and increasing over the post-treatment study period. However they are not statistically significant at the 5% significance level as shown in Figures 3.8 to 3.10.

Results for any foster child in the household are shown in Table 3.9 and Figures 3.11 to 3.13 show estimates with 95% confidence intervals. Relative to the OLS results (Table 3.6), shows the Callaway & Sant'anna correction method again estimates smaller effects. I find no evidence of any significant

pre-treatment effects. I do find that the treatment effect averaged across group cohorts was significant five years following the legalization of same-sex marriage ($I_{t=5}$). This effect is positive, significant at the 5% significance level, and appears to be largely driven by male same-sex households choosing to foster a child. Interpreting the estimate $I_{t=5}$ for the overall sample (column 1), on average same-sex couples were .16 pp more likely to foster a child five years following same-sex marriage legalization at the 5% significance level. However, I find no significant effect on same-sex households raising a foster child when averaging dynamic treatment effects over the post-treatment period as shown in the second row of Table 3.9.

3.6 Conclusion

This study examined whether same-sex couples are more likely to parent a child following the passage of same-sex marriage legalization. Using a difference-in-differences strategy and correcting for staggered treatment timing per Callaway and Sant'Anna (2021), I find an increasing trend in the overall likelihood same-sex are parenting any child or adopting a child, but only find a significant effect of same-sex marriage legalization on the likelihood of same-sex households fostering a child. These estimates are relatively small in magnitude to estimated those using the naive OLS-based difference-in-differences strategy. These results are suggestive evidence that although same-sex marriage legalization has not significantly increased overall rates of parenthood among same-sex couples, more same-sex couples have chosen to foster a child. This could potentially be due to greater access of child placement services following legal marriage equality as well as increasing demand by same-sex couples to parent.

One potential issue with estimating the short-term effect of marriage legalization on same-sex couples' decision to raise a child is the behavioral lag between couples' decision to raise a child and completion of the process to become a parent. Over time as younger cohorts with more exposure to same-sex marriage access may be more adaptable to changing social norms and tolerant of non-traditional parenting among same-sex couples. Therefore, it is possible to expect that same-sex marriage legalization may create greater future demand for child placement services.

There are also several study limitations. First, these results rely on a sample defined using an

imputation method to identify same-sex marital status since federal statistics were not collected specifically on sexual orientation and martial status throughout the entire study period which may affect internal validity of the study's results. Another potential measurement issue is non-response error among same-sex couples. Non-response among same-sex couples less willing to identify their sexual orientation within the ACS or decline to complete the survey due to privacy concerns could cause error in extrapolating these results to the general population due to bias defining the treatment population. There is also potential selection bias and measurement error due to unobservable state characteristics such as shifting social norms which proceeded same-sex marriage legalization and cannot be distinguished separately from the effect of marriage legalization on same-sex couples' decision to raise a child.

From a child welfare policy perspective, this study cannot directly distinguish whether households adopted a child via domestic or foreign placement due to data limitations. While same-sex couples are to be more likely to foster or adopt a child following legal marriage equality, its not evident whether greater rates of adoption by same-sex couples creates more opportunities for children in the US public child welfare system.

Future research should both improve upon identification of same-sex households and marital status using linked household IRS tax return information and further investigate same-sex couples interactions with the US public child welfare system in order to assess discriminatory practices as well as same-sex couples' demand to adopt or foster a child and the effect that has on child placement rates. Related research could use follow-up surveys and federal statistical records to better assess later life outcomes for foster/adopted children placed with both heterosexual and same-sex couples.

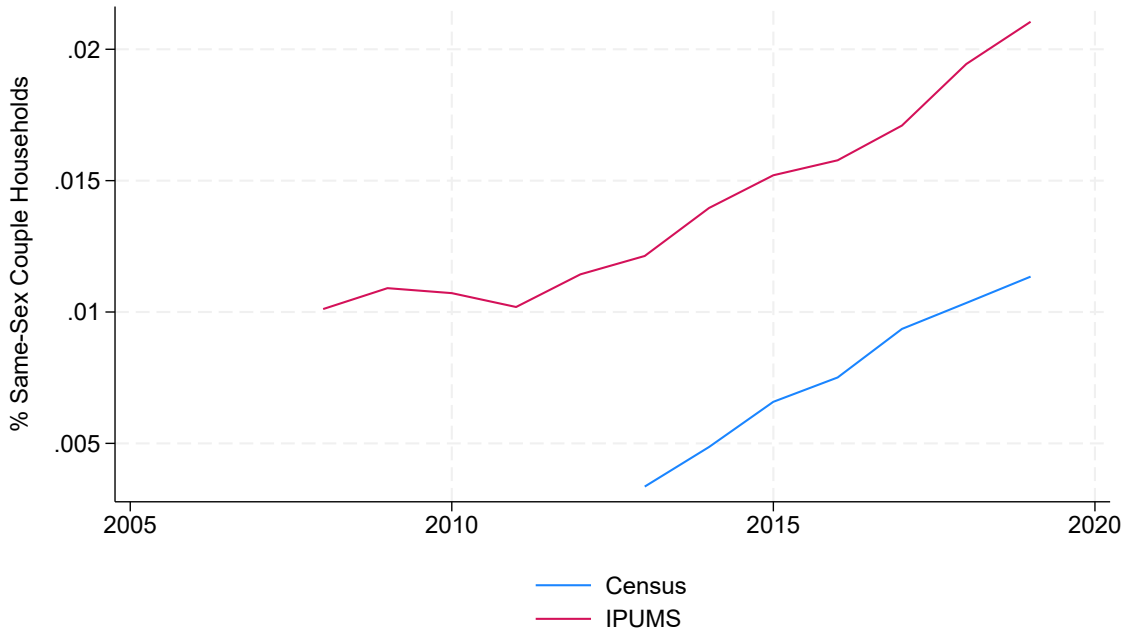
3.7 Figures and Tables

Table 3.1: Timeline of Changes Related to Same-Sex Households in the ACS

First ACS is Collected	2000
ACS Gender Question Redesigned to Reduce Misreporting	2008
Same-Sex Married Couple (SSMC) Question Added to ACS	2013
Same-Sex Marriage Federally Legal	2015
IPUMS Revises and Harmonizes ACS Spousal Linkages	2017

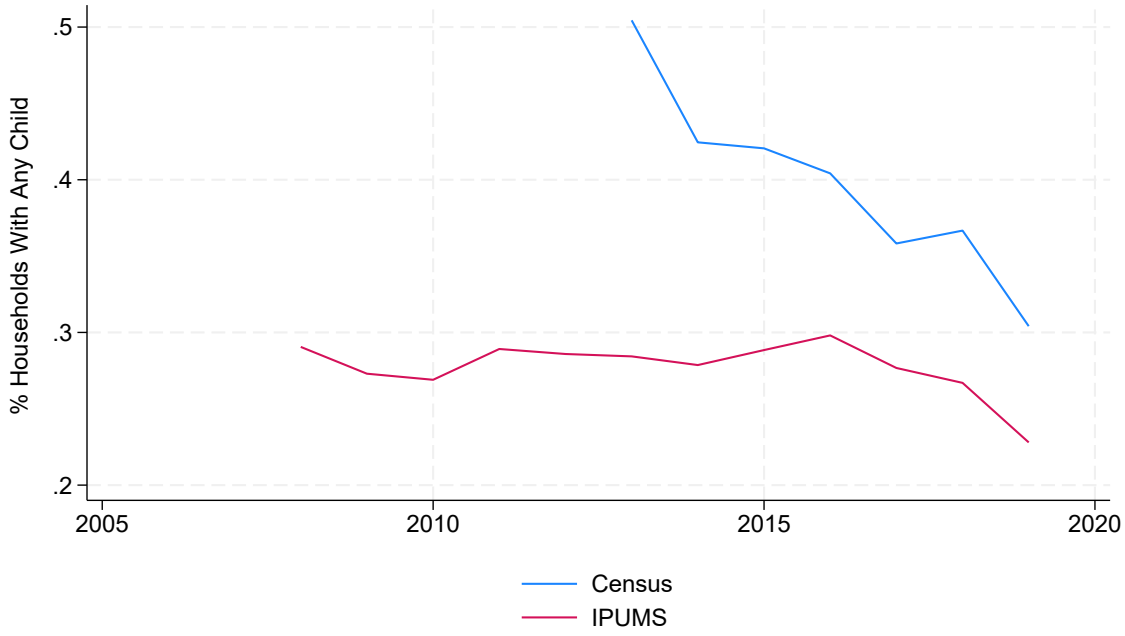
Notes: This table provides a timeline of events related same-sex marriage legalization and the measurement of same-sex households in the American Community Survey from 2000-2017.

Figure 3.1: Same-Sex Household Identification



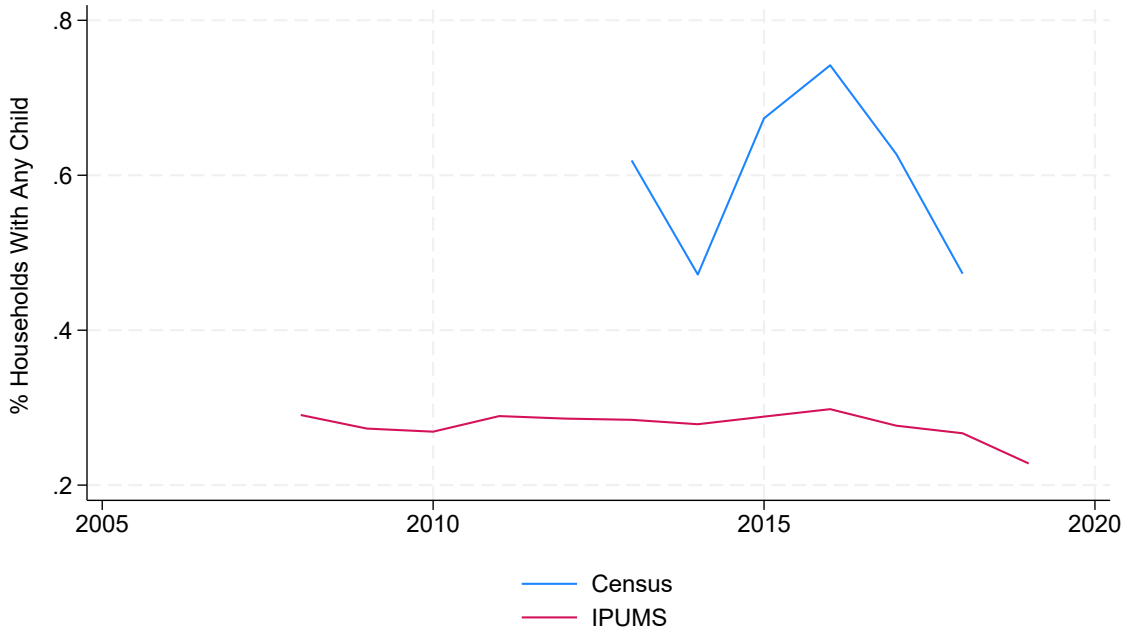
Notes: This figure compares rates of same-sex households classified in the American Community Survey from 2008-2019. "Census" are rates of married same-sex households classified using the official Census survey instrument on married same-sex couples. "IPUMS" are rates of same-sex households, married and unmarried cohabitating partners, classified using IPUMS method which relies on intra-household relationships. Estimates for the "Census" series are unavailable pre-2013.

Figure 3.2: Any Child in Household: Male Same-Sex



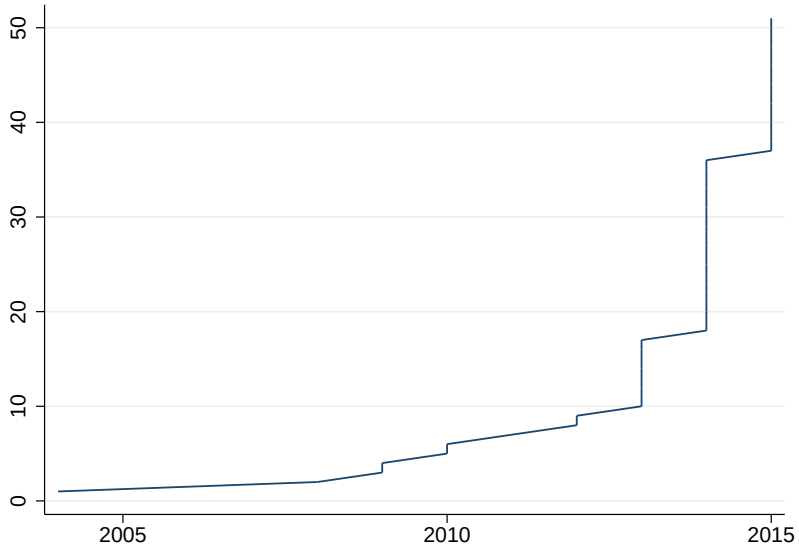
Notes: This figure compares rates of male same-sex households which are raising a child as classified in the American Community Survey from 2008-2019. "Census" are rates of married same-sex households classified using the official Census survey instrument on married same-sex couples. "IPUMS" are rates of same-sex households, married and unmarried cohabitating partners, classified using IPUMS method which relies on intra-household relationships. Estimates for the "Census" series are unavailable pre-2013.

Figure 3.3: Any Child in Household: Female Same-Sex



Notes: This figure compares rates of female same-sex households which are raising a child as classified in the American Community Survey from 2008-2019. "Census" are rates of married same-sex households classified using the official Census survey instrument on married same-sex couples. "IPUMS" are rates of same-sex households, married and unmarried cohabitating partners, classified using IPUMS method which relies on intra-household relationships. Estimates for the "Census" series are unavailable pre-2013.

Figure 3.4: Count of States(+DC) with Same-Sex Marriage Laws, 2004-2015



Notes: This figure shows a chronological timeline for the number of states which legalized same-sex marriage from 2004-2015. In 2015 same-sex marriage was legalized at the federal level.

Table 3.2: Household Characteristics of Same-Sex and Heterosexual Households

	Same-Sex		Heterosexual	
	Pre-SSML	Post-SSML	Pre-SSML	Post-SSML
Child residing in household	0.2874	0.2677	0.8031	0.7871
Adopted child in HH	0.0408	0.0414	0.0199	0.0157
Foster child in HH	0.0057	0.0070	0.0021	0.0023
Married	0.0428	0.4655	1.0000	1.0000
Age	35.6923	34.6442	36.0546	36.0855
Race: White	0.8185	0.7701	0.7908	0.7665
Race: Non-white	0.1815	0.2299	0.2092	0.2335
Hispanic	0.1348	0.1668	0.1726	0.1783
Less than HS ed.	0.0491	0.0369	0.0909	0.0731
High School ed.	0.1315	0.1264	0.1987	0.1659
Some College	0.3227	0.3067	0.3111	0.2841
Bachelor's Degree	0.2929	0.2945	0.2524	0.2824
Advanced Degree	0.2038	0.2355	0.1470	0.1944
Total family income (000's - 2010$)	57.1080	80.7451	90.4131	102.1699
Live in metro area	0.8740	0.8831	0.7816	0.8190
Observations	11004	19165	998342	1065673

Notes: Sample includes married heterosexual households as well as both married and unmarried cohabitating same-sex households where the household reference person and their spouse/parent are both between the age 25-45. Information on state same-sex marriage legalization was collected and harmonized from the Pew Research Center, the National Council of State Legislatures and the Movement Advancement Project. Population demographics were obtained from the 1-year American Community Survey (ACS).

Table 3.3: Household Characteristics of Same-Sex Households by Gender

	Male		Female	
	Pre-SSML	Post-SSML	Pre-SSML	Post-SSML
Child residing in household	0.1810	0.1409	0.3825	0.3748
Adopted child in HH	0.0234	0.0277	0.0564	0.0530
Foster child in HH	0.0041	0.0054	0.0072	0.0084
Married	0.0410	0.4274	0.0444	0.4976
Age	36.1138	35.0666	35.3158	34.2874
Race: White	0.8319	0.7936	0.8065	0.7503
Race: Non-white	0.1681	0.2064	0.1935	0.2497
Hispanic	0.1410	0.1741	0.1293	0.1606
Less than HS ed.	0.0505	0.0326	0.0478	0.0405
High School ed.	0.1156	0.1142	0.1457	0.1367
Some College	0.3145	0.2787	0.3301	0.3304
Bachelor's Degree	0.3092	0.3253	0.2784	0.2685
Advanced Degree	0.2103	0.2492	0.1979	0.2240
Total family income (000's - 2010$)	67.7941	93.7503	47.5619	69.7577
Live in metro area	0.8965	0.9091	0.8539	0.8612
Observations	5208	8769	5796	10396

Notes: Sample includes married heterosexual households as well as both married and unmarried cohabitating same-sex households where the household reference person and their spouse/parent are both between the age 25-45. Information on state same-sex marriage legalization was collected and harmonized from the Pew Research Center, the National Council of State Legislatures and the Movement Advancement Project. Population demographics were obtained from the 1-year American Community Survey (ACS).

Table 3.4: Diff-in-Diff: Any Child Residing in the Household

	(1) All	(2) Female	(3) Male
Same-Sex Couple	-0.485^{***}	-0.389^{***}	-0.594^{***}
	(0.008)	(0.007)	(0.009)
Marriage Legal	0.003	0.002	0.003
	(0.002)	(0.002)	(0.002)
Marriage Legal X Same-Sex Couple	0.006	0.016^{*}	-0.012
	(0.005)	(0.008)	(0.009)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Age FE	Yes	Yes	Yes
Observations	2094184	2080207	2077992
Mean of Dep. Variable	0.788	0.792	0.791
R^2	0.080	0.067	0.074

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares with robust standard errors clustered at the state-level are reported in parentheses. The unit of observation are household-state-year cells from 2008-2019. All estimates control for age, year, and state fixed-effects. Additional control variables omitted from the table were the head of household's race and ethnicity (white or Hispanic), the head of household's education level, family total income, and metropolitan status.

Table 3.5: Diff-in-Diff: Adopted Child Residing in the Household

	(1) All	(2) Female	(3) Male
Same-Sex Couple	0.022***	0.037***	0.004*
	(0.003)	(0.005)	(0.002)
Marriage Legal	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
Marriage Legal X Same-Sex Couple	0.006*	0.002	0.010***
	(0.003)	(0.005)	(0.003)
Year FE	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*
Demographic Controls	*Yes*	*Yes*	*Yes*
Age FE	*Yes*	*Yes*	*Yes*
Observations	2094184	2080207	2077992
Mean of Dep. Variable	0.018	0.018	0.018
R^2	0.004	0.004	0.003

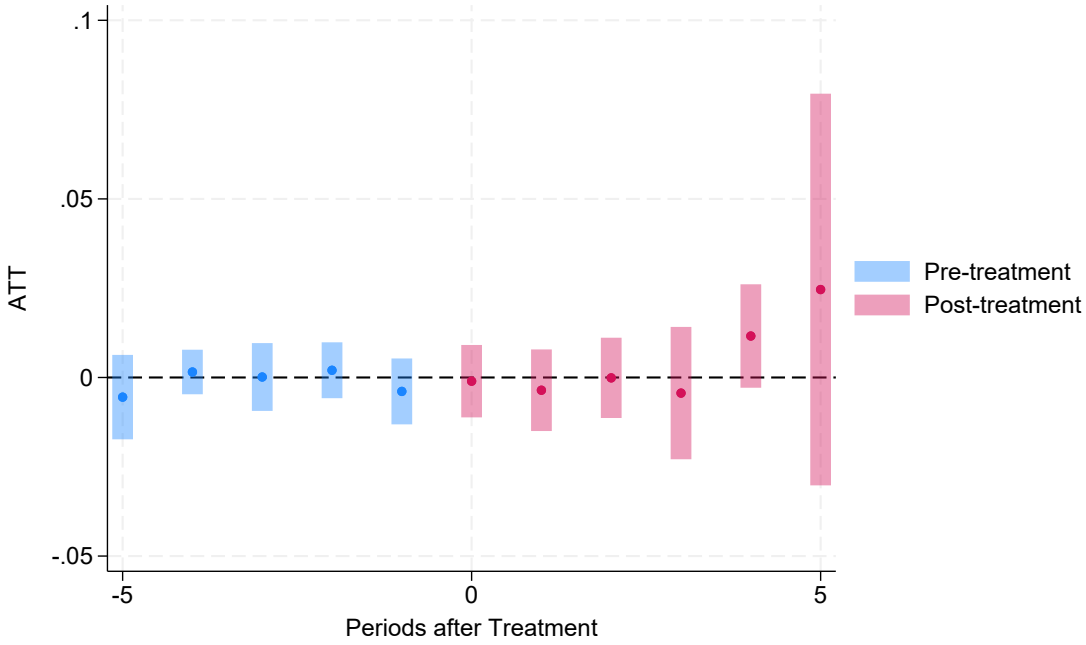
Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares with robust standard errors clustered at the state-level are reported in parentheses. The unit of observation are household-state-year cells from 2008-2019. All estimates control for age, year, and state fixed-effects. Additional control variables omitted from the table were the head of household's race and ethnicity (white or Hispanic), the head of household's education level, family total income, and metropolitan status.

Table 3.6: Diff-in-Diff: Foster Child Residing in the Household

	(1) All	(2) Female	(3) Male
Same-Sex Couple	0.004***	0.005***	0.002**
	(0.001)	(0.001)	(0.001)
Marriage Legal	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
Marriage Legal X Same-Sex Couple	0.001	0.001	0.001
	(0.001)	(0.002)	(0.001)
Year FE	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*
Demographic Controls	*Yes*	*Yes*	*Yes*
Age FE	*Yes*	*Yes*	*Yes*
Observations	2094184	2080207	2077992
Mean of Dep. Variable	0.002	0.002	0.002
R^2	0.001	0.001	0.001

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from ordinary least squares with robust standard errors clustered at the state-level are reported in parentheses. The unit of observation are household-state-year cells from 2008-2019. All estimates control for age, year, and state fixed-effects. Additional control variables omitted from the table were the head of household's race and ethnicity (white or Hispanic), the head of household's education level, family total income, and metropolitan status.

Figure 3.5: Any Child - All Same-Sex Households



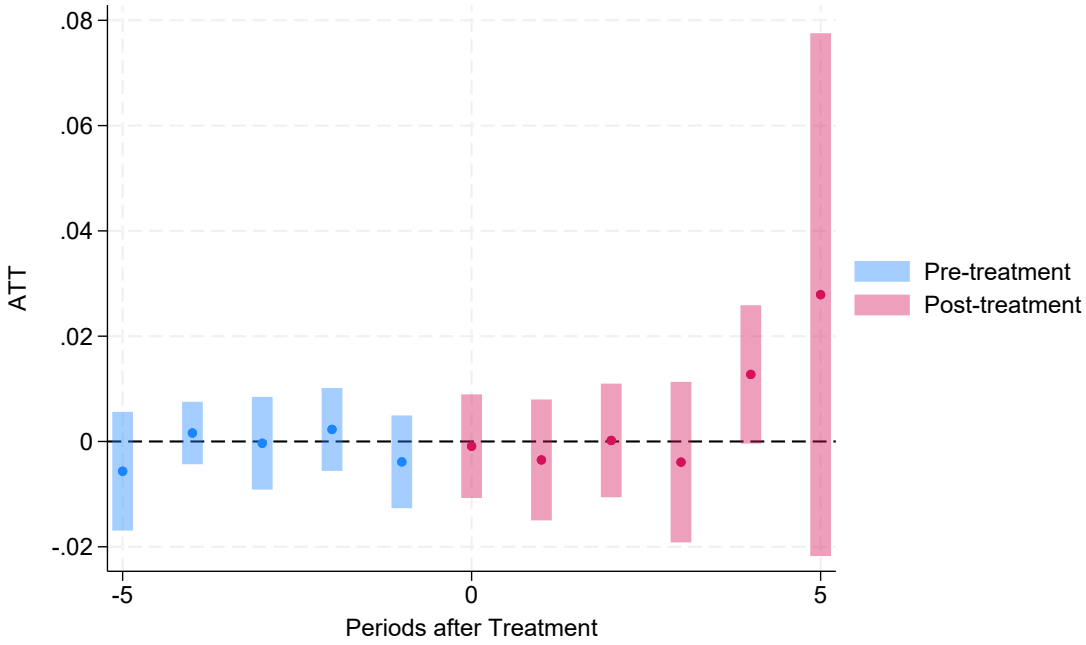
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Table 3.7: Callaway & Sant'anna: Any Child

	(1) All	(2) Male	(3) Female
Pre-treatment Average	0.0000	0.0000	0.0000
	(0.0001)	(0.0001)	(0.0001)
Post-treatment Average	−0.0002	−0.0002	−0.0004
	(0.0003)	(0.0002)	(0.0002)
$I_{t=-5}$	0.0000	0.0000	0.0000
	(0.0006)	(0.0006)	(0.0006)
$I_{t=-4}$	0.0004	0.0004	0.0004
	(0.0004)	(0.0003)	(0.0003)
$I_{t=-3}$	0.0000	0.0001	0.0000
	(0.0005)	(0.0005)	(0.0005)
$I_{t=-2}$	−0.0002	−0.0003	−0.0003
	(0.0005)	(0.0005)	(0.0005)
$I_{t=-1}$	0.0000	0.0000	0.0000
	(0.0003)	(0.0003)	(0.0003)
$I_{t=0}$	−0.0006	−0.0005	−0.0006
	(0.0003)	(0.0003)	(0.0003)
$I_{t=1}$	0.0002	0.0003	0.0002
	(0.0002)	(0.0003)	(0.0003)
$I_{t=2}$	−0.0006	−0.0005	−0.0006
	(0.0004)	(0.0004)	(0.0004)
$I_{t=3}$	−0.0009	−0.0007	−0.0009
	(0.0005)	(0.0005)	(0.0005)
$I_{t=4}$	−0.0011	−0.001	−0.0011
	(0.0007)	(0.0008)	(0.0006)
$I_{t=5}$	0.0016	0.0012	0.0006
	(0.0005)	(0.0005)	(0.0006)

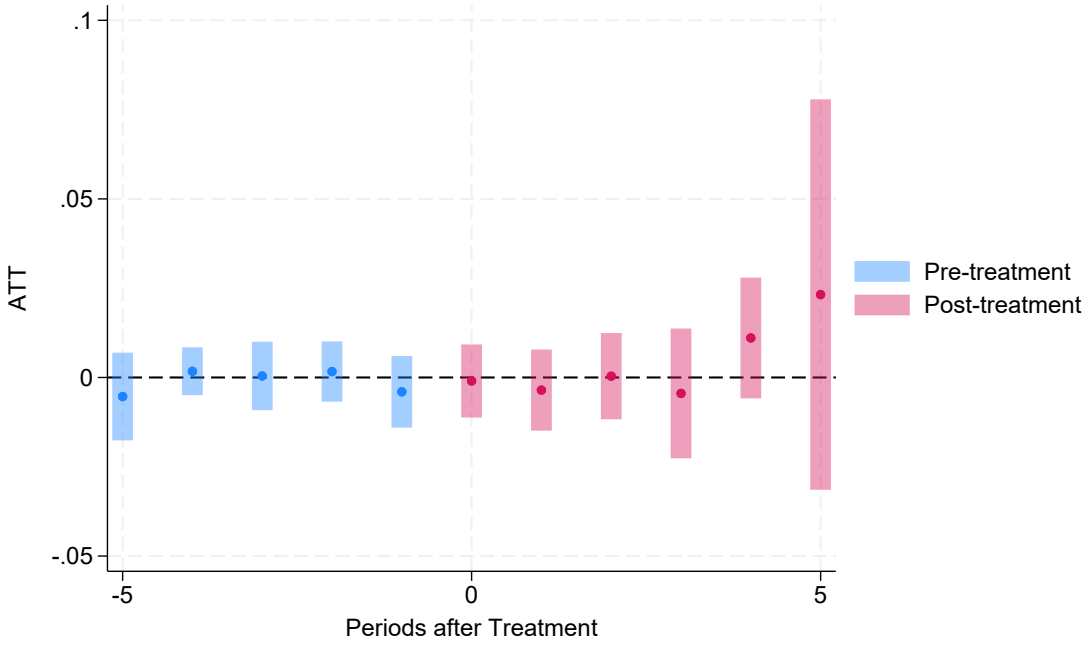
Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from difference-in-difference approach with Callaway & Sant'anna corrections and wild bootstrapped standard errors clustered at the state-level are reported in parentheses. The unit of observation are household-state-year cells from 2008-2019. All estimates control for age, year, and state fixed-effects. Additional control variables omitted from the table were the head of household's race and ethnicity (white or Hispanic), the head of household's education level, family total income, and metropolitan status.

Figure 3.6: Any Child - Male Same-Sex Households



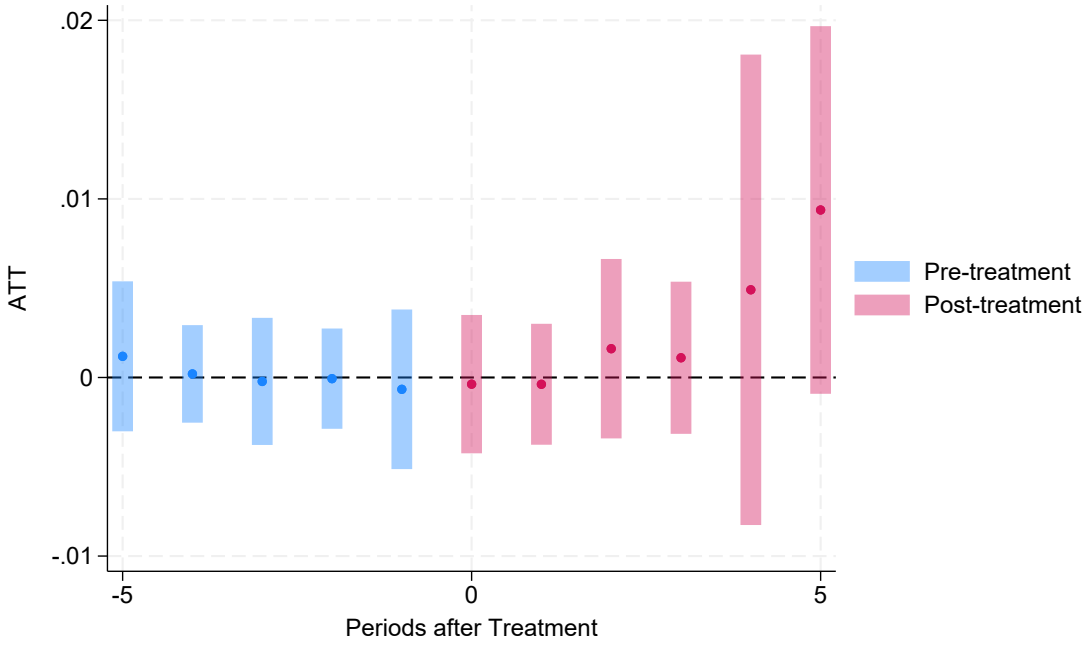
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Figure 3.7: Any Child - Female Same-Sex Households



Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Figure 3.8: Adopted Child - All Same-Sex Households



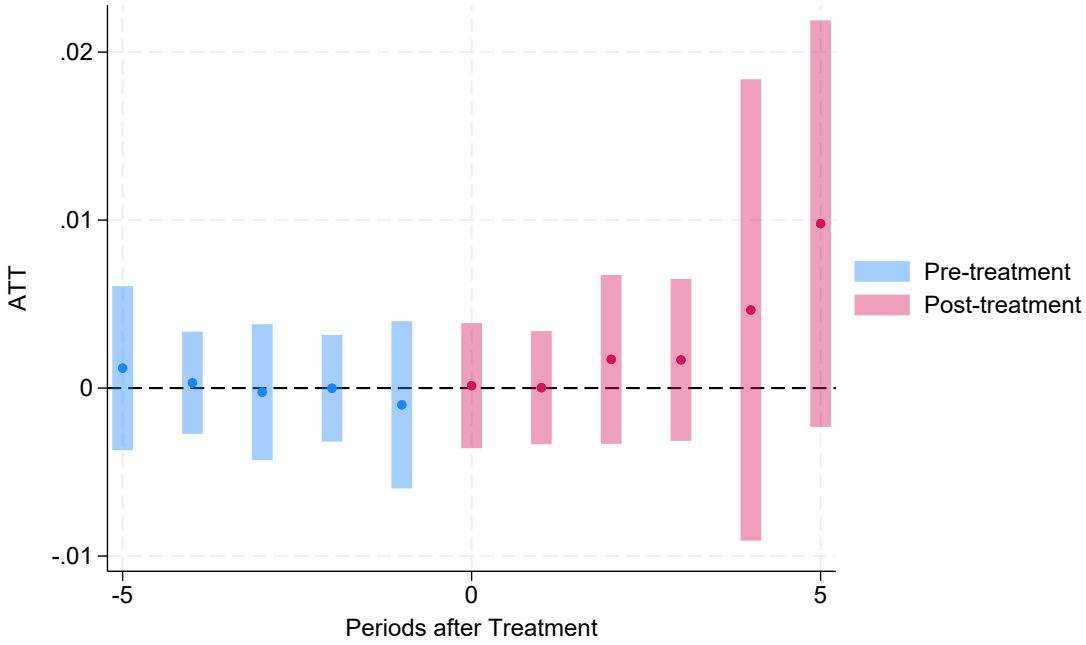
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Table 3.8: Callaway & Sant'anna: Adopted Child

	(1) All	(2) Male	(3) Female
Pre-treatment Average	0.0001	0.0001	0.0001
	(0.0003)	(0.0002)	(0.0003)
Post-treatment Average	0.0027	0.0030	0.0027
	(0.0016)	(0.0017)	(0.0018)
$I_{t=-5}$	0.0012	0.0012	0.0013
	(0.0014)	(0.0015)	(0.0014)
$I_{t=-4}$	0.0002	0.0003	0.0002
	(0.0009)	(0.0009)	(0.0009)
$I_{t=-3}$	−0.0002	−0.0002	−0.0002
	(0.0012)	(0.0013)	(0.0012)
$I_{t=-2}$	−0.0001	0.0000	−0.0001
	(0.0009)	(0.0010)	(0.0011)
$I_{t=-1}$	−0.0007	−0.0010	−0.0006
	(0.0015)	(0.0015)	(0.0015)
$I_{t=0}$	−0.0004	0.0001	−0.0004
	(0.0013)	(0.0012)	(0.0013)
$I_{t=1}$	−0.0004	0.0000	−0.0003
	(0.0011)	(0.001)	(0.0012)
$I_{t=2}$	0.0016	0.0017	0.0020
	(0.0017)	(0.0016)	(0.0016)
$I_{t=3}$	0.0011	0.0017	0.0010
	(0.0014)	(0.0015)	(0.0016)
$I_{t=4}$	0.0049	0.0046	0.0051
	(0.0044)	(0.0043)	(0.0045)
$I_{t=5}$	0.0094	0.0098	0.0088
	(0.0035)	(0.0038)	(0.0038)

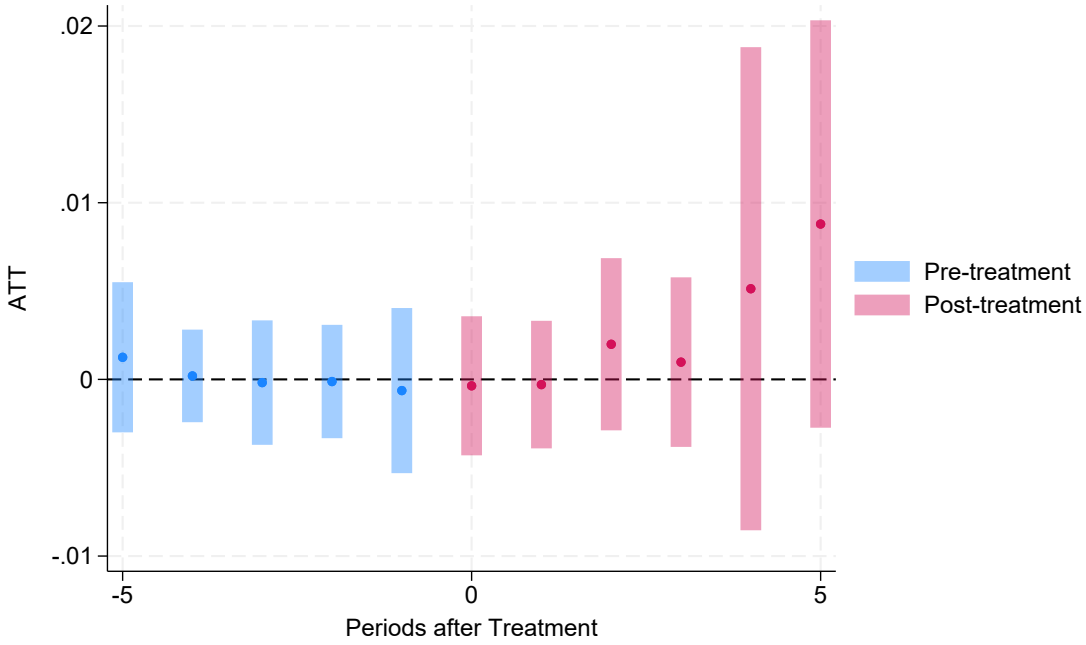
Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from difference-in-difference approach with Callaway & Sant'anna corrections and wild bootstrapped standard errors clustered at the state-level are reported in parentheses. The unit of observation are household-state-year cells from 2008-2019. All estimates control for age, year, and state fixed-effects. Additional control variables omitted from the table were the head of household's race and ethnicity (white or Hispanic), the head of household's education level, family total income, and metropolitan status.

Figure 3.9: Adopted Child - Male Same-Sex Households



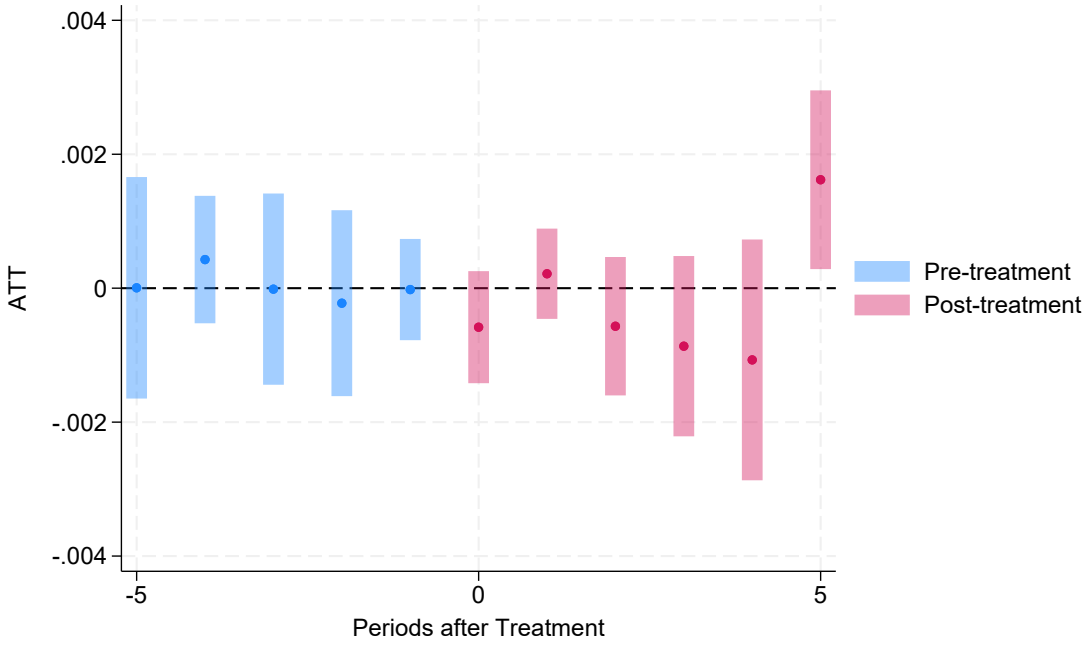
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Figure 3.10: Adopted Child - Female Same-Sex Households



Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Figure 3.11: Foster Child - All Same-Sex Households



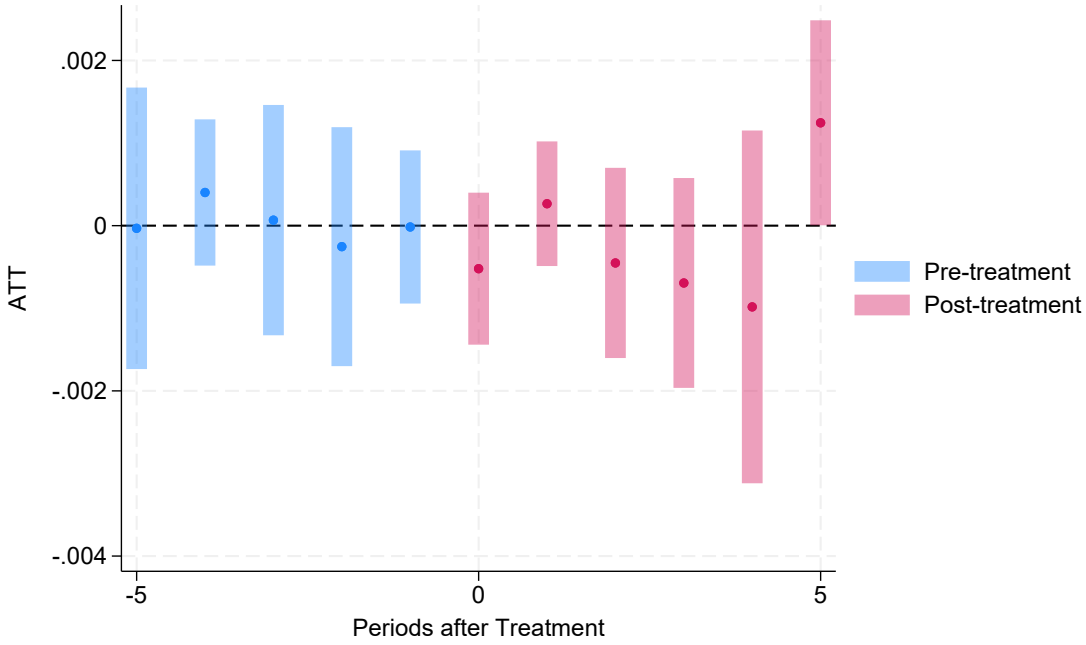
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Table 3.9: Callaway & Sant'anna: Foster Child

	(1) All	(2) Male	(3) Female
Pre-treatment Average	0.0000	0.0000	0.0000
	(0.0001)	(0.0001)	(0.0001)
Post-treatment Average	−0.0002	−0.0002	−0.0004
	(0.0003)	(0.0002)	(0.0002)
$I_{t=-5}$	0.0000	0.0000	0.0000
	(0.0006)	(0.0006)	(0.0006)
$I_{t=-4}$	0.0004	0.0004	0.0004
	(0.0004)	(0.0003)	(0.0003)
$I_{t=-3}$	0.0000	0.0001	0.0000
	(0.0005)	(0.0005)	(0.0005)
$I_{t=-2}$	−0.0002	−0.0003	−0.0003
	(0.0005)	(0.0005)	(0.0005)
$I_{t=-1}$	0.0000	0.0000	0.0000
	(0.0003)	(0.0003)	(0.0003)
$I_{t=0}$	−0.0006	−0.0005	−0.0006
	(0.0003)	(0.0003)	(0.0003)
$I_{t=1}$	0.0002	0.0003	0.0002
	(0.0002)	(0.0003)	(0.0003)
$I_{t=2}$	−0.0006	−0.0005	−0.0006
	(0.0004)	(0.0004)	(0.0004)
$I_{t=3}$	−0.0009	−0.0007	−0.0009
	(0.0005)	(0.0005)	(0.0005)
$I_{t=4}$	−0.0011	−0.0010	−0.0011
	(0.0007)	(0.0008)	(0.0006)
$I_{t=5}$	0.0016**	0.0012**	0.0006
	(0.0005)	(0.0005)	(0.0006)

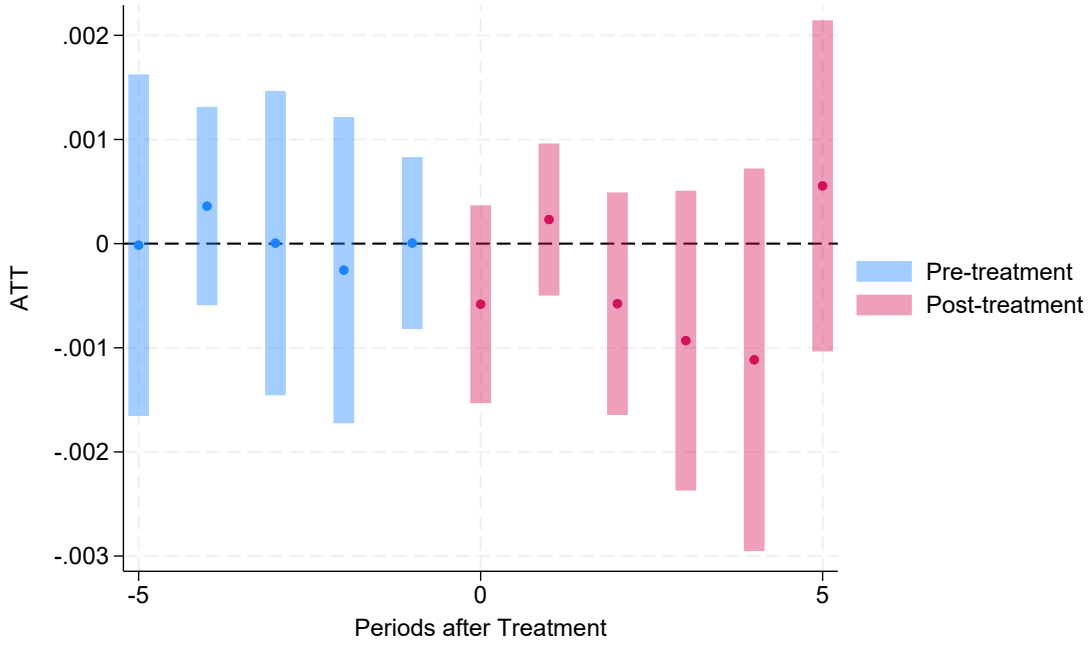
Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Results from difference-in-difference approach with Callaway & Sant'anna corrections and wild bootstrapped standard errors clustered at the state-level are reported in parentheses. The unit of observation are household-state-year cells from 2008-2019. All estimates control for age, year, and state fixed-effects. Additional control variables omitted from the table were the head of household's race and ethnicity (white or Hispanic), the head of household's education level, family total income, and metropolitan status.

Figure 3.12: Foster Child - Male Same-Sex Households



Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Figure 3.13: Foster Child - Female Same-Sex Households



Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period. The vertical bars represent 95% confidence intervals. Standard errors are computed using wild bootstrapping clustered at the state level.

Chapter 4

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Chapter 5

Appendix

5.1 Chapter 1

5.1.1 Identifying Public Housing in the 1940 Census

To identify public housing developments, public housing project building addresses were obtained using Sanborn fire insurance maps drawn during the mid-1930s to 1940. From 1867 until 1960, the Sanborn Map Company produced fire insurance maps for more than 12,000 US towns and cities. Since these maps were used to inform fire insurance companies of potential liabilities, they were highly detailed and included street names, property boundaries, building use, as well as house and block numbers.[1] Addresses collected using Sanborn maps were then geographically cross-referenced using Google Maps.

Next, Census enumeration district maps available through One-Step Webpages were used to identify each public housing development's 1940 Census enumeration district.[2] Prior to modern computing, the US Census was collected by trained enumerators. Using a standardized procedure, enumerators walked through assigned districts to enumerate each household resident.[3] Once 1940 Census enumeration districts containing public housing developments were identified, households within each district were manually flagged as public housing residents by matching the Census-enumerated street address with the address of public housing developments obtained from the Sanborn fire insurance maps.[4]

Table 5.1: Distribution of Children Age 6-18 by Public Housing Exposure (Non-Missing Income)

Age (years)	< 1	$1-2$	$2-3$	$3-4$	Total
6	19	50	131	0	200
7	536	462	744	89	1831
8	544	489	776	92	1901
9	513	467	705	98	1783
10	509	442	665	96	1712
11	462	400	667	89	1618
12	471	379	621	110	1581
13	429	307	520	84	1340
14	358	316	521	93	1288
15	365	266	470	95	1196
16	325	267	437	89	1118
17	298	234	425	74	1031
18	349	269	438	93	1149
Total	5178	4348	7120	1102	17748
Observations	17748				

Authors' calculations of complete-count 1940 Census data (Ruggles et al., 2021)

[1] They also included information as to the size, shape and construction of dwellings, fire walls, locations of windows and doors, sprinkler systems, and types of roofs.
https://www.loc.gov/collections/sanborn-maps/articles-and-essays/introduction-to-the-collection/

[2] Morse and Weintraub 2017

[3] Enumerators would revisit households to collection information for absent individuals if the respondent could not provide adequate information during the initial visit.

[4] A more detailed explanation of the public housing resident identification strategy can be found in Allen and Van Riper (2020)

5.1.2 Flow Chart of Constructing/Identifying Eligible Households

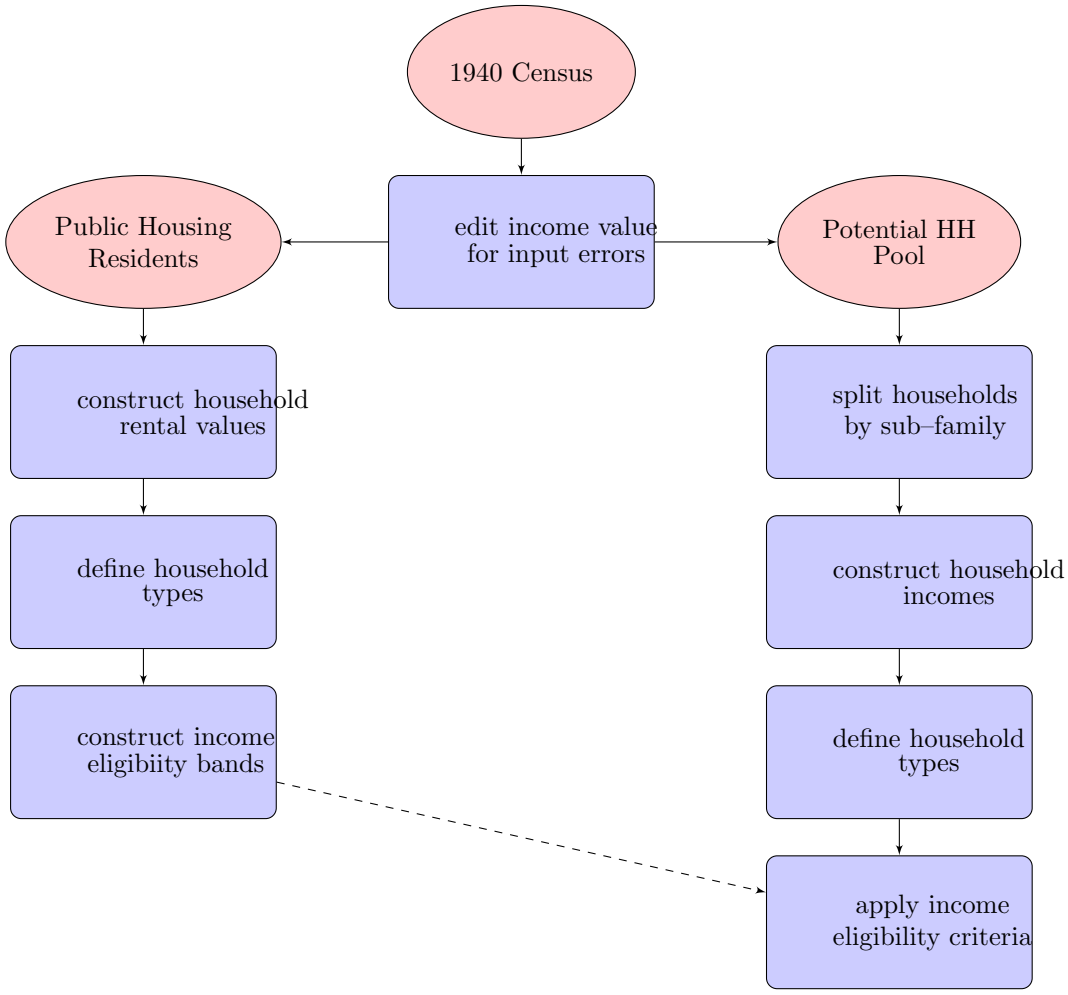


Table 5.2: Logit - Child Currently Attends School

	Unweighted		CEM Weighted		CEM k2k Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Public Housing	1.643***	1.289	1.629***	1.774***	1.501***	1.540***
	(0.108)	(0.203)	(0.108)	(0.248)	(0.134)	(0.241)
Public Housing × Female		0.998		0.897*		0.831*
		(0.043)		(0.059)		(0.086)
Public Housing × Non-white		1.738***		1.504***		1.584***
		(0.093)		(0.152)		(0.217)
Public Housing × Older than CSL age		0.924		0.831		0.953
		(0.158)		(0.145)		(0.200)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1894741	1894741	255476	255476	22738	22738
Mean of Dep. Variable	0.862	0.862	0.898	0.898	0.901	0.901
R^2	0.328	0.337	0.307	0.315	0.348	0.365

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

Table 5.3: Logit - Child Ontime for School Completion Requirements

	Unweighted		CEM Weighted		CEM k2k Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Public Housing	1.234	0.898	1.135	1.036	1.250***	1.196**
	(0.164)	(0.153)	(0.122)	(0.156)	(0.087)	(0.090)
Public Housing × Female		0.966		0.943		0.930
		(0.072)		(0.071)		(0.075)
Public Housing × Non-white		1.348*		1.199		1.138
		(0.228)		(0.261)		(0.230)
Public Housing × Older than CSL age		1.446*		2.336***		1.538***
		(0.290)		(0.327)		(0.214)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	1894741	1894741	255476	255476	22738	22738
Mean of Dep. Variable	0.749	0.749	0.771	0.771	0.711	0.711
R^2	0.126	0.142	0.106	0.121	0.174	0.186

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

Table 5.4: Logit - Child Employed in Labor Force

	Unweighted		CEM Weighted		CEM k2k Weighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Public Housing	0.504***	1.130	0.549***	0.758	0.610***	0.450*
	(0.063)	(0.246)	(0.070)	(0.308)	(0.087)	(0.188)
Public Housing × Female		0.581***		0.615**		0.950
		(0.095)		(0.119)		(0.113)
Public Housing × Non-white		0.529***		0.615**		0.517**
		(0.115)		(0.150)		(0.137)
Public Housing × Older than CSL age		0.862		1.039		1.660
		(0.170)		(0.369)		(0.680)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	742623	742623	59592	59592	6386	6386
Mean of Dep. Variable	0.244	0.244	0.227	0.227	0.191	0.191
R^2	0.273	0.281	0.277	0.285	0.210	0.234

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

Table 5.5: Heterogeneous Exposure Summary Results (Logit)

	Currently Attends School		Ontime to Complete Schooling		Employed in Labor Force	
	(1)	(2)	(3)	(4)	(5)	(6)
Duration in Public Housing	1.242***	0.861	1.036	1.011	0.949	1.458
	(0.100)	(0.138)	(0.074)	(0.080)	(0.071)	(0.338)
Female × Duration in Public Housing		1.181***		1.002		1.042
		(0.068)		(0.032)		(0.087)
Non-white × Duration in Public Housing		0.907		1.011		1.105
		(0.108)		(0.135)		(0.184)
Older than CSL age × Duration in Public Housing		1.514**		1.476*		0.600**
		(0.255)		(0.336)		(0.128)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	19190	19190	19190	19190	6200	6200
Mean of Dep. Variable	0.907	0.907	0.739	0.739	0.155	0.155
R^2	0.331	0.350	0.186	0.198	0.223	0.261

Notes: *** Denotes significance at 1%, ** at 5%, and * at 10%. Standard errors clustered at the state-level are reported in parentheses. The unit of observation are children. All estimates control for age, and state fixed-effects. Models (2), (4), and (6) also control for child (female, non-white, old enough to be exempt from compulsory schooling, US citizenship) and household demographics (total household income, whether household income was missing, whether both parents were present in the household, whether there were three or more children in the household, and indicator variables for the number of household members [1-2, 3, 4, 5, 6 or more]).

5.2 Chapter 2

5.2.1 Missing Observations for Substance Abuse Treatment Admissions

The following state-year observations are not available in the public-use TEDS-A data:

- 2006: Alaska, District of Columbia
- 2007: Alabama
- 2008: Mississippi
- 2009: District of Columbia, Mississippi
- 2010: Mississippi
- 2014: South Carolina
- 2015: Oregon, South Carolina
- 2016: Georgia, Oregon

Therefore these state-year observations are not included in the adult substance abuse treatment admissions panel. For the child-level panel, there are no missing state-year-age cells (10,098). When merged with the adult-level panel, I lose 216 state-year-age cells, therefore the OLS and IV regressions are done using 9,882 cells.

5.2.2 Inter-temporal Treatment Measure Correlation

Figure 5.1: Correlation: Mean Pre-2010 Oxycodone Supply to 2006 Oxycodone Supply

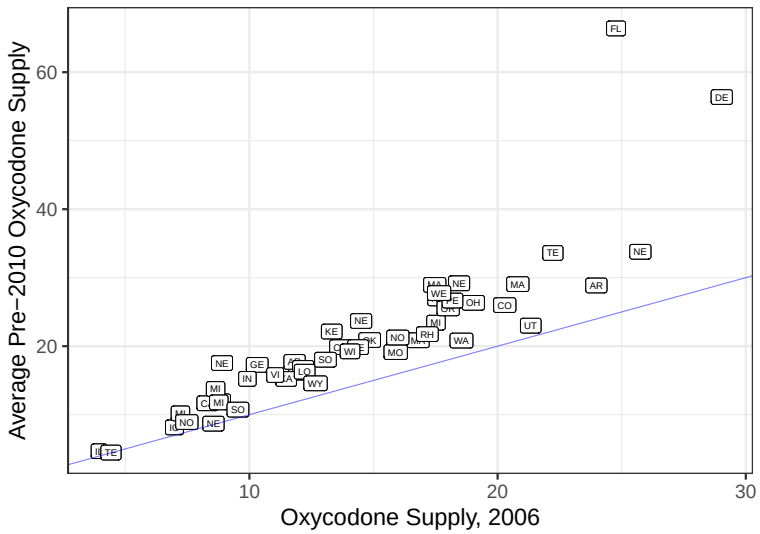


Figure 5.2: Correlation: Mean Pre-2010 Oxycodone Supply to 2010 Oxycodone Supply

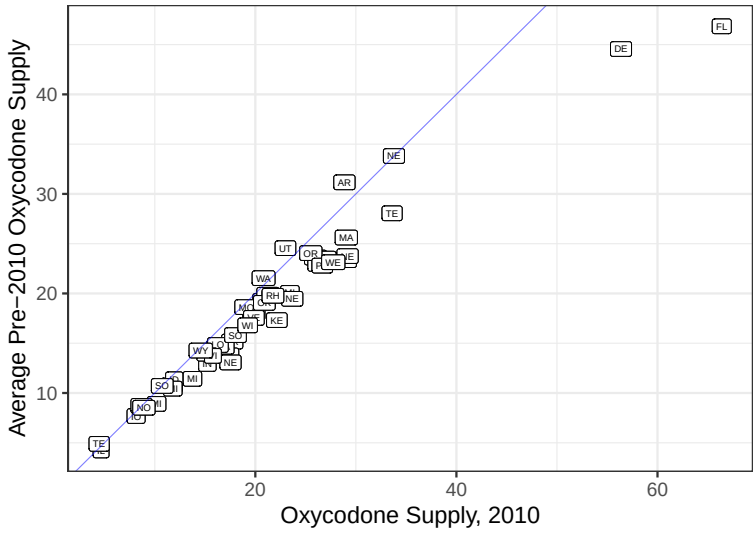
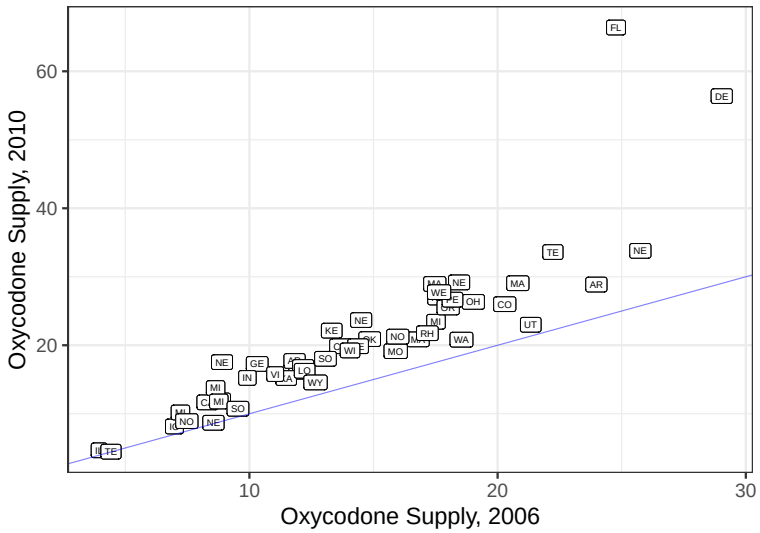


Figure 5.3: Correlation: 2006 to 2010 Oxycodone Supply



5.3 Chapter 3

5.3.1 Additional Tables

Table 5.6: State Same-Sex Marriage Legalization Enactment Dates

Massachusetts	2004
Connecticut	2008
Iowa	2009
Vermont	2009
District of Columbia	2010
New Hampshire	2010
New York	2011
Maine	2012
Washington	2012
California	2013
Delaware	2013
Hawaii	2013
Maryland	2013
Minnesota	2013
New Jersey	2013
New Mexico	2013
Rhode Island	2013
Alaska	2014
Arizona	2014
Colorado	2014
Idaho	2014
Illinois	2014
Indiana	2014
Kansas	2014
Montana	2014
Nevada	2014
North Carolina	2014
Oklahoma	2014
Oregon	2014
Pennsylvania	2014
South Carolina	2014
Utah	2014
Virginia	2014
West Virginia	2014
Wisconsin	2014
Wyoming	2014
Alabama	2015
Arkansas	2015
Florida	2015
Georgia	2015
Kentucky	2015
Louisiana	2015
Michigan	2015
Mississippi	2015
Missouri	2015
Nebraska	2015
North Dakota	2015
Ohio	2015
South Dakota	2015
Tennessee	2015
Texas	2015