ABSTRACT

Title of Dissertation: ESSAYS IN LABOR AND HEALTH ECONOMICS

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Chapter 1 investigates the impact of the opioid crisis on the foster care system, motivated by a growing literature suggesting that the opioid crisis has negatively impacted children. This paper builds on prior work by separately identifying the causal effects of parental illicit opioid use and prescription opioid (PO) misuse on foster care entry and caseload after 2010. Two baseline opioid supply measures from 2000, at the onset of the opioid crisis, instrument for post-2010 changes in opioid overdose death rates among adults of prime parenting age. Using county-level mortality data and administrative foster care records, I find that moving from the 10th to the 90th percentile of change in the illicit opioid death rate leads to a 1.1 (2.4) standard deviations larger percent growth in foster care entry (caseload). In contrast, a 10th to 90th percentile difference in the change in PO misuse does not significantly affect total entry or caseload. Female illicit opioid use has larger effects than male illicit opioid use. Effects are similar across child age at the time of removal. Illicit

opioid use has similar effects across all placement settings, whereas PO misuse only increases entry to kinship care.

Chapter 2 studies how an expansion of public health insurance affects self-employment dynamics. I investigate this question in the context of the Affordable Care Act (ACA) Medicaid expansion, which made Medicaid available to all non-elderly adults with family income up to 138% of the federal poverty level. By increasing access to health insurance outside of wage employment, expanding Medicaid eligibility could (i) induce some wage workers to become self-employed, and (ii) increase persistence in self-employment for the currently self-employed. Using the 2003-2016 CPS-ASEC linked to respondents' earnings histories from tax returns, I estimate the effect of expanding Medicaid eligibility on the probability of self-employment, wage employment to self-employment transition, and self-employment persistence among childless adults, the group that saw the largest increase in Medicaid eligibility as a result of the expansion. Using difference-in-differences that takes into account pre-ACA cross-county variation in population shares of "at-risk" groups and triple differences, I find suggestive evidence that the expansion of Medicaid eligibility may have increased persistence in self-employment for those with a relatively high valuation of health insurance, but no effects on wage employment to self-employment transitions or self-employment rates.

Chapter 3 (with Katharine Abraham, John Haltiwanger, Kristin Sandusky, and James Spletzer) examines the role of self-employment in older workers' transitions to retirement. Self-employment rates rise with age, especially past the age of 50. Using unique integrated survey and administrative data, we find the share of the employed who are primarily self-employed more than doubles from age 47-52 to 65-70 – rising from under 10% to more than 20%. This growth reflects the differential patterns by age of all of the transitions among wage and salary employment, self-employment and non-employment. There is a sharp decline in the likelihood that workers switch from self-employment to wage and salary employment with age, but not the reverse. The share of wage and salary workers who transition to non-employment each year rises more rapidly with age between 53-58 and 65-70 than is the case for the self-employed. Just as important, there is a much sharper decline with age in the pace of transitions from non-employment to wage and salary employment than in the pace of transitions from non-employment to self-employment. The interaction of these changing transition rates, as opposed to simply their individual effects, plays a large role in accounting for the increase in the self-employment rate with age. We investigate how education, cumulative earnings over the prior 20 years, and earnings volatility over the prior 20 years affect these changing transition dynamics by age. We find, for example, that wage and salary workers who are more educated and have higher cumulative earnings are more likely to move to self-employment and less likely to move to non-employment, with both of these effects larger at older ages.

ESSAYS IN LABOR AND HEALTH ECONOMICS

by

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Dissertation submitted to the Faculty of the Graduate School of the
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Preface

This dissertation spans a range of topics in labor and health economics, motivated by pressing socioeconomic concerns or by an interest in understanding the effects of policy changes. My most recent area of interest is the causes and consequences of the opioid crisis in the United States. Rising rates of opioid abuse and overdose deaths over the past two decades have been linked to significant health and economic changes, but we have less evidence on the spillover effects of adult opioid abuse on children. Chapter 1 addresses this question by studying the effect of parental opioid abuse on foster care involvement of children. This is an important type of change to children's environments because foster care entry potentially has negative effects later in life, affects a significant portion of children, and is costly to the public.

I am also interested in how health policy affects employment decisions. In particular, health care reform can impact the labor market by changing the availability, price, and quality of health insurance and health care, which interact with workers' and firms' decisions in the labor market. In Chapter 2, I study whether a public health insurance expansion affects self-employment decisions.

Chapter 3 is coauthored with Professors Katharine Abraham and John Haltiwanger and Drs. Kristin Sandusky and James Spletzer, and is part of our broader research agenda to better measure and understand self-employment activity. The version presented here is largely based on a publicly available working paper written for the Stanford Institute for Economic Policy Research Working Longer and Retirement Conference in 2018, with minor modifications to the text made by the author of this dissertation, Claire Hou. All material are used with permission from all coauthors.

Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the view of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information has been disclosed (Chapter 2: CBDRB-FY21-CES011-002; Chapter 3: DRB-B0127-CDAR-20180927, CBDRB-FY22-134).

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I am indebted to Professor Katharine Abraham, who has advised my own work and has also been a wonderful co-author. Much of what I have learned about empirical labor economics comes from our collaborative research on self-employment. The rigor of her thinking and writing and the depth of her expertise have been invaluable and inspirational.

I must also thank the other co-authors of Chapter 3: Dr. Kristin Sandusky and Dr. James Spletzer made my time as an intern at the U.S. Census Bureau an enjoyable and interesting experience. Their experience with and knowledge of administrative data has been instrumental to our collaborations and also to Chapter 2, which uses the same data infrastructure as our joint work. Many thanks to Professor John Haltiwanger, who first introduced me to this research group.

I also had the great pleasure of working with Professor Nolan Pope. His second-year field course made me excited about applied microeconomics, and his practical suggestions and advice have significantly improved my research. Thanks also to Professor Michel Boudreaux for participating in my dissertation committee.

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A special thanks goes to the staff of the Department of Economics and Professor John Shea, who helped me navigate the administrative side of the graduate program, and to many of my fellow PhD students for their friendship and advice.

Finally, I thank my family and friends for their love, patience, and support.

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List of Abbreviations

2SLS	2-Stage least squares
3SLS	3-Stage least squares
ABP	Alternative Benefit Plan
ACA	Affordable Care Act
ACS	American Community Survey
AFCARS	Adoption and Foster Care Analysis & Reporting System
ARCOS	Automation of Reports and Consolidated Orders System
ASFA	Adoption and Safe Families Act
CDC	Centers for Disease Control and Prevention
CHIP	Children's Health Insurance Program
CI	Confidence interval
COVID-19	Coronavirus disease 2019
CPS	Child Protective Services; Current Population Survey
CPS-ASEC	Current Population Survey Annual Social and Economic Supplement
DEA	Drug Enforcement Administration
DER	Detailed Earnings Records
DHHS	Department of Health and Human Services
DiD	Difference-in-differences
EHB	Essential health benefit
EIN	Employer Identification Number
EITC	Earned Income Tax Credit
ESHI	Employer-sponsored health insurance
FD	First differences
FE	Fixed effects
FFPSA	Family First Prevention Services Act
FPL	Federal Poverty Level
FY	Fiscal Year

GDP	Gross Domestic Product
HIV/AIDS	Human immunodeficiency virus infection and acquired immunodeficiency syndrome
HRS	Health and Retirement Study
ICD-10	International Classification of Disease, Tenth Revision
IHS	Inverse hyperbolic sine
IV	Instrumental variables
KFF	Kaiser Family Foundation
LD	Long differences
LFP	Labor force participation
MAT	Medication-assisted treatment
MCD	Multiple Cause of Death
MEF	Master Earnings File
NAICS	North American Industry Classification System
NCANDS	National Child Abuse and Neglect Data System
NCHS	National Center for Health Statistics
NDACAN	National Data Archive on Child Abuse and Neglect
NE	Non-employment
OD	Overdose
OLS	Ordinary least squares
OOP	Out of pocket
PDMP	Prescription drug monitoring program
PIK	Protected Identification Key
PO	Prescription opioid
PSID	Panel Study of Income Dynamics
PUMA	Public Use Microdata Area
RCT	Randomized controlled trial
RD	Regression discontinuity
RF	Reduced-form
RHS	Retirement History Survey

SCHIP	State Child Health Insurance Program
SE	Self-employment
SEER	Surveillance, Epidemiology, and End Results
SFY	State fiscal year
SIPP	Survey of Income and Program Participation
SSI	Supplemental Security Income
SUD	Substance use disorder
SW	Sanderson-Windmeijer (F-statistic)
UI	Unemployment Insurance
UMR	Universal mandated reporting
WS	Wage and salary

Chapter 1: The Opioid Crisis and Foster Care Dynamics

1.1 Introduction

Over the past two decades, the rate of drug overdose deaths in the United States increased more than threefold, from 6.2 deaths per 100,000 population in 2000 to 21.6 deaths per 100,000 by 2019 (CDC 2020a). Opioids have been the primary driver of this increase: the number of drug overdose deaths involving any opioid increased by nearly 500% during this period. This alarming growth in overdose deaths, along with the negative consequences of opioid misuse, abuse, and addiction, constitute one of the most serious public health challenges facing the U.S. today. The opioid crisis shows no signs of slowing down, and in fact has deepened during the ongoing COVID-19 pandemic. 12-month provisional counts of opioid overdose deaths increased by 35.7%, from 48,663 in October 2019 to 66,047 in October 2020 (NCHS 2021), an order of magnitude larger than the 2.9% increase in opioid overdose deaths over the same period one year earlier.

Beyond first-order effects on drug overdose mortality, the opioid crisis has been linked to changes in a broad range of social, economic, and health outcomes: declines in labor force participation and employment rates,[1] increased use of social insurance programs and welfare benefits,[2] adverse impacts on infant health and pregnancy outcomes,[3] and increases in child maltreatment and the rate of children living away from parents.[4] Estimates of the total annual societal cost of the opioid crisis range from \$179 billion (Davenport et al. 2019) to over \$1 trillion (Florence et al. 2021).

Despite this growing body of research on the widespread costs of the opioid crisis, we are only beginning to understand its impact on the foster care system, a major component of child welfare services in the U.S.[5] Time series evidence and anecdotal reports suggest a link between increased opioid abuse among parents and the recent uptick in children entering foster care. The rate of children under age 18 living in foster care (per 1,000 children under age 18) began to increase in 2011 after a decade-long decline (Fig 1.1). 5.8 out of every 1,000 children were in foster care on the last day of FY 2017, a 14% increase since FY 2011 (U.S. DHHS 2020). Mirroring this recent rise in the foster care population, entry into foster care per 1,000 children increased by 8% between 2011 and 2017. Fig 1.2 shows that the increase in foster care entry coincides with the rapid rise of overdose deaths involving illicit opioids (heroin and non-pharmaceutical fentanyl) starting in 2010. Reports from professionals working in child welfare and media outlets corroborate these trends. In interviews with caseworkers and court professionals across eleven states, Ghertner et al.

[1]Krueger (2017); Aliprantis et al. (2019); Beheshti (2019b); Park and Powell (2021); Powell (2021)

[2]Beheshti (2019b); Park and Powell (2021); Arteaga and Barone (2021)

[3]Gihleb et al. (2019); Ziedan and Kaestner (2020)

[4]Evans et al. (2022); Buckles et al. (2020)

[5]Gihleb et al. (2018) and Dallman (2020) address the connection between the opioid crisis and foster care entry, both doing so by estimating the effect of supply-side opioid control policies on foster care admissions. Section 2.3 discusses these two studies in more detail.

(2018) found that in every jurisdiction interviewed, practitioners perceived that increased caseloads were in large part due to parental opioid abuse. The popular press has covered the issue with headlines such as "The Opioid Plague's Youngest Victims: Children in Foster Care" (Lachman) and "The opioid crisis is straining the nation's foster-care systems" (Stein and Bever).

Foster care placement is not a rare event: 5% to 6% of all children in the U.S. will have been in foster care at some point between birth and the age of 18 (Wildeman and Emanuel 2014). Being placed in foster care potentially increases the likelihood of future criminal justice involvement and teenage motherhood, reduces the likelihood of high school graduation, and reduces earnings (Doyle 2007, 2008; Warburton et al. 2014). Placements also incur large direct public expenditures: in FY 2018, $35,000 per child were spent on foster care placement costs, which is more than twice the per-pupil public spending on K-12 education (Rosinsky et al. 2021; Urban Institute). Given these costs and the significant risk of experiencing foster care during childhood, whether there is a causal relationship between parental opioid abuse and the increasing burden on the foster care system is an important question for child welfare and public policy.

This paper estimates the effect of parental opioid abuse on foster care flows and stocks, separately identifying the effects of illicit opioid use (i.e., use of heroin and synthetic opioids such as fentanyl) and prescription opioid (PO) misuse on foster care admissions and caseload. The sample period spans 2010 to 2018, when the opioid crisis shifted from prescription opioid misuse to dramatic increases in overdose deaths due to heroin and fentanyl. Given potentially different treatment effects of illicit opioid use and PO misuse on the risk of foster care entry arising from differences in their harmful effects on users, I distinguish between the two types of opioid abuse. To better capture longer-run changes in foster care outcomes over the entire sample period, the analysis is specified in terms of long differences between averaged levels over 2010-11 and averaged levels over year pairs at the end of the period. Rates of illicit opioid and prescription opioid overdose death among prime parenting ages are used as proxies for unobserved rates of parental illicit opioid use and PO misuse, respectively.

To address potential omitted variables and measurement error bias in OLS estimates of the effect of parental illicit opioid and PO mortality on foster care outcomes, I employ an instrumental variables (IV) estimation strategy that uses measures related to a county's prescription opioid supply observed in the year 2000, at the onset of the opioid crisis, to instrument for post-2010 changes in illicit and prescription opioid death rates. Specifically, the main instruments are the supply of oxycodone as a share of total PO supply and the per-capita total PO supply in 2000. At the beginning of the opioid crisis, there was substantial but not highly correlated spatial variation in both the disproportionate prescribing of oxycodone (measured by the oxycodone share) and the overall prescription opioid density. These supply measures are equilibrium quantities that reflect different dimensions of initial geographic variation in exposure to several subsequent supply-side prescription opioid control policies. The policies, which typically restricted access to or reduced the supply of abusable prescription opioids, shifted both illicit opioid and PO mortality by inducing substitution toward illicit opioids and away from prescription opioids. The initial supply measures are analogous to the “shares” in the standard shift-share Bartik instrument, which capture variation across units during a baseline period in exposure to subsequent aggregate shocks.

I combine a variety of data sources for the empirical analysis. Data on foster care admissions and caseload come from the Adoption and Foster Care Analysis & Reporting System (AFCARS), a federally mandated data collection system covering all children in foster care in the fifty states and Washington, D.C. AFCARS provides case-level information on dates of removal from home and discharge from foster care, reasons for removal, child demographics, and the placement setting. Opioid overdose death rates are constructed using Multiple Cause of Death (MCD) microdata, which are based on death certificates for U.S. residents. The initial PO exposure measures are based on retail prescription opioid quantities reported in the Drug Enforcement Administration's data collection system, which tracks controlled substance transactions from manufacturers and distributors.

I find that the increase in parental illicit opioid use between 2010-11 and 2017-18 increased both flows into foster care and the foster care caseload. During this period, a 1 standard deviation (sd) larger change in the illicit opioid death rate is associated with a 0.84 sd larger percent change in foster care entry, and with a 1.06 sd larger percent change in caseloads. The effect of parental illicit opioid use on caseloads was driven by new entries into foster care rather than by entry cohorts of previous years staying longer in the system, as indicated by the lack of an effect on the continuation rates of prior entry cohorts. In contrast, post-2010 changes in parental prescription opioid misuse only increased relative foster care (i.e. kinship care, where the foster home is provided by a biological relative) entry and caseload, with no evidence of a significant effect on total entry and caseload. A 1 sd larger change in the PO-only death rate is associated with a 0.15 sd larger percent change in entry to kinship care, and with a 0.6 sd larger percent change in kinship care caseloads. To alleviate the concern that the instruments may be correlated with other trends affecting the foster care system, I show that the IV estimates are stable across the inclusion of a comprehensive set of controls that account for initial conditions and changes in demographics, economic and social conditions, other substance abuse factors, welfare generosity, and the supply of social assistance establishments. I provide further support for instrument exogeneity by showing that in a subsample where the first stage relationship is zero, the reduced-form estimates are also zero. The results are also qualitatively similar across a battery of sensitivity checks.

To put effect magnitudes in perspective, for a county with median entry rate in 2010, moving from the 10th to the 90th percentile of the change in the illicit opioid death rate implies an increase of 2.5 admission per 1,000 children by 2018, compared to a counterfactual of no change in the illicit opioid death rate during this period.[6] This increase in entry leads to an additional \$6.1 million (2018 \$) in direct public expenditures for the county with median entry rate in 2010. A county with median caseload rate in 2010 would experience an increase of 6.9 cases per 1,000 children by 2018,[7] which is associated with an additional \$7.8 million in direct public expenditures. Alternatively, if we consider a counterfactual in which all counties in the U.S. were assigned a change between 2010 and 2018 in the illicit

[6] In 2010, the median foster care entry rate was 4.07 admissions per 1,000 children, and the county with median entry rate had 431 admissions. The increase of 2.5 entries per 1,000 children by 2018 is equivalent to 169 additional admissions.

[7] In 2010, the median foster care caseload rate was 5.87 cases per 1,000 children, and the county with median caseload rate had 241 cases. The increase of 6.9 cases per 1,000 children by 2018 is equivalent to an increase of 217 cases.

opioid death rate that is equivalent to the actual 10th percentile change during this period, there would have been 6,000 fewer foster care admissions, or 70% of the observed aggregate increase in admissions. This hypothetical reduction in entry would have saved $200 million in foster care placement costs.

I also document heterogeneous effects of parental illicit opioid use and PO misuse on foster care entry and caseload. First, illicit opioid use increased entry to both kinship care and non-relative placements (i.e. non-relative foster homes, group homes, and institutions), whereas PO misuse only increased entry to kinship care. This pattern is consistent with a different likelihood that other family members are available to care for the child among parents who are primarily misusing POs compared to those who have turned to heroin or fentanyl. Second, while increases in both male and female illicit opioid use raised foster care entry/caseload, the effect of female illicit opioid use is significantly larger. This is consistent with illicit opioid use by mothers having a stronger impact on the risk of out-of-home placement, which could be explained by factors such as reporting requirements for substance-exposed newborns and the much higher prevalence of single mother households as compared to single father households. I do not find different effects of illicit opioid use by child age at the time of removal. I also explore whether effects are different based on the degree of centralization of the state child welfare system, the presence of a universal mandated reporting law, and whether parental substance abuse is included in the state civil definition of child maltreatment. I find that the effects of parental illicit opioid use on foster care outcomes do not differ by these child welfare policies and contexts.

A small but growing literature in economics has examined how the opioid crisis has affected children and families. Gihleb et al. (2018), Dallman (2020), and Evans et al. (2022) study the response of foster care admissions or child maltreatment rates to supply-side prescription opioid control policies. Buckles et al. (2020) take a broader view of children's outcomes by examining the impact of the drug crisis over the past three decades on parental absence and the rate of children living with grandparents. The present paper contributes to this literature in four ways.

First, I identify the effects of both parental illicit opioid use and parental PO misuse on foster care dynamics using a just-identified IV model in which illicit opioid use and PO misuse are treated as separate endogenous explanatory variables. At the individual parent level, we have good reason to expect that illicit opioid use and PO misuse could have different effects on the risk of children being removed from the home. Heroin use, for instance, leads to more severe negative consequences for users more quickly, compared to PO misuse. Opioid users who transitioned from POs to heroin report that "life started unraveling at a much faster pace" after the transition to heroin, that the drug "completely takes over you", and that the withdrawal symptoms, especially from intravenous use of heroin, are much worse (Monico and Mitchell 2018). Moreover, although heroin is considered to be cheaper than prescription opioid pills within opioid user networks (Cicero et al. 2012; Monico and Mitchell 2018), greater variation in purity and dramatic increases in tolerance contribute to regular heroin use being more expensive over time, such that it is more costly over the long run to maintain a heroin addiction versus a prescription opioid addiction. Heroin is also more likely to be injected, increasing the risk of contracting blood-borne viral infections such as hepatitis B and C and HIV (Beheshti 2019a; CDC 2020b, 2021a). To the extent that these consequences of illicit opioid use reduce parents' capacity to care for their children more so than PO misuse, the two types of opioid abuse could differentially affect the likelihood that an allegation of child maltreatment will be made to Child Protective Services, which begins the process that may result in foster care placement.

Second, I add to this literature estimates of new parameters: the causal effects of parental illicit opioid use and PO misuse on foster care entry and caseload. These estimates complement the parameter estimates from Gihleb et al. (2018) and Evans et al. (2022), which are informative about the effects of specific supply-side opioid policies on foster care entry and child maltreatment, respectively. Dallman (2020) uses the one-time shock of the OxyContin reformulation as an instrument to estimate the effect of opioid abuse on foster care entry. In addition to these effects of opioid control policies on children's outcomes that operate through changes in parents' use of licit and illicit opioids, we would also like to know the direct effects of parental PO misuse and illicit opioid use on those outcomes. Related to this point, the identifying variation in this paper relies on two dimensions of initial exposure to the opioid crisis, as captured by the disproportionate prescribing of oxycodone and overall PO density in 2000. Previous work has shown that this initial exposure had enduring impacts on the trajectory of opioid abuse and overdose deaths across localities (Alpert et al. 2019). The IV estimates are therefore the effects on foster care entry or caseload of post-2010 changes in illicit opioid use and PO misuse induced by exposure to the supply of POs a decade earlier.

The results add to the two existing studies on foster care admissions by highlighting the role of this exposure at the beginning of the crisis in generating changes in foster care dynamics.

Third, I leverage county-level variation in foster care trends. Given that some of the largest child welfare systems in the U.S. are administered at the county level (e.g. Los Angeles County Department of Children and Family Services, Administration for Children's Services in New York City), with differences across counties in funding, policymaking, and worker training (Child Welfare Information Gateway 2018; Slack and Berger 2020), prior work using state-level variation in foster care admissions (Gihleb et al. 2018; Dallman 2020) may miss important cross-county heterogeneity in foster care trends. Similarly, as will be shown below in the first stage results, there is substantial variation across counties in rates of PO and illicit opioid deaths in both levels and changes. Estimating the IV model using state-level data reveals that cross-state variation in the instruments does not allow the first stage relationship to be precisely estimated, indicating that my empirical strategy requires county-level variation in baseline PO supply exposure and opioid overdose death.

Finally, in addition to foster care entry, the outcome that has been the sole focus of prior studies, this paper provides results on a wider set of outcomes and also finds several dimensions of heterogeneity. I examine entry, caseload, and the continuation rates of past entry cohorts (i.e., the share of entry cohorts from prior years that remain in care by the end of the current fiscal year). The caseload, which is the stock of children who have not been discharged by the end of a given fiscal year, measures the net burden on the foster care system at a point in time. By estimating effects on the continuation rates of past entry cohorts, this paper shows that increases in the caseload due to increased illicit opioid use is driven entirely by new entries into foster care. Examining all three outcomes within the same IV framework provides a fuller picture of how the opioid crisis has affected the dynamics of foster care beyond the entry margin. This study is also the first to document differential impacts of illicit opioid use and PO misuse on entry to kinship care versus non-relative placements and heterogeneity by gender-specific illicit opioid use.

This paper also relates to a broader literature on the determinants of foster care placement and child maltreatment. Factors that have been identified include welfare generosity (Paxson and Waldfogel 2003; Swann and Sylvester 2006), labor market conditions (Lindo et al. 2018), family resources (Paxson and Waldfogel 2002), female incarceration (Swann and Sylvester 2006), and abortion availability (Bitler and Zavodny 2002, 2004). In addition to the studies already discussed linking the opioid crisis to foster care entry and child maltreatment, Cunningham and Finlay (2012) find that methamphetamine use increased foster care admissions in the late 1990s. This paper contributes to this literature by identifying parental illicit opioid use as an important contributing factor to the rise in foster care entry and caseload after 2010.

The findings of this paper point to an important negative externality of adult illicit opioid use: the entry of children into foster care and a corresponding increase in the caseload burden. While children who enter foster care make up a small share of all children who are subject to an investigated maltreatment report[8] and an even smaller share of the overall

[8]During 2010 to 2018, children entering foster care made up 7.5%-9.1% of all children who were subject to an investigated maltreatment report, meaning they received an investigation or alternative response from a Child Protective Services agency. Between 2.8 and 3.5 million children were subject to an investigated

child population,[9] they are a policy-relevant group because they are at particularly high risk of adverse outcomes such as increased rates of involvement with juvenile justice and adult criminal justice systems (Doyle 2007, 2008) and increased likelihood of experiencing homelessness (Dworsky et al. 2013) and mental health problems (Clausen et al. 1998), relative to nationally representative samples. The causal link between parental illicit opioid use and foster care entry may be addressed by policies and programs targeting families involved in the child welfare system due to parental substance use. For example, the Regional Partnership Grant Program uses competitive federal funding to support partnerships between child welfare agencies and organizations providing substance use disorder treatment in order to improve the outcomes of children who were in, or at risk of, out-of-home placement due to parental substance use (U.S. DHHS 2019). Another promising direction is legislation passed in 2018 (discussed in Section 1.2.1) that allows local child welfare agencies to use federal funding more flexibly, including for substance abuse and mental health treatment for at-risk families in order to prevent the use of foster care. In terms of opioid-related policy, the specific role of illicit opioids identified in this study highlights the need for policies that take into account the clandestine nature of markets for illicit opioids. Given the relatively limited scope for supply-side intervention in these markets, it may be useful to consider demand-side policies such as expanding insurance coverage for Substance Use Disorder (SUD) treatment.

report, based on tabulation of National Child Abuse and Neglect Data System (NCANDS) Child File data by Kids Count Data Center.

[9] Children entering foster care during 2010-18 made up 0.3%-0.4% of the total child population ages 0-17, and the population of children in foster care made up 0.5%-0.6% of the total child population.

1.2 Background

1.2.1 Foster care

Foster care is a social service program that provides temporary, substitute, out-of-home care for children experiencing abuse and neglect. When an allegation of child maltreatment is reported to state or local child welfare services[10], the allegation is first screened during an intake process. Those that are screened in may be investigated by a caseworker to determine if abuse or neglect has occurred and if there is risk of child maltreatment in the future. If there are safety concerns for the child that cannot be addressed by a safety plan and there is significant risk of child maltreatment, then the child is removed from the home and placed in foster care. The child may be placed with relatives (kinship foster care), with unrelated foster parents, in a group home, or in a residential treatment center. Placement in a foster family home with relatives or non-relatives are the most common, respectively accounting for 33% and 46% of entries during FY 2010-18. Among children who exited during this period, the median length of stay was 1.05 years. Children may re-enter foster care multiple times, and may live in more than one foster family home during a single placement. Foster care is a major component of the overarching U.S. child welfare system, which is responsible for identifying, responding to, and addressing alleged acts of maltreatment, guided by a three-part federal mandate to promote the safety, permanency (whether with the family of origin or through out-of-home care), and well-being of children (Berger and Slack 2020).

[10]Reporters may be legally obligated to report suspected maltreatment, e.g., teachers, health care workers, and law enforcement, or may be voluntary.

Foster care services are provided by state and county Child Protective Services (CPS) agencies, under the guidance of federal mandates and with partial federal reimbursement of expenditures. Thirty-nine states, Washington, D.C., and Puerto Rico operate state-administered systems, nine states have county-administered, state-supervised systems, and two states have hybrid systems where CPS functions are divided between the state and counties.[11] As a result, child welfare practice and policy vary substantially across states and, in some cases, across counties within a state.[12] In both county-administered and state-administered systems, there is anecdotal evidence of considerable variation across counties in screening (whether a report of maltreatment is investigated by CPS), substantiation (whether the alleged maltreatment is substantiated after an investigation), and the decision to remove a child (Geen and Tumlin 1999). This gives rise to heterogeneity in foster care entry and caseload trends, which I am able to capture through a county-level analysis. Foster care costs make up a large share of child welfare expenditures: in state fiscal year (SFY) 2018, state and local child welfare agencies spent $33 billion using a combination of federal, state, local, and other funds, nearly half of which was spent on out-of-home placements, compared with 19% of expenditures spent on adoption and guardianship and 15% on preventive services (Rosinsky et al. 2021). The federal government reimburses states for a portion of costs associated with foster care services for eligible children through the Title IV-E Foster Care Program. In SFY 2018, states spent $2.6 billion in federal IV-E Foster Care Program funds,

[11]The nine states with county-administered systems are California, Colorado, Minnesota, New York, North Carolina, North Dakota, Ohio, Pennsylvania, and Virginia. Nevada and Wisconsin are the two states with hybrid systems.

[12]Even in state-administered systems, CPS is generally organized at the county level, since counties carry out activities such as case-level decision-making and supervision (Font and Maguire-Jack 2019).

which are used for foster care maintenance payments and administrative costs.

Between 1985 and 1996, the number of U.S. children living in foster care sharply increased from 276,000 to over 500,000. Several factors have been cited as causes of the increases in caseloads during this period, including the crack cocaine and HIV/AIDS epidemics in the late 1980s (U.S. DHHS 1999; Groze et al. 1994), methamphetamine use over the 1990s (Cunningham and Finlay 2012), and higher rates of female incarceration and decreases in welfare benefits as a result of welfare reform (Swann and Sylvester 2006). In response to this growth in the foster care population and to the concern that children were remaining in foster care for long periods or experiencing multiple placements, Congress enacted the Adoption and Safe Families Act (ASFA) in 1997. ASFA intended to shorten time to permanency by establishing timelines for moving children out of foster care, and emphasized the health and safety of the child as the paramount factor in considering placement options (O'Flynn 1999; U.S. DHHS). During the next decade, the number of children living in foster care fell by about 30% before reaching a historic low of 375,000 in 2012. This decline may have been partly due to changes in child welfare practice and state system reforms in response to the requirements of ASFA (Golden and Macomber 2009). The foster care population then steadily increased to 428,000 by 2017 before declining slightly to 425,000 in 2018. My empirical analysis focuses on this most recent period of increase in foster care caseloads and admissions. Although relatively small when placed within the long-term trend in the size of the foster care population, understanding the causes of this recent increase is an important policy question, given substantial public expenditures on foster care and direct effects of foster care placement on children's outcomes later in life, which are briefly summarized below.

In addition to incurring direct public expenditures of nearly $15 billion, the social cost of foster care placement also includes the effects of foster care on children's later life outcomes independent of the environmental factors that contributed to their entry. Using an IV approach exploiting the fact that most allegations of child maltreatment are assigned to investigators rotationally and that the propensity to recommend removal varies across investigators, Doyle (2007) finds that for children on the margin of removal–i.e., those who are induced into foster care due to investigator assignment–placement into foster care increases rates of juvenile delinquency and teenage motherhood and reduces earnings in young adulthood. Doyle (2008) uses the same estimation strategy to examine effects on adult crime and finds higher rates of arrest, conviction, and imprisonment for children who are placed into foster care. Warburton et al. (2014) use a similar IV based on case worker administrative discretion, complemented by a different source of plausibly exogenous variation where the child removal rate abruptly and substantially increased following a high-profile judicial inquiry. Both instruments indicate that for 16- to 18-year old males, placement into foster care reduces the likelihood of high school graduation and increases welfare receipt as young adults, although the marginal at-risk child to whom these effects apply is likely different across the two instruments. The results of these studies apply to children who are on the margin of entering foster care, and it is unknown how different this population may be from the population of children entering foster care as a result of the opioid crisis. To the extent that these are the only studies to address endogeneity problems in estimating the effects of foster care by using quasi-experimental variation, and so long as the overlap between the two child populations is not zero, the IV estimates are still informative about the potential social (and private) costs associated with increased foster care entry in the form of negative effects on medium- and long-term child outcomes.

Finally, it is worth noting that the time period covered by the empirical analysis is one in which major reforms to the child welfare system were beginning to be implemented, reflecting a shift in emphasis toward preventing child maltreatment and foster care placement. At the federal level, the Family First Prevention Services Act (FFPSA) was a spending bill signed into law in February 2018, representing the most significant change to federal child welfare finance since the establishment of the Title IV-E entitlement in 1980. Federal Title IV-E funding previously could be used only for foster care and adoption assistance expenses. Under the new law, funds can be used for 12 months of services aimed at preventing the use of foster care for a wide range of families identified as being at risk of child removal. There are three areas of service: substance abuse treatment, mental health services, and in-home parent skill-based programs. This flexibility allows child welfare agencies to serve families that have not been reported, which is a significant departure from traditional policy and practice (Berger and Slack 2020). At the state level, 20 states implemented Title IV-E child welfare waiver demonstration projects between 2012 and 2019 that included foster care prevention services, with one of the explicit goals being to decrease entry and re-entry rates.[13]

[13] Waivers allow states to use federal funds more flexibly to test different approaches to child welfare service delivery and financing. https://www.acf.hhs.gov/cb/programs/child-welfare-waivers/active-projects

The geographic scope of these projects varied across states, with interventions rolling out only in a number of counties in some states and statewide in others.

1.2.2 Related evidence

A small set of descriptive studies using county- or state-level panel data has documented a positive relationship between substance use and child maltreatment. Ghertner et al. (2018) estimate significant positive associations of the all-drug overdose death rate and drug-related hospitalization rate with multiple child maltreatment indicators, including rates of foster care entry and child maltreatment reports, controlling for county characteristics and state prescription opioid policies. The unique contributions of this study are interviews with practitioners and administrators in child welfare and substance use treatment. Through these interviews, the authors find that professionals working in the field perceive the increase in foster care placements to be in large part linked to parental substance abuse. Caseworkers also report higher degrees of child neglect and difficulty finding kinship care in cases involving parental opioid use. Quast (2018) estimates state-specific associations between foster care admissions and the opioid prescribing rate over 2010-15 and finds substantial variation in the magnitude and direction of state-level associations. This may be due to differences across states in the existence and nature of a legal basis for removing children from the home due to parental drug use. Anderson (2019) estimates a positive relationship between the incidence of grandparents raising grandchildren and the opioid prescription rate using state-level data from the ACS, but the association becomes insignificant using county-level data.

Economists have recently begun to study how children and families have been impacted by both the opioid crisis and by policies aimed at mitigating the crisis, with somewhat mixed findings. Buckles et al. (2020) study the impact of the drug crisis over the past three decades on children's living arrangements using data from the CPS-ASEC, focusing on rates of children without their mother or without their father in the household and the rate of children living in a household headed by a grandparent. Given that a child's current living arrangement is a function of her stock of prior experiences, exposure to the drug crisis is measured as the cumulative number of drug deaths of likely parents over the course of a child's life up until when she is observed in the survey, normalized by the number of likely parents in her birth year. Variation across states and over time in children's exposure to triplicate prescription laws is then used to instrument for cumulative exposure to the drug crisis.[14] The two-stage least squares (2SLS) estimates imply that had the drug crisis stayed at 1996 levels, there would be 1.5 million fewer children living away from at least one parent and half a million fewer children living in a household headed by a grandparent in 2015.

Several other studies take a more indirect approach and examine the effects of various supply-side opioid control policies on a range of child and family outcomes, including foster care admissions, child maltreatment, the incidence of neonatal abstinence syndrome and other infant health measures, and maternal behaviors. One type of policy examined by multiple studies is state prescription drug monitoring programs (PDMPs), electronic databases that contain provider and patient prescription histories involving controlled substances.

[14]State triplicate programs were an early form of prescription drug monitoring implemented between 1939 and 1982. OxyContin was marketed much less aggressively in states with triplicate programs, leading to lower growth in oxycodone use and drug overdose deaths in triplicate states.

Researchers have also focused on the OxyContin reformulation, which sharply reduced the supply of abusable prescription opioids, and "pill mill" laws, which impose strict regulations on pain management clinics in an effort to curb opioid prescribing without medical indication. Estimating the effects of opioid control policies on child outcomes is an indirect approach to the question of how children have been impacted by the opioid crisis because, with the exception of Ziedan and Kaestner (2020) and Dallman (2020), these studies do not provide evidence that the policies in question affect the severity of the opiois crisis in their sample (although other prior work has shown that mandatory-access PDMPs and the OxyContin reformulation significantly affected opioid prescribing/supply/overdose deaths), but the implicit assumption is that the observed effect of opioid control policies on child welfare is through changes in prescription opioid misuse and illicit opioid use.

Evans et al. (2022) estimate the effects of two supply-side interventions, the OxyContin reformulation and must-access PDMPs, on alleged and substantiated child maltreatment. The effect of the reformulation is identified using event study and trend-break specifications that leverage cross-county variation in pre-intervention exposure to Rx opioids, which is proxied by the population-weighted mean number of opioid prescriptions per capita in a county over the four years prior to the reformulation. The authors find a significant positive effect on the number of children with substantiated physical abuse or neglect (an increase of 13% relative to the mean for a 1 sd increase in initial opioid prescription per capita), and also provide some evidence that effects are concentrated in counties without access to therapeutic substitutes. The difference-in-differences (DiD) model used to estimate the effect of must-access PDMP implementation shows increases in both alleged and substantiated maltreatment, with larger effects in a subsample of counties with above-median pre-intervention Rx opioid exposure. This result is at odds with Gihleb et al. (2018), which finds that must-access PDMPs reduced foster care admissions by 10%. Evans et al. (2022) suggest that the divergence may be explained by differences in the assignment of states to treatment status and the timing of treatment status. Dallman (2020) uses an alternative empirical strategy to examine foster care admissions citing parental drug use. He uses the OxyContin reformulation supply shock to instrument for opioid abuse rates, proxied by opioid abuse treatment admission rates. The state-level supply of oxycodone in 2010 generates variation in pre-reformulation exposure to POs. He finds that a 10% increase in the opioid abuse treatment admission rate caused an additional 2.4 foster care entries involving parental drug use per 100,000 children.

In addition to child maltreatment and foster care admissions, prior work has also examined the effects of PDMPs on infant health and outcomes during pregnancy. Using a DiD analysis of opioid control policies and an instrumental variables approach where opioid control policies instrument for PO use, Ziedan and Kaestner (2020) find that reductions in PO use (proxied by PO sales) as a result of state opioid control policies led to modest improvement in infant health in terms of birth weight and the incidence of low birth weight, and also increased prenatal care. Consistent with the result that PDMPs improve infant health, Gihleb et al. (2019) estimate that the adoption of PDMPs reduced the incidence of neonatal abstinence syndrome by 10%. However, they find small or null effects on the incidence of low birth weight, premature births, and infant mortality.

1.3 Empirical Framework

The main objective of this paper is to characterize the extent to which the opioid crisis, and specifically parental opioid abuse, contributed to the post-2010 increase in foster care admissions and caseloads. I examine annual flows (entries) into care and annual caseload, which is determined by flows into and out of foster care. New entries are the most appropriate measure of external stress to the foster care system: they arise when child welfare agencies respond to changes in children's home environments. I also examine annual caseload in order to understand the impact of the opioid crisis on the size of the foster care population. One reason for focusing on the caseload rather than exits is that discharges are more heavily influenced by internal agency processes and practices than admissions,[15] and therefore do not properly reflect the same socioeconomic changes that may have contributed to foster care entry.

To estimate the effects of parental illicit opioid use and prescription opioid misuse on foster care entry and caseloads, consider a specification relating the change in foster care entry/caseload to changes in the two types of parental opioid misuse over a fixed period relative to averaged levels over 2010-11 (i.e. the long difference relative to the average over

[15]For example, exits consistently spike in the months leading up to summer, likely reflecting a preference for delaying discharge of children until after the school year ends (Wulczyn et al. 2007).

years 2010-11):

$$\Delta_{\bar{t}} Y_c = \beta_1 \Delta_{\bar{t}} \text{IllicitOpUse}_c + \beta_2 \Delta_{\bar{t}} \text{RxOpMisuse}_c + \Delta_{\bar{t}} X_c' \theta + \Delta_{\bar{t}} \eta_c \tag{1.1}$$

Y_c is the inverse hyperbolic sine (IHS) of the number of foster care admissions or caseloads in county c. The inverse hyperbolic sine transformation is applied to foster care entry and caseload counts in order to reduce the substantial right-skewness of the count variable distributions while preserving the zeros.[16] IllicitOpUse and RxOpMisuse are the rates of illicit opioid use and prescription opioid misuse among parents with dependent children, respectively. X is a vector of time-varying county and state characteristics that may also affect foster care entry and caseload. $\Delta_{\bar{t}}$ denotes the difference between averaged levels over years 2010-11 and averaged levels over pairs of endpoint years $\bar{t}$. For the main results, I use the long difference between averaged levels over 2010-11 and averaged levels over three pairs of endpoint years at the end of the sample period: averaged levels over 2015-16, 2016-17, and 2017-18.

Examining changes relative to 2010-11 is motivated by two trends previously discussed. 2010 was a turning point in the opioid crisis: overdose deaths involving heroin began to rapidly increase, followed by synthetic opioids starting in 2013 (CDC 2021b). In comparison, the rate of drug overdose deaths involving prescription opioids plateaued after 2010 and has declined slightly since 2016, although the magnitude of this decline is small compared to the

[16]This is consistent with prior studies of foster care admissions and caseload, which have applied log or IHS transformations to foster care admission or caseload counts (Paxson and Waldfogel 2003; Cunningham and Finlay 2012; Gihleb et al. 2018).

magnitude of the increase in the illicit opioid overdose death rate. To the extent that illicit opioid use and PO misuse may have different effects on parents' capacity to care for their children and the risk of foster care entry, focusing on this period with both types of opioid abuse is relevant for understanding how the opioid crisis has impacted the foster care system. Second, foster care entry grew rapidly from the mid-1980s to the late 1990s before starting to decline in 2000. Fig 1.2 shows that this decline continued for a decade before admissions began to rise again. 2010-11 appears to be the inflection point marking the beginning of this most recent increase in admissions, which has been hypothesized to be causally linked to parental opioid misuse. Moreover, zooming in on the post-2010 period, the increase in the admissions rate coincided with the rise of illicit opioid overdose deaths.

In the main results, long differences are taken between averaged levels 2010-11 and averaged levels over pairs of endpoint years to account for noise in the timing of the realization of the hypothesized effects. For example, consider a parent who begins to abuse opioids at some time x. There is inherent randomness in when the opioid abuse and its associated consequences may lead to a maltreatment allegation being reported to CPS. Following the report being made, CPS will need to screen the allegation, investigate the case, determine whether the maltreatment is substantiated, and if so, whether to remove the child from the home. These steps introduce additional uncertainty in the timing of actual entry into foster care. All else equal, if x is closer to the end of the year, the effect on foster care entry is unlikely to be realized within the year. Averaging levels over two (or more) consecutive years reduces the noise in timing and increases the probability of observing changes to foster care entry and caseload caused by changes in parental opioid abuse, relative to taking the long difference between annual levels. Although the main results use the long difference between averaged levels over pairs of endpoint years, results are qualitatively similar when annual levels are used for the long difference (see Appendix Tables A.22 and A.23).

The structural equation in (1.1) is specified in long differences rather than in first differences at an annual frequency, in order to reflect that the parameters of interest are the contributions of parental illicit opioid use and PO misuse to the observed longer-run increase in foster care entry and caseload since 2010-11. Models in levels or first differences are identified using within-county year-to-year changes, which contain substantial transitory noise (especially for an outcome, such as foster care entry, that is rare at the population level) and are unlikely to capture the underlying relationship between parental opioid abuse and foster care admissions. Moreover, as I note in more detail below, prescription and illicit opioid overdose death rates will be used as proxies for unobserved parental PO misuse and illicit opioid use, respectively, introducing measurement error to the independent variables. Assuming a classical measurement error model, estimates obtained from a specification in long-differences should be less inconsistent than those from a first-differences specification.

Two issues arise when (1.1) is estimated by OLS. First, changes in parental opioid abuse may be correlated with trends in other determinants of foster care admissions.[17] For example, counties with larger growth in parental opioid abuse could also experience a general decline in the strength of social institutions, which independently affects foster

[17] Long differences can either exacerbate or reduce this bias, depending on the length of the long difference relative to the lengths of the unobserved trends.

care entry. $Cov(\Delta_{\bar{t}}\eta_c, \Delta_{\bar{t}}\text{IllicitOpUse}_c) \neq 0$ and $Cov(\Delta_{\bar{t}}\eta_c, \Delta_{\bar{t}}\text{RxOpMisuse}_c) \neq 0$ imply that OLS estimates of β_1 and β_2 will be biased. Second, the use of the opioid mortality rate as a proxy for the rate of parental opioid abuse introduces measurement error into the regressors. Indeed, I present evidence in Section 1.4.2 that is consistent with the presence of classical measurement error in my measures of parental opioid abuse.

1.3.1 Instrumental variables estimation strategy

To address these sources of bias, I use an instrumental variables strategy that, via the first stage of two-stage least squares, predicts the change in illicit and prescription opioid overdose death rates between averages 2010-11 and averaged levels at the end of my sample period ($\Delta_{\bar{t}}\text{IllicitOpDeath}_c$ and $\Delta_{\bar{t}}\text{RxOpDeath}_c$, which are proxies for $\Delta_{\bar{t}}\text{IllicitOpUse}_c$ and $\Delta_{\bar{t}}\text{RxOpMisuse}_c$, respectively) using two measures of a county's baseline supply of POs in 2000, at the onset of the opioid crisis. The idea underlying this strategy is that these supply measures, which are observed in an initial year well before the sample period, predict post-2010 trajectories of opioid abuse/overdose deaths, and are plausibly independent of other factors affecting foster care outcomes. The instruments measure initial local exposure to prescription opioids at a time when PO misuse had just begun to ramp up, catalyzing what would eventually become the opioid crisis. I also show evidence below that the first stage has predictive power in part because the instruments are correlated with a county's exposure to subsequent supply-side opioid policies that affected opioid overdose death rates. This provides policy-induced variation in opioid deaths that stems from differences in initial

PO supply and is conditionally exogenous to other determinants of foster care dynamics post-2010.

The instruments used for the main analysis are the following county-level measures:

1. The supply of oxycodone as a share of the total supply of oxycodone and hydrocodone in 2000: $\text{OxyShare}_{c,2000} = \text{oxycodone}_{c,2000}/(\text{oxycodone}_{c,2000} + \text{hydrocodone}_{c,2000})$, and
2. The per-capita supply of oxycodone and hydrocodone combined in 2000 (in milligrams per county resident):

   $\text{RxOpSupply}_{c,2000} = (\text{oxycodone}_{c,2000} + \text{hydrocodone}_{c,2000})/\text{population}_{c,2000}$

Oxycodone is the active ingredient in OxyContin, a brand-name extended-release opioid medication introduced by Purdue Pharma in 1996. Initially falsely marketed as having lower potential for abuse than other oxycodone products due to its timed-release properties, OxyContin became one of the leading drugs of abuse in the U.S. by the mid-2000s and has been identified as a key factor in the increasing prevalence of prescription opioid abuse and overdose deaths during the past two decades.[18] In April 2010, Purdue Pharma introduced a reformulated version of OxyContin which was designed to be abuse-deterrent, and only the reformulated version has been distributed to pharmacies since August 2010. The introduction of abuse-deterrent OxyContin and subsequent discontinuation of the original formulation represented a large national shock to the supply of abusable prescription opioids. Fig 1.3 shows that the supply of oxycodone increased steadily starting in the early 2000s and first began to decline in 2010 Q2, coinciding exactly with the reformulation. Moreover, the

[18]See Alpert et al. (2018), Alpert et al. (2019), and references therein for more background on the rise of OxyContin abuse and Purdue Pharma's marketing tactics to promote OxyContin for treating chronic non-cancer pain. For a journalistic account of the history of Purdue Pharma and its role in the opioid crisis, see Meier (2018).

supply of all POs tracks the trend in the oxycodone supply; oxycodone accounted for 35% of all POs on average during 2002-17, which is larger than the average share of any single other opioid medication. A larger value of $\text{OxyShare}_{c,2000}$ indicates that oxycodone was disproportionately prescribed in a county relative to hydrocodone, a clinical substitute that is also commonly misused.[19] This measure therefore reflects differential cross-county exposure to disproportionate prescribing of one of the most commonly abused POs, as well as exposure to the OxyContin reformulation, which specifically reduced the supply of oxycodone.

The other baseline measure, the per-capita supply of oxycodone plus hydrocodone, is a proxy for the supply density of all prescription opioids in 2000.[20] Counties with relatively higher initial PO density were more exposed to POs overall in 2000. They may also be more affected by state-level supply control policies targeting opioids generally, such as PDMPs and pain clinic regulations. To the extent that the OxyContin reformulation reduced the total supply of abusable POs, initial PO density should also reflect differential exposure to the reformulation. The two instruments capture cross-county variation in different dimensions of initial exposure to the supply of prescription opioids (which in turn is correlated with exposure to subsequent supply-side opioid control policies), which translates to different post-2010 trends in illicit opioid and PO-only overdose deaths.[21]

[19] A study of 3,520 opioid-dependent individuals entering drug treatment programs found that oxycodone and hydrocodone were the drugs of choice in 75% of all patients (Cicero et al. 2013).

[20] Only oxycodone and hydrocodone were reported in ARCOS for the year 2000, the earliest publicly available year of data. Reports for years 2001-2017 contain a larger set of opioids. Oxycodone and hydrocodone combined accounted for the majority of the prescription opioid supply during this time, ranging from 53% to 62%. To check that results are not sensitive to any reporting peculiarities in 2000, I also use ARCOS data from 2001, the earliest year with the larger set of opioids, to construct the baseline supply measures.

[21] The instruments can be thought of as being analogous to the "shares" in the usual shift-share Bartik-style instrument. That is, they measure differential exposure across counties to state- and nation-level supply-side opioid control policies, which are the "shifts" or common shocks. Goldsmith-Pinkham et al. (2020) show that

The geographic distribution of the oxycodone share and PO density in 2000 are shown in Fig 1.4 and 1.5, respectively. Fig 1.4 shows that the spread of the oxycodone share is large, ranging from a low of 9% in Uvalde County, Texas to a high of 88% in Franklin County, Missouri. The share of oxycodone in the local prescription opioid supply is relatively high in counties in the Northeast, upper Midwest, parts of the Southwest, and Missouri and Florida. In contrast, Fig 1.5 shows that the cross-sectional variation in PO supply is driven by a smaller set of counties: the highest levels of PO density in 2000 are concentrated in Kentucky, West Virginia, and the Southeast. Another way to visualize this difference is to compare histograms of the two baseline exposure measures, shown in Fig 1.6. The distribution of initial PO density is right-skewed, with a median density of 91 mg PO per capita but 5% of counties having a PO density above 200 mg per capita. The initial share of oxycodone is approximately normally distributed, with a mean and median of 0.49.

The two baseline PO supply measures are initial county characteristics that predict the trajectory of different phases of the opioid crisis. Counties with high initial PO density are clustered in regions that have been disproportionately affected by the first wave of the crisis, characterized by increasing PO misuse and overdose deaths. This can be observed in Fig 1.8, which plots heat maps of changes in the PO-only overdose death rate during 1999-2010 (left) and 2010-2018 (right). Comparing Fig 1.5 to the left panel shows that areas with higher initial PO density generally experienced larger increases in PO deaths during the first decade of the crisis. This observation raises the concern that initial PO density may be correlated

Bartik-style instruments are numerically equivalent to using the shares as instruments, and argue that the exogeneity condition is therefore best stated in terms of these shares.

with other trends, either pre- or post-2010, that influence child maltreatment and foster care entry. I address this concern by including in the IV models a comprehensive set of factors that may affect foster care outcomes (both post-2010 long-differenced changes and initial levels in 2000), and showing that controlling for such factors does not substantively affect the magnitude and significance of IV estimates. Turning to the oxycodone share instrument, counties with high initial oxycodone share are more spread out throughout the country, reflecting the geographic dispersion of increasing heroin and synthetic opioid deaths that characterizes the second wave of the crisis. This is illustrated in Fig 1.9: while there are clusters of large increases during 2010-2018 in the illicit opioid death rate in the Northeast and Appalachia, much of the rest of the country also experienced substantial increases in illicit opioid deaths. Consistent with these differences in the geographic distribution of the two initial supply measures, their correlation is only 0.12 (Fig 1.7) despite being correlated by construction.

To achieve a strong first stage, the initial PO supply measures in 2000 should predict changes in prescription and illicit opioid overdose death rates starting in 2010. If the initial supply measures are indeed related to policy intensity or exposure, we then need that these various policies affect both types of opioid overdose death differently. The following discussion provides background on two major supply-side policies which are the focus of the analysis in Appendix Section A.2 aiming to elucidate the channels underlying the first stage relationship.

The first supply-side policy is the reformulation of OxyContin in 2010, which has been found to affect illicit and prescription opioid deaths. In particular, Alpert et al. (2018) show that the OxyContin reformulation generated a large differential increase in heroin deaths in states with the highest initial rates of OxyContin misuse beginning in the year following the reformulation, consistent with rapid substitution to heroin by consumers in response to the abrupt reduction in the supply of abusable POs. Similarly, Evans et al. (2019) conclude that the reformulation was largely responsible for the post-2010 increase in the heroin death rate, estimating that each prevented prescription opioid death was offset by an additional heroin death. Given these findings, we would expect counties with higher initial oxycodone share or higher initial PO density to see a larger increase after 2010 in the illicit opioid overdose death rate and a larger reduction in the PO-only overdose death rate.

While the OxyContin reformulation was a national shock to the PO supply, states have implemented a variety of opioid control policies in response to the misuse of prescription opioids. By reducing the supply of opioids that are prescribed and tightening access to POs, state-level opioid control policies have the potential to reduce PO misuse and overdose death, but may also induce greater use of illicit substitutes by some opioid-dependent individuals. The most common type of state-level opioid control policy is a prescription drug monitoring program, a centralized database that records patients' history of prescribed medications containing controlled substances, including prescription opioids. Studies that distinguish between voluntary (or operational) PDMPs, which do not require health care providers to check the database at the time of prescribing or dispensing medication, and mandatory-access PDMPs, which require providers to query the system before writing a prescription, have found that PDMP mandates reduced prescription opioid deaths (Grecu et al. 2019; Meinhofer 2018). However, echoing the finding that the OxyContin reformulation led to substitution toward illicit opioids, mandatory-access PDMPs have been found to increase heroin overdose deaths (Mallatt 2020; Kim 2021).

Empirical evidence that the aforementioned supply-side policies have heterogeneous effects on PO and illicit opioid overdose deaths depending on levels of the initial PO supply measures would bolster our confidence that the first stage relationship is partly due to the instruments reflecting a county's relative exposure to common policy shocks. To directly assess the extent to which the baseline supply measures reflect counties' differential exposure to the OxyContin reformulation and PDMPs during the sample period, I estimate the effect of these policies on opioid overdose death rates, following empirical approaches used in the literature and allowing the effects to vary by either the initial oxycodone share or initial PO density. The specifications and detailed results are presented in Appendix Section A.2.

Having established that levels of the baseline PO supply measures are expected to be correlated with differential post-2010 changes in illicit and prescription opioid deaths across counties, the first stage equations are:

$$\begin{aligned} \Delta_{\bar{t}}\text{IllicitOpDeath}_c &= \alpha_1 \text{OxyShare}_{c,2000} + \alpha_2 \text{RxDensity}_{c,2000} + \Delta_{\bar{t}} X_c' \psi_1 + X_{c,2000}' \psi_2 + \Delta_{\bar{t}} u_c \\ \Delta_{\bar{t}}\text{RxOpDeath}_c &= \gamma_1 \text{OxyShare}_{c,2000} + \gamma_2 \text{RxDensity}_{c,2000} + \Delta_{\bar{t}} X_c' \lambda_1 + X_{c,2000}' \lambda_2 + \Delta_{\bar{t}} v_c \end{aligned} \tag{1.2}$$

As before, $\Delta_{\bar{t}}$ denotes the long difference between averaged levels 2010-11 and averaged levels over pairs of endpoint years $\bar{t}$. IllicitOpDeath and RxOpDeath are drug overdose deaths involving any illicit opioid (heroin or synthetic opioids) and involving prescription opioids only, respectively, per 100,000 population ages 18-57. The opioid overdose death rates are proxies for rates of illicit opioid use and PO misuse among parents, the unobserved independent variables of interest. The second stage (structural) equation is

$$\Delta_{\bar{t}} Y_c = \kappa_1 \Delta_{\bar{t}} \text{IllicitOpDeath}_c + \kappa_2 \Delta_{\bar{t}} \text{RxOpDeath}_c + \Delta_{\bar{t}} X_c' \phi_1 + X_{c,2000}' \phi_2 + \Delta_{\bar{t}} \epsilon_c \qquad (1.3)$$

$\Delta_{\bar{t}} Y_c$ is the long difference of (the inverse hyperbolic sine of) foster care entry and caseload counts.[22] The IV model also controls for a large set of (long-differenced) county and state level characteristics that potentially affect foster care dynamics in the vector $\Delta_t X_c$, and for the initial levels of these characteristics in 2000 in the vector $X_{c,2000}$. Equation (1.3) is estimated by 2SLS, where the equations in (1.2) are the first stage equations. Heteroskedasticity-consistent standard errors are used, and each observation is weighted by the county population ages 0-17 in 2018.

A causal interpretation of the IV estimates requires instrument relevance and satisfying the exclusion restriction. Instrument relevance can be empirically verified by examining the magnitude and direction of the first stage estimates; I show in the results that the initial PO supply measures are significantly predictive of post-2010 changes in illicit and prescription opioid death rates, and that the directions of the coefficients are largely consistent with prior findings on the effects of opioid control policies on overdose death rates.

The exclusion restriction requires that the instruments affect foster care outcomes only

[22] The direction, significance, and pattern of IV coefficients are similar if rates (number of entries per 1,000 population under age 18) are used instead. See Appendix Table A.20.

through their effects on parental illicit opioid use and PO misuse. While baseline measures of PO supply should not have a direct effect on outcomes since the supply measures are from the year 2000, they might be correlated with other determinants of foster care outcomes. We may be especially concerned that the initial PO supply measures may be correlated with other time-varying county characteristics that could independently affect child neglect or abuse and subsequent placement in foster care. I address this first by including a rich set of covariates that are intended to capture factors that plausibly affect child maltreatment and foster care outcomes and that may also be correlated with a county's initial prescription opioid supply density and oxycodone share. The IV model controls for (1) the long-differenced change in these covariates relative to 2010-11, to account for differential trends during the sample period that may be correlated with baseline exposure measures, and (2) the initial levels of these covariates in the year 2000, to account for differences in initial conditions that may influence the subsequent evolution of foster care admissions and caseload. I also show in Appendix Section A.4.1 that in a subsample where the first stage is zero, the reduced-form relationship between the initial exposure measures and foster care outcomes is also zero. This bolsters the plausibility of the identifying assumption.

Five categories of controls are included in the preferred specification. (1) Differences across counties in demographic composition are controlled for by county population shares of age groups 0-19, 20-24, 55-64, 65+, share female, share non-white, and log of the population ages 0-17. (2) The county unemployment rate, labor force participation (LFP) rate, and child poverty rate control for differences in economic conditions. The share of adults with own children under age 18 in the state who are below twice the poverty threshold and who are also single parents and the state teenage birth rate proxy for the prevalence of low-income single parents and teenage mothers respectively, both of which increase the risk of foster care entry. (3) The county rate of 100% alcohol-attributable deaths among ages 18-57 proxies for parental alcohol abuse. The density of substance abuse treatment facilities measures the local availability of substance abuse treatment, which may affect (i) measurement error in the explanatory variables by changing the risk of overdose death for a given level of opioid abuse and (ii) use of non-opioid drugs. (4) Public expenditures. The shares of state and local expenditures on public welfare and housing and community development proxy for public spending on the social safety net, which may affect the likelihood of child maltreatment and foster care entry among at-risk families. The share of expenditures on correctional institutions proxies for funding to other service systems that place children into foster care, such as juvenile correctional facilities. (5) The supply of child welfare services and other social assistance are controlled for by the density of (i) child and youth services establishments, which include adoption agencies and foster care placement services, and (ii) community food services, temporary shelters, and emergency and other relief services. These covariates were chosen based on prior findings on the determinants of foster care entry[23] as

[23]Reduced welfare benefits as a result of welfare reform in the 1990s have been shown to increase the number of children in foster care and the incidence of child maltreatment (Paxson and Waldfogel 2003; Swann and Sylvester 2006). Labor market conditions and family resources matter for child maltreatment: Lindo et al. (2018) find that male employment is associated with reductions in child maltreatment, and female employment is associated with increases in child maltreatment. Paxson and Waldfogel (2002) show that the share of families with two non-working parents and those with incomes below 75% of the poverty line are positively associated with rates of child maltreatment. Additionally, Swann and Sylvester (2006) identify higher rates of female incarceration as contributing to the rise in foster care admissions during the 1990s. For more detailed discussions, see Doyle and Aizer (2018) and Barbell and Freundlich (2001).

well as direct examination of the correlation between the baseline exposure measures and trends that potentially affect foster care outcomes. In the main results, I will show that the IV estimates are stable as these sets of covariates are progressively added, suggesting that the estimated effects are not being driven by correlation between the instruments and a large set of observable characteristics.

1.4 Data and Measurement

The empirical analysis uses administrative case-level foster care data and mortality microdata based on death certificates, both of which are aggregated to the county-year level. These sources are described below in more detail. A variety of data sources, listed in Appendix Section A.1, are used to construct county- and state-level covariates. Additionally, I discuss measurement error in the independent variables in Section 1.4.2.

1.4.1 Foster care

Data on the number of foster care entries and exits are from the Adoption and Foster Care Analysis & Reporting System (AFCARS) 6-month Foster Care Files for FY 2010 Period A-FY 2016 Period B and FY 2015 Period B-FY 2020 Period A,[24] made available by the National Data Archive on Child Abuse and Neglect (NDACAN). Because the two files have some time overlap, I use records from the older file for FY 2010-2015 and records from the newer file for FY 2016-2018, so that I have data spanning FY 2010-2018. AFCARS is a

[24]Data are organized into two 6-month periods per fiscal year. Period A is Oct 1 through March 30; Period B is April 1 through Sept 30. The unit of time in the aggregated foster care data is the fiscal year.

federally mandated data collection system: states are required to collect data on all children served by the foster care system during the federal fiscal year. The Foster Care Files provide case-level information on child demographics, dates of removal and discharge, reasons for removal, and the county that is responsible for the case. I aggregate the case-level data to county-year counts of admissions and caseloads ages 0-17. Annual admissions is defined as the number of children whose date of most recent removal was during the fiscal year. Annual caseload is defined as the number of children who entered care prior to the end of the current fiscal year and have not been discharged from their latest foster care spell by the end of the current fiscal year, as reported for the second semiannual reporting period. Counties with fewer than 1,000 records are not identified to protect confidentiality, so the aggregated data cover the subset of counties that are identified in the case-level files every year of the sample period. The final foster care data is a balanced panel of 1,193 counties during FY 2010-2018.[25] These counties account for 86% of the U.S. population under age 18 and 87% of foster care entries under age 18. This county-year dataset is used to construct the outcomes of interest, IHS foster care entry and caseload, and entry and caseload rates (count per 1,000 population ages 0-17).

Table 1.1 presents descriptive statistics of the case-level data for children who entered foster care in the subset of identified counties at any point during FY 2010-18, and for entries in all counties during FY 2010-18. The average characteristics of children entering care in

[25] An additional reason for focusing on years 2010 and later is the availability of foster care data pre-2010. While the foster care data extend back to 2000, a different criterion was used in AFCARS to mask county identifiers in years prior to 2010, resulting in a much larger number of counties not being identified in those years. I construct a balanced panel of counties spanning 2000-2018; this sample contains only 115 counties, which is 10% of the counties in my main estimation sample. I repeat the analysis using the restricted sample going back to 2000 and find an insignificant first stage, partly due to being under-powered.

the subset of counties are very similar to that of the overall population of children entering care; entries in identified counties are slightly younger and more likely to be Black. 29% of children entering care are Black, and 22% are of Hispanic origin. The average age at latest removal is just under 7 years old, and children are removed an average of 1.3 times, including the current removal. Title IV-E foster care maintenance payments are being paid on behalf of 29% of entries.[26] The most commonly cited reasons for removal are neglect (60% of entries) and parental drug use (31% of entries).[27]

1.4.2 Opioid overdose deaths and initial exposure measures

The independent variables of interest are rates of prescription opioid misuse and illicit opioid use among parents with dependent children. Since parental opioid abuse is not observed, drug poisoning deaths per 100,000 population involving (1) natural and semi-synthetic opioids (e.g. oxycodone and hydrocodone) alone and (2) heroin or synthetic opioids excluding methadone (e.g. fentanyl and its analogs) among prime parenting ages are used to proxy for parental prescription opioid misuse and illicit opioid use, respectively. Prime parenting ages are defined as ages 18-57. This age range captures the majority of individuals in their prime parenting years, i.e., having a child aged 0-17. In 2018, 97% of all births were

[26]To be eligible for federal reimbursement under the Title IV-E Foster Care Program, a child must have been removed from a family considered "needy" based on criteria established in 1996 under the Aid to Families with Dependent Children program. Nationally, less than half of children in out-of-home placements are covered under Title IV-E (Rosinsky et al. 2021). In contrast, nearly all children in foster care are categorically eligible for Medicaid.

[27]Reasons for latest removal are indicated by caseworkers. An entry can have multiple reasons or no reasons, so shares add up to more than 1. Additional reasons not listed in the table are: sexual abuse, death of a parent, abandonment, relinquishment, and reasons related to the child such as behavioral problems and disabilities.

to mothers between 18 and 40 years old; 94% of births in which the father's age is known were to fathers between 20 and 44 years old.[28] I use the range of mother's age at birth to define the bounds of the prime parenting age range.

Annual county-level opioid type-specific rates of overdose deaths are calculated using Multiple Cause of Death (MCD) microdata for years 1999-2018, which were obtained by application to the National Center for Health Statistics (NCHS). Each record in the microdata is based on information extracted from death certificates filed in the vital statistics office of each state and D.C., so these data provide a census of deaths for U.S. residents. Each death record has a single underlying cause of death and up to twenty additional multiple causes, classified using the Tenth Revision of the International Classification of Disease (ICD-10), demographic information, and counties of occurrence and residence. I follow the CDC and prior literature in identifying opioid overdose deaths. First, drug poisoning deaths are identified as those with an underlying cause of death ICD-10 code of X40-44, X60-64, X85, or Y10-14. There are four opioid-related drug identification codes: T40.1 - heroin, T40.2 - natural and semi-synthetic opioids, T40.3 - methadone, and T40.4 - synthetic opioids excluding methadone. Illicit opioid overdose deaths are defined as drug poisoning deaths with a multiple cause of death ICD-10 code of T40.1 or T40.4. Prescription opioid-only overdose deaths are identified as drug poisoning deaths where the only opioid-related multiple cause of death code is T40.2. The reason for coding PO-only overdose death in this particular way is that from 2012 to 2018, the share of opioid overdose deaths involving more than one

[28]These numbers are from NCHS Vital Statistics Natality public-use data 2018, accessed via CDC WONDER Online Database.

opioid type increased from 12% to 32%; two-thirds of this increase in multi-opioid deaths was accounted for by deaths involving heroin and synthetic opioids, and 32% was accounted for by deaths involving natural/semi-synthetic opioids in combination with heroin or synthetic opioids (combinations involving methadone accounted for the remaining 1%). If PO overdose death were not restricted to the natural/semi-synthetic opioid code alone, it would also be partially picking up illicit opioid use.[29] Given that I emphasize the distinction between PO misuse and illicit opioid use and allow them to have different effects on foster care dynamics, it is important to exclude heroin and synthetic opioids from PO overdose deaths.[30] I aggregate the microdata to annual county-level counts of each type of opioid overdose death ages 18-57, and then use single year of age county population estimates from the National Cancer Institute's SEER program to calculate the rate of death per 100,000 population.

Initial prescription opioid supply measures. Data on the retail distribution of opioids are used to construct the instruments, the oxycodone share and the per capita supply of POs in 2000. These data come from the Drug Enforcement Administration (DEA)'s Automation of Reports and Consolidated Orders System (ARCOS), an automated and comprehensive drug reporting system where manufacturers and distributors of Schedule I (e.g. heroin) and II (e.g. oxycodone, hydrocodone, fentanyl) materials are required to report their controlled substances transactions. Supply data by 3-digit ZIP code for every state and D.C. are available annually starting in 2000. Only oxycodone and hydrocodone were reported in ARCOS in 2000; beginning in 2001, reports contain data for a broader set of

[29]The analogous argument can be made for restricting illicit opioid death to only heroin and synthetic opioid codes. Results are very similar when this definition of illicit opioid death is used.

[30]Acknowledging that the absence of heroin and synthetic opioids reported in an overdose death does not necessarily indicate that these drugs were not used.

opioids (meperidine, fentanyl base, oxycodone, hydrocodone, morphine, hydromorphone). To convert the 3-digit ZIP level supply data to county level, I use a geographic correspondence file based on 2000 census geography and a population-weighted allocation factor.[31]

Summary statistics of key regression variables. Table 1.2 shows the cross-county means of these long-differenced variables. The first two rows show the means of ΔIHS FC entry and ΔIHS FC caseload, which confirm the national time trends: foster care admissions and caseload increased after 2010-11, reached a peak in 2016-17, and then declined slightly. Rates per 1,000 children show similar trends. The illicit opioid death rate grew rapidly: the average change between 2010-11 and 2012-13 was 1.7 deaths per 100,000, and by 2017-18 the average change was 16 deaths per 100,000. In contrast, the PO-only death rate slowly declined during this period. We can also get a sense of how the covariates have trended. The population share age 55 and above and the share non-white have increased. On average, the unemployment rate and child poverty rate declined. In line with its long-term decline over the past two decades, the labor force participation rate declined during the sample period. There were large declines in the teenage birth rate. The density of substance abuse treatment facilities and child and youth services establishments increased slightly. The share of state and local expenditures on public welfare also steadily increased.

Measurement error in the independent variable. Given that PO-only and illicit opioid overdose death rates among ages 18-57 are used as proxies for unobserved rates of PO misuse and illicit opioid use among parents, respectively, we may be concerned that measurement error in the independent variables will bias OLS estimates of the effect of

[31]Provided by Geocorr 2000 Geographic Correspondence Engine, Missouri Census Data Center.

parental opioid misuse on foster care outcomes. To motivate the usefulness of the IV strategy for addressing this measurement error issue, I provide evidence consistent with the presence of classical measurement error in the independent variables.

Comparing estimates from fixed-effects (FE), first differences (FD), and long differences (LD) models can serve as a diagnostic for the presence of measurement error in the independent variable.[32] Following McKinnish (2008)'s method, I examine the relationship between opioid overdose death rates and foster care admissions using FE, FD, and LD models estimated by OLS. The estimating equation for FD and LD models is given by (1.3).[33] For the FD model, $\Delta_{\bar{t}}$ denotes the difference between the averaged level over years t and $t-1$ and the averaged level over years $t-2$ and $t-3$; for the j-period LD model, $\Delta_{\bar{t}}$ denotes the difference between the averaged level over years t and $t-1$ and the averaged level over years $t-(j+1)$ and $t-(j+2)$. The FE model is

$$Y_{ct} = \theta_1 \text{IllicitOpDeath}_{ct} + \theta_2 \text{RxOpDeath}_{ct} + X'_{ct}\alpha + \delta_c + \gamma_t + \epsilon_{ct} \tag{1.4}$$

δ_c and γ_t are county and year fixed effects, respectively. Levels in this FE model are also averages over years t and $t-1$. Results are shown in Table 1.3. Focusing on coefficients on

[32] Griliches and Hausman (1986) show that, under the assumption that the true signal is independent of the measurement error and stationary with a declining correlogram, for $T > 2$: (1) the inconsistency of FE and LD is less than that of FD; (2) the inconsistency of the $T-1$ LD estimator is less than that of the FE estimator; (3) the relative inconsistency of differences shorter than $T-1$ and FE depends on specific characteristics of the correlation structure; (4) in the presence of measurement error and as long as the signal and error are uncorrelated, estimates from FE, FD, and LD should be attenuated toward zero.

[33] The FE model omits initial levels of the control variables in 2000 since it includes county fixed effects, which control for time-invariant county characteristics. To make estimates from the FD and LD models comparable to those from the FE model, I also omit initial levels of the controls from the FD and LD models. The FD and LD models include the full set of control variables described in Section 1.3.1, first-differenced and long-differenced respectively. The FE model includes the full set of time-varying controls in levels.

the illicit opioid OD death rate in the first column, the FD estimate is smaller in magnitude than the FE estimate (although neither are statistically significant), and the LD estimates increase as longer differences are used. The 5-year and 6-year LD estimates are similar to the FE estimate. These patterns are suggestive of classical measurement error in the independent variable attenuating OLS estimates toward zero. To examine whether the changing samples across the models are driving the results, I also estimate all models using the 6-year long differences sample, which contains only observations from 2016-18. These estimates are presented in Appendix Table A.1. While the magnitude of the FE estimate is reduced (which likely reflects declines in foster care entry rates starting in 2017) and is smaller than the FD estimate, the LD estimate increases as longer differences are used, again consistent with the presence of classical measurement error.

1.5 Results

In this section, I first establish that the instruments significantly predict post-2010/11 changes in PO-only and illicit opioid overdose death rates, and validate the hypothesized policy mechanisms driving the first stage. I then discuss the main 2SLS estimates for foster care entry and caseload. I examine heterogeneity along the dimensions of foster care placement type, gender-specific illicit opioid use and PO misuse, child age, and state child welfare system features/contexts.[34] Finally, I summarize results from a variety of tests of

[34]I am also interested in examining the impact of parental illicit opioid use and PO misuse on foster care admissions by child race – specifically, estimating separate IV models for White and Black admissions, using the corresponding race-specific opioid death rates as the endogenous regressors. However, the instruments do not consistently predict changes in Black illicit opioid and PO-only overdose death rates, resulting in a weak first stage in the 2SLS estimation for Black foster care admissions. For this reason, I am not able to

internal validity, and quantify the estimated effects.

1.5.1 First stage: using initial exposure measures to predict post-2010 changes in opioid death rates

Estimates from the first stage regression (equation 1.2) of the post-2010 change in the illicit opioid or PO-only death rate on the two initial PO supply measures and the full set of covariates are presented in Table 1.4. Focusing first on the change in illicit opioid deaths in column 1, both exposure measures significantly predict the post-2010 change in the illicit opioid death rate: counties with higher initial PO density or oxycodone share experienced larger increases in illicit opioid death. The top panel indicates that a one standard deviation (sd) increase in the initial share of oxycodone increases the 2010/11-2015/16 change in the illicit opioid death rate by 2.3 deaths per 100,000 (14% of the average change). A one-sd increase in initial PO density increases the same 2010/11-2015/16 change by 1.8 illicit opioid deaths per 100,000 (11% of the average change).[35] The coefficients on both measures are slightly larger when using the 2016/17 average level or 2017/18 average level as the endpoint, as can be seen in the middle and bottom panels.

As discussed in Section 1.3.1, one major supply-side policy that is likely to be driving this positive relationship between both initial PO supply measures and post-2010 changes in illicit opioid deaths is the reformulation of OxyContin in 2010, which sharply reduced the

determine whether the effects of parental opioid abuse on foster care admissions differ across race.

[35]One-quarter of the 1,193 counties in the sample saw a reduction in the illicit opioid death rate between 2010 and 2016, while three-quarters experienced an increase. The positive coefficients on the initial PO supply measures can be thought of as a higher initial oxycodone share or PO density being associated with a larger increase or smaller reduction in the illicit opioid death rate.

supply of oxycodone and of abusable POs overall. To directly examine the trajectories of the illicit opioid death rate after the reformulation in counties with varying levels of exposure to the supply shock, I estimate event study specifications that interact year indicators with each initial exposure measure; the estimating equation and a more detailed discussion of the results can be found in Appendix Section A.2. Echoing the findings for illicit opioid deaths from Table 1.4, the event study estimates indicate that counties with higher initial oxycodone share or higher initial PO density experienced larger increases in the illicit opioid death rate after the 2010 reformulation, with the difference becoming significant starting in 2014. This is consistent with the OxyContin reformulation inducing a greater degree of substitution to illicit opioids in areas that were initially more exposed to oxycodone prescribing as well as areas with initially higher overall PO density.

The positive relationship between the initial PO supply measures and the change in illicit opioid deaths could also reflect counties' differential exposure to state implementation of PDMPs, as these programs reduced opioid prescribing and restricted access to prescription opioids.[36] I use DiD specifications to estimate the average effect of PDMPs on opioid death rates, allowing the effects to differ by initial oxycodone share or initial PO density. Appendix Section A.2 presents the estimating equation and discusses the DiD results. For the purpose of validating a particular mechanism driving the first stage, the DiD estimates for illicit opioid deaths show that counties with higher initial PO exposure by either measure saw larger increases in the illicit opioid death rate after PDMP implementation, consistent with the instruments capturing differential exposure to PDMPs. The increase in illicit opioid

[36]See references in Section 4.1 of the review article Maclean et al. (2020).

deaths suggests substitution to illicit opioids in response to reduced PO supply and access, similar to the effect of the reformulation.

The instruments also significantly predict the post-2010 change in the PO-only death rate. Column 2 of Table 1.4 shows that while higher initial PO density is associated with reductions in PO-only deaths after 2010, higher initial oxycodone share predicts increases in PO-only deaths after 2010. The top panel indicates that a one-sd increase in the initial oxycodone share increases the 2010/11-2015/16 change in the PO-only death rate by 0.7 deaths per 100,000 (18% of the average increase of 3.7 deaths per 100,000 among the 550 counties that saw an increase in the PO-only death rate during this period). When the 2017-18 average is the endpoint, the effect magnitude is smaller but still positive and significant. A one-sd increase in the initial PO density reduces the 2010/11-2015/16 change in the PO-only death rate by 1.2 deaths per 100,000 (28% of the average reduction of 4.4 deaths per 100,000 among the 643 counties that saw a decrease or no change in the PO-only death rate).[37] The negative coefficient on PO density is consistent with a greater degree of substitution away from prescription opioid misuse in counties that were more opioid-dense at baseline after the OxyContin reformulation. The reformulation event study estimates in the right panel of Appendix Fig A.2 confirm this pattern.

The positive relationship between initial oxycodone share and post-2010 changes in PO-only deaths is surprising, given that higher oxycodone share counties saw larger post-

[37] Slightly over half of the 1,193 counties in the sample saw a reduction or no change in the PO-only death rate between 2010-11 and 2015-16, while the remaining counties experienced an increase. The negative coefficient on PO density can be interpreted as higher initial PO density being associated with a larger reduction or a smaller increase in the PO-only death rate, and vice versa for the positive coefficient on oxycodone share.

2010 increases in illicit opioid deaths and were therefore expected to exhibit a greater degree of substitution away from prescription opioid misuse. To further investigate the relationship between initial oxycodone share and the change in PO-only deaths, I estimate the first stage equations including only one exposure measure at a time. Appendix Table A.3 shows these estimates for the 2010/11-2015/16 change in opioid death rates, as the pattern of coefficients is similar using the 2010/11-2016/17 change. Columns 3 and 6 are replicated from Table 1.4 for comparison. Focusing on the change in the PO-only death rate, columns 4-6 show that while higher initial oxycodone share on its own predicts a larger increase in the PO-only death rate after 2010, conditioning on initial PO density nearly doubles the coefficient on oxycodone share. Appendix Table A.4 shows that when the 2010/11-2017/18 change is used, the initial oxycodone share on its own is not significantly related to the change in PO-only deaths, but its coefficient becomes positive and significant after initial PO density is included. These results indicate that controlling for initial PO supply density, counties with larger initial oxycodone share saw larger increases in PO-only deaths after 2010. The reformulation event study estimates for the PO-only death rate, plotted in the left panel of Appendix Fig A.2, confirm this pattern. If we assume that initial oxycodone share primarily reflects exposure to the reformulation, this finding suggests that reformulation-induced substitution away from prescription opioid misuse is not the only force influencing the evolution of the PO-only death rate after 2010. For instance, DiD estimates of the effect of PDMPs on PO overdose deaths indicate that mandatory PDMPs reduced the PO-only death rate more in counties with higher initial PO density.

So far, I have shown that the instruments, the oxycodone share and PO supply density measured at the beginning of the opioid crisis in 2000, are highly predictive of changes in illicit and prescription opioid death rates in the post-2010 period. The hypothesized mechanism for these strong correlations is that the instruments capture variation in initial exposure to the supply of POs and to the disproportionate prescribing of oxycodone, which generated differential responses across counties of opioid overdose deaths to supply-side shocks that occurred well after the initial year. Results from the OxyContin event study and the PDMP DiD specification support this idea by showing that the effects of these policy shocks on opioid deaths differ depending on the two initial PO supply measures. Given the significance of the first stage, we would expect sufficiently large conditional F-statistics on the excluded instruments in each of the two first stage equations to alleviate any concern about weak instruments. Indeed, as I discuss in more detail in the main 2SLS results below, the Cragg-Donald and Sanderson-Windmeijer F-statistics for IV models with multiple endogenous regressors are above 15 in both first stage equations in the preferred model including the full set of covariates. In light of the fact that the usual t-ratio procedure in combination with a modest threshold for F (e.g. 10) yields distortions in inference, I additionally provide 95% confidence intervals using a procedure that adjusts critical values of t based on the first-stage F-statistic (Lee et al. 2020).

1.5.2 Foster care entry

The effects of parental illicit opioid use and prescription opioid misuse on foster care admissions are estimated according to equation (1.3) by 2SLS, with oxycodone share and PO density in 2000 as the excluded instruments in the first stage. I focus on the long difference between levels averaged over two endpoint years[38] for the main estimates (average 2010-11 and average 2015-16, 2016-17, or 2017-18), but I also graphically show the IV estimates as a function of the endpoint for the long difference relative to 2010-11 for each pair of endpoint years between the average over 2012-13 and the average over 2017-18. Additional results using the rate of foster care entry or caseload per 1,000 children ages 0-17 (Appendix Tables A.20, A.21) and using annual levels to define the long difference (Appendix Tables A.22- A.25) are similar in terms of the significance and direction of coefficients and the pattern of changes in coefficients as covariates are added.

Table 1.5 presents the main 2SLS estimates for entries into foster care; each panel uses a different average of two endpoint years for the difference relative to 2010-11. All columns control for initial levels in 2000 of the full set of covariates. Long-differenced covariates are progressively added in each column, as indicated at the bottom of the table. All three panels show a positive and significant relationship between changes in the illicit opioid death rate among prime parenting ages and the percent change in foster care entry,[39] but an insignificant

[38]In the following discussion, these long differences are occasionally referred to by the later of the two consecutive years. Unless stated otherwise, the endpoints of the long differences refer to averaged levels over consecutive year pairs.

[39]The outcome variable is a difference in the IHS of the foster care entry count, which approximates the percent change in the entry count.

relationship between foster care entry and the PO-only death rate in the same age range.

In the first panel, the coefficient on illicit opioid death from the specification including the full set of covariates (Column 6) implies that a 1 sd increase in the change in the illicit opioid death rate is associated with a 1.15 sd increase in the percent change in foster care entry (1 sd in ΔIllicit opioid death rate=12.6 deaths per 100,000 ages 18-57; 1.15 sd in ΔIHS entry=32.6 pp). Alternatively, we can compare counties at different percentiles of the distribution of the change in the illicit opioid death rate. Moving from the 10th (0, i.e. no change) to the 90th percentile (+27.1 deaths per 100,000) of the change in the illicit opioid death rate during 2010/11-2015/16 is associated with a 80 percentage point (pp) larger percent increase in foster care admissions. This is 2.47 sd in the distribution of the percent change in foster care entry between 2010/11 and 2015/16. Although the associated increase in the percent change in foster care entry seems large in percentage point units, the scale of the increase is reasonable when considered in standard deviation units. Moving from the 10th to the 90th percentile of the change in the illicit opioid death rate is a 2.15 sd increase, which is associated with a 2.47 sd increase in the percent change in foster care entry. The lower bound of the 95% confidence interval for the 2SLS estimate implies that moving from the 10th to the 90th percentile of the change in the illicit opioid death rate is associated with a 17 pp larger percent increase in entry, or 0.5 sd. The next two panels indicate that the magnitude of the effect of parental illicit opioid use on foster care entry are smaller for the 2010/11-2016/17 or 2010/11-2017/18 period. Between 2010-11 and 2017-18 (2016-17), a 1 sd increase in the change in the illicit opioid death rate is associated with a 0.84 (0.49) sd increase in the percent change in foster care entry.

Moving across columns 1 through 6, the magnitude and statistical significance of the positive effect of parental illicit opioid use on foster care entry remain stable as sets of long-differenced covariates are included. This suggests that the positive IV estimate is not being driven by correlation between the initial exposure measures and changes in a wide range of observable area characteristics. Using the average 2015-16 as the endpoint yields a positive estimate of the effect of PO misuse on foster care entry of a similar magnitude as illicit opioid use, although the estimate is not statistically significant due to larger standard errors. However, the point estimate is close to zero using later years as the interval endpoint (second and third panels), indicating that PO misuse did not have a direct effect on foster care entry. Examining the coefficients on the controls, higher initial population shares of ages 20-24 and ages 65+, higher initial LFP rate, lower initial share female, and lower initial share of state and local expenditures on welfare are significantly correlated with a larger post-2010 increase in foster care entry. In terms of long-differenced controls relative to 2010-11, larger increases in the child poverty rate and the LFP rate and a larger decline in the density of substance abuse treatment facilities are associated with a larger increase in foster care entry.

The bottom of each panel reports the first stage F-statistics of tests of weak identification associated with the specification in each column. The Cragg-Donald F-statistic (Cragg and Donald 1993) can be used to test the null hypothesis of weak instruments using critical values provided in Stock and Yogo (2002). The Sanderson-Windmeijer (SW) F-statistic (Sanderson and Windmeijer 2016) is calculated separately for each endogenous regressor, the change in the illicit opioid death rate and the change in the PO-only death rate.[40] Although the Cragg-Donald F and the SW F's for both endogenous regressors are always above the conventional weak instrument critical value of 10 for all specifications and all three long difference intervals in Table 1.5, they are below the distortion-corrected F threshold of 104.7 that is necessary to actually achieve 5% significance with no distortion to the size and coverage rates of the inference procedure (Lee et al. 2020). For IV estimates from the preferred specification that includes all covariates (column 6), I report the 95% confidence interval obtained using Table 3 of Lee et al. (2020), which contains critical values for $|t|$ as a function of the first stage F-statistic.[41] Using the distortion-adjusted 95% CIs still indicates that the positive effect of parental illicit opioid use on foster care admissions is significantly different from zero.

To examine how estimates change with the length of the long difference, Fig 1.10 and 1.11 plot the IV coefficients on ΔIllicit op mort and Δ PO-only mort as a function of the endpoint year pairs used to compute the long difference relative to 2010-11, using F-dependent critical values for the 95% CIs. We saw in Table 1.5 that prescription opioid misuse had no significant relationship with foster care admissions, and this holds when earlier years are used as the endpoint of the long difference. The pattern of the illicit opioid coefficient is more interesting: the magnitude of the contribution of parental illicit opioid use to foster care entry decreases as we look over a longer time horizon relative to 2010-

[40]The SW F-statistic is a modification to the conditional first stage F for the case of multiple endogenous regressors proposed in Angrist and Pischke (2009) that corrects the variance formula in the denominator.

[41]This function $c(F)$ is such that under the null hypothesis, $Pr[t^2 > c(F)] \leq 0.05$, for any values of the nuisance parameters $E[F]$ and the correlation between the structural equation and first-stage errors. The 95% CI is computed as $\hat{\beta}_{IV} \pm c(F_{sw}) * \hat{SE}(\hat{\beta}_{IV})$, where F_{sw} is the Sanderson-Windmeijer F-statistic.

11, although the coefficients are not significantly different from each other. Using different endpoint year pairs results in slightly different estimates of the increase in foster care entry associated with a given change in the (differenced) illicit opioid death rate, but it does not change the qualitative conclusion that increased parental illicit opioid use in the post-2010 period significantly increased foster care admissions.

This pattern of a decline in the IV estimate as the long difference is extended relative to 2010-11 is likely related to the decline in the entry rate starting in 2016 seen in the national data (Figure 1.2). States that drove this decline since 2016 are geographically diverse: in my sample, the ten states with the largest percent decrease in foster care admissions (averaged across counties) between 2016 and 2018 included states on the East Coast, in the Midwest, the South, and the Southwest.[42] One speculative reason for these declines in entry is the implementation of child welfare waiver demonstrations, which allowed states to use federal Title IV-E funding to provide a variety of programs and services.[43] These interventions often have explicit programmatic goals of preventing foster care entry and/or reducing reentry, with 15 out of the 27 waiver demonstrations implemented since 2012 placing special emphasis on foster care prevention (Graham 2021). Notably, seven out of the ten states with the largest reductions in entry since 2016 in my sample implemented a demonstration waiver between 2013 and 2016, with an end date in 2019.[44] While differences

[42]The ten states were Mississippi, Arkansas, Nebraska, New Jersey, Arizona, Delaware, D.C., Utah, Oklahoma, and Indiana.

[43]Examples include: differential response, a CPS practice that allows for alternative response pathways other than initiating an investigation when a report of maltreatment is screened in; intensive in-home family preservation services; and linking families to community services such as substance abuse and psychiatric services.

[44]Indiana's demonstration began in 1998, during the first round of projects, and was extended in the second round that began in 2012.

across states in the types of interventions provided, the research design used for evaluating the demonstration, and the outcomes studied preclude drawing general conclusions about the effects of the demonstrations on entry into foster care (Graham 2021), this suggests that state demonstration waivers are a plausible contributor to the decline in the entry rate since 2016.

To get a sense of the potential bias of OLS estimates of the effects of parental opioid misuse on foster care entry, I compare the 2SLS estimates to OLS estimates of equation 1.3, which are presented in Table 1.6. The OLS estimates are also shown in red in Fig 1.10 and 1.11. OLS estimates of the relationship between the change in the illicit opioid overdose death rate and the change in foster care entry are notably different from the 2SLS estimates. Focusing on the specification including the full set of controls (column 6), OLS estimates in the first two panels are not significantly different from zero and are roughly an order of magnitude smaller than the 2SLS estimates. Using the 2010/11-2017/18 LD, the OLS estimate is positive but one-quarter the size of the 2SLS estimate. Assuming OLS estimates are biased by omitted variables and measurement error, the fact that 2SLS estimates are larger indicates that OLS estimates are biased downward. The downward bias of OLS estimates and the stability of the 2SLS estimates across the inclusion of controls are consistent with classical measurement error in the proxies for parental opioid abuse attenuating the OLS estimates toward zero. OLS estimates may be biased downward by omitted variables if counties with larger growth in parental illicit opioid use experienced trends negatively correlated with foster care admissions. This is plausible if, for example, child welfare agencies in places with larger increases in illicit opioid use shift toward family treatment courts and other approaches that integrate substance abuse treatment with the child welfare system, which may be effective at reducing foster care entry.

Reduced-form estimates. We may also want to know the direct effect of the instruments on the change in foster care admissions. This provides a check on the IV estimates and directly relates levels of the initial PO supply measures in 2000 to the post-2010 change in foster care entry. I estimate reduced-form (RF) regressions according to equation (1.2), where the outcome variable is instead the long-differenced change in IHS entry. Appendix Fig A.3 visualizes the RF estimates from the specification including all controls by plotting marginal effects calculated as follows. Each initial exposure measure is divided into deciles, and predicted responses are obtained by holding one initial exposure measure at its mean within each decile, letting all other covariates (including the other exposure measure) take their actual values, and taking the average of the linear predicted response across all observations. This gives the marginal effect of each initial exposure measure on the post-2010 change in entry, adjusting for the other exposure measure and changes in county characteristics. The left panel shows that higher oxycodone share in 2000 is associated with a larger percent change in entry, although none of the predicted responses are significantly different from zero. The right panel shows that initial PO density has a significant positive marginal effect on the change in entry. Given that both instruments have a positive effect on the post-2010 change in the illicit opioid death rate, these patterns are consistent with the positive IV estimate on the change in illicit opioid deaths.

In addition to total foster care entry, the IV model is estimated for entry by reason cited for the removal in order to examine whether the increase in total entry due to parental illicit opioid use is driven by particular forms of child maltreatment. There are several issues related to the reporting of reasons for removal, and in particular the reporting of parental substance abuse as a reason: counties differ in reporting practices, and caseworkers do not always reliably record parental substance abuse as a reason for removal even when it is present (Ghertner et al. 2018). Federal and state efforts to improve reporting reliability further complicate interpretation of the data, as the observed increase in the share of foster care entries citing parental substance abuse since 2000 (Meinhofer and Angleró-Díaz 2019) may reflect both improved reporting and a true increase in this share. Given these concerns, I consider total entries as the main measure of foster care entry, and discuss results for entry by reasons for removal in Appendix Section A.3. I do not find evidence that the increase in total entries is concentrated in any specific type of maltreatment.

1.5.3 Foster care caseload

In addition to changes in total new entries, we are also interested in net changes to caseloads as a basic indicator of how foster care as a system is evolving over time. To directly examine the effects of parental illicit opioid use and PO misuse on the size of the foster care population, I estimate equation (1.3) by 2SLS with annual foster care caseload as the dependent variable. Notice that the caseload in year t is determined by (i) the rate of new entries in t from the population of children not in foster care and (ii) the rates at which children who entered in years $t-1$,..., $t-k$ remain in care at the end of year t, i.e., the continuation rates in t of entry cohorts from $t-1$,..., $t-k$. An increase in the caseload over time could be due to an increase in the rate of new entry, increases in continuation rates, or a combination of the two. To examine whether parental opioid misuse has affected the rates at which prior entry cohorts remain in foster care, I also estimate effects on continuation rates, limiting outcomes to the $t-1$ and $t-2$ continuation rates.[45] The $t-1$ continuation rate, for example, is defined as the share of children entering foster care in FY $t-1$ who remain in care at the end of FY t.

2SLS estimates for the change in foster care caseload and the change in prior entry cohorts' continuation rates are summarized in Table 1.7, where the endpoint of the long difference is the 2017-18 average, and all columns include the full set of controls. Panel A shows the IV estimates, and Panel B shows the corresponding OLS estimates for comparison.[46] In the first column of Panel A, the coefficient on illicit opioid deaths implies that between 2010-11 and 2017-18, a 1 sd increase in the change in the illicit opioid death rate is associated with a 1.06 sd increase in the percent change in foster care caseload (1 sd in ΔIllicit opioid death rate=18.8 deaths per 100,000 ages 18-57; 1.06 sd in ΔIHS caseload=40.3 pp). Alternatively, moving from the 10th (+0.94 deaths per 100,000) to the 90th percentile (+43.1 deaths per 100,000) of the change in the illicit opioid death rate, a 2.2 sd increase, increases the percent change in the foster care caseload by 2.4 sd. The full caseload results

[45] Because one year of the sample period is lost for each additional annual entry cohort further back in time (e.g., the $t-1$ continuation rate is missing in 2010 since 2010 is the first year of my sample period), I limit the number of past annual entry cohorts examined to two.

[46] Full sets of results for each outcome showing the other two LD endpoints and specifications with fewer controls are in Appendix Tables A.8, A.9, A.10, and A.11.

in Appendix Table A.8 show that, as with the entry estimates, the caseload 2SLS estimates remain positive and significant across the addition of controls. Back in the summary table, the first column of Panel B shows the OLS estimates for caseload: similar to the entry OLS estimates, the OLS estimates of the effect of illicit opioid use on caseload are attenuated toward zero. Based on the OLS estimates for entry and caseload, we would have concluded that the change in parental illicit opioid use did not affect the foster care system after 2010.

Fig 1.12 and 1.13 plot 2SLS estimates as a function of the long difference endpoint year pairs. Fig 1.12 shows that the pattern of the estimated effect of parental illicit opioid use on caseload is similar to the effect on entry as we vary the length of the long difference relative to 2010-11: the effect magnitude declines as we look over a longer time horizon from 2010-11. The 95% CI for the 2SLS estimate excludes zero using the last three year pairs of the sample period as the long difference endpoint. It is also interesting to note the positive coefficients on the change in PO-only deaths in Fig 1.13. Although the 95% F-dependent CIs in the figure do not rule out a null effect of parental PO misuse on foster care caseloads, the magnitudes are larger than that of the illicit opioid death coefficients and are significant at the 10% level. This suggests a potential positive effect of parental PO misuse on caseload that was not seen for entry.

Reduced-form estimates. As with the entry results, I visually show the RF estimates by plotting the covariate-adjusted average response of the percent change in foster care caseload as a function of the within-decile mean of each initial exposure measure. This is shown in Appendix Fig A.4. The positive and significant marginal effects in the left panel indicate that the positive IV estimate on the change in the illicit opioid death rate is driven by a positive relationship between the oxycodone share in 2000 and the post-2010 change in foster care caseload. By comparison, the adjusted predicted response of the change in caseload is flat across deciles of initial PO density. This also contrasts with the RF estimates for foster care entry, which showed a significant and positive marginal effect of initial PO density on the post-2010 change in entry.

We saw in section 1.5.2 that the post-2010 rise in parental illicit opioid use significantly increased new entries into foster care, and the caseload estimates indicate that this increase in entry translated into a rise in caseloads. To test whether the effect of parental illicit opioid use on foster care caseloads is also operating through changes in the continuation rates of prior entry cohorts, the IV model is estimated for $t-1$ and $t-2$ continuation rates. 2SLS estimates are presented in columns 2 and 3 of Table 1.7. Note that because we are looking one year back with each additional prior entry cohort and thus losing years at the beginning of the sample period, the long difference for the $t-1$ ($t-2$) continuation rate is relative to averaged levels 2011-12 (averaged levels 2012-13), while the endpoint shown in this summary table is still the average over 2017-18 (full results are in Appendix Tables A.10 and A.11). Estimates in these two columns indicate that parental illicit opioid use and PO misuse did not significantly affect the continuation rates of the two previous entry cohorts. To the extent that the null effects on the continuation rates of more recent past entry cohorts can be extrapolated to entry cohorts further back in time, this finding indicates that the effect of parental illicit opioid use on caseloads is driven by an increase in new entries into foster care.

1.5.4 Heterogeneity: placement setting

Conditional on entering foster care, the type of placement could influence child outcomes. Placement in a relative foster home, also known as kinship care, is thought to be less traumatic for children (Doyle and Aizer 2018). As a result, a major shift in foster care policy over the past 30 years has been to prioritize finding relatives of the child to provide a foster home before turning to non-relatives. Although there are few RCTs to examine the effects of kinship care on child outcomes relative to other placement types, the available evidence suggests that children in kinship care may have better outcomes than children placed with non-relatives in terms of placement stability, behavioral development, and mental health (Winokur et al. 2014). Additionally, there is anecdotal evidence that in child welfare cases involving parental opioid abuse, caseworkers sometimes have trouble finding other relatives to care for the child due to multiple family members abusing opioids (Ghertner et al. 2018). If the lack of other relatives is a systematic phenomenon when it comes to parents who have been affected by the opioid crisis, we would expect effects to be concentrated in entry to a non-relative foster home or a group home/institution. I estimate the entry and caseload IV models separated out by the three main placement types: non-relative foster home, relative foster home, and group home or institution. During 2010-2018, the share of total admissions placed in kinship care increased from 28% to 35%, while the share in group homes or institutions declined from 16% to 12.5%; the share in non-relative foster homes remained the same at 47%.[47] These patterns likely reflect, in part, federal and state policy shifts away from the use of congregate care and the long-term trend in emphasizing placement with kin when possible.

Table 1.8 presents 2SLS estimates for the change in foster care entry by placement setting from the IV model including the full set of covariates. First comparing the illicit opioid death coefficients across columns in the first two panels, parental illicit opioid use increased entry across all three placement types: the coefficients are not statistically different from each other.[48] Interestingly, PO misuse only increased entry into kinship care. The PO misuse point estimates are larger than the illicit opioid use estimates, although they're not statistically different. Using the longest LD interval (the third panel), effect magnitudes decline and are not different from zero. The 2SLS estimates for caseload in Appendix Table A.15 show a similar pattern as entry, but the larger effect of PO misuse on cases placed in kinship care is now statistically different from that of illicit opioid use. Again, illicit opioid use increased caseload in all three placement settings (the effect is largest for kinship care cases, and statistically different from the other two coefficients), whereas PO misuse only increased kinship care cases.

Considering that child welfare agencies generally try to find relatives first, the different effects of illicit opioid use and PO misuse on entry/caseload by placement setting suggests

[47] These shares are slightly different from the shares presented in Table 1.1, descriptive statistics of foster care entries 2010-18, because the former are calculated as the average of county-level shares (entries in a particular placement divided by total entries in the county), and the latter are shares calculated from the pooled case-level data.

[48] I test whether the coefficients on Δ Illicit opioid death rate are different from each other by first writing the equations in the three columns as a system of equations and estimating the parameters of interest by three-stage least squares (3SLS), then testing for equality of the coefficients.

that the availability of other family members to provide a foster home may differ across populations of parents who are misusing POs versus those who have turned to illicit opioids. In particular, the results are consistent with foster care cases due to parental illicit opioid use being less likely to have other relatives who can care for the child. Although I do not have direct evidence on the differential availability of family members, I compare the characteristics of individuals who reported misusing pain relievers in the past year (corresponding to PO misuse) to that of individuals who reported using heroin in the past year (corresponding to illicit opioid use, although this does not include synthetic opioids) in the National Survey on Drug Use and Health in 2012-13. Appendix Table A.16 shows this comparison: those who misused pain relievers are positively selected relative to those who used heroin. They are significantly more likely to have graduated from college, to have family income above $75,000, to have health insurance coverage, and to be married. Given this positive selection, it is plausible that parents who are misusing POs are also more likely to have extended family who are able to provide a home for their child, whether as kinship foster care or as an informal arrangement.

1.5.5 Heterogeneity: gender-specific parental opioid abuse

The effect of parental illicit opioid use on entry to foster care may differ depending on parents' gender for several reasons. First, more children live in single mother households than single father households. During 2010-18, 23% of children under age 18 in the U.S. lived in single mother households, compared to 4% of children who lived in single father households.[49] Second, prenatal drug exposure is one way in which families come into contact with the child welfare system: the Child Abuse Prevention and Treatment Act requires states to have policies and procedures in place to notify CPS agencies of substance-exposed newborns. Infants showing evidence at birth of exposure to drugs and alcohol are included in the definition of child maltreatment in 14 states and D.C., and 19 states and D.C. have specific reporting procedures for such infants (Child Welfare Information Gateway 2016). Third, prior work has shown differential associations of foster care outcomes with other gender-specific explanatory variables (Swann and Sylvester 2006). Although these reasons suggest that female illicit opioid use may have a stronger effect on foster care entry, the direction of gender heterogeneity in the effect of parental illicit opioid use is ultimately an empirical question.

In Table 1.9, the entry IV model is estimated using changes in gender-specific rates of illicit opioid and PO-only overdose death as the endogenous regressors (IV estimates for caseload are in Appendix Table A.17). All columns include the full set of covariates in long differences and initial year 2000 levels. In the third panel, the first-stage F-statistics in the female opioid death specification are below the Stock-Yogo threshold, raising concerns about weak instruments. For this reason, I focus on the first two panels. Comparing between columns 1 and 2, the 2SLS estimates indicate that both female and male illicit opioid use significantly increased foster care admissions. The effects of female illicit opioid use on entry are consistently larger and statistically different from that of male illicit opioid use.[50] The

[49] Based on Annual Social and Economic Supplements of the Current Population Survey.

[50] I test whether the coefficients on female and male Δ Illicit opioid death rate are different from each other in the same way as for entry/caseload by placement setting – see section immediately above.

caseload results show a similar pattern of a larger and statistically different effect of female illicit opioid use.

Column 1 in the first panel of Table 1.9 implies that a 1 sd increase in the change in the female illicit opioid death rate (7.4 female deaths per 100,000 females ages 18-57) is associated with a 1.22 sd increase in the percent change in foster care entry during 2010/11-2015/16. The effect of the same 1 sd increase in the change in the male illicit opioid death rate (18.7 male deaths per 100,000 males ages 18-57) during this period is associated with a 1.16 sd increase in the percent change in foster care entry, even though the standard deviation of the distribution of ΔMale illicit opioid death rate is 2.5 times that of the distribution of ΔFemale illicit opioid death rate. These findings are consistent with maternal illicit opioid use having a stronger impact on children's out-of-home placement, which is potentially related to factors such as reporting policies for substance-exposed newborns and a substantially higher share of children living in single mother households as compared to single father households.

1.5.6 Heterogeneity: child age

I explore whether the increase in foster care entry due to parental illicit opioid use differs by age of the child at removal. This is a relevant dimension of heterogeneity if the effects of foster care on children's later outcomes differ depending on their age at home removal/entry into foster care. Doyle (2007) shows that for children on the margin of placement, increases in the juvenile delinquency rate and teenage birth rate due to foster care placement were larger for older children at the time of the initial investigation. Separately, if special policies and procedures regarding reporting of and child welfare responses to substance-exposed infants play an important role in the entry of these children into foster care, we may expect to observe differential effects on entry of children under one year old.

IV models are estimated separately for the change in IHS entry of children under age 1 (infant), ages 1-5 (preschool), ages 6-12 (grade school), and ages 13-17 (teenager). Results are presented in Table 1.10. All columns include the full set of covariates. In the first two panels, the effect of parental illicit opioid use on entry are similar across age groups. In the third panel, corresponding to estimates using the 2010/11-2017/18 LD, effects are significant only for the two older age groups. Comparing across columns, the positive effect on entry is consistently significant for ages 6-12 and 13-17. Appendix Table A.18 shows 2SLS estimates for age-specific foster care caseloads. Comparing across columns in each panel, the effects of parental illicit opioid use on caseloads are consistently positive and significant for ages above one year old, although there is still a positive effect on cases under the age of one in the first two panels. Taken together, these results suggest that the effect of parental illicit opioid use on entry and caseload are not different across children's age at the time of removal.

1.5.7 Heterogeneity: state child welfare systems

States vary widely in the administration and operation of their child welfare systems, and these differences may affect the observed relationship between parental opioid abuse and entry into foster care. There are many dimensions of heterogeneity at each stage of a family's involvement with CPS (reporting, screening, investigation, and response), as well as contextual differences such as the degree of centralization (i.e., state versus county administration of the child welfare system). It is unclear ex ante what stages and which differences within each stage are most likely to mediate the estimated effects on foster care entry. In contrast, contextual differences potentially affect multiple stages of CPS involvement and may be more likely to generate heterogeneous effects of illicit opioid use on foster care outcomes. I examine two dimensions of contextual difference: whether parental substance abuse is included in the civil definition of child maltreatment, and the degree of centralization of the child welfare system. I also test for heterogeneity by the presence of a universal mandated reporting (UMR) law. I test for differences in the effect of parental illicit opioid use on foster care outcomes by each type of state policy by augmenting the second stage structural equation as follows:

$$\begin{aligned}\Delta_{\bar{t}} Y_c = {} & \kappa_1 \Delta_{\bar{t}} \text{IllicitOpDeath}_c + \kappa_2 \Delta_{\bar{t}} \text{RxOpDeath}_c \\ & + \kappa_3 (\text{Policy}_s \times \Delta_{\bar{t}} \text{IllicitOpDeath}_c) + \kappa_4 (\text{Policy}_s \times \Delta_{\bar{t}} \text{RxOpDeath}_c) \\ & + \Delta_{\bar{t}} X_c' \phi_1 + X_{c,2000}' \phi_2 + \Delta_{\bar{t}} \mu_c\end{aligned} \tag{1.5}$$

Policy_s is an indicator for whether state s has the policy in question during the sample period. A separate indicator is created for each of the three policies: state centralization, UMR, and parental substance use being included in the definition of child maltreatment. Because the third policy was not constant during the sample period (i.e. some states introduced statutes between 2010 and 2018), the estimation sample for this policy is limited to states where the policy status did not change during this period. In equation (1.5), in addition to $\Delta_{\bar{t}}\text{IllicitOpDeath}_c$ and $\Delta_{\bar{t}}\text{RxOpDeath}_c$, $\text{Policy}_s \times \Delta_{\bar{t}}\text{IllicitOpDeath}_c$ and $\text{Policy}_s \times \Delta_{\bar{t}}\text{RxOpDeath}_c$ are also endogenous regressors. Therefore, I use the initial PO supply exposure measures in 2000 each interacted with the policy indicator as additional instruments. Excluded instruments in the first stage are $\text{OxyShare}_{c,2000}$, $\text{RxOpSupply}_{c,2000}$, $\text{Policy}_s \times \text{OxyShare}_{c,2000}$, and $\text{Policy}_s \times \text{RxOpSupply}_{c,2000}$. A significant estimate of κ_3 (κ_4) in either direction would indicate that the effect of parental illicit opioid use (PO misuse) on foster care entry/caseload differs by the presence of a particular state policy. I first describe each type of state policy before presenting the IV estimates.

The relationship between parental illicit opioid use and inflows to foster care may differ across states due to differences in the civil definition of child abuse or neglect, to the extent that these definitions provide a legal basis for removing children from the home due to a parent's substance abuse. There are three types of laws that are relevant for illicit opioid use: (1) infant exposure to drugs, alcohol, or other controlled substances is included in the definition of child abuse or neglect; (2) exposing children to the manufacture, possession, or distribution of illegal drugs is considered child endangerment; (3) using a controlled substance that impairs the caregiver's ability to adequately care for the child is considered child abuse or neglect (Quast 2018). These codified definitions could provide the necessary justifications for a caseworker to recommend removal,[51] and they could also influence general policies and practices of child welfare agencies in the state. Because these state statutes are not constant

[51] For example, California's State Code states that in cases where "the child has suffered, or there is substantial risk that the child will suffer, serious physical harm or illness, as a result of the inability of the parent... to provide regular care for the child due to the parent's... substance abuse", the child is "within the jurisdiction of the juvenile court which may adjudge that child to be a dependent child of the court." (California Welfare & Institutions Code §300)

across the sample period (more states have added subsets of the three types of laws over time), I limit the sample as follows. States that had at least one of the three types of laws as of May 2009 are assigned a value of 1 for this policy indicator. States without any of the three laws as of May 2009 and did not add any by April 2015 are assigned a value of 0 for the policy indicator.[52] This results in 179 counties being dropped from the estimation sample.

I also examine heterogeneity by whether the state child welfare system is centralized (state-administered) or decentralized (county-administered or hybrid). In state-administered systems, the state has centralized control over child welfare funding, policymaking, training for workers, etc. There are differences across local agencies based on the state administrative structure that could affect the relationship between the rate of parental illicit opioid use and foster care placement. For instance, a survey fielded in 2002 as part of the National Study of Child Protective Services Systems and Reform Efforts found that compared to agencies in county-administered systems, state-administered agencies had more staff on average and had a higher percentage of workers who strictly specialize in conducting either screening/intake or investigations (U.S. DHHS 2003). Eleven states have a county-administered or hybrid child welfare system[53], and the remaining are state-administered. The policy indicator for the type of centralization takes a value of 1 if the state is state-administered, and is 0 otherwise.

The third type of policy is the presence of a Universal Mandated Reporting law, or UMR. All states require professionals who work with children, such as health care providers

[52]Child Welfare Information Gateway provides lists of states with each type of law, updated every few years. The version closest to the end of the sample period is current through April 2015. Archived versions of these reports, "Parental Drug Use as Child Abuse", were obtained by request. States assigned a value of 1 for the policy indicator are AK, AR, CO, DC, DE, FL, IA, IL, IN, KS, KY, MN, MO, MT, ND, NY, RI, SC, SD, TX, VA, and WI. States dropped from the sample are AZ, CA, LA, OK, WA, and WV.

[53]CA, CO, MN, NC, ND, NV, NY, OH, PA, VA, and WI.

and teachers, to report known or suspected child maltreatment. In addition, some states have enacted UMR laws, which require all adults to report child maltreatment. UMRs may reinforce the effect of parental illicit opioid use on foster care placement if they facilitate reporting by a wider range of adults and increase substantiated reports of maltreatment, but evidence on whether UMRs increase substantiated child maltreatment reports remains inconclusive. Eighteen states had a UMR law as of April 2010, and policy status was constant during the sample period.[54] The UMR policy indicator takes a value of 1 if the county is in one of these eighteen states, and is 0 otherwise.

Table 1.11 shows 2SLS estimates of equation (1.5) for foster care entry, using average 2010/11 - average 2017/18 long difference[55]. Each column corresponds to a different policy, indicated in the first row above the coefficients. The coefficients on the interaction terms Policy $\times$ ΔIllicit op mort and Policy $\times$ ΔPO-only mort in the first two columns indicate that the effects of parental illicit opioid use and PO misuse did not differ by the degree of centralization of the child welfare system or the presence of a UMR law. In the third column, the model interacts changes in illicit opioid and PO-only deaths with an indicator for whether parental drug use is included in the state civil definition of child maltreatment. The 2SLS estimates are not well-identified, as indicated by the low first-stage F-statistics, and therefore the estimate of κ_3 is not informative about whether the effect of parental illicit opioid use on foster care entry differed by this aspect of the definition of child maltreatment. Analogous 2SLS estimates for caseload in Appendix Table A.19 indicate that the effect of illicit opioid

[54]DE, FL, ID, IN, KY, MD, MS, NE, NH, NJ, NM, NC, OK, RI, TN, TX, UT, and WY.

[55]Estimates are similar if average 2015-16 or 2016-17 is used as the endpoint.

use on caseload also did not differ by policy context. Overall, these results suggests that the observed increase in foster care admissions due to parents' illicit opioid use are not dependent on these particular features of the state child welfare system. There are additional differences across states in reporting requirements, screening/intake, investigations, and child welfare response, and it may be interesting for future research to determine whether these other differences mediate the relationship between parental opioid abuse and foster care entry.

1.5.8 Tests of internal validity

This section summarizes the results of a placebo test and a variety of robustness checks, which provide evidence supporting the internal validity of the IV estimates. A detailed discussion of the tests can be found in Appendix Section A.4.

In the results so far, we have seen that IV estimates of the post-2010 increases in foster care entry and caseload due to the increase in parental illicit opioid use remain stable in both magnitude and significance as controls are gradually added. While this suggests that the estimated positive effects are not due to correlation between the initial exposure measures and changes in observables such as demographic composition and economic conditions, we might still be concerned that the initial exposure measures are correlated with unobservable trends that influence foster care outcomes. To address this potential concern, I conduct an informal check of the IV identifying assumption based on the idea that in a sample where the first stage is zero, the reduced-form (RF) estimates should also be zero if the exclusion restriction holds.[56] While passing this test cannot verify the exclusion restriction, it would bolster our

[56]This check is sometimes referred to as the zero-first-stage test.

confidence that the instruments do not affect foster care outcomes through channels other than changes in illicit opioid use and PO misuse. I implement this placebo test in a sample of states where we would expect a weak first-stage relationship due to an early prescription policy–triplicate prescription programs–that limited initial exposure in 2000 to oxycodone and to prescription opioids in general. I first confirm that in the triplicate sample (the sample of states identified by Alpert et al. (2019) as having triplicate prescription programs by the beginning of the opioid crisis), the first stage estimates are close to zero relative to estimates from the non-triplicate sample, and are all not statistically significant. I then show that the reduced-form estimates for foster care entry and caseload are also zero in the triplicate sample, indicating that, in at least one instance when the instruments did not affect the paths of opioid deaths after 2010, they were also unrelated to foster care outcomes.

I assess the robustness of the main IV estimates in several ways. First, I show that IV estimates are similar if the foster care entry or caseload rate is used as the dependent variable instead of the IHS of entry or caseload count. Second, we might be concerned that the results are sensitive to the specification of the long difference. I show that the effects of illicit opioid use on entry and caseload are not substantively changed by using annual levels to define the long difference. Third, to check that the estimates are not being driven by reporting peculiarities in the 2000 ARCOS opioid retail supply data used for the main instruments, I estimate the IV models for foster care entry and caseload using instruments constructed from 2001 data. The results are similar to the main estimates. I also use alternative instrument definitions and find similar entry and caseload effects. Fourth, I show that estimates are not sensitive to the set of states included in the sample by omitting the counties of one state at a time from the sample and re-estimating the IV models. Finally, I estimate the IV models without weighting each observation by the county child population in 2018. I find that the unweighted entry and caseload effects of illicit opioid use are slightly smaller, but still positive and statistically significant.

1.5.9 Discussion

In summary, the post-2010 increase in parental illicit opioid use raised foster care admissions and caseloads. In contrast, changes in parental PO misuse only increased kinship foster care. The increase in caseloads attributed to rising illicit opioid use was driven by new entries into foster care, with no effect of illicit opioid use on the continuation rates of entry cohorts from prior years. I find heterogeneity in the effect of illicit opioid use and PO misuse across foster care placement types: illicit opioid use increased entry to both non-relative and relative foster homes, as well as to congregate care, whereas PO misuse increased only entry to relative foster homes. This is consistent with differential availability of other family members who can provide a foster home among parents who are primarily misusing POs versus those who have turned to heroin and fentanyl. Female illicit opioid use has significantly larger effects on admissions and caseloads than male illicit opioid use, perhaps due to the gendered division of child care responsibilities and the fact that a substantially larger share of children live in single mother households as compared to single father households. The effects of illicit opioid use on entry and caseload are not significantly different across child age. Finally, the effect of parental illicit opioid use on foster care outcomes did not differ by the degree of centralization of the state child welfare system, the presence of a universal mandated reporting law, or whether parental drug use is considered a form of child maltreatment in the state civil code.

To scale effect sizes for foster care entry and caseload, I consider the county with median entry or caseload rate per 1,000 children ages 0-17 in 2010.[57] In my sample, the median entry rate in 2010 was 4.07 entries per 1,000 children (431 admissions) and the median caseload rate was 5.87 cases per 1,000 children (241 cases). I assume a counterfactual of no change in the illicit opioid death rate between 2010 and 2018, and multiply the 2SLS estimates for entry or caseload (using the 2010/11-2017/18 long difference of averaged levels) by the 10th to 90th percentile move in Δillicit opioid death rate of +42.1 deaths per 100,000. This gives one measure of the percent increase in entry/caseload caused by the increase in illicit opioid use during 2010-2018. I then apply this percent increase to the entry (caseload) count of the county with median entry (caseload) rate in 2010. This calculation yields 169 additional admissions, or an increase of 2.5 admission per 1,000 children, and 217 additional cases on net, or an increase of 6.9 cases per 1,000 children, for the median entry or caseload rate county in 2010, relative to a counterfactual of no change in illicit opioid deaths.

Note that the endogenous regressors are illicit opioid and PO-only death rates among all individuals ages 18-57. Ideally, we would want to use opioid deaths among parents to proxy for parental opioid abuse, but the mortality data do not record whether the decedent

[57] The median entry rate county in 2010 was Genesee County, Michigan, which contains the city of Flint. The median caseload rate county in 2010 was Mohave County, Arizona.

had dependent children. To obtain the effect of parental illicit opioid use on foster care outcomes, the IV estimates need to be adjusted for the fact that not all opioid deaths ages 18-57 were parental opioid deaths. I make the simple assumption that parental opioid deaths ages 18-57 are a constant share α of all opioid deaths in this age range, $0 < \alpha < 1$. The IV estimates should then be scaled by $\frac{1}{\alpha}$ to obtain the effect of parental illicit opioid use. α is operationalized as the share of individuals ages 18-57 who have dependent children, which implicitly assumes that the share of all opioid deaths in this age range attributable to parents is exactly proportional to parents' share of the population ages 18-57. Using the ACS 2010-2018, I estimate α as the share of individuals ages 18-57 who have at least one child of their own in the household who is under age 18.[58] This proportion is 36.6%, so IV estimates should be inflated by $1/0.366 = 2.7$. Applying this scaling factor to the 2010/11-2017/18 IV estimates for the effects of parental illicit opioid use on foster care entry and caseload raises the implied increases in admissions and cases for the median entry and caseload counties just discussed. For the median entry rate county, the 10th to 90th percentile difference in the change in the illicit opioid death rate implies an increase of 456 admissions by 2018, or 5.6 entries per 1,000 children. For the median caseload rate county, the implied increase is 586 additional cases by 2018, or 17.3 cases per 1,000 children. It is important to emphasize that this is a crude way to scale up the estimates that requires strong simplifying assumptions, and the implied growth in foster care entries and cases should be regarded as approximations.

Total direct public expenditures on out-of-home placement costs were $32,000 per child

[58] I calculate this share in each ACS year and take the average over annual estimates 2010-2018.

in FY 2016.[59] For the median entry rate county in 2010, the implied increase in admissions would incur an additional \$6.1 million (2018 \$), or \$762,500 annually. For the median caseload rate county in 2010, the direct cost is \$7.8 million associated with the implied increase in caseload, or \$975,000 annually.[60] These are lower bounds on the total cost of additional entry or caseload due to parental illicit opioid use during 2010-2018, since they do not take into account the private and social costs of the potential negative effects of foster care placement on adult outcomes later in life.

In addition to the implied increases in entry/caseload for the median county in 2010, we can also scale the IV estimates across all counties in the U.S. to understand the size of the effect of parental illicit opioid use in the aggregate. To do this, consider a counterfactual where every county is assigned a change in the illicit opioid death rate during 2010-18 which is equal to the actual 10th percentile change in the illicit opioid death rate during this period. The estimated effect on entry (using the LD between two-year averages 2010-11 and 2017-18) indicates that the 10th percentile change in the illicit opioid death rate is associated with a 0.87% increase in foster care entry. Aggregating this increase across all counties, we would have seen roughly 6,000 fewer foster care admissions between 2010 and 2018, which is 70% of the actual increase in admissions. This counterfactual reduction in admissions would have saved \$200 million in foster care placement costs.

[59] Total federal, state, and local expenditures on out-of-home placement costs in SFY 2016 were roughly \$13.9 billion (Rosinsky and Williams 2018). Dividing by the 437,465 children in foster care in FY 2016 yields \$32,000 per child in out-of-home placement costs.

[60] I assume admissions are distributed evenly across each year during 2011-18, and that each admission stays the median number of months in foster care as reported in the annual AFCARS Report (https://www.acf.hhs.gov/cb/research-data-technology/statistics-research/afcars). I use the same assumptions for foster care caseload.

A limitation of this study is that foster care placement as a result of parents' illicit opioid use is only one of the many consequences of the opioid crisis for children. Given the observed increases in foster care entry and caseload, we would also expect increases in child maltreatment (Evans et al. 2022) that do not necessarily result in foster care placement. Rates of children living away from at least one parent and living with grandparents have also increased (Buckles et al. 2020). Although placement in foster care is a relatively rare event even among at-risk children (children entering foster care during the sample period made up 7.5% to 9.1% of all children who were subject to an investigated maltreatment report), it is an important outcome to understand from the perspective of children's well-being. Foster care entry has been shown to increase rates of imprisonment and teenage motherhood, and to reduce earnings and the rate of employment (Doyle 2007, 2008; Warburton et al. 2014). 60% of the admissions in my sample were placed with either an unrelated foster family or a group home/institution, which may be more disruptive to child development than living with relatives such as grandparents, whether informally or as a formal foster care placement.

1.6 Conclusion

The opioid crisis of the past two decades has taken a toll on society beyond rising rates of opioid abuse, addiction, and overdose deaths. In particular, we may be concerned that opioid abuse by parents has negative spillover effects on children through channels such as safety of the home, employment and family resources, and the risk of incarceration and overdose death. This paper explores the effect of parental opioid abuse on a child outcome that is especially disruptive to development and potentially has adverse implications later in life–entry into foster care. I focus on the growth in foster care admissions and caseload in the post-2010 period, notable because it reversed the previous decade's decline in foster care entry and also because it coincided with a shift in the opioid crisis from prescription opioid misuse to use of illicit opioids (i.e. heroin and synthetic opioids). To account for this change in the composition of opioid abuse and potentially different treatment effects on the risk of entering foster care, the empirical analysis separately identifies the effects of parental illicit opioid use and PO misuse, which are proxied, respectively, by rates of illicit opioid and PO-only overdose death among prime parenting ages. To estimate the causal effects of the two types of parental opioid abuse on foster care outcomes, baseline PO supply measures from year 2000 are used to instrument for long-differenced changes relative to 2010 in illicit opioid and PO-only death rates. I show evidence for supply-side policy mechanisms underlying the first stage and provide support for the plausibility of the exclusion restriction.

The IV estimates suggest that the change after 2010 in parental illicit opioid use significantly increased total foster care entry and caseload: between 2010-11 and 2017-18, a 1-sd increase in the change in the illicit opioid death rate ages 18-57 was associated with a 0.84 sd (1.06 sd) increase in the percent change in foster care entry (caseload). In contrast, parental PO misuse did not directly affect total entry or caseload, but it did significantly increase kinship foster care entry and cases. To my knowledge, this paper is the first in the literature to quantify the causal link between adult illicit opioid use and the recent increase in foster care entry. The magnitude of the effect of illicit opioid use on admissions is comparable to the positive elasticity (1.54) of foster care admissions with respect to methamphetamine use estimated by Cunningham and Finlay (2012). The distinct role of illicit opioid use also confirms previous results that the OxyContin reformulation, which induced substitution toward illicit opioids, increased child maltreatment and foster care admissions (Dallman 2020; Evans et al. 2022). Unlike these studies, however, I use variation in changes in opioid deaths induced by different dimensions of exposure to the PO supply at the very beginning of the opioid crisis, which highlights how differential initial exposure to the opioid crisis has led to cascading effects on the next generation.

This paper also adds to the literature evidence of heterogeneity in the effects of parental opioid abuse on foster care outcomes. Although PO misuse did not affect total entry, it increased entry to kinship care, whereas illicit opioid use increased entry to both kinship care and non-relative placement settings. This suggests differences in the availability of other family members to take care of the child between parents using illicit opioids and those misuing POs. I also find a larger effect of female illicit opioid use, as compared to male illicit opioid use, on foster care entry and caseload, a novel result that merits further study to uncover the reasons for heterogeneity by gender-specific illicit opioid use. Although I find that effects do not differ across child age, it may be interesting to examine whether there is heterogeneity by child age in states that have specific reporting procedures for prenatal drug exposure.

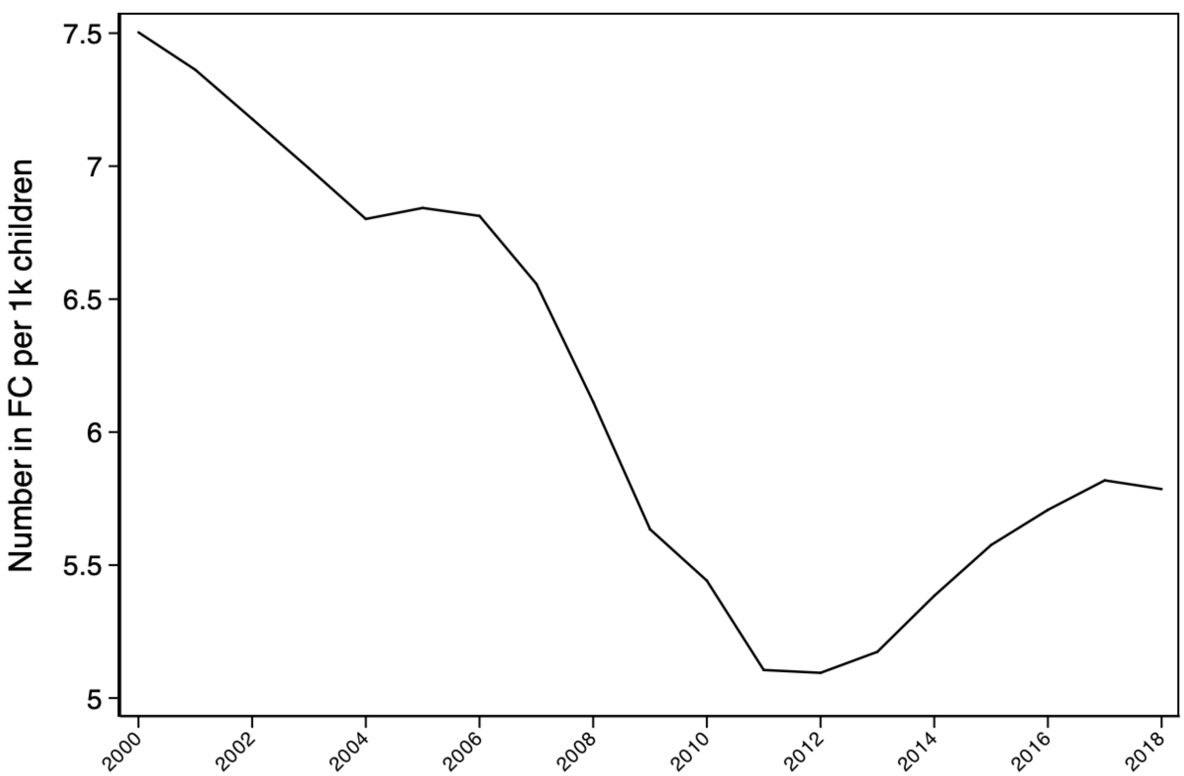
These results highlight the need for child welfare policies that integrate substance abuse and mental health treatment. The Family First Prevention Services Act of 2018 took an important step in this direction by allowing states to use federal Title IV-E funds for up to 12 months of services aimed at preventing at-risk children from entering foster care. These services include substance abuse and mental health treatment provided by a qualified clinician, and must meet evidence-based requirements (Children's Defense Fund). More generally, collaboration between child welfare agencies and service systems that address substance abuse may reduce entry into foster care due to parental illicit opioid use. The specific role of illicit opioid use in increasing foster care admissions and caseload after 2010 also has implications for opioid-related policy. Given the limited effectiveness of illicit drug prohibitions by law enforcement (Maclean et al. 2020), it may be worthwhile to consider demand-side policies if the goal is to reduce illicit opioid use. One such policy is expanding the use of medication-assisted treatment (MAT) for opioid use disorder, a highly effective but underutilized type of treatment (Bouchery 2021). Recent growth in the number of physicians who are trained and DEA-waivered to prescribe buprenorphine, an opioid agonist, has increased potential access to MAT (Dick et al. 2015). Another demand-side policy issue is health insurance coverage for SUD treatment. The Affordable Care Act (ACA) specifically listed SUD treatment as an essential benefit that must be covered by most plans, including Medicaid expansion programs. Despite changes in coverage through the ACA and other government efforts to expand the use of treatment and recovery support services, rates of SUD treatment remain very low and have not increased in recent years (Bouchery 2021).

The effects of parental illicit opioid use on foster care outcomes can be viewed as the "tip of the iceberg" when it comes to the overall impact of the opioid crisis on children.

Given increases since 1980 in the rate of children living away from at least one parent and the rate of children living with grandparents (Buckles et al. 2020), an examination of these outcomes using the framework of this paper would disentangle the contributions of different phases of the opioid crisis to broader changes in children's home environments. Parental opioid abuse may also affect their children's physical and mental health and performance in school, which in turn have ramifications in adulthood. Future research should also examine these important child outcomes.

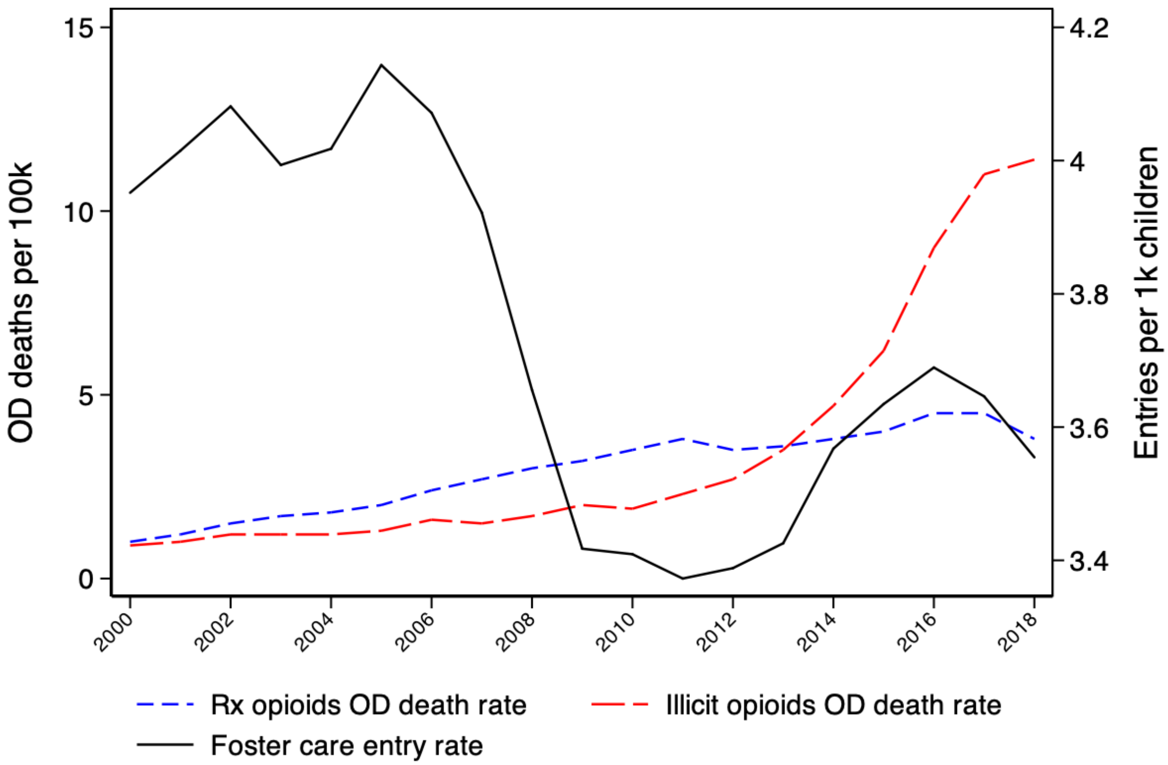
1.7 Figures

Figure 1.1: Children under age 18 in foster care, 2000-2018



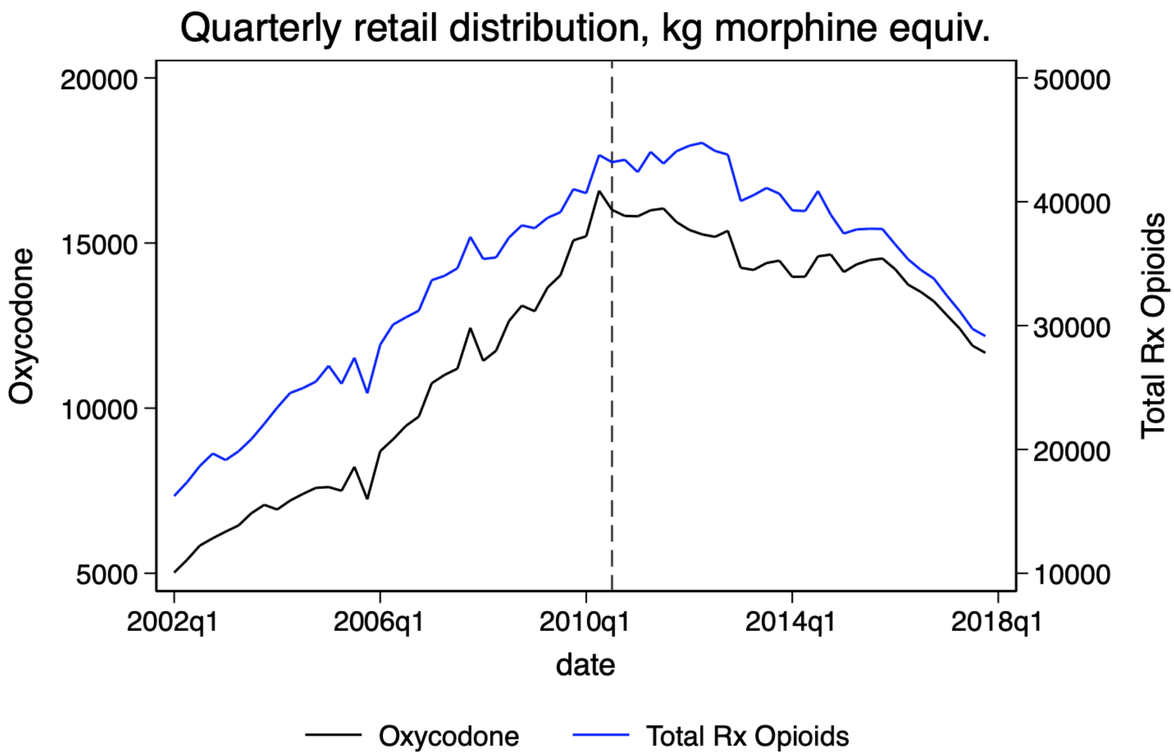
Child Trends analysis of data from AFCARS. Children are defined as being in foster care if they entered prior to the end of the current fiscal year and have not been discharged from their latest foster care spell by the end of the current fiscal year. Rates are based on Census Bureau estimates of the population of children ages 0 to 17 as of July 1st of the respective year.

Figure 1.2: Foster care admissions and opioid overdose deaths, 2000-2018



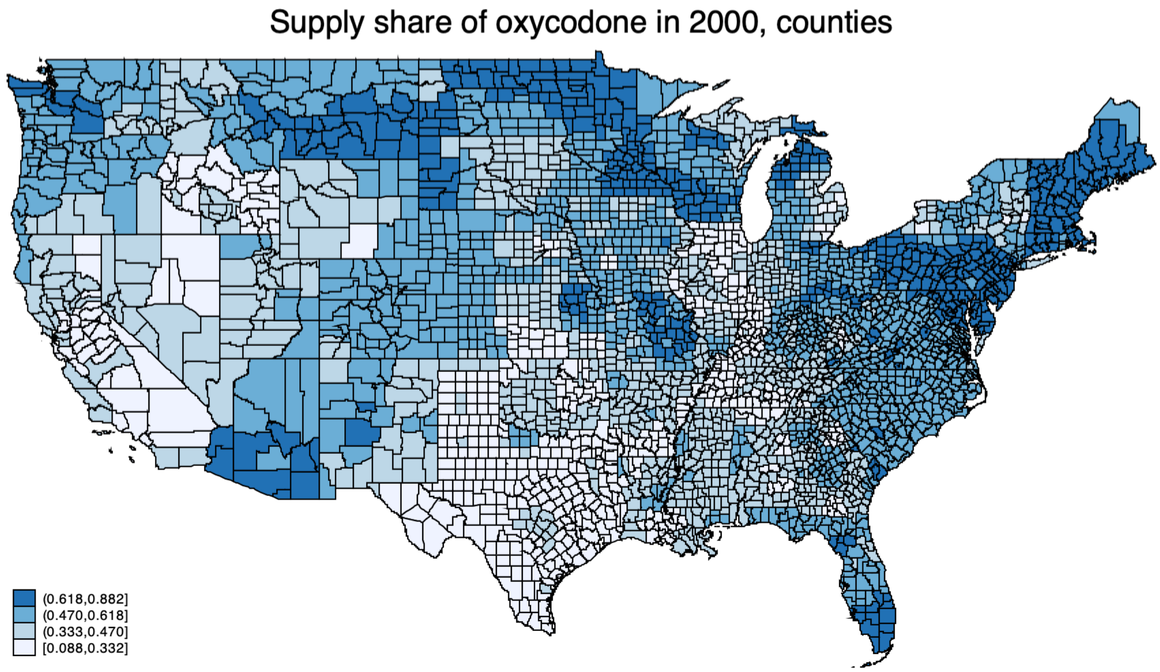
National data from CDC WONDER and AFCARS. OD death rates are calculated from CDC Multiple Cause of Death data. Rx opioids: drug overdose deaths involving natural and semi-synthetic opioids. Illicit opioids: drug overdose deaths involving heroin or synthetic opioids. Foster care entry rate is calculated from case-level data collected through AFCARS.

Figure 1.3: National supply of oxycodone and all Rx opioids



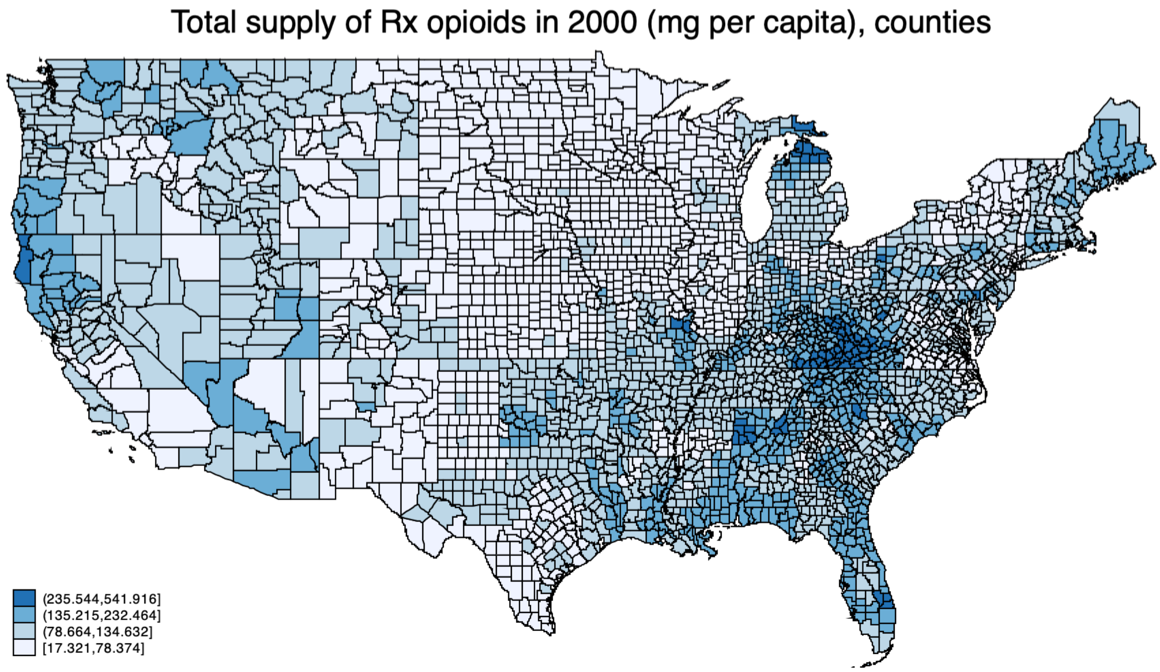
Aggregated from state quarterly retail opioid distribution data from ARCOS. Total Rx Opioids series is the sum of oxycodone, hydrocodone, morphine, hydromorphone, fentanyl base, and meperidine in Morphine Gram Equivalent units.

Figure 1.4: Geographic variation in oxycodone share in 2000



Data: DEA ARCOS Report 1, Retail Drug Distribution by 3-digit ZIP code. Converted to county level using population-based allocation factors.

Figure 1.5: Geographic variation in Rx opioid supply in 2000



Data: DEA ARCOS Report 1, Retail Drug Distribution by 3-digit ZIP code. Converted to county level using population-based allocation factors.

Figure 1.6: Distributions of baseline exposure measures

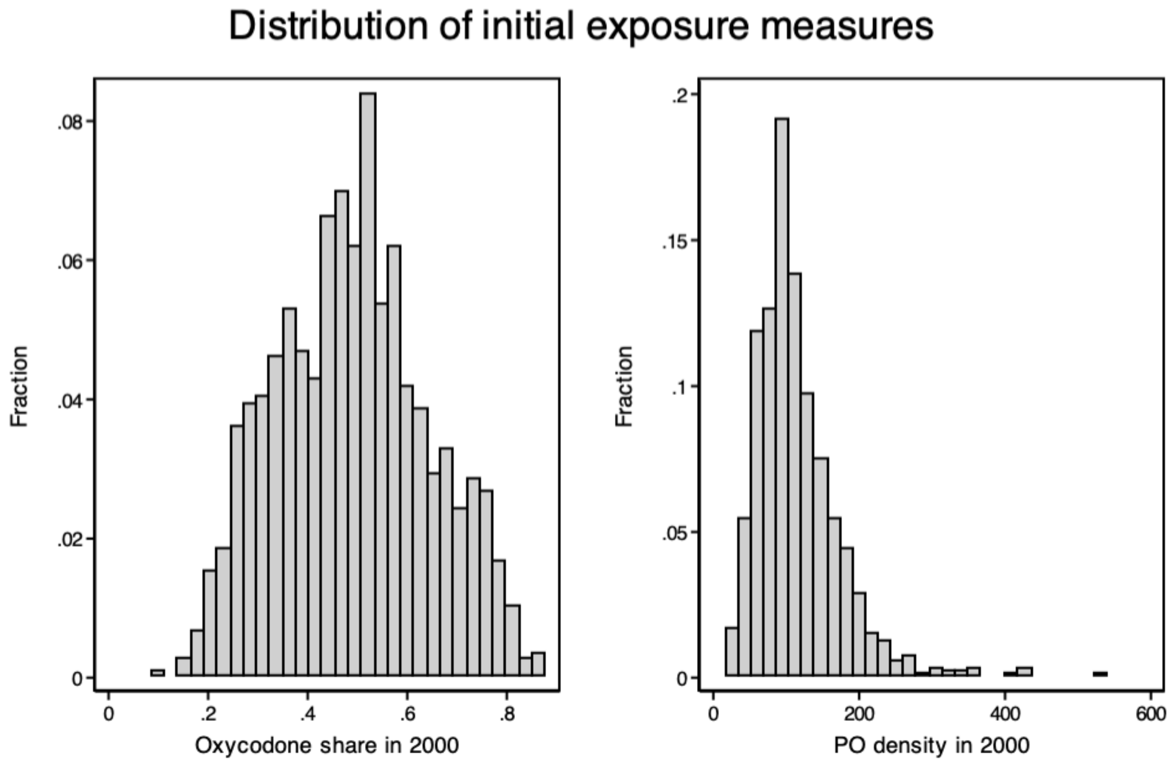


Figure 1.7: Correlation between the two baseline exposure measures

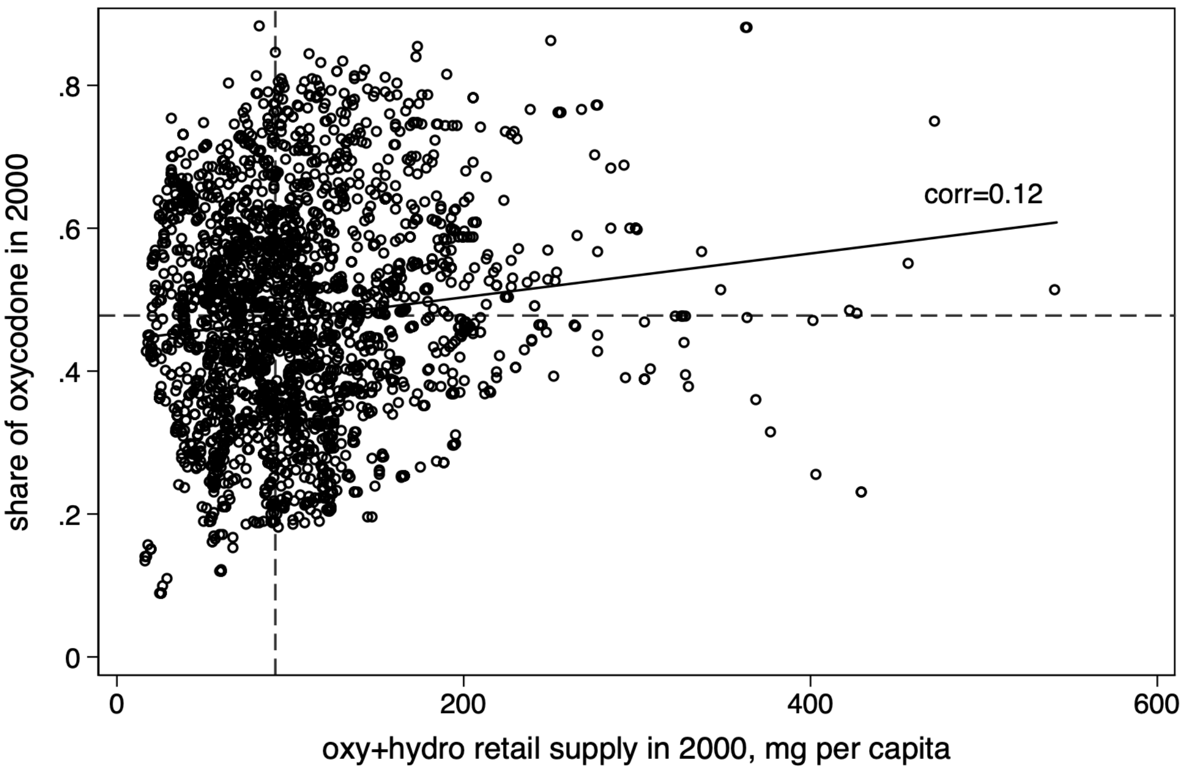
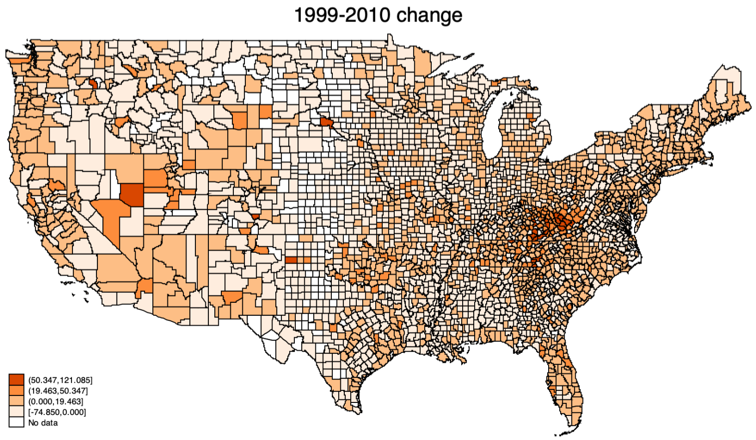


Figure 1.8: PO-only overdose death rate



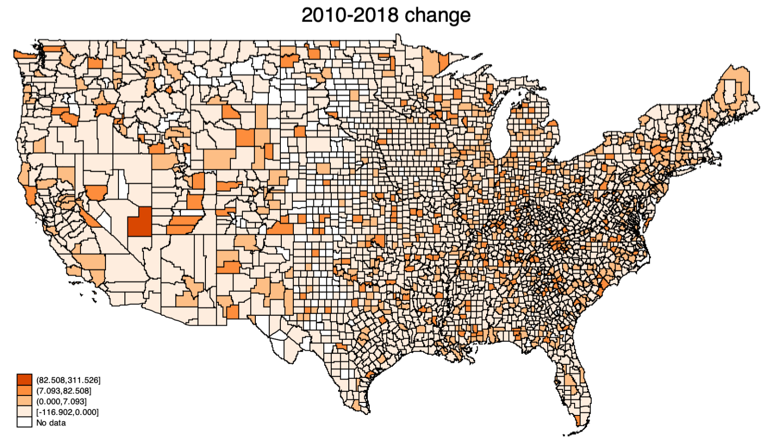


Figure 1.9: Illicit opioid overdose death rate

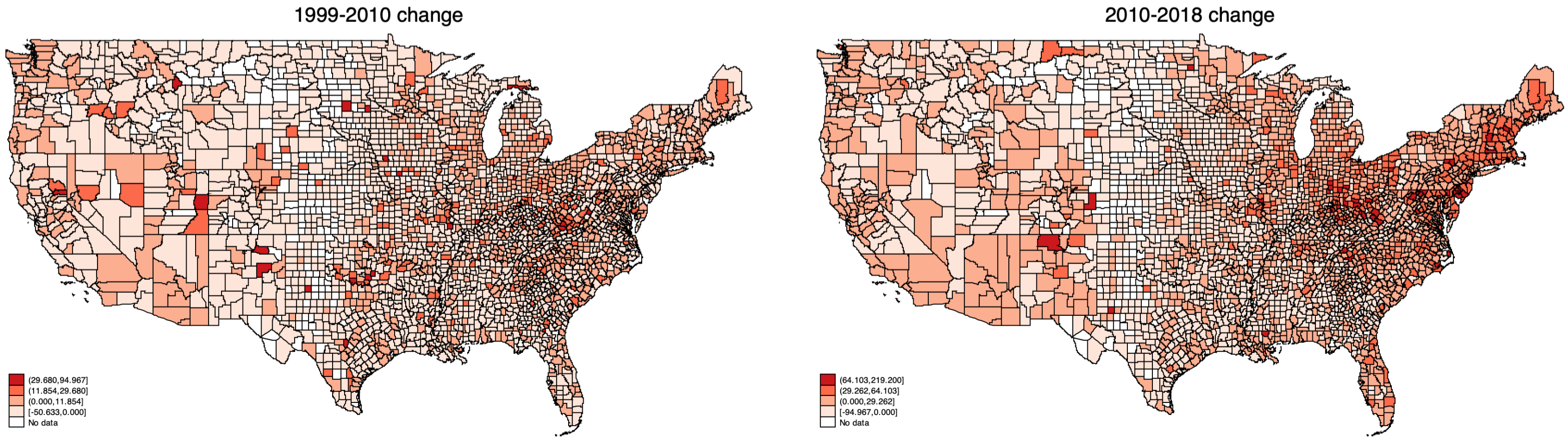
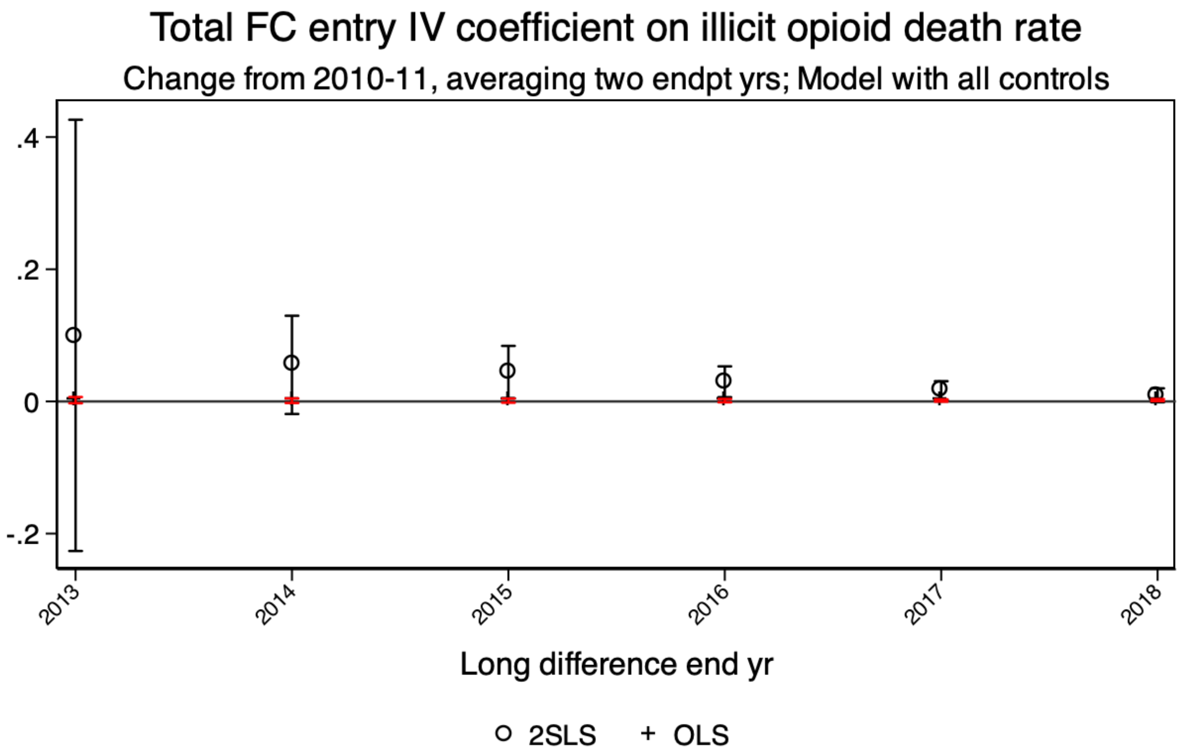
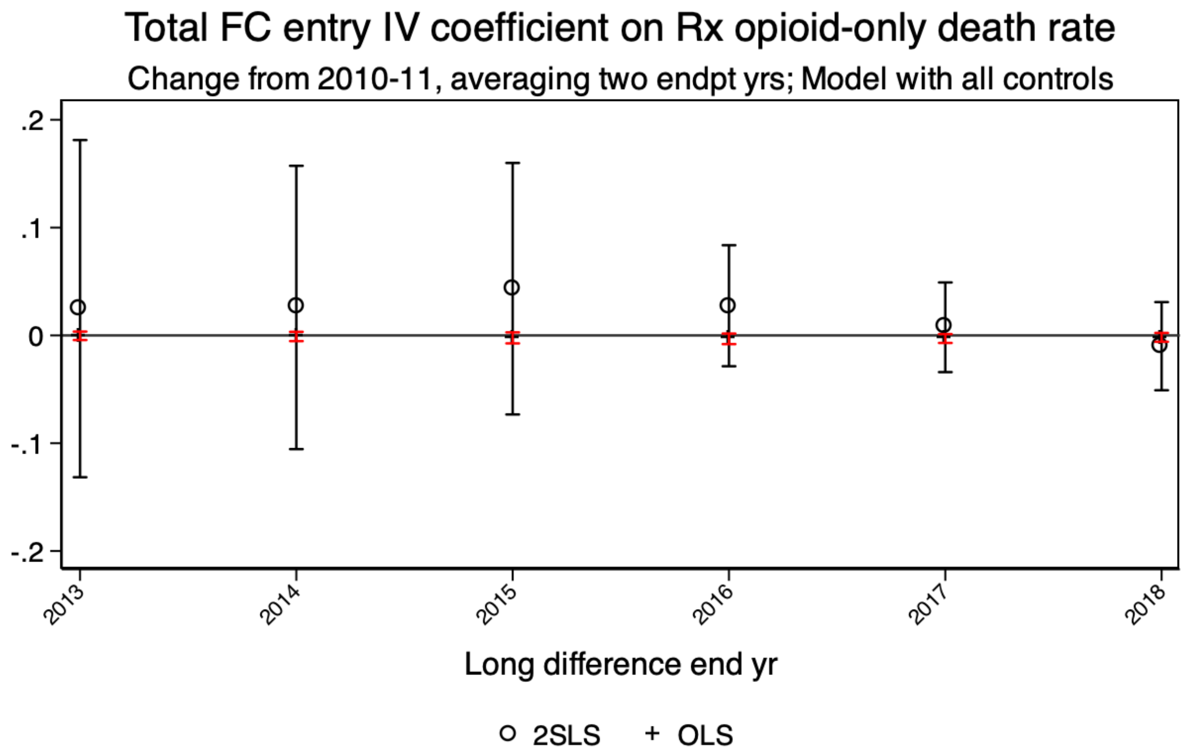


Figure 1.10: IV estimates of effect of ΔIllicit opioid use on ΔFoster care entry



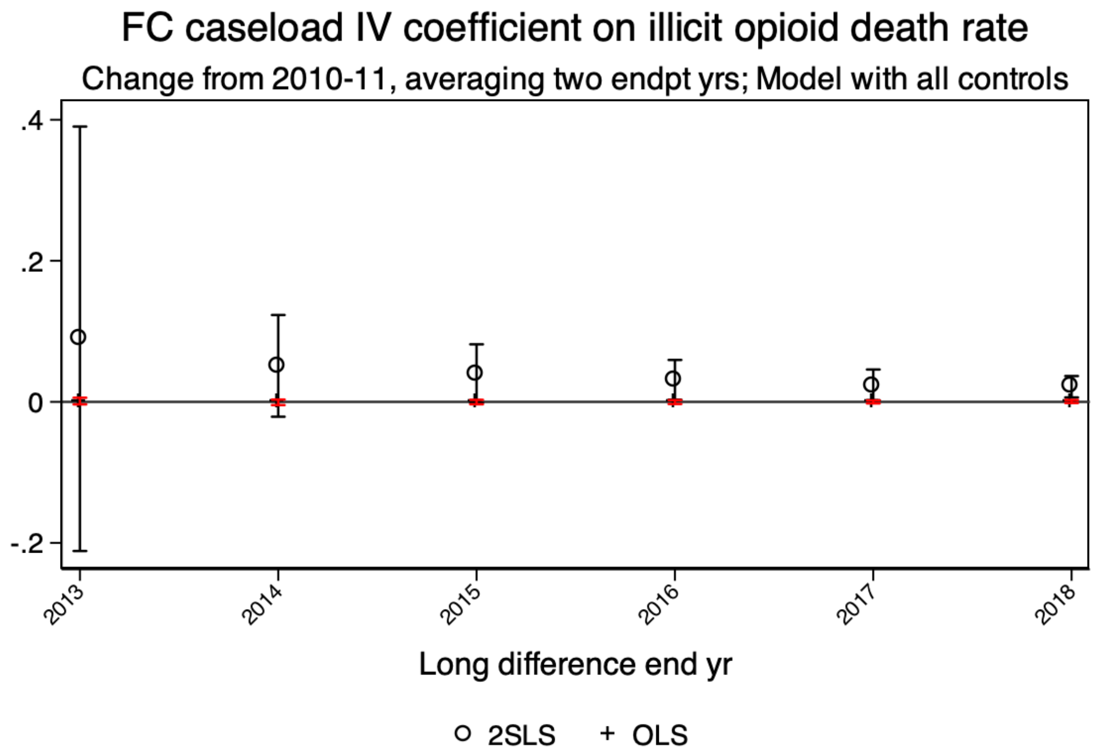
The 2016, 2017, and 2018 point estimates correspond to ΔIllicit op mort coefficients in Table 1.5, column 4. 95% confidence intervals for 2SLS estimates are computed using F-dependent critical values for $|t|$ provided in Table 3 of Lee et al. (2020).

Figure 1.11: IV estimates of effect of ΔRx opioid misuse on ΔFoster care entry



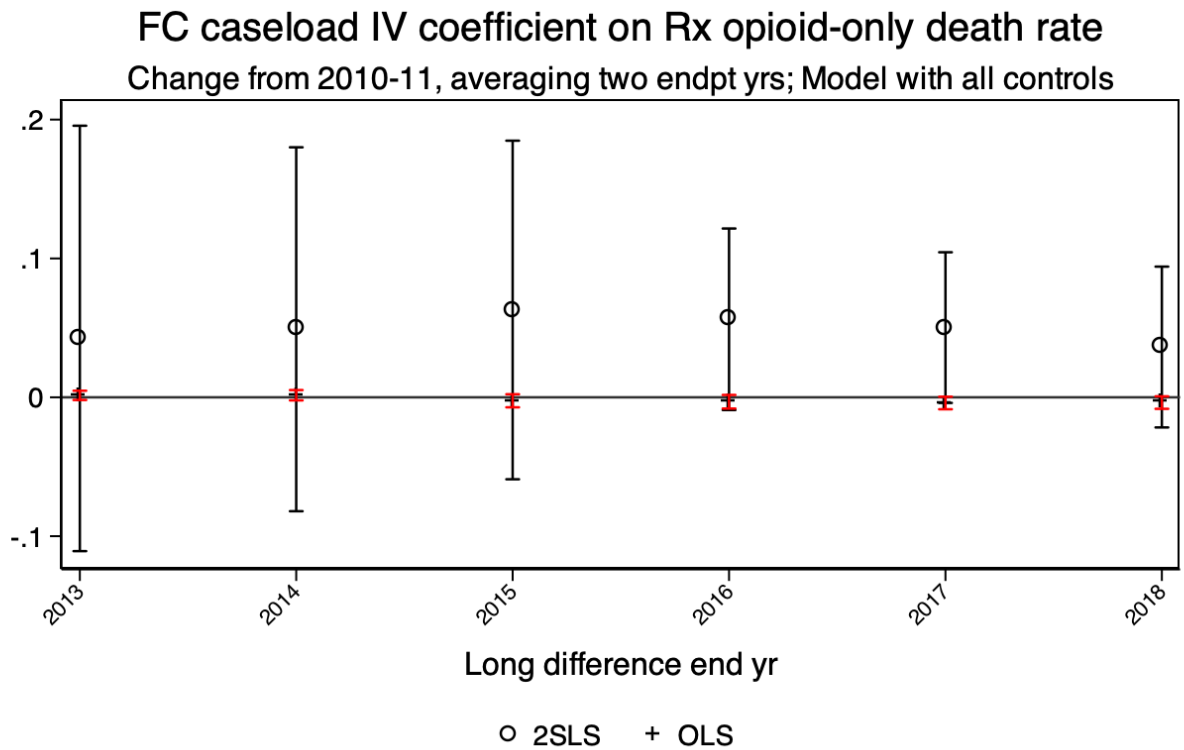
The 2016, 2017, and 2018 point estimates correspond to ΔPO-only mort coefficients in Table 1.5, column 4. 95% confidence intervals for 2SLS estimates are computed using F-dependent critical values for $|t|$ provided in Table 3 of Lee et al. (2020).

Figure 1.12: IV estimates of effect of ΔIllicit opioid use on ΔFoster care caseload



The 2016, 2017, and 2018 point estimates correspond to ΔIllicit op mort coefficients in Table A.8, column 4. 95% confidence intervals for 2SLS are computed using F-dependent critical values for $|t|$ provided in Table 3 of Lee et al. (2020).

Figure 1.13: IV estimates of effect of ΔRx opioid misuse on ΔFoster care caseload



The 2016, 2017, and 2018 point estimates correspond to ΔPO-only mort coefficients in Table A.8, column 4. 95% confidence intervals for 2SLS are computed using F-dependent critical values for $|t|$ provided in Table 3 of Lee et al. (2020).

1.8 Tables

Table 1.1: Foster care entry descriptive statistics

	Identified counties		All counties	
	Mean (SD)	N	Mean (SD)	N
Child characteristics				
Female	0.49	2,086,079	0.49	2,295,942
White	0.61	2,086,583	0.63	2,296,496
Black	0.29	2,086,583	0.27	2,296,496
Other race	0.06	2,086,583	0.06	2,296,496
Hispanic origin	0.22	2,046,786	0.21	2,254,406
Age at first removal	6.45 (5.56)	2,086,115	6.51 (5.56)	2,295,997
Age at latest removal	6.85 (5.70)	2,086,583	6.90 (5.70)	2,296,496
Total number of removals	1.25 (0.61)	2,085,383	1.25 (0.61)	2,295,168
Title IV-E FC payments	0.29	2,086,071	0.29	2,295,889
Eligible for Medicaid	0.77	2,085,627	0.77	2,295,430
Current placement setting				
Placed with relatives	0.33	2,086,583	0.33	2,296,496
Unrelated foster family	0.46	2,086,583	0.46	2,296,496
Group home or institution	0.14	2,086,583	0.14	2,296,496
Reason for latest removal				
Neglect	0.60	2,048,729	0.59	2,257,274
Parental drug use	0.31	2,048,729	0.31	2,257,274
Parental alcohol abuse	0.06	2,048,729	0.06	2,257,274
Physical abuse	0.14	2,048,731	0.14	2,257,276
Parental incarceration	0.07	2,048,729	0.08	2,257,274
Inability to cope	0.16	2,048,729	0.16	2,257,274
Inadequate housing	0.10	2,048,607	0.10	2,257,152

Source: AFCARS 6-month Foster Care Files, FY 2010-18. Identified counties: all foster care entries ages 0-17 where the county FIPS code is identified. All counties: all foster care entries ages 0-17. Means of indicator variables except for age at first and latest removal and total number of removals, which are continuous variables. An entry may have multiple or no reasons for removal, and not all reasons for removal are shown.

Table 1.2: Summary statistics of key regression variables and covariates

	2010/11-2012/13	2010/11-2013/14	2010/11-2014/15	2010/11-2015/16	2010/11-2016/17	2010/11-2017/18
Δ IHS foster care entry	-0.003	0.021	0.041	0.046	0.038	0.007
	(0.231)	(0.272)	(0.308)	(0.326)	(0.341)	(0.359)
Δ IHS foster care caseload	-0.015	0.007	0.040	0.058	0.064	0.056
	(0.209)	(0.252)	(0.300)	(0.331)	(0.358)	(0.382)
Δ FC entry rate (per 1k children)	-0.001	0.091	0.180	0.229	0.231	0.146
	(1.021)	(1.261)	(1.440)	(1.582)	(1.657)	(1.708)
Δ FC caseload rate (per 1k children)	-0.037	0.099	0.301	0.428	0.509	0.520
	(1.356)	(1.887)	(2.346)	(2.751)	(3.046)	(3.179)
Δ Illicit opioid OD death rate (per 100k)	1.710	3.476	5.841	9.567	13.836	15.971
	(3.248)	(5.182)	(8.215)	(12.613)	(17.604)	(18.843)
Δ PO-only OD death rate (per 100k)	-0.371	-0.487	-0.536	-0.611	-0.802	-1.473
	(3.133)	(3.476)	(3.697)	(4.021)	(4.320)	(4.443)
Δ Share age 0-19	-0.610	-0.865	-1.092	-1.290	-1.475	-1.666
	(0.270)	(0.377)	(0.479)	(0.573)	(0.662)	(0.750)
Δ Share age 20-24	0.130	0.120	0.044	-0.089	-0.230	-0.345
	(0.336)	(0.460)	(0.563)	(0.642)	(0.690)	(0.719)
Δ Share age 25-54	-0.611	-0.871	-1.078	-1.245	-1.387	-1.527
	(0.449)	(0.631)	(0.796)	(0.948)	(1.073)	(1.167)
Δ Share age 55-64	0.325	0.470	0.617	0.752	0.849	0.905
	(0.209)	(0.295)	(0.377)	(0.458)	(0.535)	(0.609)
Δ Share age 65+	0.765	1.142	1.507	1.873	2.245	2.630
	(0.309)	(0.435)	(0.546)	(0.643)	(0.735)	(0.829)
Δ Share female	-0.023	-0.034	-0.049	-0.060	-0.067	-0.071
	(0.147)	(0.182)	(0.208)	(0.225)	(0.241)	(0.255)
Δ Share non-white	0.450	0.693	0.947	1.209	1.478	1.748
	(0.355)	(0.528)	(0.710)	(0.908)	(1.113)	(1.322)
Δ Unemployment rate	-1.628	-2.593	-3.629	-4.271	-4.728	-5.212
	(0.816)	(1.049)	(1.279)	(1.543)	(1.735)	(1.826)
Δ Labor force participation rate	-0.881	-1.302	-1.620	-1.669	-1.551	-1.484
	(1.014)	(1.248)	(1.384)	(1.482)	(1.645)	(1.857)
Δ Child poverty rate	0.391	-0.048	-0.825	-1.956	-3.093	-3.813
	(1.579)	(1.695)	(1.777)	(1.936)	(2.163)	(2.392)
Δ Share low-income single parent	0.176	0.071	-0.143	-0.532	-0.916	-1.241
	(0.394)	(0.444)	(0.409)	(0.472)	(0.523)	(0.554)
Δ Teenage birth rate	-5.045	-7.680	-9.826	-11.871	-13.682	-15.178
	(1.309)	(1.815)	(2.400)	(3.014)	(3.642)	(4.161)
Δ Alcohol death rate	0.325	0.633	1.137	1.537	1.714	1.924
	(3.203)	(3.359)	(3.652)	(3.895)	(4.112)	(4.376)
Δ Treatment facility density	0.226	0.275	0.395	0.607	0.672	0.705
	(1.670)	(1.739)	(1.852)	(2.006)	(2.255)	(2.776)
Δ Child and youth services density	0.067	0.089	0.187	0.259	0.195	0.237
	(0.923)	(1.010)	(1.128)	(1.246)	(1.308)	(1.608)
Δ Other social services density	0.108	-0.010	-0.041	-0.003	-0.037	-0.137
	(1.111)	(1.130)	(1.191)	(1.289)	(1.472)	(1.721)
Δ Public welfare share of state+local exp	0.698	1.254	2.211	3.115	3.322	3.456
	(0.931)	(1.068)	(1.531)	(2.316)	(2.471)	(2.751)
Δ Housing+community dev share of state+local exp	-0.112	-0.200	-0.260	-0.317	-0.345	-0.353
	(0.232)	(0.294)	(0.317)	(0.347)	(0.375)	(0.392)
Δ Corrections share of state+local exp	-0.056	-0.072	-0.081	-0.114	-0.167	-0.202
	(0.157)	(0.188)	(0.210)	(0.187)	(0.169)	(0.193)

Each cell is the mean of the long difference (column) of the indicated variable (row); standard deviation in parentheses below the mean. All long differences use 2-year averaged levels. N=1,193 counties. All statistics are weighted by the county's 2018 population under age 18.

Table 1.3: OLS estimates of the relationship between opioid overdose rates and foster care admissions

Estimation Method	Illicit opioid OD death rate	PO-only OD death rate	N
County fixed effects	0.0010	-0.0010	9,544
	(0.0007)	(0.0015)	
First differences	0.0008	-0.0007	7,158
	(0.0006)	(0.0011)	
2-year Long differences	0.0009	-0.0011	5,965
	(0.0007)	(0.0015)	
3-year Long differences	0.0009	-0.0012	4,772
	(0.0007)	(0.0018)	
4-year Long differences	0.0010	-0.0008	3,579
	(0.0007)	(0.0019)	
5-year Long differences	0.0011	-0.0002	2,386
	(0.0007)	(0.0020)	
6-year Long differences	0.0015*	-0.0008	1,193
	(0.0008)	(0.0023)	

Outcome variable is IHS foster care admissions count. Levels of all variables are averages over two consecutive years. County fixed effects model is given by equation (1.4). First differences and j-period long differences models are given by equation (1.3); see Section 1.4.2. Each row shows the OLS coefficients on the illicit opioid OD death rate and PO-only OD death rate. All regressions are weighted by county population under age 18, and include the full set of controls: demographic composition, socioeconomic conditions, other substance abuse factors, public expenditure shares, and the supply of child welfare services and other social assistance. Standard errors clustered at county level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 1.4: First stage estimates

	(1) ΔIllicit op mort	(2) ΔPO-only mort
LD 2010/11-2015/16		
OxyShare 2000	14.9441***	4.3662***
	(4.3187)	(1.0811)
PO density 2000	0.0315***	-0.0216***
	(0.0118)	(0.0047)
Dep. var. mean	9.567	-0.611
R^2	0.401	0.183
LD 2010/11-2016/17		
OxyShare 2000	17.9676***	4.6262***
	(5.8661)	(1.1354)
PO density 2000	0.0577***	-0.0276***
	(0.0180)	(0.0052)
Dep. var. mean	13.836	-0.802
R^2	0.425	0.165
LD 2010/11-2017/18		
OxyShare 2000	22.5237***	2.9645**
	(6.1842)	(1.2348)
PO density 2000	0.0736***	-0.0328***
	(0.0197)	(0.0054)
Dep. var. mean	15.971	-1.473
R^2	0.475	0.184
OxyShare 2000 mean (SD): 0.491 (0.1521)		
PO density 2000 mean (SD): 115.897 mg/resident (56.827)		

N=1,193 counties. LD using 2010-11 average and 2015-16, 2016-17, or 2017-18 average. All regressions are weighted by the county's 2018 population under age 18, and include the full set of controls in long differences relative to 2010-11 average and their initial levels in 2000. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table 1.5: IV estimates for foster care entry, average of two endpoint years

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry	(4) Δ IHS Entry	(5) Δ IHS Entry	(6) Δ IHS Entry
LD 2010/11-2015/16						
Δ Illicit op mort	0.0270***	0.0256***	0.0280***	0.0283***	0.0297***	0.0297***
	(0.0086)	(0.0078)	(0.0079)	(0.0081)	(0.0087)	(0.0086)
tF 95% CI						(.0064, .053)
Δ PO-only mort	0.0283	0.0269	0.0243	0.0224	0.0277	0.0275
	(0.0307)	(0.0283)	(0.0250)	(0.0245)	(0.0230)	(0.0231)
tF 95% CI						(-.0288, .0837)
Dep. var. mean	0.046	0.046	0.046	0.046	0.046	0.046
Cragg-Donald F	15.023	17.157	20.432	20.799	24.236	23.902
Illicit op mort SW F	19.122	21.778	20.679	20.257	17.608	17.823
PO-only mort SW F	17.537	19.747	22.399	22.307	26.235	25.798
LD 2010/11-2016/17						
Δ Illicit op mort	0.0153***	0.0143***	0.0157***	0.0163***	0.0164***	0.0163***
	(0.0047)	(0.0044)	(0.0049)	(0.0051)	(0.0052)	(0.0051)
tF 95% CI						(.0019, .0308)
Δ PO-only mort	0.0071	0.0046	0.0081	0.0069	0.0076	0.0075
	(0.0215)	(0.0203)	(0.0190)	(0.0188)	(0.0174)	(0.0174)
tF 95% CI						(-.0342, .0491)
Dep. var. mean	0.038	0.038	0.038	0.038	0.038	0.038
Cragg-Donald F	16.563	18.104	19.024	19.165	21.492	21.371
Illicit op mort SW F	22.700	23.026	18.222	17.768	15.899	15.995
PO-only mort SW F	20.919	22.431	24.478	24.522	28.229	28.025
LD 2010/11-2017/18						
Δ Illicit op mort	0.0095**	0.0082**	0.0086**	0.0088**	0.0093**	0.0093**
	(0.0037)	(0.0035)	(0.0041)	(0.0042)	(0.0041)	(0.0041)
tF 95% CI						(-.0013, .0199)
Δ PO-only mort	-0.0094	-0.0168	-0.0142	-0.0140	-0.0100	-0.0100
	(0.0171)	(0.0165)	(0.0170)	(0.0171)	(0.0161)	(0.0161)
tF 95% CI						(-.0508, .0308)
Dep. var. mean	0.007	0.007	0.007	0.007	0.007	0.007
Cragg-Donald F	18.022	18.881	15.909	15.569	18.870	18.826
Illicit op mort SW F	25.045	23.735	20.010	19.710	20.750	20.845
PO-only mort SW F	22.787	22.689	19.441	18.995	22.102	21.993
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table 1.6: OLS estimates for foster care entry, average of two endpoint years

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry	(4) Δ IHS Entry	(5) Δ IHS Entry	(6) Δ IHS Entry
LD 2010/11-2015/16						
Δ Illicit op mort	0.0018	0.0018	0.0014	0.0013	0.0012	0.0012
	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0013)	(0.0013)
Δ PO-only mort	-0.0049**	-0.0043*	-0.0034	-0.0033	-0.0031	-0.0032
	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0025)	(0.0025)
Dep. var. mean	0.046	0.046	0.046	0.046	0.046	0.046
R^2	0.107	0.114	0.136	0.138	0.138	0.138
LD 2010/11-2016/17						
Δ Illicit op mort	0.0021**	0.0020**	0.0014*	0.0014	0.0014	0.0014
	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
Δ PO-only mort	-0.0042**	-0.0038*	-0.0033	-0.0032	-0.0028	-0.0029
	(0.0021)	(0.0021)	(0.0020)	(0.0021)	(0.0021)	(0.0021)
Dep. var. mean	0.038	0.038	0.038	0.038	0.038	0.038
R^2	0.129	0.136	0.162	0.163	0.164	0.164
LD 2010/11-2017/18						
Δ Illicit op mort	0.0029***	0.0028***	0.0023***	0.0023***	0.0020**	0.0020**
	(0.0009)	(0.0009)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Δ PO-only mort	-0.0027	-0.0023	-0.0021	-0.0020	-0.0017	-0.0018
	(0.0021)	(0.0020)	(0.0020)	(0.0020)	(0.0021)	(0.0021)
Dep. var. mean	0.007	0.007	0.007	0.007	0.007	0.007
R^2	0.153	0.163	0.179	0.180	0.187	0.189
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table 1.7: IV and OLS estimates for foster care caseload and prior entry cohorts' continuation rates

	(1) Δ IHS Caseload	(2) Δ $t-1$ Cont. Rate	(3) Δ $t-2$ Cont. Rate
Panel A: IV estimates			
Δ Illicit op mort	0.0214***	0.0002	0.0006
	(0.0059)	(0.0017)	(0.0015)
tF 95% CI	(.0062, .0366)	(-.0061, .0065)	(-.0066, .0077)
Δ PO-only mort	0.0362	0.0024	0.0091
	(0.0228)	(0.0061)	(0.0079)
tF 95% CI	(-.0216, .094)	(-.0176, .0224)	(-.0299, .0482)
Dep. var. mean	0.056	0.048	0.027
N	1,193	1,178	1,190
Cragg-Donald F	18.826	10.681	5.720
Illicit op mort SW F	20.845	9.155	6.020
PO-only mort SW F	21.993	10.732	6.242
Panel B: OLS estimates			
Δ Illicit op mort	0.0005	-0.0001	-0.0002
	(0.0011)	(0.0002)	(0.0002)
Δ PO-only mort	-0.0038	-0.0004	0.0001
	(0.0023)	(0.0007)	(0.0006)
Dep. var. mean	0.056	0.048	0.027
N	1,193	1,178	1,190
R^2	0.198	0.079	0.117

Column 1: long difference is taken between averaged levels 2010-11 and 2017-18. Column 2: the $t-1$ continuation rate is defined as the share of children entering foster care in FY $t-1$ who remain in care at the end of FY t. Long difference is taken between averaged levels 2011-12 and 2017-18. LD is relative to averaged levels over 2011-12 because prior FY continuation rate is missing for the first year of the sample period, 2010. Column 3: the $t-2$ continuation rate is defined as the share of children entering foster care in FY $t-2$ who remain in care at the end of FY t. Long difference is taken between averaged levels 2012-13 and 2017-18. LD is relative to averaged levels over 2012-13 because $t-2$ FY continuation rate is missing for the first 2 years of the sample period, 2010-11. Instruments for the IV models in panel A are the oxycodone share and per-capita PO supply in 2000. All columns include the full set of covariates. Observations are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table 1.8: IV estimates for foster care entry, by placement setting

	(1) ΔIHS Entry, Non-relative foster home	(2) ΔIHS Entry, Relative foster home	(3) ΔIHS Entry, Group home/institution
LD 2010/11-2015/16			
Δ Illicit op mort	0.0377***	0.0368**	0.0289**
	(0.0103)	(0.0150)	(0.0128)
tF 95% CI	(.0098, .0655)	(-.0037, .0774)	(-.0059, .0638)
Δ PO-only mort	0.0058	0.0891**	-0.0239
	(0.0265)	(0.0420)	(0.0349)
tF 95% CI	(-.0587, .0703)	(-.0131, .1913)	(-.1087, .0608)
Dep. var. mean	0.020	0.183	0.046
Cragg-Donald F	23.902	23.902	23.902
Illicit op mort SW F	17.823	17.823	17.823
PO-only mort SW F	25.798	25.798	25.798
LD 2010/11-2016/17			
Δ Illicit op mort	0.0116**	0.0191*	0.0159*
	(0.0050)	(0.0112)	(0.0095)
tF 95% CI	(-.0024, .0256)	(-.0124, .0506)	(-.0106, .0424)
Δ PO-only mort	-0.0178	0.0642*	-0.0426
	(0.0173)	(0.0385)	(0.0317)
tF 95% CI	(-.059, .0234)	(-.0278, .1561)	(-.1185, .0332)
Dep. var. mean	0.036	0.211	-0.017
Cragg-Donald F	21.371	21.371	21.371
Illicit op mort SW F	15.995	15.995	15.995
PO-only mort SW F	28.025	28.025	28.025
LD 2010/11-2017/18			
Δ Illicit op mort	0.0053	0.0107	0.0076
	(0.0042)	(0.0116)	(0.0089)
tF 95% CI	(-.0056, .0162)	(-.0192, .0407)	(-.0152, .0305)
Δ PO-only mort	-0.0247	0.0248	-0.0558*
	(0.0173)	(0.0411)	(0.0330)
tF 95% CI	(-.0687, .0193)	(-.0795, .1291)	(-.1397, .0281)
Dep. var. mean	0.011	0.198	-0.106
Cragg-Donald F	18.826	18.826	18.826
Illicit op mort SW F	20.845	20.845	20.845
PO-only mort SW F	21.993	21.993	21.993

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Endogenous regressors in both columns are Δ Illicit opioid OD death rate and Δ PO-only OD death rate. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions include the full set of covariates, and are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table 1.9: IV estimates for foster care entry: gender-specific opioid overdose death rates

Regressors:	(1) ΔIHS Entry Female deaths	(2) ΔIHS Entry Male deaths
LD 2010/11-2015/16		
Δ Illicit op mort	0.0540***	0.0203***
	(0.0168)	(0.0058)
tF 95% CI	(.0092, .0988)	(.0042, .0363)
Δ PO-only mort	0.0514	0.0187
	(0.0409)	(0.0167)
tF 95% CI	(-.0701, .1729)	(-.0219, .0592)
Dep. var. mean	0.046	0.046
Cragg-Donald F	9.568	24.026
Illicit op mort SW F	18.147	17.096
PO-only mort SW F	13.325	26.192
LD 2010/11-2016/17		
Δ Illicit op mort	0.0333***	0.0108***
	(0.0120)	(0.0033)
tF 95% CI	(-.0062, .0727)	(.0019, .0196)
Δ PO-only mort	0.0240	0.0034
	(0.0311)	(0.0126)
tF 95% CI	(-.0684, .1164)	(-.026, .0328)
Dep. var. mean	0.038	0.038
Cragg-Donald F	8.810	24.154
Illicit op mort SW F	10.601	17.522
PO-only mort SW F	13.557	31.324
LD 2010/11-2017/18		
Δ Illicit op mort	0.0200**	0.0061**
	(0.0101)	(0.0026)
tF 95% CI	(-.0185, .0584)	(-.0002, .0123)
Δ PO-only mort	-0.0016	-0.0094
	(0.0307)	(0.0113)
tF 95% CI	(-.1248, .1216)	(-.0365, .0177)
Dep. var. mean	0.007	0.007
Cragg-Donald F	5.703	24.497
Illicit op mort SW F	8.170	26.942
PO-only mort SW F	7.952	28.320

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. In column 1 (2), the endogenous regressors are female (male) Δ Illicit opioid OD death rate and Δ PO-only OD death rate. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions include the full set of covariates, and are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table 1.10: IV estimates for foster care entry, by child age

	(1) Δ IHS Entry, Age <1	(2) Δ IHS Entry, Age 1-5	(3) Δ IHS Entry, Age 6-12	(4) Δ IHS Entry, Age 13-17
LD 2010/11-2015/16				
Δ Illicit op mort	0.0324***	0.0323***	0.0316***	0.0248***
	(0.0105)	(0.0100)	(0.0095)	(0.0083)
tF 95% CI	(.0019, .0629)	(.0031, .0615)	(.0065, .0567)	(.0022, .0474)
Δ PO-only mort	0.0335	0.0337	0.0321	0.0140
	(0.0275)	(0.0265)	(0.0276)	(0.0249)
tF 95% CI	(-.0365, .1035)	(-.0328, .1002)	(-.0349, .0992)	(-.0473, .0753)
Dep. var. mean	0.139	0.035	0.127	-0.055
Cragg-Donald F	19.967	20.964	23.970	22.328
Illicit op mort SW F	14.187	14.730	19.317	17.703
PO-only mort SW F	22.305	23.025	25.859	24.589
LD 2010/11-2016/17				
Δ Illicit op mort	0.0181***	0.0165**	0.0173***	0.0170***
	(0.0068)	(0.0065)	(0.0059)	(0.0060)
tF 95% CI	(-.0037, .0399)	(-.0032, .0362)	(.0013, .0332)	(.0003, .0337)
Δ PO-only mort	0.0158	0.0145	0.0045	0.0004
	(0.0229)	(0.0208)	(0.0197)	(0.0223)
tF 95% CI	(-.0438, .0753)	(-.0378, .0668)	(-.0421, .0511)	(-.0544, .0552)
Dep. var. mean	0.161	0.024	0.124	-0.085
Cragg-Donald F	15.622	17.693	22.906	19.413
Illicit op mort SW F	11.590	12.803	17.426	15.697
PO-only mort SW F	20.195	23.012	29.640	25.246
LD 2010/11-2017/18				
Δ Illicit op mort	0.0085	0.0062	0.0097**	0.0114**
	(0.0052)	(0.0051)	(0.0046)	(0.0054)
tF 95% CI	(-.0063, .0233)	(-.0079, .0203)	(-.0018, .0212)	(-.0031, .0259)
Δ PO-only mort	-0.0104	-0.0100	-0.0141	-0.0107
	(0.0206)	(0.0189)	(0.0180)	(0.0214)
tF 95% CI	(-.0671, .0462)	(-.0606, .0405)	(-.0588, .0306)	(-.0668, .0455)
Dep. var. mean	0.158	-0.009	0.102	-0.138
Cragg-Donald F	14.383	16.234	20.354	16.826
Illicit op mort SW F	15.141	16.818	23.626	18.842
PO-only mort SW F	16.589	18.453	23.874	19.517

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 child population corresponding to the age range of the outcome variable in each column; all columns include the full set of covariates. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table 1.11: IV estimates for foster care entry, interaction with state policies

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry
Policy	**State-admin. CWS**	**UMR**	**Maltrt defn incl parental drug use**
Policy × Δ Illicit op mort	0.0042	-0.0026	0.0194
	(0.0112)	(0.0056)	(0.0316)
tF 95% CI	(-.0464, .0549)	(-.015, .0099)	$(-\infty,\infty)$
Policy × Δ PO-only mort	-0.0215	-0.0381	0.0940
	(0.0399)	(0.0274)	(0.1019)
tF 95% CI	(-.1377, .0947)	(-.106, .0298)	(-1.807, 1.9951)
Δ Illicit op mort	0.0077	0.0106**	0.0139*
	(0.0052)	(0.0045)	(0.0082)
tF 95% CI	(-.0051, .0204)	(-.002, .0233)	$(-\infty,\infty)$
Δ PO-only mort	0.0093	0.0079	-0.0579
	(0.0300)	(0.0222)	(0.0588)
tF 95% CI	(-.0696, .0882)	(-.0567, .0725)	(-.2826, .1668)
Dep. var. mean	0.007	0.007	0.034
N	1,193	1,193	1,014
Cragg-Donald F	4.252	6.922	0.779
Policy × Illicit op mort SW F	6.839	41.510	1.175
Policy × PO-only mort SW F	14.269	24.470	3.817
Illicit op mort SW F	24.830	15.615	3.226
PO-only mort SW F	19.303	14.164	8.511

Long difference is taken between averaged levels 2010-11 and 2017-18. Instruments: oxycodone share in 2000, per-capita PO supply in 2000, and each interacted with the corresponding state policy indicator for the policy in each column. All regressions include the full set of covariates and are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Chapter 2: The Effect of Expanding Medicaid Eligibility on Self-Employment Dynamics

2.1 Introduction

The provision of private health insurance in the U.S. historically has been tied to wage employment at a firm. Relative to the cost of health insurance faced by an individual in the private market, employers have a cost advantage in providing health insurance to employees for several reasons. Non-wage forms of compensation, such as pensions and health insurance, are excludable from taxable income, effectively reducing the price of insurance purchased through an employer. Firms, and large firms in particular, are able to reduce adverse selection by increasing the size of the risk pool. There are additionally economies of scale generated by purchasing insurance through a group. The consequent dominance of employer-sponsored health insurance (ESHI) poses a potential barrier to self-employment. For a wage-and-salary worker with ESHI who is contemplating leaving the wage job to become self-employed, the loss of ESHI and the disruption in coverage may be a salient factor in the self-employment decision, depending on their valuation of health insurance. For the self-employed, health insurance is often obtained on the individual market in the absence of another source of coverage in the household. However, for reasons just outlined, plans offered on the individual market generally have substantially higher premiums, lower actuarial values, and less comprehensive coverage than employer-based plans.[1] Alternatively, one could choose to be uninsured and bear the risk of potentially costly adverse health events.

The Affordable Care Act (ACA) introduced into this environment a number of policy changes that increased access to health insurance outside of wage employment. This paper focuses on one policy component of the ACA, the state-level expansion of Medicaid, and studies its effect on self-employment among adults without children. Specifically, I examine the probability of being self-employed (the stock of self-employment) and labor market state transitions that contribute to the stock of self-employment. The ACA Medicaid expansion extended program eligibility to all adults with family income below 138% of the Federal Poverty Level (FPL). This opened up a significant new pathway to affordable health insurance for non-elderly low-income childless adults, because they had been categorically excluded from Medicaid in most states prior to the ACA expansion. In addition to the expansion of Medicaid, the ACA also provides premium subsidies for private health insurance purchased on health exchanges for families between 100% and 400% FPL. This reduced the price of private health insurance for low to middle-income families. Guaranteed issue regulations, which require insurers to allow enrollment regardless of health status or other factors, eased the concern that a change in employment will affect the availability and quality of health insurance. Another provision expanded coverage among young adults by allowing them to remain on their parents' health insurance plan until the age of 26.

[1]McDevitt et al. (2010) compare pre-ACA individual market plans with employer-based plans and find that: (1) the average annual adjusted premium on the individual market for a 25-year-old in 2007 was $2,262 ($5,972 for a 55-year-old), compared to $693 for single adults with employer plans; (2) individual market plans have higher deductibles (more than $2,100 versus less than $600 in 2007), out-of-pocket maximums ($5,271 versus $2,167), and out-of-pocket expenses (for the top decile of spenders, $5,502 versus $3,364) than group plans on average.

To estimate self-employment effects of the Medicaid expansion, I use a difference-in-differences (DiD) model that relies on variation in state Medicaid expansion status and pre-ACA county-level variation in proportions of the relevant childless adult population who can potentially be induced to become self-employed due to the expansion of Medicaid eligibility. These pre-ACA at-risk shares generate granular geographic variation in the potential impact of the Medicaid expansion on self-employment outcomes. They also take into account the availability of ACA Marketplace premium tax credits in both expansion and non-expansion states. For instance, the DiD model for the self-employment rate allows the effect of the expansion to vary by the pre-ACA share of employed childless adults who have employer-sponsored health insurance and whose family income is (1) below 138% FPL and (2) between 138% FPL and 400% FPL. To address the possibility of confounding trends occurring simultaneously with the Medicaid expansion, I also estimate a triple differences model comparing self-employment outcomes of childless adults to that of parents within the same state and year. This strategy leverages the fact that while low-income childless adults experienced a significant increase in Medicaid income limits due to the ACA expansion (the national median Medicaid income limit for childless adults increased from 0% to 138% FPL between 2013 and 2014 in expansion states), low-income parents saw a much smaller increase (the national median Medicaid income limit for parents increased from 101% to 138% FPL in expansion states) since they were already eligible for Medicaid prior to the expansion. We would expect any self-employment effects of the expansion to be relatively concentrated in childless adults.

The DiD and triple difference models are estimated using the Annual Social and Economic Supplement to the Current Population Survey (CPS-ASEC), 2003-2016 (reference years 2002-2015), linked to the respondents' longitudinal tax-based earnings histories sourced from the Census Bureau's Detailed Earnings Records (DER). I use self-employment and wage-and-salary earnings from the DER to construct (1) the self-employment outcomes and labor market state transitions that are the dependent variables of interest in both sets of models and (2) measures of prior labor market experience that are used as controls. I also use individual demographic information from the CPS-ASEC as individual-level controls in all regressions. This unique dataset linking household survey data to longitudinal earnings based on tax returns allows me to estimate effects of the Medicaid expansion on transitions to self-employment and persistence in self-employment controlling for a wide range of individual characteristics. The main DiD estimation sample consists of employed individuals aged 26-64 who have no children of their own in the household. Since the triple difference models compare childless adults to parents, the estimation sample is expanded to all employed individuals aged 26-64.

I do not find evidence that the ACA Medicaid expansion affected either the extensive or the intensive margin of self-employment one to two years after the expansion. A marginally significant positive effect on the rate of dominant job self-employment becomes null once controlling for self-employment experience in the past 10 years. I find no effect on self-employment earnings and the share of total earnings from self-employment. These null results could be explained by the fact that only a very small share of childless adults can potentially be induced by expanded Medicaid eligibility to become self-employed: the pre-ACA share of employed childless adults who have employer-sponsored health insurance and family income below 138% FPL is only 2% on average across counties. I show that the minimum effects of the expansion within at-risk groups that would generate statistically significant effects in the full sample are implausibly large. Although a simple solution is to limit the sample to low-income childless adults, as Lee (2019) does, using income to select samples raises concerns about endogenous income reporting and labor supply responses to the Medicaid expansion.[2]

The overall lack of an effect on the probability of self-employment and self-employment earnings should be interpreted with caution, since the null results in the full sample of childless adults do not allow me to rule out significant effects in the at-risk group that are not detectable in the full sample. In addition, the sample period ends in 2015, while the Medicaid expansion in the states included in the sample took place between January 2014 and February 2015. This relatively short post-expansion period may not be enough time to observe changes in self-employment decisions. Given these limitations, the next draft of this paper will incorporate additional years of data and restrict the sample to less-educated childless adults, where a greater share of the sample is likely to become eligible for Medicaid as a result of the ACA expansion.

Examining labor market state transitions, I find no effect on the transition from wage employment to self-employment. While there is also no effect on persistence in self-

[2] I discuss in more detail the reasons for my sample choices relative to the samples in Lee (2019) in Section 2.3.

employment at mean values of pre-ACA at-risk population shares, I find a 6.7% post-expansion increase in the rate of persistence in self-employment for a 1 standard deviation increase in the pre-ACA share of self-employed childless adults who have directly purchased health insurance and have family income below 138% FPL. Given that this group spent, at the median, 34% of family income on health insurance premiums in 2010, taking up Medicaid could substantially reduce health insurance premium expenditures and increase persistence in self-employment by (1) increasing the amount of resources available for business operations and capital investment and (2) reducing the pressure to switch to wage employment in order to reduce health insurance premiums. Note that those with income below 138% FPL and directly purchased health insurance on the individual market prior to the ACA likely had relatively high valuations of health insurance, so the effect on self-employment persistence may not apply to the broader population of self-employed childless adults whose income would qualify them for Medicaid under the ACA expansion.

This paper is closely related to Lee (2019), who estimates the effect of the ACA Medicaid expansion on self-employment in two samples of lower-income childless adults using CPS-ASEC data. Similar to Lee (2019), as well as other prior work on labor market effects of the ACA Medicaid expansion, my DiD approach exploits cross-state differences in Medicaid expansion status. In addition to this basic state-level policy variation, however, I introduce to my DiD specifications county-level measures intended to capture the likelihood that an individual's self-employment decision is affected by the Medicaid expansion through specific channels. For example, I interact the Medicaid expansion treatment indicator with M^*, the county-level pre-ACA share of employed childless adults aged 26-64 who have family income below 138% FPL and are covered by ESHI. Having family income below 138% FPL would qualify them for Medicaid after eligibility expansion; being covered by ESHI increases the cost of transitioning from wage employment to self-employment, if ESHI is provided by the individual's own employer. M^* thus measures the share of the relevant population who could be subject to self-employment lock and whose choice of employment state may be altered by having access to Medicaid. Because an individual's family income and health insurance coverage are likely to be correlated with the regression error term, M^* is constructed as a county-level share. While Lee (2019) explores mechanisms of the self-employment effects by interacting the DiD treatment indicator with health insurance demand factors, my approach does not rely on individual characteristics affecting health insurance demand, which may be correlated with unobserved heterogeneity across individuals in expansion and non-expansion states that also affects the propensity to become self-employed. These county-level measures also allow me to take into account premium subsidies for plans offered through ACA health exchanges, which expanded health insurance coverage for the higher-income childless adults included in my sample. This channel is captured by interacting the Medicaid expansion indicator with E^*, the pre-ACA share of employed childless adults aged 26-64 who have family income between 138% and 400% FPL and are covered by ESHI.

In the CPS-DER data, information on self-employment is based on net self-employment earnings reported annually on the 1040 Schedule SE, the tax form used to calculate the amount of the self-employment tax. I also make use of wage and salary earnings data from all Form W-2's filed by an individual's employers in a given year. The administrative data offer several advantages over the CPS-ASEC data used in Lee (2019). First, I am able to study the intensive margin of self-employment by estimating effects on net self-employment earnings and the share of total earnings from self-employment, conditional on being self-employed in t and $t + 1$. This allows me to consider how expanded eligibility for Medicaid affects the relative importance of self-employment among multiple sources of earnings, which is an informative outcome if, for instance, the transition from dominant wage employment to dominant self-employment occurs over a time frame longer than the one-year period I use to define labor market state transitions. Second, I use 10 years of earnings history for the CPS-ASEC respondents in my sample to construct measures of prior labor market experience, which are included in the DiD and triple difference regressions to control for potential unobserved heterogeneity across individuals in expansion versus non-expansion states that are correlated with the propensity to enter self-employment. Finally, whereas the definition of self-employment in Lee (2019) includes both unincorporated and incorporated self-employment, the self-employment activity that I observe in the tax data is unincorporated sole proprietorship, a business owned and run by one individual, with no distinction between the business and the owner. This is the simplest and least expensive business structure to establish. Given that the lower-income population affected by the ACA Medicaid expansion is not likely to have access to large amounts of capital or to establish businesses with a complex structure, unincorporated sole proprietorship may be a more relevant definition of self-employment in the context of the Medicaid expansion.

Relative to the broader literature investigating the relationship between the availability and price of health insurance and self-employment, this paper provides suggestive evidence that an expansion of public health insurance may increase persistence in self-employment for lower-income childless adults with high valuations of health insurance. Whereas previous studies focus on the transition from wage employment to self-employment (Holtz-Eakin et al. (1996), Fairlie et al. (2011), Lee (2019)), I additionally examine persistence in self-employment, which is an interesting outcome for two reasons. First, duration is a key dimension in the typology of self-employment. Research on self-employment distinguishes between self-employment out of necessity (e.g., when opportunities in the wage sector are limited), which tends to be transitory, and self-employment motivated by opportunity (e.g., voluntarily leaving a wage job to start a business), which tends to persist longer (Evans and Leighton (1989), Block and Sandner (2009), Humphries (2021)). Second, persistence in self-employment contributes to the self-employment rate, an important statistic for understanding labor market activity.

2.2 Conceptual Framework

Self-employed workers are a highly heterogeneous group in terms of work arrangements. For example, an independent contractor who performs occasional tasks for fixed payments and the owner of an incorporated business with an established client base are both categorized as self-employed. In thinking about self-employment in lower-income populations that become eligible for Medicaid as a result of the ACA expansion, I draw on the conceptual distinction made in the literature between entrepreneurs and other business owners. While entrepreneurs play a key role in theories of economic growth and business cycles, the majority of the self-employed fall into the "other business owners" category: they simply provide goods and services directly to consumers without the intermediation of firms, largely in the form of small-scale sole proprietorships that have no or few employees. The goal for this group of self-employed individuals is not necessarily business growth. Rather, if opportunities in the wage sector are limited or do not match well to the worker's skills, self-employment may offer a better alternative. In this case, the monetary return from self-employment combined with the value of non-pecuniary benefits are at least equal to the wage compensation they would receive as an employee. In contrast, the defining feature of entrepreneurs is that they receive economic profit—a return above the wages and interest that they could receive in alternative work activities (Balkin 1989).

Empirical evidence supports this characterization of the non-entrepreneurial self-employed. Using a longitudinal sample of white men interviewed in the 1970s and 1980s, Evans and Leighton (1989) find that workers with lower wages, a history of frequent job changes, or more unemployment experience are more likely to switch to self-employment, controlling for assets and education. For a sample of nascent small business owners first identified in 2005, Hurst and Pugsley (2011) document that at the time of business formation, most business owners have no plans to grow or innovate in the future. For more than half of these owners, non-pecuniary benefits such as being one's own boss and flexibility in work hours was a major factor in the decision to start a business. Thus, there are both push and pull factors that contribute to the decision to enter non-entrepreneurial self-employment. Lower-income workers could be "pushed" into self-employment by lack of opportunity in the wage sector: they may lack the skills and educational credentials required for better-paid wage jobs, or face discrimination in hiring and on the job. They could also be "pulled" in by the non-pecuniary benefits of self-employment, such as the flexibility of work schedules.

Given low wage levels and other labor market disadvantages of the population targeted by the Medicaid expansion, it is this type of self-employment–characterized by the small scale of the enterprise and the provision of a "relatively standardized good or service to an existing customer base" (Hurst and Pugsley 2011)–in which we would expect the newly eligible to engage as a result of increased access to health insurance outside of wage employment. This potential mechanism is most relevant for previously ineligible individuals who are in low-wage jobs that provide employer-based health insurance, as they may be reluctant to leave or reduce hours at the wage job due to the eventual loss of ESHI.[3] I test the "self-employment lock" channel by examining the Medicaid expansion-induced change in the probability of transitioning from wage-and-salary employment to self-employment. All else equal, the effect of becoming eligible for Medicaid on self-employment is expected to be larger for wage workers with higher valuations of health insurance, e.g. those who are older, have health conditions, or have family members in bad health. Note that lower-wage workers who become eligible for Medicaid as a result of the ACA expansion are substantially less likely to have health insurance through their own employer compared to higher-wage workers. In 2010, only a quarter of workers in the bottom quintile of the hourly wage distribution had health coverage

[3] Anecdotally, some low-income microentrepreneurs have cited Medicaid as an important form of "bridge" support while their business became more established (Sherraden et al. 2004).

through their own employer, compared with just over half of workers in the next quintile up, and just under 80% of workers in the top quintile (Schmitt 2014). Although lower-wage workers with own ESHI is admittedly not a majority of all lower-wage workers, potential "self-employment lock" within this group is important for understanding how the health insurance system can interact with labor market decisions: the provision of health insurance through firms may be keeping some of these workers in their low-wage jobs, which is sub-optimal if the workers would receive a higher return from self-employment than their wage compensation.

Becoming eligible for Medicaid could also increase persistence in self-employment for workers who were already self-employed by (i) expanding the budget set of those with individual market health insurance who now can substantially reduce their insurance premium, or (ii) reducing uninsured risk for those who do not have any health coverage. Sherraden et al. (2004) find that some low-income microentrepreneurs took wage jobs in order to obtain health benefits after losing Medicaid coverage, further suggesting that the Medicaid expansion could increase persistence in self-employment by reducing the need to additionally take on wage work, thus allowing more time to be allocated to self-employment. Empirically, an increase in persistence corresponds to an increase in the probability of staying self-employed from one year to the next (SE-to-SE), and to reductions in the probabilities of exiting self-employment (SE-to-WS and SE-to-N).

After Medicaid was expanded under the ACA, the eligibility income limit for childless adults has remained fairly low at just 138% of the poverty level. Because income for Medicaid eligibility is determined on a current basis[4], a self-employed Medicaid recipient may lose Medicaid coverage if her business reports sufficiently high net income even temporarily. Medicaid income limits could therefore disincentivize business growth in the long run, but this potential disincentive effect may be offset by the increased availability of subsidized private health insurance for individuals who are above Medicaid income limits but below 400% FPL in the post-ACA policy environment.

After the ACA, expectations about the availability of affordable health insurance outside of wage employment depends on the expected path of self-employment earnings, which interacts with both Medicaid eligibility and ACA Marketplace premium subsidies. Assuming no other sources of income, a single self-employed individual with no dependent children qualifies for Medicaid under the expansion between the initial period of business formation and when her business generates net income of at least $17,236 (using 2019 FPL for a one-person household). In the case that the business survives and self-employment income increases above this amount but is below $49,960, she becomes ineligible for Medicaid, but can purchase private health insurance in the ACA Marketplace at a discount. The amount of the premium subsidy is determined by the difference between the cost of a "benchmark" plan (the second-lowest cost silver plan available to the individual) and a premium cap rate that increases with income. However, because subsidized premiums for Marketplace plans are substantially more expensive than the nominal cost-sharing paid by Medicaid enrollees, the cost to the individual of maintaining coverage discontinuously increases at the

[4] In theory, Medicaid eligibility depends on monthly income. States verify the attestation of income and other eligibility factors made at application and renewals against electronic data sources. In practice, these data sources are not always up to date, and verifying income information is more difficult for the self-employed than for wage workers (Solomon 2019).

Medicaid income limit. While Medicaid expansion status varies by state, ACA premium subsidies are available in all states. In expansion states, the ACA created a "bundle" of health insurance coverage that reduces the price of being self-employed: at low levels of self-employment earnings–specifically, below 138% FPL–coverage is provided through Medicaid at very little cost to the beneficiary; at higher levels of self-employment earnings–above the Medicaid income limit but below 400% FPL–subsidized individual coverage is available, where the size of the subsidy generally decreases with earnings. In non-expansion states, a self-employed individual with earnings between one and four times the poverty level would qualify for subsidized private coverage through ACA health exchanges. If individuals decide to become self-employed based on the expected trajectory of returns to self-employment and the associated health insurance options at each level of returns, then the effect of the Medicaid expansion on entry to self-employment is not separable from that of the ACA premium subsidies. In other words, using self-employment trends in non-expansion states as the counterfactual does not allow me to difference out the effect of the premium subsidies on entry to self-employment. Self-employment effects estimated using DiD are therefore interpreted as the total effect of expanded Medicaid eligibility and the expectation that in the longer run, one can obtain subsidized health insurance through the ACA Marketplace.

So far, I have characterized the type of self-employment that is most relevant for those who become newly eligible for Medicaid and discussed the intuition for how the Medicaid expansion could affect self-employment decisions. Next, I describe in more detail the predicted effect of the ACA Medicaid expansion on transitions into and out of self-employment among the newly eligible by comparing a two-period model of labor market state decisions before and after the ACA, in expansion versus non-expansion states. I make the following simplifying assumptions:

1. Health insurance is a homogeneous good across different sources of coverage (employer-provided, Medicaid, directly purchased on the individual market), and it is a binary characteristic.
2. All wage workers have access to and take up ESHI.
3. There are no alternative sources of health coverage within the household, so those without ESHI can either choose to be uninsured, or must directly purchase a private plan on the individual market or qualify for Medicaid in order to obtain coverage.

Individuals have preferences over health insurance coverage and returns from being in one of three labor market states, net of the cost of health insurance premiums. Omitting the individual subscript, the utility of individual i in year t is

$$U(E_t - P_t, H_t)$$

where E_t is earnings plus non-pecuniary benefits, P_t is the amount of the health insurance premium, and $H_t = 1$ if i has health insurance coverage, and $H_t = 0$ if i is uninsured. U is strictly increasing in the first argument, and $U(.,1) > U(.,0)$. The possible labor market states and their associated values are: wage employment (WS), $E = E^{WS}$; self-employment (SE), $E = E^{SE}$; or non-employment (NE; includes unemployment and out of the labor force), $E = E^{NE}$. For WS and SE, E is earnings plus the non-pecuniary value of wage employment or self-employment. For NE, E is cash and in-kind benefits received while not working plus the non-pecuniary value of non-employment. Individuals are in one of these three states in year t, and choose the labor market state in year $t+1$ that provides her with the highest utility.

Scenario 1: wage-and-salary in the initial period. First consider a wage employed lower-income adult with no children in year t. Prior to the ACA, she would most likely not be eligible for Medicaid, and there was no health exchange to provide subsidized private health insurance. Her utility in t is $U(E^{WS} - P^{G}, 1)$, where P^{G} is the employee share of the group health insurance premium. Continuing in WS provides the same utility in $t+1$. Switching to SE gives

$$\begin{cases} U(E^{SE} - P^{I}, 1) & \text{if insured} \\ U(E^{SE}, 0) & \text{if uninsured.} \end{cases} \tag{2.1}$$

where P^{I} is the premium for a private plan purchased on the individual market. Suppose that $E^{SE} \geq E^{WS}$, i.e., the expected monetary return from self-employment plus non-pecuniary benefits such as flexibility and autonomy are at least as high as wage earnings (plus any non-pecuniary value of the wage job). If health insurance could be purchased on a per-worker basis by both firms and the self-employed at the same price and wages were perfectly flexible, then the worker would choose the sector with the highest return. However, as previously discussed, premiums for non-group plans are substantially higher than the employee share of the cost of ESHI: $P^{I} > P^{G}$. Thus, it could be the case that $P^{I} - P^{G} > E^{SE} - E^{WS}$ if health coverage is maintained: the cost of health insurance deters the individual from switching to self-employment when it would otherwise be optimal to do so. She can alternatively choose to be uninsured, but the utility cost of going without health insurance may be high enough such that $U(E^{WS} - P^G, 1) > U(E^{SE}, 0)$.

If instead she were to enter non-employment, her utility in $t + 1$ would be

$$\begin{cases} U(E^{NE} - P^I, 1) & \text{if insured} \\ U(E^{NE}, 0) & \text{if uninsured.} \end{cases} \tag{2.2}$$

I assume that non-employment provides the lowest return among the three labor market states, $E^{NE} < E^{WS} \leq E^{SE}$. $E^{NE} < E^{WS}$ and $P^I > P^G \implies E^{NE} - P^I < E^{WS} - P^G$, so she would never choose to become non-employed and purchase individual market health insurance. If her valuation of health insurance is sufficiently low and the utility value of non-employment increases relative to her wage, it may be optimal to become non-employed and be uninsured.

Now suppose the same wage employed lower-income childless adult is choosing her employment sector in $t + 1$ in the post-ACA period in an expansion state. Her utility from wage employment in t is the same as before, $U(E^{WS} - P^G, 1)$. If her income is below 138% FPL and she becomes eligible for Medicaid as a result of the ACA expansion, switching to SE in $t+1$ gives utility $U(E^{SE} - P^M, 1) = U(E^{SE}, 1)$. She would not choose to be uninsured in this case, since P^M, the premium for Medicaid, is (nearly) zero. The ACA individual mandate additionally imposed a tax penalty on those without coverage, further increasing the cost to the individual of being uninsured. Our assumption that $E^{SE} \geq E^{WS}$ implies $E^{SE} > E^{WS} - P^G \implies U(E^{SE}, 1) > U(E^{WS} - P^G, 1)$, so that it is now optimal to switch to self-employment in $t + 1$. If her income is between 138% FPL and 400% FPL, she would be ineligible for Medicaid but can receive a premium subsidy α for a private plan purchased on an ACA health exchange. α is a function of her income level (because the premium cap rate is progressive), the ACA rating area where the individual resides, and age. In this case, switching to SE in $t+1$ gives

$$\begin{cases} U(E^{SE} - (P^{I} - \alpha), 1) & \text{if insured} \\ U(E^{SE} - z, 0) & \text{if uninsured.} \end{cases} \tag{2.3}$$

where z is the aforementioned tax penalty on the uninsured. The ACA premium subsidies reduce the cost of private health insurance if it is purchased on the health exchanges. It is optimal to switch from wage employment to self-employment if $E^{SE} - (P^{I} - \alpha) > E^{WS} - P^{G}$ or $E^{SE} - E^{WS} > (P^{I} - \alpha) - P^{G}$, i.e., returns from self-employment exceed wage earnings by an amount larger than the difference between the subsidized premium and the employee share of group health insurance. Relative to the pre-ACA period, premium subsidies should increase the likelihood of switching to self-employment among marginal wage employed individuals for whom $E^{SE} \geq E^{WS}$. In the uninsured case, if the utility cost of being uninsured was already high enough to deter entry to self-employment prior to the ACA, then the tax penalty will exacerbate this effect.

In states that did not expand Medicaid, the wage employed lower-income childless adult would not qualify for Medicaid, but she would still have access to subsidized private insurance on the ACA exchanges if her income is between 100% FPL and 400% FPL. Notably, childless adults with income below the poverty level in non-expansion states fall into the ACA "coverage gap", as their income is too low to qualify for premium subsidies, but they remain ineligible for Medicaid. For the wage employed within this group, the ACA did not generate any incentives to switch to self-employment.

In expansion states after the ACA, non-employed childless adults would qualify for Medicaid based on income. Switching to NE in $t+1$ provides $U(E^{NE}, 1)$. Relative to the pre-ACA period, the expansion of Medicaid eligibility induces more entry from wage work to non-employment: if the utility value of non-employment increases relative to wages, the worker can exit employment and maintain health insurance coverage. In non-expansion states, this group likely falls into the ACA "coverage gap", and is ineligible for subsidized private health insurance and Medicaid. As a result, the utility of entering non-employment in $t+1$ in non-expansion states after the ACA remains the same as before the ACA.[5]

Scenario 2: self-employed in the initial period. Next, consider a lower-income childless adult who is self-employed in t and choosing her employment sector for $t+1$ in the pre- versus post-ACA period, in expansion and non-expansion states. Prior to the ACA, her utility in year t is given by (2.1). Wage employment would give utility $U(E^{WS}-P^{G}, 1)$, and I maintain the assumption that $E^{SE} \geq E^{WS} > E^{NE}$. In the insured case, it is optimal to switch to WS if $E^{WS} - P^{G} > E^{SE} - P^{I}$, or $P^{I} - P^{G} > E^{SE} - E^{WS}$. This can happen if the premium for individual market health insurance, P^{I}, increases, or the return to SE relative to that of WS decreases. If the self-employed worker were uninsured in t, it is optimal to switch to WS in $t+1$ if the increase in utility from gaining ESHI is larger than $E^{SE} - (E^{WS} - P^{G}) > 0$.

[5]Non-employed individuals living in a non-expansion state would likely be exempt from the ACA tax penalty for being uninsured. In particular, they can claim an exemption if they can demonstrate that they would have qualified for Medicaid had their state expanded the program, or if their income does not meet the federal tax filing requirement. See https://www.healthcare.gov/health-coverage-exemptions/forms-how-to-apply/.

This may occur if the worker's valuation of health insurance increases. As for entering non-employment in $t+1$, her utility is given by (2.2). Since non-employment provides the lowest return of the three labor market states, she would not choose to switch from SE to NE.

After the ACA and in states that have expanded Medicaid, a self-employed lower-income childless adult has utility

$$\begin{cases} U(E^{SE}, 1) & \text{if insured by Medicaid} \\ U(E^{SE} - (P^I - \alpha), 1) & \text{if insured by private Exchange plan} \\ U(E^{SE} - z, 0) & \text{if uninsured.} \end{cases}$$

If the self-employed worker is insured through either Medicaid or private health insurance purchased on an ACA exchange, the value of switching to wage employment in the next period is reduced relative to the pre-ACA environment. $E^{SE} \geq E^{WS} \implies U(E^{SE}, 1) > U(E^{WS} - P^G, 1)$, so there is no incentive to be wage employed in $t+1$ if the worker becomes eligible for and enrolls in Medicaid as a result of the expansion. If the worker directly purchases individual market health insurance on an ACA exchange, a premium subsidy α reduces the cost of coverage. It would be optimal to switch to WS if $E^{WS} - P^G > E^{SE} - (P^I - \alpha)$, or $P^I - P^G - \alpha > E^{SE} - E^{WS}$. For self-employed workers who prefer to be insured, the ACA Medicaid expansion and premium subsidies increase the likelihood of persisting in self-employment by reducing the cost of remaining in this labor market state. On the other hand, for self-employed workers who prefer to be uninsured, the tax penalty z imposed by the individual mandate increases the cost of remaining self-employed, and thus increases the probability of switching to wage employment. In states that did not expand

Medicaid, self-employed childless adults with income between one and four times FPL may still have experienced some reduction in the cost of being self-employed through subsidized private health insurance on ACA exchanges.

Exiting to non-employment in $t+1$ in expansion states provides $U(E^{NE}, 1)$ via Medicaid coverage. If the self-employed worker is covered by Medicaid in the initial period, she would not switch to NE. If she is covered by subsidized private health insurance, she would enter non-employment if $E^{SE} - E^{NE} < P^{I} - \alpha$, i.e., if the cost of a subsidized Exchange plan exceeds the additional return from self-employment. If she is self-employed and uninsured in t, exiting to non-employment is optimal if the utility value of gaining health coverage exceeds the drop in income (net of the tax penalty for being uninsured). In non-expansion states, self-employed childless adults are not eligible for Medicaid but may qualify for premium subsidies. If they are insured through a subsidized private plan, it is never optimal to exit to non-employment and maintain coverage, since they would lose the premium subsidy and also experience a drop in income.

Scenario 3: non-employed in the initial period. Prior to the ACA, a childless adult non-employed in t has utility given by (2.2). For simplicity, assume that the premium for group health insurance is such that the individual would always prefer to be insured if group health insurance is available. Switching to WS gives $U(E^{WS} - P^{G}, 1)$; switching to SE provides utility given by (2.1). If she prefers to be insured when self-employed, the employment sector choice depends on $E^{SE} - E^{WS}$, the additional return from self-employment, compared to $P^{I} - P^{G}$, the additional cost of individual market health insurance. If she prefers to be uninsured when self-employed, then self-employment is optimal if $E^{SE} - (E^{WS} - P^{G})$ exceeds her valuation of health insurance.

In expansion states post-ACA, utility in non-employment is $U(E^{NE}, 1)$; expanded Medicaid eligibility increases the value of remaining in non-employment in $t+1$. If she were to become employed in $t+1$ and qualifies for Medicaid, it is optimal to choose self-employment. If instead she is not eligible for Medicaid but is eligible for subsidized private health insurance, the choice of WS versus SE depends on the comparison of $E^{SE} - E^{WS}$ with $(P^{I} - \alpha) - P^{G}$. Relative to the pre-ACA period, the Medicaid expansion and premium subsidies reduce the cost of being self-employed. However, the tax penalty for uninsurance increases the cost of being self-employed and uninsured. In non-expansion states, the additional cost of individual market health insurance when self-employed is offset by the ACA premium subsidy, but the reduction in the cost of being self-employed post-ACA is smaller as compared to expansion states.

2.3 Prior Literature

The literature that addresses the relationship between health insurance factors and self-employment can be organized into two groups: studies examining the effects of health care reform on a range of labor market outcomes, including self-employment, and studies that specifically frame the empirical analysis as estimating the magnitude of "entrepreneurship lock".

Health care reforms often include major legislation that changes the availability and price of health insurance, other individual incentives to obtain health insurance, and employer incentives to offer health insurance benefits. A number of recent studies have empirically tested the predictions made by, for example, the static model of labor supply or the job lock hypothesis on the labor market effects of these health insurance-related policy changes. A subset of these studies specifically examine the labor market effects of the ACA Medicaid expansion, while others estimate the overall effect of entire health care reforms, such as the ACA and the 2006 Massachusetts health care reform. There are also a few studies that exploit changes to state public health insurance programs. This paper is most closely related to Lee (2019) and Callison and Sicilian (2018), both of whom estimate the effect of the ACA Medicaid expansion on self-employment.

Callison and Sicilian (2018) estimate the effect of the Medicaid expansion on a range of labor market outcomes in lower-income populations, but self-employment is not the focus of their study. I center my discussion on Lee (2019), who studies the effect of the ACA Medicaid expansion on the probability of self-employment and self-employment entry and exit among lower-income childless adults. Using a variety of identification strategies and CPS-ASEC data for years 2003-2017, the author finds that the expansion of Medicaid eligibility significantly increased the self-employment rate among lower-income childless adults, and that this occurred through new self-employment entrants. In two lower-income samples of childless adults (below 138% FPL and below 300% FPL), a propensity score weighted DiD analysis yields a 10% to 14% increase in the rate of self-employment in expansion states relative to non-expansion states. Another approach that uses the Medicaid expansion to instrument for Medicaid coverage yields a large local average treatment effect for compliers:

IV estimates imply that the expansion led to a doubling of the probability of self-employment among lower-income childless adults who enrolled in Medicaid after becoming newly eligible. These self-employment effects are mainly driven by increases in the one-year rate of self-employment entry, and are larger for those with no alternative sources of health insurance coverage.

The main samples in Lee (2019) are childless adults aged 26-64 below 138% FPL and below 300% FPL, motivated by the fact that lower-income childless adults experienced the largest increase in Medicaid eligibility as a result of the expansion, and that higher-income childless adults were not affected by the expansion. However, using income to select samples raises endogeneity concerns.[6] First, labor supply and income may respond to the Medicaid expansion. For example, self-employed workers close to but above the 138% FPL income threshold for eligibility might reduce their reported income in order to qualify for Medicaid. This could reflect a true reduction of labor supply or income misreporting. Saez (2010) and Mortenson and Whitten (2020) provide empirical evidence of taxpayer bunching at kink points that maximize refundable tax credits, such as the Earned Income Tax Credit (EITC), which is mainly driven by reporting of self-employment income. Although these studies do not directly shed light on whether Medicaid income limits induce bunching,[7] they

[6]Lee (2019) acknowledges these issues and uses an alternate sample of lower-education (high school or less) childless adults as a sensitivity check. Estimates using the lower-education sample are quite a bit smaller than the main DiD results, and statistical significance is reduced for most outcomes.

[7]As a robustness check to their main analysis of bunching at kink points in the tax schedule, Mortenson and Whitten (2020) look for bunching at Medicaid eligibility thresholds during the period 2002-2011. They find no evidence of bunching, although the authors note that federal income tax data do not contain all the types of income and deductions included in the definition of qualifying income for Medicaid. In addition, the period under study is prior to the ACA Medicaid expansion. The substantial increase in Medicaid income thresholds for childless adults generated by the expansion could have induced bunching behavior in the post-ACA period.

demonstrate the possibility of strategic reporting of self-employment income in response to incentives created by the tax code. The response of reported self-employment income and labor supply to the expansion of Medicaid eligibility could be substantial, since Medicaid creates a "notch" in the budget set (as reporting earnings above the limit results in complete loss of benefits) as opposed to kinks (e.g. kink points created by benefits phase-in and phase-out in the EITC schedule) and provides large benefits. Given that I use self-employment data based on self-employment income reported on the Form 1040 Schedule SE, rather than reports of self-employment from a household survey as in Lee (2019), this type of sample selection would be especially problematic for the unbiasedness of my estimates.

A second and related concern is that the Medicaid expansion changes the composition of the population of lower-income childless adults differentially in expansion and non-expansion states. If these compositional changes occur in unobserved characteristics and are correlated with self-employment trends, DiD estimates of self-employment effects of the Medicaid expansion will be biased. These sources of potential bias motivate my choice to include in the main analysis sample all childless adults aged 26-64 who are employed in $t + 1$. The tradeoff to reducing endogenous sample selection is that 44% of my sample are higher-income childless adults who are unaffected by either the ACA Medicaid expansion or the Marketplace premium subsidies.[8] This will tend to bias my estimates toward zero.

Evidence on the self-employment effects of other health care reforms is mixed. Olds (2016) estimates the effect of eligibility for the State Child Health Insurance Program

[8]On average over 2002-15, 47% of the sample of childless adults in expansion states aged 26-64 and employed in $t + 1$ have total family income above 400% FPL. This share is 42% for the sample of childless adults in non-expansion states.

(SCHIP), a Medicaid-like program targeted towards children, on self-employment among parents using a variant of regression discontinuity (RD). The effects are the largest among the studies discussed, likely because they are local effects around the RD threshold: SCHIP increased the self-employment rate among parents by 15% and the number of incorporated businesses by 36%. Two studies examining the 2006 Massachusetts health care reform come to different conclusions using similar methodologies but different data sources. Applying DiD and synthetic controls to panel tax return data, Heim and Lurie (2014) find that health reform implementation led to a decline in the rate of taxpayers earning a majority of income from self-employment, but no effects for other self-employment outcomes. In contrast, DiD estimates using the CPS-ASEC from Niu (2014) suggest a positive and significant, albeit transitory, effect of the Massachusetts reform on the probability of self-employment. Heim and Yang (2017) conduct an early analysis of the effect of the ACA on the level and composition of self-employment. Using two identification strategies–DiD exploiting pre-ACA differences across states in community rating and guaranteed issue regulations, and triple differences combining the traditional approach of Holtz-Eakin et al. (1996) with the policy regime change created by the ACA–the authors do not find any significant effect on the probability of self-employment. Since the CPS monthly data available to the authors were through July 2014, only seven months after the health insurance exchanges and subsidies were implemented, it is possible that not enough time had passed for any self-employment effects to be detected in household survey data.

Changes in the availability of affordable health insurance outside of wage employment could affect labor supply if some wage workers are employed solely to maintain employer-sponsored coverage, or if means-tested public health insurance programs have work disincentive effects. A number of studies have addressed these questions by examining the effect of health insurance reforms on the extensive and intensive margins of labor supply, but there does not seem to be a consensus on either extensive or intensive margin effects. Dague et al. (2017) and Garthwaite et al. (2014) study the labor supply effects of changes to state public health insurance programs in Wisconsin and Tennessee, respectively. Using quasi-experimental methods, both find evidence of a significant degree of employment lock among low-income childless adults. Garthwaite et al. (2014) estimate an especially large positive effect of losing Medicaid on labor supply: approximately 60% of those who lost public health insurance increased labor supply along the extensive margin within two years after disenrollment. In contrast, an evaluation of the Oregon Health Insurance Experiment, an expansion of Oregon's Medicaid program by lottery, found no effects on employment or earnings (Baicker et al. 2014). Duggan et al. (2017) fall in between these two sets of divergent results, finding that labor supply effects of the ACA were zero on average, but that this masks significant heterogeneity in labor force participation changes across areas with different income distributions.

Studies on the labor supply effects of the ACA Medicaid expansion (Peng et al. 2018; Kaestner et al. 2017; Leung and Mas 2018; Gooptu et al. 2016) generally estimate small and insignificant effects. A notable exception is Peng et al. (2018), who find a significant but transitory 1.3% decrease in employment concentrated in low-wage industries one year after expansion. Identification in this paper comes from variation in Medicaid expansion status within contiguous border county-pairs in neighboring states; estimation uses the Quarterly Census of Employment and Wages, a tabulation of employment and wages of establishments that report to state UI systems. The divergence in results might arise from the data used, which is based on establishment rather than household reports, and/or the within-contiguous county pair approach, which intuitively addresses spatial heterogeneity but operates on the untested assumption that cross-border contiguous counties are appropriate control groups (Neumark et al. 2014).

The other strand of the literature aims explicitly to test the "entrepreneurship lock" hypothesis, which suggests that the employer-based health insurance system in the U.S. has reduced the supply of entrepreneurs. Several studies provide evidence on this question by estimating the effect of the availability and price of health insurance on self-employment. Identification in several cases hinges on the comparison of self-employment outcomes between groups of individuals who are expected to have different valuations of health insurance due to differential availability of alternative sources of coverage and variation in health characteristics (e.g. age, self-reported health status). Holtz-Eakin et al. (1996), Wellington (2001), and Fairlie et al. (2011) use variants of this approach to estimate the effect of health insurance coverage on the probability of (entering) self-employment. Fairlie et al. (2011) compare the probability of transitioning to self-employed business ownership between those who have no alternative source of health insurance and have poor family health (likely to place a higher value on current ESHI) and those who have an alternative source of coverage and have good family health, among workers with ESHI. The authors find evidence of entrepreneurship lock for men of 33% relative to an annual base business creation rate of 3%. Holtz-Eakin et al. (1996) use a similar strategy, estimating a logit model of the probability of entering self-employment as a function of own ESHI, spouse's health insurance, their interaction, plus observable characteristics. This reveals no statistically or economically significant effect of health insurance portability on the probability of transition to self-employment. However, as Gumus and Regan (2015) point out, using spousal coverage to indicate an alternative source of coverage raises concerns: own health insurance status is not necessarily orthogonal to the availability of spousal coverage, and occupational choices of a couple could be jointly made in a way that is correlated with the propensity to enter self-employment.[9]

Given these concerns, researchers have leveraged policy changes as a source of plausibly exogenous variation in the availability and price of health insurance. A series of amendments to the 1986 Tax Reform Act allowed the self-employed to deduct an increasingly larger portion of their health insurance premium from taxable income, thus reducing the after-tax price of health insurance for the self-employed. Gumus and Regan (2015) use these tax deductions to estimate the effect of the tax price of health insurance on entry to and exit from self-employment, finding no effect on entry and limited effects in reducing exits from self-employment. Fairlie et al. (2011) exploit eligibility for Medicare at age 65 in an RD design: comparing business ownership among those just above age 65 to those just below

[9] Consider a scenario where the family is covered by the wife's ESHI, but she would like to leave her current job to start a small business. Knowing this would entail loss of coverage, the husband obtains a job with health insurance that covers dependents. The alternative source of health insurance would then be endogenous to becoming self-employed.

age 65, they find an increase in business ownership rates in the month an individual turns 65, and no such increases around other ages 55-75.

Overall, evidence from this group of studies on the existence and magnitude of entrepreneurship lock is mixed: Holtz-Eakin et al. (1996) and Gumus and Regan (2015) find little to no effect of health insurance factors on self-employment, whereas Wellington (2001) and Fairlie et al. (2011) estimate entrepreneurship lock for men on the order of 23% to 33%.[10] All of these studies rely on self-employment reports from household surveys, including both short panels (PSID, SIPP, 1-year panels of the CPS) and cross-sections (CPS-ASEC). Holtz-Eakin et al. (1996) and Fairlie et al. (2011) focus on transitions from wage employment to self-employment, whereas Gumus and Regan (2015) examine both WS-to-SE and SE-to-WS. In contrast, my paper examines the stock of self-employment, the WS-to-SE transition, and persistence in self-employment, and does so using self-employment earnings reported on tax returns. The former allows me to identify which transitions are driving any potential response of the self-employment stock to expanded Medicaid eligibility. The latter is important given that the rate of self-employment (as a share of all employed) measured in household surveys is substantially lower than what is indicated by tax-based data sources,[11] and this divergence has grown over the past two decades (Abraham et al. 2018).

[10]Effect size as a percent of a base self-employment or business creation rate.

[11]Abraham et al. (2018) compare self-employment rates from several tax filings (Nonemployers, 1099-MISC individuals and businesses, Schedule SE Self-Employment Tax) and from household surveys (CPS-ASEC, CPS monthly, ACS) from 1996 to 2015, and find that the rate of self-employment based on tax data is higher by 2 to 8 percentage points than the rate based on household survey data, depending on the specific sources compared and the year.

2.4 Policy Background

The Affordable Care Act legislated a sweeping set of reforms intended to increase health insurance coverage and reduce health care costs. Implementation of the various provisions began in 2010 and continued until 2020. Major aspects of the law include: the creation of state health insurance exchanges (Marketplaces) where individuals and small businesses can purchase health insurance, with premium subsidies available to low- and middle-income households; expansion of Medicaid to all adults below 138% FPL; an employer mandate requiring employers with 50 or more full-time equivalent employees to offer health insurance or pay a penalty; a tax penalty on individuals without a sufficient level of health insurance coverage, also known as the individual mandate;[12] an extension of dependent coverage until age 26; and modified guaranteed issue and community rating regulations in the private non-group health insurance market, which prohibit insurers from denying coverage based on pre-existing conditions and limit the extent to which insurers can vary premiums based on health status, respectively. This paper focuses on the state-level expansion of Medicaid and, to a smaller extent, premium subsidies for Marketplace plans.

Medicaid is a public health insurance program for low-income individuals and the primary source of health insurance coverage for low-income families and individuals in the U.S. Structured as a jointly financed partnership between the federal government and states, Medicaid is optional for states, although all fifty states participate. Under the current

[12] This penalty was the greater of $695 per person or 2.5% of household income when it went into full effect in 2016. Tax legislation enacted in December 2017 eliminated this tax penalty starting in 2019, effectively repealing the individual mandate.

financing scheme, the federal government matches state spending for eligible beneficiaries without pre-set limits. The federal match share is determined by a progressive formula based on state per capita income. The federal core requirements mandate coverage, up to specified income levels, for certain eligibility groups: pregnant women, infants and children, parents, and persons with disabilities and persons aged 65 or older through Supplemental Security Income (SSI) or other state-optional eligibility pathways. All states must meet minimum federal standards in order to participate and receive federal matching dollars, but above this minimum there is significant variation across states in income eligibility thresholds and covered populations. Table 2.1 shows the national median and range of income eligibility limits for different non-disabled, non-elderly adult populations in 2013, a year prior to the expansion. Among adults, Medicaid income limits were most generous for pregnant women and substantially less so for parents. Non-disabled, non-elderly adults without children were generally categorically excluded: before the expansion, only nine states (including DC) covered this group.

As part of its broader goal of expanding health insurance coverage, the ACA expanded Medicaid eligibility to nearly all non-elderly adults with income up to 138% FPL (about $17,774 for an individual in 2021), with states having the option to set higher income thresholds. In June 2012, the Supreme Court ruled in *National Federation of Independent Business v. Sebelius* that the ACA was constitutional except for the federal government's authority to enforce the mandatory expansion of Medicaid coverage. This effectively gave individual states the option to expand Medicaid. Currently, 34 states (including DC) have adopted the expansion; 3 states passed a measure on the November 2018 ballot to expand Medicaid to 138% FPL; the remaining 14 states have not adopted the expansion (KFF 2019). Of the states that have adopted the expansion, six did so in the two years prior to the June 2012 Supreme Court decision.[13] All six early expansion states previously covered some low-income adults through solely state- or county-funded programs (KFF 2012). In the majority of remaining states, coverage under the expansion became effective in January 2014.

The ACA Medicaid expansion opened a significant new pathway to affordable health insurance for non-elderly childless adults with income up to 138% FPL, as they had previously been excluded from the program in most states. Most adults qualifying for Medicaid under the expansion receive "Alternative Benefit Plans" (ABPs), which must include the same ten essential health benefits as ACA Marketplace plans.[14] Table 2.1 compares Medicaid income limits for non-disabled, non-elderly adult eligibility groups in 2013 to 2014, the first year that the program expansion took effect in most adopting states. Prior to the expansion, pregnant women were the only adult population for which income limits were relatively generous. Parents of dependent children were eligible in every state but subject to income limits that were quite low: in 2013, the income eligibility limit was no more than 64% FPL in half of all states. For a family of four, this amounts to about $15,000 or less. Notably, adults without dependent children were categorically excluded in all but nine states prior to the expansion. This group therefore saw the largest change in Medicaid eligibility, as the national median

[13] The early expansion states (CA, CT, DC, MN, NJ, and WA) expanded Medicaid to low-income adults through the ACA option and/or Section 1115 waiver authority.

[14] The ten EHBs are: ambulatory patient services (outpatient care), emergency services, hospitalization, pregnancy/maternity/newborn care, mental health/substance use disorder services, prescription drugs, rehabilitative services, laboratory services, preventive and wellness services, and pediatric services.

income limit rose to 138% FPL as a result of the expansion. Eligibility was also expanded on average for low-income parents. These average increases mask variation across states in income eligibility limits depending on whether the state has expanded Medicaid: in non-expansion states, low-income adults without dependent children remained ineligible for the program. Table 2.2 shows this by comparing changes in the median income limit from 2013 to 2014 in expansion versus non-expansion states. Relative to childless adults, parents in expansion states experienced a much smaller increase in the income eligibility limit. Although parents in non-expansion states are eligible for Medicaid, the median income limit slightly tightened, and remains low at just below half of the poverty line.

Another major component of the ACA was the establishment of health insurance exchanges, or Marketplaces, and income-based tax credits to subsidize premiums for private health insurance plans purchased through the exchanges. Plans available through the exchanges are standardized into four "metal" tiers based on their actuarial value (the share of expected medical spending that is paid by the plan): 60% for bronze, 70% for silver, 80% for gold, and 90% for platinum. To be eligible for a premium tax credit, an individual or family must have household income between one and four times FPL, not have access to affordable coverage through an employer,[15] and be ineligible for any form of public health insurance (Medicaid, Medicare, CHIP, etc.). This last eligibility rule means that the effective income limits are 138% to 400% FPL in states that expanded Medicaid, and 100% to 400% FPL in non-expansion states. The ACA sets a cap on the maximum amount that has to

[15] An employer-sponsored plan is considered affordable if the employee's contribution (for own coverage only, not considering the cost of adding family members) is less than 9.78% of household income.

be spent on the monthly premium of a "benchmark" plan (the second-lowest cost silver plan available to the individual/family through their state's health exchange); the cap is a share of household income and increases with income.[16] The amount of the tax credit is the difference between the premium for the benchmark plan and the premium cap. Thus if a person enrolls in a more expensive Marketplace plan, the subsidy rate relative to the actual full premium will be smaller. The tax credit may be received at the time of purchase to be applied toward the monthly premium, or can be claimed later when filing tax returns. In addition to the subsidy for monthly health insurance premiums, the ACA also provides a cost-sharing subsidy which sets maximum out-of-pocket (OOP) spending limits when health care services are used (deductibles, copayments, and coinsurance). An individual/family is eligible if they are eligible for a premium tax credit and have household income between 100% and 250% FPL. Unlike the premium tax credit, cost-sharing subsidies can only be applied toward silver plans, and are automatically applied when an eligible person enrolls in a silver plan. By lowering OOP costs, cost-sharing subsidies increase the actuarial value of silver plans.

2.5 Data and Measurement

The empirical analysis uses records from the CPS-ASEC linked to tax data containing 10-year earnings histories for the same individuals from the Census Bureau's Detailed Earnings Records. The CPS-ASEC contains individual, family, and household characteristics that

[16] The premium cap rate in 2019 was: 2.08% for 100%-133% FPL; 3.11%-4.15% for 133%-150% FPL; 4.15%-6.54% for 150%-200% FPL; 6.54%-8.36% for 200%-250% FPL; 8.36%-9.86% for 250%-300% FPL; 9.86% for 300%-400% FPL.

are not available in the tax data; the tax data track the same individual over time, allowing me to observe transitions into and out of self-employment and to construct measures of labor market experience. The linked dataset makes it possible to estimate DiD and triple difference regressions of self-employment transitions controlling for a rich set of individual characteristics.

2.5.1 CPS-DER data

The CPS is a nationally representative household survey administered monthly to over 65,000 households in the U.S. The basic monthly questionnaire asks a set of labor force and demographic questions for all individuals aged 16 and older in the household. The CPS-ASEC is an annual supplement administered each spring to CPS households that collects information on employment, income, demographics, and family structure. Data on employment and income refer to the previous calendar year, whereas demographic data refer to the time of the survey. I use CPS-ASEC's 2003-2016, which contain information for reference years 2002-2015. I rely on the CPS-ASEC for information on the following individual characteristics: age, gender, marital status, presence of own children under age 18, foreign-born status, race, education, and the state and county of residence. The sample is restricted to CPS-ASEC respondents for whom a Protected Identification Key (PIK)–an encrypted Social Security Number–is available, since the PIK is the individual identifier used to link CPS-ASEC respondents to their tax-based earnings records.

The DER are an extract from the Social Security Administration's Master Earnings File (MEF) of nearly all workers in the U.S. They contain tax-based wage and self-employment earnings records from 1978 to 2016 for each CPS-ASEC respondent for whom there is a PIK, regardless of when the CPS-ASEC interview occurred. Self-employment earnings are reported by taxpayers on the 1040 Schedule SE, the form used to calculate the tax due on net earnings from self-employment. Unincorporated sole proprietors and partners with $400 or more in net earnings are required to file a Schedule SE. The DER also contain the Employer Identification Number (EIN) and the wage and salary earnings from that EIN for each Form W-2 that an individual receives in a given year. For each reference year 2002-2015, the CPS-ASEC is matched by PIK to longitudinal earnings data from the DER. The PIK is missing for 20 to 30% of CPS-ASEC records, depending on the year. CPS-ASEC records with a PIK are re-weighted to represent the population age 16 and older using the propensity score method in Abraham et al. (2018). I refer to the resulting dataset, the subset of CPS-ASEC respondents with a PIK matched to their DER earnings histories, as the CPS-DER. For each individual, longitudinal earnings data from the DER are relabeled relative to their CPS-ASEC reference year.[17] After relabeling, I keep the past 10 years of historical earnings, current earnings, and one year of future earnings for all individuals in the sample. The 10 years of historical earnings are used to construct aggregate measures of labor market experience used as individual-level controls in the regressions. Current and one-year-ahead earnings are used to define year t to $t+1$ labor market state transitions that are dependent variables of interest.

[17]For example, for an individual with CPS-ASEC reference year 2002, I have information on 24 years of historical SE and WS earnings (1978-2001), contemporaneous earnings (2002), and 14 years of future earnings (2003-2016). For an individual with CPS-ASEC reference year 2015, I have 37 years of historical earnings (1978-2014), contemporaneous earnings (2015), and 1 year of future earnings (2016).

2.5.2 Measurement of labor market outcomes

Three labor market states are defined based on W2 wage earnings and Schedule SE self-employment earnings recorded in the DER. An individual is classified as "dominant job wage and salary" (WS) if she has positive annual DER earnings and more than half of those earnings are derived from wage and salary work. An individual is classified as "dominant job self-employed" (SE) if she has positive annual DER earnings and more than half of those earnings are derived from self-employment. The third labor market state of "not employed" (NE) occurs when the individual has no DER earnings in a given year. Because earnings on tax forms are reported annually, I cannot distinguish between self-employment during only part of the year and full-year self-employment. In order to exclude from the definition of transitions situations where an individual is briefly self-employed within the year but self-employment is not her primary source of earnings, I use a dominant job concept to define the transitions. Transitions are defined from t, the CPS-ASEC reference year, to $t+1$. This is consistent with estimating the specifications below on the probability of being self-employed in $t+1$, since transitions from t to $t+1$ contribute to stocks at $t+1$. Specifying transitions in this way assumes that becoming eligible for Medicaid in t affects self-employment decisions between t and $t+1$. Transitions are conditional on the labor market state in t (e.g. the estimation sample for WS-to-SE is restricted to individuals who are dominant job wage and salary in t).

2.5.3 Sample restrictions

The main estimation sample is the CPS-DER 2002-2015, excluding early expansion states (California, Connecticut, D.C., Minnesota, New Jersey, Washington), restricted to individuals age 26-64 who are employed in $t+1$ and have no children of their own in the household. The six early expansion states, which undertook the ACA Medicaid expansion in 2010-11, had already been covering some low-income adults through solely state- or county-funded programs (KFF 2012). Early opt-in allowed states to cover enrollees in these programs under Medicaid prior to 2014, and receive matching federal funding for a portion of the costs through the Federal Matching Assistance Percentage. This leads to two concerns: childless adults in early expansion states were not exposed to as large of an increase in public health insurance eligibility as did their counterparts in expansion states that previously excluded childless adults from any public coverage. The availability of state- and county-funded programs prior to the Medicaid expansion may also be correlated with unobserved trends in, for example, program generosity for low-income adults in other safety net and income support programs, thus confounding the effect of expanded Medicaid eligibility. To bypass these issues, I exclude early expansion states.

The age restriction limits the confounding effects of two policies that change the availability of health insurance based on age. The ACA young adult provision was implemented in 2010 to allow those aged 19 to 25 to be covered by their parents' private health plans. Previously, individuals could no longer be covered by their parents' health insurance upon turning 19. By extending the availability of health insurance through parents, the provision may affect self-employment in this age group by shifting young adults on the margin into self-employment when they would otherwise have chosen wage employment in order to gain employer-sponsored health insurance. At the other end of the age range, individuals automatically qualify for Medicare once they turn 65, again increasing access to affordable health insurance.[18]

The DiD sample is limited to adults with no children of their own in the household because this population experienced the largest increase in Medicaid eligibility as a result of the expansion. Prior to the ACA expansion, childless adults were categorically excluded from the program in most states. The triple difference approach exploits this feature of the expansion by comparing outcomes for childless adults and parents in the same state-year, so the estimation sample for triple difference regressions is expanded to all (employed) adults aged 26-64. Finally, the sample is restricted to adults who are employed in $t + 1$, since I am primarily interested in how expanded Medicaid eligibility changes the relative value of self-employment to wage employment for those who are working. Note that the stock of self-employment depends on the full set of labor market state transitions, and a complete decomposition would need to examine transitions to and from non-employment. Becoming eligible for Medicaid could affect transitions from employment to non-employment due to "employment lock".[19] It is less clear how the Medicaid expansion affects transitions from non-employment to self-employment: Medicaid availability could increase the probability of

[18]Fairlie et al. (2011) use this discrete change in health insurance coverage to estimate the magnitude of entrepreneurship lock and find a significantly higher rate of business ownership among male workers just over age 65 compared to those just under 65.

[19]For example, Garthwaite et al. (2014) find a large increase in labor supply following disenrollment from Tennessee's state Medicaid program, consistent with the notion that some individuals remain employed only to secure health insurance.

NE-to-SE by raising the relative value of self-employment or through improved health, but the labor supply disincentive effect of Medicaid pushes in the opposite direction.

2.6 Empirical Strategy

The first part of the research question is concerned with the effect of expanded Medicaid eligibility on self-employment rates among employed adults without children, in a post-ACA environment where subsidized individual market health insurance was also available to those with family income up to 400% FPL. Given that the self-employment rate at any point in time depends on transitions from other labor market states as well as persistence in self-employment, I then proceed to examine the effect of the Medicaid expansion on labor market state transition probabilities.[20]

2.6.1 Difference-in-difference

To estimate the effects of expanded Medicaid eligibility on the self-employment rate, I use a difference-in-differences design that exploits cross-state variation in whether Medicaid eligibility was extended to all adults with family income below 138% FPL, and pre-ACA cross-county variation in the share of employed individuals age 26-64 without children who can potentially be induced to become self-employed as a result of increased availability of affordable health insurance through (i) expanded Medicaid eligibility and (ii) premium subsidies for private health insurance purchased on ACA health exchanges. I estimate the

[20]The probability of self-employment at a given point in time more accurately depends on the full set of labor market transitions, but I focus on transitions that are hypothesized to respond to becoming eligible for Medicaid.

following equation:

$$y_{icst+1} = \beta_1 Exp_{st} + \beta_2(Exp_{st} \times M_c^*) + \beta_3(Exp_{st} \times E_c^*) + X'_{icst}\theta + \gamma_t + \mu_c + \phi_c t + \epsilon_{icst} \tag{2.4}$$

Exp_{st} is an indicator of whether Medicaid eligibility was expanded in state s in year t. Exp_{st} turns on in expansion states in post-expansion years, which is equivalent to interacting an expansion state indicator with a post-expansion year indicator. M_c^* corresponds to cross-county variation (i) above and is defined, for a county c, as the pre-ACA share of employed individuals age 26-64 with no children in the household who report having employer-sponsored health insurance coverage and whose total family income is no more than 138% FPL. E_c^* is analogously defined as the share of the same population who report having ESHI coverage and whose total family income is between 138% and 400% FPL.[21] γ_t and μ_c are full sets of year and county effects respectively, and $\phi_c t$ are county-specific time trends. $\phi_c t$ control for the differential evolution of factors (e.g. aggregate economic conditions) correlated with self-employment propensities across counties with different pre-ACA income distributions. Note that the main effects M_c^* and E_c^* drop out since county effects are included. All regressions are weighted using the CPS-ASEC person weight adjusted to account for missing PIKs;

[21] M_c^* and E_c^* are computed using pooled single years of the American Community Survey (ACS) from 2008 to 2010 at the Public Use Microdata Area (PUMA) level. Using the equivalency file between Census 2000-based PUMAs and counties with population-based allocation factors, values of M_c^* and E_c^* are a weighted average of PUMA-level values, where the weights are the allocation factors. Ideally, these measures should identify those who are the policyholders of ESHI. Because of data limitations in the ACS, I cannot identify whether an individual covered by ESHI is the policyholder. Finally, note also that M_c^* and E_c^* are an extension of the empirical approach in Duggan et al. (2017), which uses the pre-ACA share of the population in a PUMA who are uninsured and have family income in the two program eligibility ranges– i.e., the share that can potentially gain health insurance coverage through expanded Medicaid or subsidized private insurance–to generate geographic variation in ACA treatment intensity. Descriptive statistics for M_c^*, E_c^*, and similar measures used in the transition regressions are presented in Table 2.4.

standard errors are clustered at the county level.

X'_{icst} is a vector of individual demographic characteristics and prior labor market experience measures for person i in CPS-ASEC reference year t. Demographics include age group dummies, education dummies, gender, marital status, foreign-born status, and an indicator for white race.

I include three prior labor market experience summary measures as of year t (i.e. using earnings histories in years $t-1$ to $t-10$) that potentially affect the propensity to be self-employed: an indicator for having had any positive self-employment earnings in the past 10 years, (the inverse hyperbolic sine of) cumulative discounted total real earnings over the past 10 years (E_{it}), and total earnings volatility over the past 10 years (σ_{it}). $E_{it} = \sum_{n=1}^{10}(1+r)^n e_{i,t-n}$, where $e_{i,t-n}$ is real earnings in year $t-n$ and r=0.02. Total earnings volatility is calculated as follows. Let $\gamma_{is} = (e_{is} - e_{i,s-1})/z_{is}$ and $z_{is} = \frac{1}{2}(e_{is} + e_{i,s-1})$, where, as above, e_{is} is real earnings in year s. Let P_{is} be the number of years with $z_{is} > 0$. Define

$$\sigma_{is} = \big[(\frac{1}{P_{is}-1}) \sum_{k=0}^{P_{is}-1} (\gamma_{i,s-k} - \bar{\gamma}_{is})^2\big]^{\frac{1}{2}} \tag{2.5}$$

where the bar represents the mean of γ_{is} and squared deviations are only taken for the years in which $z_{is} > 0$. I adjust prior labor market experience for an individual's age when she is observed in the CPS-ASEC by interacting each measure with the set of age group dummies. The inclusion of these work experience controls is motivated in part by findings in the literature on the individual correlates of self-employment transitions. Having past self-employment experience may reduce the earnings uncertainty associated with entering self-employment, and thereby increase the expected utility of self-employment. Prior work has consistently found that past self-employment is significantly associated with a higher probability of becoming self-employed, conditional on various sets of demographic characteristics, other labor market experience measures, and wealth (Evans and Leighton (1989), Lin et al. (2000), Taylor (2001), Georgellis et al. (2005)). Household wealth is another factor that has been identified as an important determinant of entry to self-employment (e.g., Evans and Leighton (1989), Evans and Jovanovic (1989), Holtz-Eakin et al. (1994), Taylor (2001), Fairlie and Krashinsky (2012)). Own wealth directly affects the availability of start-up capital as well as the amount of available collateral, which matters for credit access. I use cumulative earnings over the past 10 years as a proxy for an individual's accumulated wealth. Finally, I include total earnings volatility to capture unanticipated changes in earnings that might have been associated with adverse events such as job loss and health problems. Such otherwise unobserved events may affect an individual's propensity to switch from wage employment to self-employment or to persist in self-employment. Prior labor market experience thus helps to explain variation across individuals in self-employment propensities, which may increase the precision of estimated effects. A second reason for including these work experience measures is that they can help to control for confounding trends, a plausible concern given that other aspects of the ACA may have generated systematic time-varying differences between expansion and non-expansion states that affect self-employment decisions.

This specification is estimated for the following measures of self-employment (stock outcomes) in $t + 1$: Pr(dominant job self-employed), Pr(self-employed only), Pr(both wage employment and self-employment). I also examine the intensive margin of self-employment by estimating this model for the share of total earnings from self-employment and log self-employment earnings, both conditional on having self-employment earnings. By interacting M_c^* with the expansion indicator, I consider how self-employment effects of the ACA Medicaid expansion varies according to the share of the at-risk group in the relevant population, i.e., the share of employed childless adults who may be tied to wage jobs by employment-based health insurance and who would newly qualify for Medicaid under the expansion. In addition to extending Medicaid eligibility in states that chose to expand, the ACA provides premium tax credits to individuals with family income up to 4 times FPL for private health insurance plans purchased through a health exchange. By lowering the monthly payments for individual market plans, the premium tax credits increase access to health insurance outside of wage employment for individuals between 138% and 400% FPL in expansion states, and between 100% and 400% FPL in non-expansion states. I take this into account by allowing the Medicaid expansion treatment effect to vary with E_c^*, the share of employed individuals age 26-64 with no children in the household who are covered by ESHI and eligible for Exchange premium subsidies. For this group, the cost of potentially losing ESHI when becoming self-employed is offset by the availability of subsidized private health insurance.

Because individual factors relevant for being in these two groups–ESHI coverage and family income–are likely correlated with unobserved characteristics that affect self-employment propensities, M_c^* and E_c^* are county-level measures that (i) should be correlated with an individual's probability of being in the at-risk group, and (ii) are plausibly orthogonal to individual-level omitted variables. Framed in this way, equation (2.4) can be thought of as the reduced-form equation of an instrumental variables (IV) approach where M_c^* and E_c^* are used to instrument for an individual's probability of being in the respective at-risk group. E_c^* and M_c^* are de-meaned, so that β_1 is interpreted as the self-employment effect of expanded Medicaid eligibility for employed childless adults in a county with average pre-ACA shares of the two at-risk groups. I scale β_2 and β_3 by the standard deviation of M_c^* and E_c^*, respectively, so that β_2 (β_3) is the additional change in the probability of self-employment for a county with a 1-sd higher pre-ACA share of employed childless adults who are in the Medicaid (premium subsidy) at-risk group.

Since the self-employment rate in $t+1$ is determined by labor market state transitions from t to $t+1$, I also estimate the effects of increased Medicaid eligibility on a subset of transitions, conditional on the state in year t. First consider those who are dominant job wage and salary in t. For an individual with employer health insurance choosing between remaining WS or entering SE in $t+1$, becoming eligible for Medicaid reduces the cost of self-employment and reduces the value of remaining in wage employment, all else equal. We would therefore expect an increase in $Pr(SE_{t+1} = 1|WS_t = 1)$ (and mechanically a decrease in $Pr(WS_{t+1} = 1|WS_t = 1)$, since the sample is limited to the employed in $t+1$). For this transition, I estimate a specification of the same form as equation (2.4), where M_c^* and E_c^* are defined as the pre-ACA share of wage employed individuals age 26-64 without children in a county c who are covered by ESHI and have family income under 138% FPL and between 138% FPL and 400% FPL, respectively. As before, M_c^* and E_c^* are county-level measures used to instrument for a wage-and-salary individual's probability of being at risk of switching to self-employment due to becoming eligible for Medicaid or Marketplace premium subsidies.

Among individuals who are self-employed in t, labor market decisions for the next period may also be impacted by becoming eligible for Medicaid. Unlike individuals in the self-employment stock regression sample or those who are dominant job WS in t, these workers start out in t as dominant job SE, making self-employment in $t+1$ a relevant state under consideration for everyone in the sample. I therefore begin with the simpler DiD model in equation (2.6), which includes the expansion indicator Exp_{st} and the same set of controls as before. The coefficient of interest is α_1, the effect of expanded Medicaid eligibility on the likelihood of persisting in SE (SE-to-SE) among those who are dominant job self-employed at t.

I then investigate two potential mechanisms depending on the individual's source of health insurance prior to the expansion, and how they might affect the probability of persisting in SE. This is described by equation (2.7). First, taking up Medicaid shifts the resource constraint outward for a self-employed person with directly purchased private health insurance, since premium and cost-sharing amounts in Medicaid are very low and capped at 5% of household income (Brooks et al. 2018). Relative to individual market health insurance, this represents a reduction in the cost of maintaining health coverage to the self-employed individual.[22] Shifting out of the budget constraint may increase persistence in self-employment by freeing up resources that can be reallocated to, for example, business operations. I test this resource constraint channel by introducing a county-level measure

[22]Switching from private health insurance to Medicaid likely entails changes in the quality of coverage, given significant disparities in access to care for patients covered by Medicaid versus privately insured patients. In 2011, 31% of physicians were unwilling to accept new Medicaid patients, compared to 18% of physicians who would not accept new privately insured patients (Decker 2012).

of the at-risk group's share of the relevant population, P_c^M, and interacting it with the Medicaid expansion indicator in equation (2.7) below. P_c^M, for a county c, is the pre-ACA share among self-employed childless adults age 26-64 who have directly purchased health insurance coverage and family income below 138% FPL. We would expect to see an increase in the probability of SE-to-SE (corresponding mechanically to a reduction in the probability of SE-to-WS), and larger effects for individuals in areas with higher values of P_c^M.

Second, a self-employed individual without any health insurance coverage faces uninsured risk of potentially costly illnesses and accidents. Relative to remaining uninsured, enrolling in Medicaid requires little additional cost and insures against adverse health events. A reduction in uninsured risk lowers the cost of remaining self-employed, so we would expect an increase in the probability of SE-to-SE among self-employed uninsured individuals who become eligible for Medicaid as a result of the ACA expansion. To test this risk reduction channel, I interact U_c^M, the county-level share of uninsured with family income below 138% FPL among self-employed childless adults age 26-64, with the Medicaid expansion treatment indicator. As we noted for the first set of DiD regressions for self-employment stocks, the availability of premium subsidies for private health plans on ACA exchanges also increased access to health insurance outside of wage employment during the same time period that the Medicaid expansion occurred in the majority of expansion states. I account for potential effects of the premium subsidies on transitions from self-employment by interacting county-level measures of the population share of the at-risk groups, P_c^E and U_c^E, with the expansion indicator. P_c^E and U_c^E are defined in the same way as P_c^M and U_c^M, except that the family income range is 138% FPL to 400% FPL, with 400% FPL being the maximum income level eligible for premium subsidies. As before, these at-risk shares are de-meaned, so that α_1 is the effect of expanding Medicaid on persistence in self-employment for a county with average levels of P_c^M, U_c^M, P_c^E, and U_c^E.

$$y_{icst,t+1} = \alpha_1 Exp_{st} + X'_{icst}\theta + \gamma_t + \mu_c + \phi_c t + \mu_{icst} \tag{2.6}$$

$$\begin{aligned} y_{icst,t+1} = {} & \alpha_1 Exp_{st} + \alpha_2(Exp_{st} \times P_c^M) + \alpha_3(Exp_{st} \times U_c^M) \\ & + \alpha_4(Exp_{st} \times P_c^E) + \alpha_5(Exp_{st} \times U_c^E) \\ & + X'_{icst}\theta + \gamma_t + \mu_c + \phi_c t + \eta_{icst} \end{aligned} \tag{2.7}$$

2.6.2 Triple difference

Suppose that simultaneous with the Medicaid expansion, there are trends in health insurance coverage that potentially affect the self-employment decision and systematically differ between expansion and non-expansion states. This is the case if, for example, spousal employer-sponsored health insurance as an alternate source of coverage within the household became more prevalent in expansion states during the period of expanding Medicaid eligibility. Having an alternative coverage option lowers an individual's valuation of own ESHI and may therefore encourage entry to self-employment. Using DiD would then overestimate the effect of the Medicaid expansion on the probability of self-employment; a positive DiD estimate would partly reflect the increased availability of spousal ESHI in expansion states. Because several other components of the ACA, such as the employer mandate and Exchange premium subsidies, were implemented during the same time period when most of the Medicaid expansions occurred (2014-15), this type of confounding is of particular concern. I attempt to reduce such bias by estimating a triple-difference specification that compares, within the same state-year, self-employment outcomes of childless adults to that of other adults, i.e., adults with children. This strategy exploits the fact that because childless adults were categorically excluded from Medicaid in most states prior to the ACA, the expansion of Medicaid to all adults with family income below 138% FPL differentially increased the share eligible for Medicaid in this population relative to adults with children. Table 2.2 shows that the national median Medicaid income limit for childless adults increased from 0% to 138% FPL between 2013 and 2014 in expansion states, compared to a much smaller increase from 101% to 138% FPL for parents. Correspondingly, we would expect any self-employment effects of the Medicaid expansion to be concentrated among childless adults. The estimating equation is

$$\begin{aligned} y_{ist+1} =& \beta_1 Exp_{st} + Childless_i + \phi_s + \delta_t + (\phi_s \times \delta_t) \\ &+ \beta_3(\phi_s \times Childless_i) + \beta_4(\delta_t \times Childless_i) \\ &+ \beta_5(Exp_{st} \times Childless_i) + X'_{ist}\theta + \nu_{ist} \end{aligned} \tag{2.8}$$

Outcomes of interest are self-employment stock measures in $t+1$ (the probabilities of dominant job SE, SE only, WS and SE, and SE share of total earnings), $Pr(SE_{t+1} = 1|WS_t = 1)$, and $Pr(SE_{t+1} = 1|SE_t = 1)$. $Childless_{ist}$ is an indicator that turns on if individual i has no children of her own under 18 in the household, and is equal to zero if she has at least one child of her own under 18. Exp_{st} and X'_{ist} are as previously defined. Since we are comparing childless adults to parents, the sample is expanded to CPS-ASEC individuals aged 26-64 who are employed in $t+1$. A full set of state (ϕ_s), year (δ_t), and demographic group ($Childless_i$) fixed effects, and all of the two-way interactions among these three terms are included. $Childless_i$ controls for unobserved fixed characteristics of childless adults; $\phi_s \times Childless_i$ controls for state-specific time-invariant characteristics of childless adults; $\delta_t \times Childless_i$ controls for unobserved shocks common to all childless adults in the nation in a given year. With the inclusion of $\phi_s \times \delta_t$, this model compares childless adults to parents in the same state and year. The coefficient of interest is β_5, the post-expansion change in the self-employment rate or the probability of making a given labor market state transition among childless adults relative to parents, in expansion versus non-expansion states. The identifying assumption is that in expansion states, self-employment outcomes among childless adults would have trended similarly as that of parents in the absence of expanded Medicaid eligibility. To the extent that parents in expansion states also experienced more generous Medicaid income limits, the triple difference estimate should tend to understate the effect of expanded Medicaid eligibility on relative self-employment outcomes.

2.7 Results

2.7.1 Trends in self-employment and demographics

Figures 2.1-2.5 show trends in self-employment rates, rates of transition to and persistence in self-employment, and demographic characteristics in expansion and non-expansion states from 2002 to 2015. In Figure 2.1, the rate of dominant job SE differs in the two sets of states prior to the expansion: in the three years preceding the majority of expansions in 2014, the rate of dominant job SE declined slightly in expansion states before increasing during 2013-14; in non-expansion states, this rate steadily increased during 2010-14. The rates of having only self-employment earnings trended similarly in the two sets of states in the three years preceding 2014 but were divergent during the earlier period from 2002 to 2007. Turning to Figure 2.2, we also see different trends in the WS-to-SE transition rate. In expansion states, this transition declined between 2004 and 2007 and has stayed flat at around 1% since 2007. In contrast, the WS-to-SE transition in non-expansion states saw two cycles of an increase followed by a decline between 2006 and 2014. The rate of SE persistence was also different in years preceding the expansion: between 2012 and 2014, SE persistence increased by about 2 pp in expansion states and fell by nearly 5 pp in non-expansion states. Given these pre-expansion differences in trends of self-employment rates and rates of transition to and persistence in self-employment, all DiD models include a county-specific linear trend (since the expansion indicator is interacted with county-level at-risk shares), and I add a third dimension of comparison by estimating triple difference models that compare childless adults to parents within the same state-year.

To check for potential confounding compositional changes associated with the Medicaid expansion, trends in demographics, family income distribution, and prior labor market experience are plotted for expansion and non-expansion states in Figures 2.3-2.5. The evolution of these variables was broadly similarly in the two sets of states before and after the expansion. There are substantial differences in levels for some of these variables: on average, expansion states have a higher share of childless workers who are White and who have family income above 400% FPL; they are less likely to have had any self-employment experience in the previous ten years, and have higher past cumulative earnings and lower earnings volatility over the previous ten years.

2.7.2 Probability of self-employment and self-employment earnings

Table 2.5 presents DiD estimates of the effect of expanded Medicaid eligibility on the probability of being self-employed, corresponding to equation (2.4). The outcomes in columns 1 and 2, 3 and 4, and 5 and 6 are, respectively, indicators for being dominant job self-employed in $t+1$, for having positive SE earnings only in $t+1$, and for having both WS and SE earnings in $t+1$. The second column in each pair adds prior labor market experience measures and their interaction with a set of age group indicators[23]. Estimates of the DiD parameters β_1, β_2, β_3, and the main effects of any past SE experience, cumulative earnings, and earnings volatility (corresponding to the effects of these variables for individuals ages 35-44) are shown.

DiD estimates indicate that the ACA Medicaid expansion did not have a significant effect on the extensive margin of self-employment among childless adults in the first two years of the expansion. Across the three measures of the probability of self-employment, the change in the probability of being self-employed post-ACA was not different between expansion and non-expansion states. The marginally significant positive effect on the probability of dominant SE in column 1 (a 0.8 pp increase, or 10% of the sample dominant SE rate) is rendered null by the inclusion of past earnings history, which may capture otherwise unobserved heterogeneity across workers correlated with the probability of self-employment.

[23]The age groups are 26-34, 35-44, 45-54, and 55-64; age 35-44 is the omitted group.

In particular, for an individual age 35-44, having any self-employment in the previous ten years raises the probability of self-employment in $t+1$ by between 12.8 pp and 29.6 pp (columns 2, 4, and 6). The probability of SE is highly autocorrelated: past self-employment is a significant predictor of future self-employment. The final two coefficients in columns 2 and 4 indicate that the probabilities of dominant job SE and SE only are negatively associated with cumulative earnings over the past ten years and positively associated with earnings volatility, although these effects are an order of magnitude smaller than that of past self-employment experience.

Relative to a null effect for childless adults in counties with average values of the pre-ACA at-risk population shares M^* (i.e. Medicaid at-risk share) and E^* (i.e. Marketplace premium subsidy at-risk share), the change in the probability of dominant job SE is negatively associated with M^*, the pre-ACA share of employed childless adults with ESHI and family income below 138% FPL. Using the cross-county standard deviation of M^* to scale the coefficient, there was a 1.26 pp reduction post-expansion in the probability of dominant job SE (16% of the sample probability of 7.7%) among employed childless adults in counties with a 1-sd above average Medicaid at-risk share. The spatial variation in the Medicaid at-risk share is mostly driven by differences in the family income distribution rather than the share with ESHI, indicating that lower-income counties in expansion states saw reductions in the probability of self-employment after Medicaid eligibility was expanded. This result is opposite of what would be expected if the Medicaid expansion reduced self-employment lock among wage-and-salary childless workers.

Selected coefficients from the triple difference specification (corresponding to equation (2.8)) for the three measures of the probability of self-employment are presented in Table 2.6. I show the coefficient on the interaction of the Medicaid expansion indicator and the childless indicator, which is the estimated post-expansion change in the SE probability for childless workers relative to workers with children, in expansion versus non-expansion states. As in the DiD table, the even-numbered columns also include prior labor market experience measures and their interactions with a set of age group indicators. Consistent with the DiD results, the triple difference estimates indicate no differential change in the probability of self-employment among childless workers after the Medicaid expansion.

Despite no effects of expanding Medicaid eligibility on the extensive margin of self-employment, there could still be a response on the intensive margin of self-employment. For instance, a worker who is dominant job WS and also has SE earnings might increase the share of her total earnings from self-employment without becoming dominant job SE, anticipating that she could be temporarily covered through Medicaid if she were to leave her wage job and transition to self-employment only. To examine this type of possibility, DiD and triple difference regressions are estimated for the share of total earnings from self-employment and (log of) self-employment earnings, both conditional on having positive SE earnings. The DiD and triple difference results are presented in Tables 2.7 and 2.8, respectively. Columns 1 through 4 in each table indicate that the Medicaid expansion did not significantly affect the intensive margin of self-employment among childless self-employed workers. Higher cumulative total earnings over the previous ten years increases $t+1$ self-employment earnings in absolute value, but reduces the share of self-employment in total earnings.

2.7.3 Labor market state transitions: WS-to-SE and SE-to-SE

WS-to-SE. Next, I examine effects on wage employment to self-employment transitions. Table 2.9 shows estimates from the DiD model and Table 2.10 shows estimates from the triple difference model. The DiD sample is restricted to childless adults who are dominant job wage employed in year t and employed in year $t+1$. In Table 2.9, the coefficients on the expansion indicator and its interaction with pre-ACA at-risk population shares M^*_{ws} and E^*_{ws} show that the Medicaid expansion had no significant impact on the probability of making a transition from dominant job wage employment to dominant job self-employment. Also note that only 1.2% of the sample made this transition, a very low baseline. Turning to the triple difference estimates in Table 2.10, the coefficient on the interaction of the expansion and childless adult indicators in both columns shows no post-expansion change in WS-to-SE transitions among childless adults relative to parents, in expansion relative to non-expansion states. Overall, I find no evidence of "self-employment lock" among wage workers in the context of the Medicaid expansion.

SE-to-SE. Tables 2.11 and 2.12 show, respectively, DiD and triple difference estimates for persistence in self-employment (SE-to-SE). The baseline DiD model (columns 1 and 2 of Table 2.11) and the triple difference model (columns 1 and 2 of Table 2.12) find no evidence of significant effects on persisting in self-employment. Turning to the DiD model with interactions (columns 3 and 4 of Table 2.11): relative to a null effect on self-employment persistence for childless adults in counties with average values of the pre-ACA at-risk population shares, the post-expansion change in SE persistence is positively associated with P^M, the pre-ACA share of the self-employed who have directly purchased health insurance and have family income under 138% FPL. Using the cross-county standard deviation of P^M to scale the coefficient, there was a 5.8 pp increase post-expansion in the rate of SE persistence (6.7% of the sample rate of 86%) among self-employed childless adults in counties with a 1-sd above average Medicaid at-risk share. In contrast, the post-expansion change in SE persistence is negatively associated with P^E, the pre-ACA share of the self-employed who have directly purchased health insurance and have family income in the eligible range for ACA premium subsidies, and this effect is only marginally significant. A 1-sd above average Exchange subsidy at-risk share is associated with a 5.2 pp post-expansion reduction in the rate of SE persistence (6% of the sample rate).

In 2010, among self-employed childless adults with directly purchased health insurance, those with family income in the ACA Medicaid expansion eligibility range spent a larger share of that income on health insurance premium payments than those with family income in the ACA Exchange premium subsidy eligibility range. Among those with family income below 138% FPL, the median share of family income spent on health insurance premiums was 34%, compared to a median share of 11% among those with family income between 138% and 400% FPL.[24] Given that premium and cost-sharing amounts for Medicaid enrollees are capped at 5% of household income, taking up Medicaid would substantially reduce health insurance

[24]These shares are calculated using CPS-ASEC 2011 (reference year 2010) data. 2011 is the first survey year with information on the total dollar amount that a family spent on health insurance premiums in the last 12 months.

premium expenditures among self-employed childless adults with directly purchased health insurance and family income below 138% FPL. For those with family income between 138% and 400% FPL, taking up the Exchange plan premium tax credit may not reduce premium expenditures by as much relative to the reduction for the Medicaid-eligible group. To receive the ACA premium subsidy, one must contribute toward the cost of the premium at a rate between approximately 4% to 10% of household income (the rate increases with income), and will pay more if one chooses to enroll in an Exchange plan that is more expensive than the benchmark plan. This difference in the potential reductions in premium expenditures as a share of family income might partly explain why the increase in SE persistence is only observed for counties with an above-average Medicaid at-risk share but not for counties with an above-average premium subsidy at-risk share.

2.7.4 Discussion

Among childless adults, I find no evidence of significant effects of the ACA Medicaid expansion on the extensive and intensive margins of self-employment. Triple difference models comparing childless adults to parents within the same state-year confirm this result. I also find no effect on the transition from wage employment to self-employment, and no effect on persistence in self-employment at mean values of the pre-ACA Medicaid and Exchange premium subsidy at-risk population shares. These null results across different self-employment outcomes are consistent with the small share of all employed childless adults whose self-employment decisions are plausibly affected by the Medicaid expansion.[25] To see

[25] Recall that unlike Lee (2019), I do not select my samples based on income due to the concern that income and labor supply may endogenously respond to the Medicaid expansion.

why, I calculate the minimum effect within this group ($\hat{\beta}_s$) that would generate a statistically significant DiD estimate for the rate of dominant job SE in the full sample ($\hat{\beta}_f$). In Table 2.5 column 2, $\frac{\hat{\beta}_f}{.004142} \geq 1.96 \implies \hat{\beta}_f \geq .008118$ at a 5% significance level. M^*, the pre-ACA share of employed childless adults who report having ESHI coverage with family income below 138% FPL, is 2% on average (Table 2.4). Using this as the scaling factor yields $\hat{\beta}_s \times .02 \geq .008118 \implies \hat{\beta}_s \geq 0.4059$. In other words, the effect on the rate of dominant job SE in this at-risk group would have to be at least a 40.6 pp increase, an implausibly large effect that is more than 5 times the sample dominant job SE rate of 7.7%, to detect a significant effect in the full sample. Even if we use the 99th percentile of M^*, 4.7%, the effect in the at-risk group would have to be at least a 17.3 pp increase, a more than doubling of the sample dominant job SE rate. Note that the scale of M^*_{ws}, the pre-ACA share of wage-and-salary childless adults who report having ESHI coverage with family income below 138% FPL, is similar to that of M^*. The minimum effect on the rate of WS-to-SE transition in the at-risk group would have to be a 16.9 pp increase. The trade-off in using all employed childless adults for the DiD model is that while the sample is not contaminated by endogenous income or labor supply responses, the small shares of the at-risk groups make it difficult to detect possible effects among marginal workers who may be induced by the expansion of Medicaid eligibility to become self-employed. An additional explanation for the lack of an effect on the WS-to-SE transition is that for most individuals whose dominant job is wage-and-salary, self-employment is simply not a relevant labor market state under consideration. As discussed in the conclusion below, the null results could also be related to the short post-expansion observation period, which may not allow enough time for Medicaid eligibility to shift marginal workers' decisions about their labor market state in the next year.

Relative to a null effect on self-employment persistence at average values of the pre-ACA at-risk shares among self-employed childless adults, the Medicaid expansion increased the rate of SE persistence by 6.7% for a 1-sd increase in the pre-ACA share among self-employed childless adults who have directly purchased health insurance and have family income below 138% FPL. Given that this group at the median spent 34% of total family income on health insurance premiums, taking up Medicaid as a result of expanded eligibility may increase persistence in self-employment by increasing the amount of resources that could be allocated toward the business or by reducing the pressure to transition to a wage job in order to obtain ESHI. In contrast, I find no evidence for the hypothesized risk reduction channel where Medicaid lowers the cost of remaining self-employed for those who are self-employed and uninsured. These two findings–a positive association with the share Medicaid-eligible and covered by directly purchased health insurance, and no significant association with the share Medicaid-eligible and uninsured–indicate that the effect on SE persistence is only for Medicaid-eligible self-employed childless adults with a relatively high valuation of health insurance. This result cannot be extrapolated to the overall population of self-employed childless adults who became eligible for Medicaid as a result of the expansion.

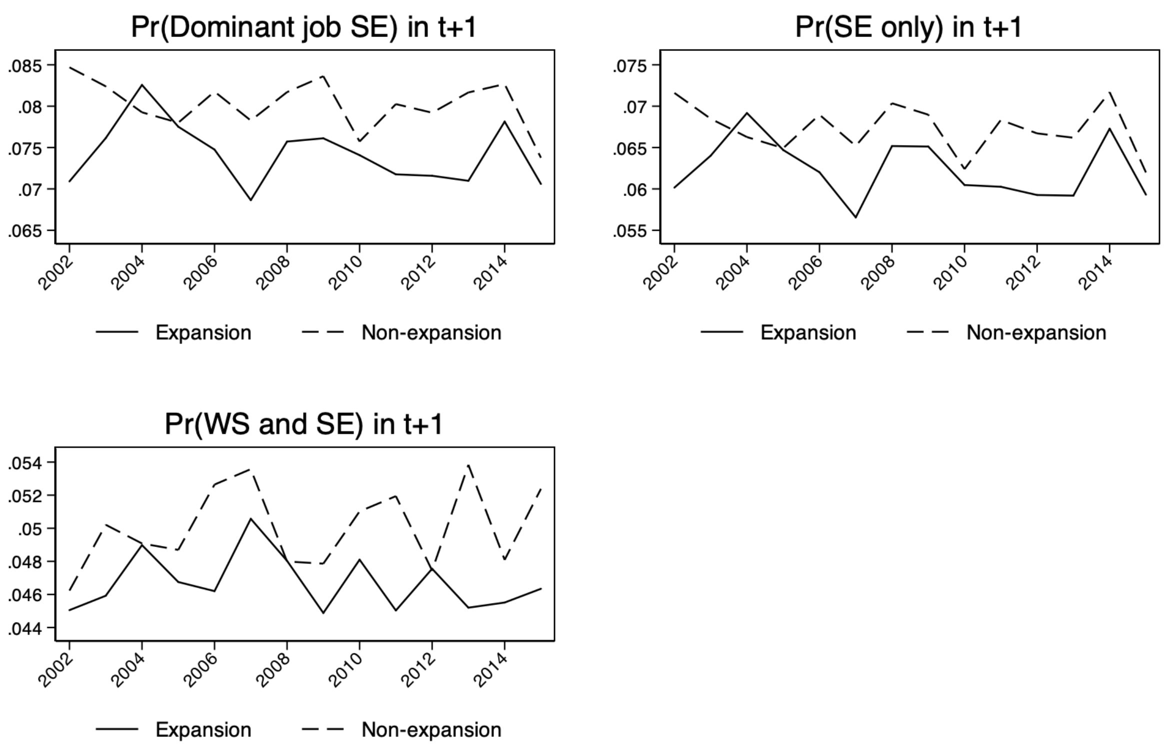
2.8 Conclusion

This paper studies how an expansion of public health insurance in the U.S. affects self-employment. Provisions of the ACA increased access to health insurance outside of wage employment, potentially changing the relative value of self-employment compared to wage employment for a subset of workers. Prior to the ACA, a wage employed person with employer-sponsored health insurance might be deterred from self-employment by the prospect of losing ESHI or disruptions in coverage. A self-employed person could either buy a plan on the individual market, likely with higher premiums and less comprehensive coverage compared to ESHI, or risk being uninsured. I focus on one component of the ACA, the expansion of Medicaid eligibility to all adults with family income below 138% FPL, while also taking into consideration the availability of Marketplace premium subsidies for families below 400% FPL. I ask how the Medicaid expansion has affected both the extensive and intensive margins of self-employment among childless adults, the group that experienced the largest increase in Medicaid eligibility due to the ACA. To estimate effects of the expansion, I use (1) a difference-in-differences strategy based on variation in state Medicaid expansion status and augmented with pre-ACA county-level population shares of at-risk groups, or groups that can potentially be induced to enter self-employment due to becoming eligible for Medicaid or premium subsidies, and (2) a triple differences approach comparing self-employment outcomes of childless adults to that of parents, who saw a much smaller increase in Medicaid income limits, within the same state-year. Using data from the CPS-ASEC linked to respondents' wage-and-salary and self-employment earnings reported on tax returns, I do not find evidence of effects on self-employment rates, self-employment earnings, and wage employment to self-employment transitions in full samples of childless adults. I find that the Medicaid expansion may have increased the rate of persistence in self-employment in counties with above average pre-ACA shares of self-employed childless adults covered by directly purchased health insurance and who are below 138% FPL, suggesting that the cost of health insurance premiums could matter for persistence in self-employment. However, this effect on self-employment persistence is only applicable to lower-income self-employed childless adults with high valuations of health insurance.

My results suggest that, at least for those with a sufficiently high valuation of health insurance, the expansion of Medicaid eligibility may have increased persistence in self-employment by reducing health insurance premiums and increasing financial resources available for the business venture. This finding complements Olds (2016) and Lee (2019), who find evidence consistent with a reduction in uninsured risk driving the positive effect of public health insurance on self-employment. The Medicaid expansion does not appear to be a significant factor in the decision to enter self-employment among those who are dominant job wage employed, partly because at-risk groups comprise very small shares of the population. Even within these groups, self-employment may not be a relevant labor market state under consideration for a majority of wage workers. In future analyses, I will restrict the sample to less-educated childless adults, since education is positively correlated with income but plausibly exogenous to the Medicaid expansion and other policy components of the ACA. Finally, due to lagged availability of the administrative tax data, the CPS-DER data used in this paper are through 2015, which is one to two years after the Medicaid expansion took place, whereas Lee (2019) uses CPS-ASEC data through 2017. Because the diffusion of information about expanded Medicaid eligibility and making a transition to self-employment may be gradual processes and behavioral adjustments take time, including additional post-expansion years of data as they become available may shed light on longer-run self-employment responses to the Medicaid expansion.

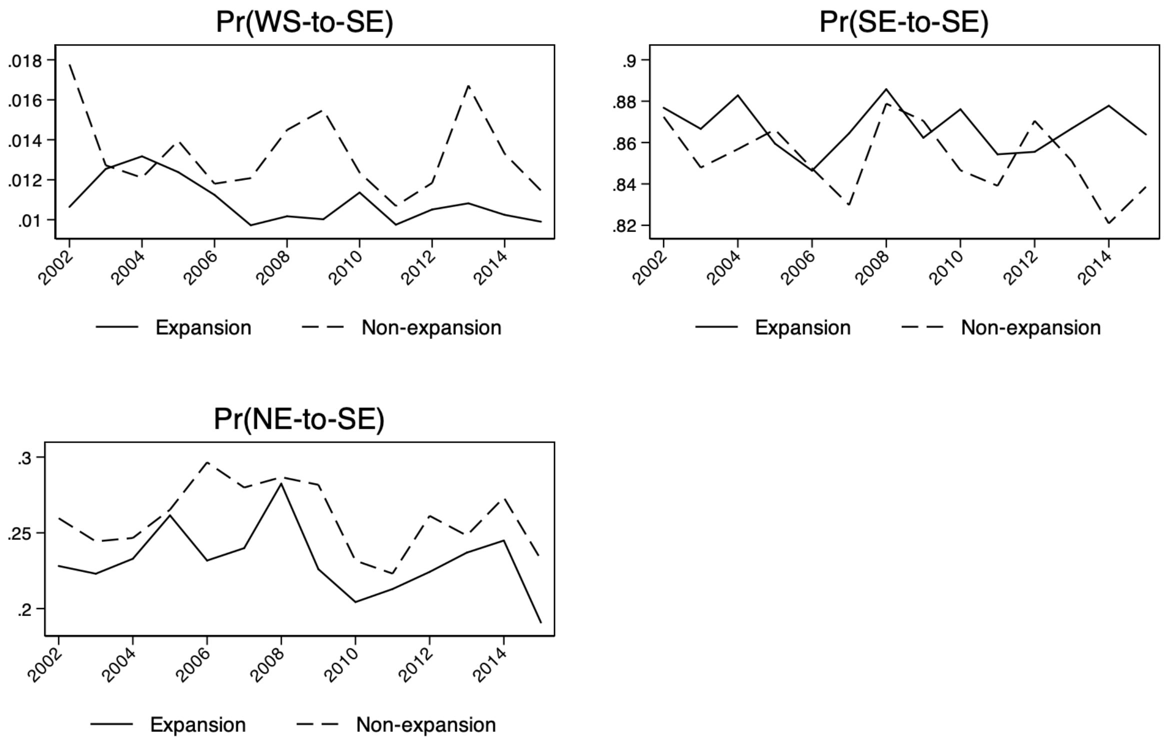
2.9 Figures

Figure 2.1: Trends in self-employment rates



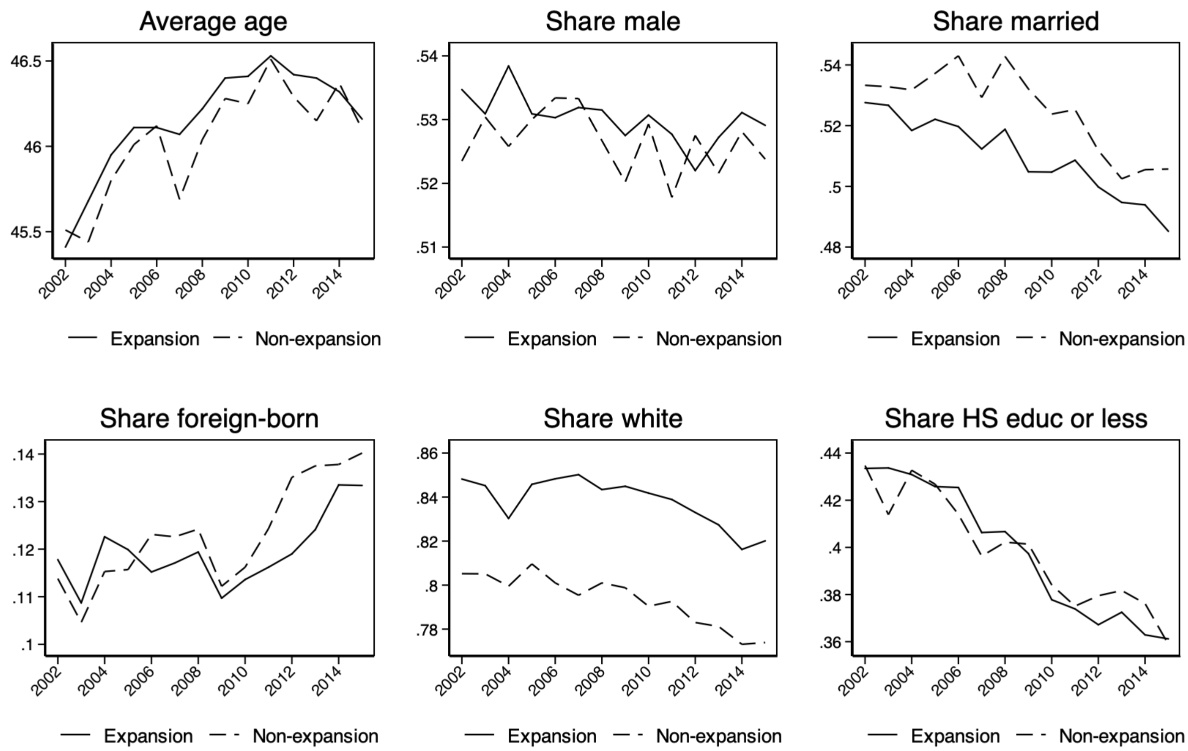
Data: CPS-DER 2002-2015, individuals age 26-64 who have positive DER earnings in $t + 1$ (1 year ahead of CPS-ASEC reference year) and have no children in the household. Rates are obtained from annual weighted sums, where weights are CPS-ASEC person weights adjusted for the probability of finding a PIK for the respondent.

Figure 2.2: Trends in rates of transition to self-employment



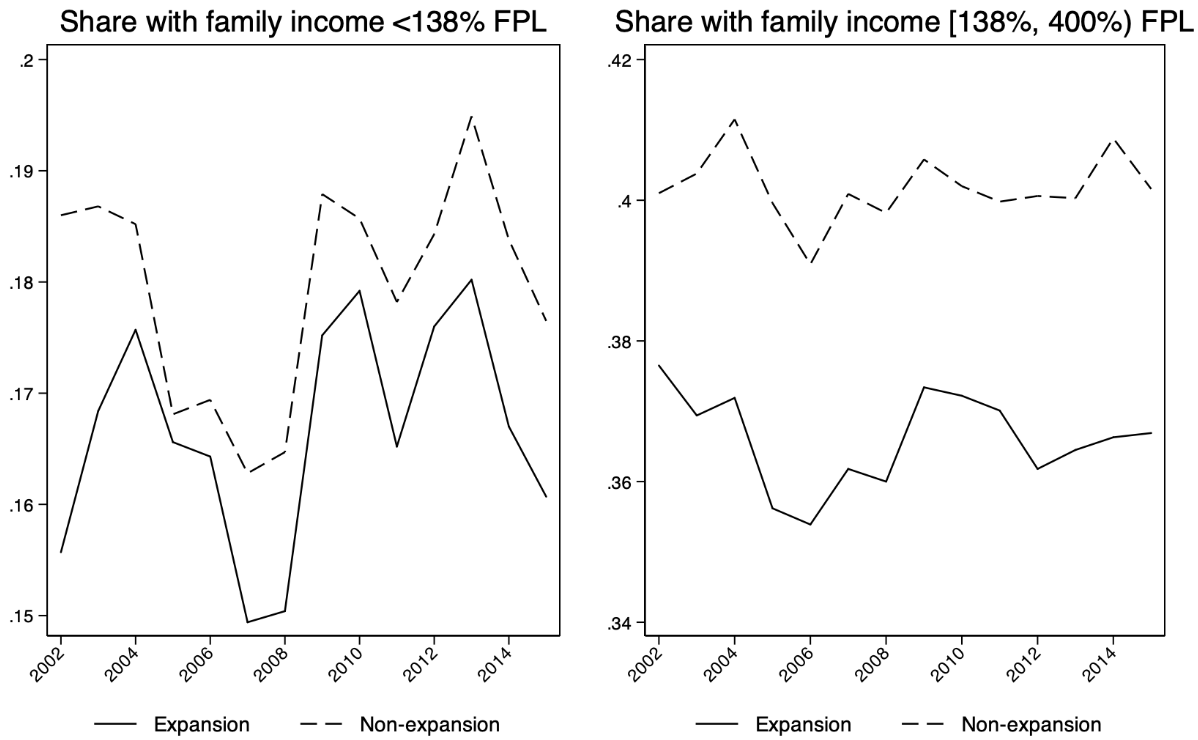
Data: CPS-DER 2002-2015, individuals age 26-64 who have positive DER earnings in $t + 1$ (1 year ahead of CPS-ASEC reference year), have no children in the household, and dominant job WS/dominant job SE/non-employed in t. Rates are obtained from annual weighted sums, where weights are CPS-ASEC person weights adjusted for the probability of finding a PIK for the respondent.

Figure 2.3: Demographic trends



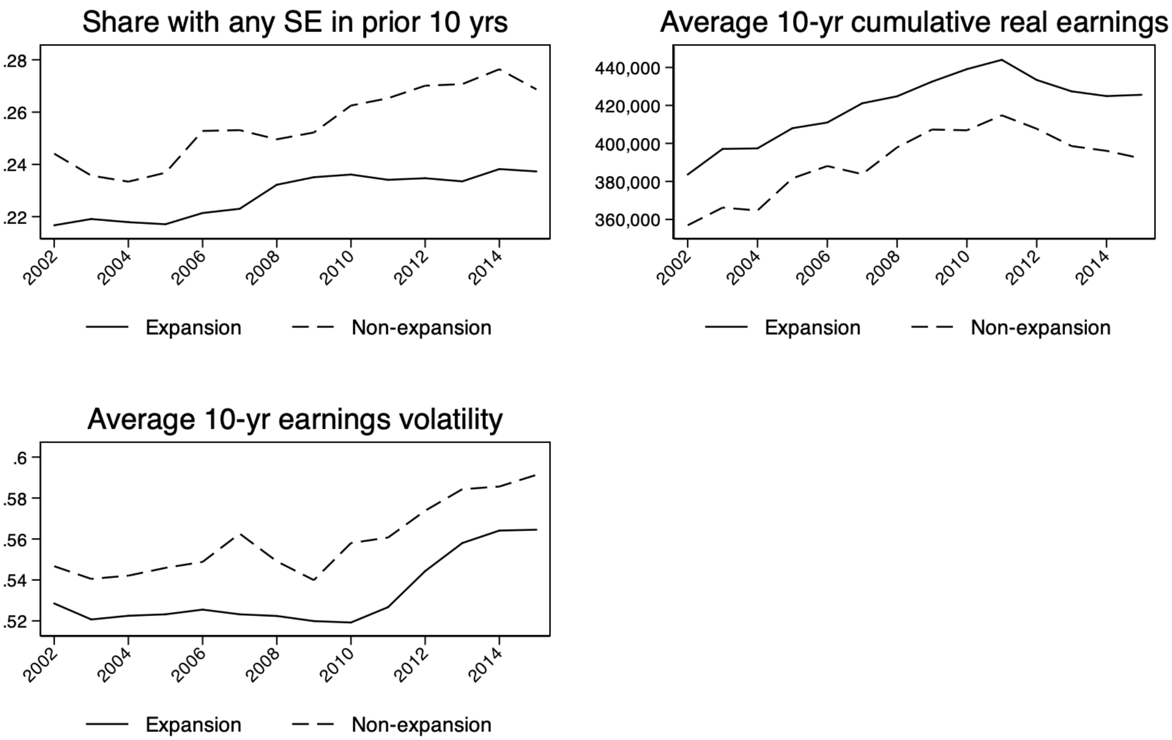
Data: CPS-DER 2002-2015, individuals age 26-64 who have positive DER earnings in $t + 1$ (1 year ahead of CPS-ASEC reference year) and have no children in the household. Shares are obtained from annual weighted sums and averages are weighted, where weights are CPS-ASEC person weights adjusted for the probability of finding a PIK for the respondent.

Figure 2.4: Trends in family income distribution



Data: CPS-DER 2002-2015, individuals age 26-64 who have positive DER earnings in $t+1$ (1 year ahead of CPS-ASEC reference year) and have no children in the household. Shares are obtained from annual weighted sums, where weights are CPS-ASEC person weights adjusted for the probability of finding a PIK for the respondent.

Figure 2.5: Trends in prior labor market experience



Data: CPS-DER 2002-2015, individuals age 26-64 who have positive DER earnings in $t+1$ (1 year ahead of CPS-ASEC reference year) and have no children in the household. Shares are obtained from annual weighted sums and averages are weighted, where weights are CPS-ASEC person weights adjusted for the probability of finding a PIK for the respondent.

2.10 Tables

Table 2.1: Medicaid income limits for non-disabled, non-elderly adult populations, as % FPL

Eligibility group	National median income limit		Income limit range	
	2013	2014	2013	2014
Pregnant women	185	205	[133, 300]	[138, 380]
Parents	64	138	[16, 215]	[16, 221]
Other non-disabled adults	0	138	[0, 211]	[0, 215]

Source: KFF (2018)

Table 2.2: National median Medicaid income limits for non-disabled, non-elderly adult populations, as % FPL

Eligibility group	Expansion states		Non-expansion states	
	2013	2014	2013	2014
Pregnant women	200	213	185	199
Parents	101	138	47	45
Other non-disabled adults	0	138	0	0

Source: KFF (2018)

Table 2.3: States included in sample, by Medicaid expansion status

Expansion	Non-expansion
Arizona	Alabama
Arkansas	Alaska
Colorado	Florida
Delaware	Georgia
Hawaii	Idaho
Illinois	Kansas
Indiana (2/1/15)	Louisiana
Iowa	Maine
Kentucky	Mississippi
Maryland	Missouri
Massachusetts	Montana
Michigan (4/1/14)	Nebraska
Nevada	North Carolina
New Hampshire (8/15/14)	Oklahoma
New Mexico	South Carolina
New York	South Dakota
North Dakota	Tennessee
Ohio	Texas
Oregon	Utah
Pennsylvania (1/1/15)	Virginia
Rhode Island	Wisconsin
Vermont	Wyoming
West Virginia	

Coverage under the ACA Medicaid expansion became effective on January 1st, 2014 in expansion states unless otherwise noted. If coverage took effect in the first half (January - June) of calendar year y, the state is considered to have expanded Medicaid starting in year y. Otherwise if coverage took effect in the second half of calendar year y (July - December), Medicaid expansion in the state is considered to have started in year $y+1$. Expansion status is defined as of the end of 2015, and the six early expansion states are excluded from the sample. Several states took up the ACA Medicaid expansion after the first half of 2015, and are defined as non-expansion states in the analysis sample: Alaska (9/1/15), Montana (1/1/16), Louisiana (7/1/16), Virginia (1/1/19), Maine (1/10/19 with coverage retroactive to 7/2/18), Idaho (1/1/20), Utah (1/1/20), Nebraska (1/1/20), Oklahoma (planned for 7/1/21), Missouri (planned for 7/1/21).

Table 2.4: Pre-ACA shares of at-risk groups among childless adults

	Mean	SD	P25	P50	P75	P95	P99
M^*	0.020	0.0087	0.014	0.019	0.025	0.035	0.047
E^*	0.248	0.0520	0.215	0.253	0.286	0.322	0.348
M^*_{ws}	0.021	0.0093	0.014	0.020	0.026	0.036	0.049
E^*_{ws}	0.268	0.0568	0.231	0.271	0.307	0.349	0.381
P^M	0.023	0.0178	0.010	0.021	0.032	0.056	0.080
P^E	0.107	0.0481	0.074	0.101	0.136	0.200	0.245
U^M	0.061	0.0387	0.032	0.054	0.081	0.140	0.179
U^E	0.115	0.0529	0.077	0.106	0.146	0.214	0.262

Based on data for 3,141 counties. All at-risk shares are calculated at the PUMA level using pooled single years of the American Community Survey, 2008-2010. County-level at-risk shares are weighted averages of PUMA-level estimates, where the weights are population-based allocation factors calculated using the equivalency file between Census 2000-based PUMAs and counties. Denominator population for M^* and E^* is employed adults aged 26-64 with no children under 18 in the household. Denominator population for M^*_{ws} and E^*_{ws} is employed adults whose main job during the ACS reference week was wage-and-salary, aged 26-64, with no children under 18 in the household. Denominator population for P^M, P^E, U^M, and U^E is employed adults whose main job during the ACS reference week was unincorporated or incorporated self-employment, aged 26-64, with no children under 18 in the household.

Table 2.5: DiD estimates for the probability of self-employment

	(1) Pr(Dominant job SE)$_{t+1}$	(2) Pr(Dominant job SE)$_{t+1}$	(3) Pr(SE only)$_{t+1}$	(4) Pr(SE only)$_{t+1}$	(5) Pr(WS and SE)$_{t+1}$	(6) Pr(WS and SE)$_{t+1}$
Exp	0.007882*	0.004764	0.006947	0.004128	0.002504	0.001577
	(0.004658)	(0.004142)	(0.004406)	(0.003953)	(0.003582)	(0.003440)
Exp $\times M^*$	-1.409**	-1.449***	-0.8984	-0.9515*	0.08218	0.1548
	(0.6213)	(0.5288)	(0.5790)	(0.5076)	(0.4206)	(0.4004)
Exp $\times E^*$	0.1239	0.06266	0.09234	0.03796	0.02882	0.004350
	(0.07883)	(0.06818)	(0.07356)	(0.06357)	(0.04683)	(0.04605)
SE past 10 years		0.2962***		0.2616***		0.1275***
		(0.01155)		(0.01112)		(0.008744)
IHS 10-yr cumulative total earnings		-0.01255***		-0.01151***		-0.0001949
		(0.001922)		(0.001877)		(0.001196)
10-yr earnings volatility		0.04150***		0.03325***		0.003186
		(0.003281)		(0.003287)		(0.002486)
Dep. var. mean	0.07678	0.07678	0.06445	0.06445	0.04821	0.04821
R^2	0.03607	0.2134	0.03655	0.1992	0.01942	0.06397
N	387,000	387,000	387,000	387,000	387,000	387,000

All specifications include year and county fixed effects, a county-specific linear trend, and individual demographic characteristics. The specifications in even-numbered columns include prior labor market summary measures and their interaction with the full set of age group dummies. Sample: CPS-DER 2002-2015, individuals age 26-64 who have positive DER earnings in $t+1$ (1 year ahead of CPS-ASEC reference year) and have no children in the household; excluding the six early expansion states. Standard errors clustered at county level. * p<0.10, ** p<0.05, *** p<0.01

Table 2.6: Triple difference estimates for the probability of self-employment

	(1) Pr(Dominant job SE)$_{t+1}$	(2) Pr(Dominant job SE)$_{t+1}$	(3) Pr(SE only)$_{t+1}$	(4) Pr(SE only)$_{t+1}$	(5) Pr(WS and SE)$_{t+1}$	(6) Pr(WS and SE)$_{t+1}$
Exp×Childless	0.005230	0.001118	0.003000	-0.0005397	0.001490	-0.0001874
	(0.003869)	(0.003274)	(0.003498)	(0.003036)	(0.004189)	(0.004109)
SE past 10 years		0.2952***		0.2622***		0.1181***
		(0.009198)		(0.007426)		(0.005184)
IHS 10-yr cumulative total earnings		-0.01405***		-0.01271***		-0.002208***
		(0.0008118)		(0.0007644)		(0.0006783)
10-yr earnings volatility		0.04883***		0.03974***		-0.0008694
		(0.001798)		(0.001503)		(0.001465)
Dep. var. mean	0.08275	0.08275	0.06828	0.06828	0.05309	0.05309
R^2	0.01408	0.1864	0.01498	0.1696	0.004476	0.05055
N	776,000	776,000	776,000	776,000	776,000	776,000

All specifications include year and state fixed effects, their interaction, each set of fixed effects interacted with the childless indicator, and individual demographic characteristics. The specifications in even-numbered columns include prior labor market summary measures and their interaction with the full set of age group dummies. Sample: CPS-DER 2002-2015, individuals age 26-64 who have positive DER earnings in $t+1$ (1 year ahead of CPS-ASEC reference year); excluding the six early expansion states. Standard errors clustered at state level. * p<0.10, ** p<0.05, *** p<0.01

Table 2.7: DiD estimates for self-employment earnings, conditional on any SE earnings

	(1) SE share of total earnings	(2) SE share of total earnings	(3) log(SE earnings)	(4) log(SE earnings)
Exp	0.0007844	-0.01741	-0.06678	-0.08765
	(0.02323)	(0.02173)	(0.08729)	(0.08357)
Exp $\times M^*$	-5.647**	-4.048	-14.13	-13.23
	(2.755)	(2.774)	(10.75)	(10.37)
Exp $\times E^*$	0.1012	-0.1414	0.1207	-0.1477
	(0.3579)	(0.3500)	(1.241)	(1.153)
SE past 10 years		0.1580***		0.7572***
		(0.03581)		(0.1164)
IHS 10-yr cumulative total earnings		-0.03470***		0.09356***
		(0.004912)		(0.01900)
10-yr earnings volatility		0.1657***		0.09414**
		(0.01298)		(0.04607)
Dep. var. mean	0.7020	0.7020	8.961	8.961
R^2	0.1563	0.2714	0.1556	0.2186
N	43,500	43,500	43,500	43,500

All specifications include year and county fixed effects, a county-specific linear trend, and individual demographic characteristics. The specifications in even-numbered columns include prior labor market summary measures and their interaction with the full set of age group dummies. Sample: CPS-DER 2002-2015, individuals age 26-64 with self-employment earnings in $t+1$ and have no children in the household; excluding the six early expansion states. Standard errors clustered at county level. * p<0.10, ** p<0.05, *** p<0.01

Table 2.8: Triple difference estimates for self-employment earnings, conditional on any SE earnings

	(1) SE share of total earnings	(2) SE share of total earnings	(3) log(SE earnings)	(4) log(SE earnings)
Exp×Childless	0.02353	0.01529	0.04326	0.03303
	(0.01602)	(0.01410)	(0.08023)	(0.07762)
SE past 10 years		0.2125***		0.7315***
		(0.02552)		(0.07935)
IHS 10-yr cumulative total earnings		-0.02771***		0.1281***
		(0.002448)		(0.01465)
10-yr earnings volatility		0.1676***		0.1067***
		(0.006401)		(0.01978)
Dep. var. mean	0.7014	0.7014	9.009	9.009
R^2	0.04785	0.1777	0.06455	0.1369
N	96,000	96,000	96,000	96,000

All specifications include year and state fixed effects, their interaction, each set of fixed effects interacted with the childless indicator, and individual demographic characteristics. The specifications in even-numbered columns include prior labor market summary measures and their interaction with the full set of age group dummies. Sample: CPS-DER 2002-2015, individuals age 26-64 with self-employment earnings in $t+1$; excluding the six early expansion states. Standard errors clustered at state level. * p<0.10, ** p<0.05, *** p<0.01

Table 2.9: DiD estimates for WS-to-SE

	(1) $Pr(SE_{t+1} = 1 \| WS_t = 1)$	(2) $Pr(SE_{t+1} = 1 \| WS_t = 1)$
Exp	-0.0008699	-0.0007870
	(0.001823)	(0.001816)
Exp $\times M^{*}_{ws}$	-0.2624	-0.2952
	(0.1961)	(0.1942)
Exp $\times E^{*}_{ws}$	-0.02251	-0.02266
	(0.02184)	(0.02204)
SE past 10 years		0.02427***
		(0.004781)
IHS 10-yr cumulative total earnings		-0.00005509
		(0.0007380)
10-yr earnings volatility		0.01589***
		(0.001711)
Dep. var. mean	0.01192	0.01192
R^2	0.01481	0.02548
N	347,000	347,000

Dependent variable is an indicator for dominant job WS in t and dominant job SE in $t + 1$. All specifications include year and county fixed effects, a county-specific linear trend, and individual demographic characteristics. The specifications in even-numbered columns include prior labor market summary measures and their interaction with the full set of age group dummies. Sample: CPS-DER 2002-2015, individuals age 26-64 who are DER dominant job wage employed in the reference year, employed in $t + 1$, and have no children in the household; excluding the six early expansion states. Standard errors clustered at county level. * p<0.10, ** p<0.05, *** p<0.01

Table 2.10: Triple difference estimates for WS-to-SE

	(1) $Pr(SE_{t+1} = 1 \| WS_t = 1)$	(2) $Pr(SE_{t+1} = 1 \| WS_t = 1)$
Exp×Childless	0.0004305	0.0002262
	(0.001572)	(0.001525)
SE past 10 years		0.02433***
		(0.004202)
IHS 10-yr cumulative total earnings		-0.0006622*
		(0.0003763)
10-yr earnings volatility		0.01481***
		(0.001276)
Dep. var. mean	0.01365	0.01365
R^2	0.002901	0.01435
N	690,000	690,000

Dependent variable is an indicator for dominant job WS in t and dominant job SE in $t+1$. All specifications include year and state fixed effects, their interaction, each set of fixed effects interacted with the childless indicator, and individual demographic characteristics. The specifications in even-numbered columns include prior labor market summary measures and their interaction with the full set of age group dummies. Sample: CPS-DER 2002-2015, individuals age 26-64 who are DER dominant job wage employed in the reference year and are employed in $t+1$; excluding the six early expansion states. Standard errors clustered at state level. * p<0.10, ** p<0.05, *** p<0.01

Table 2.11: DiD estimates for persistence in SE

	(1) $Pr(SE_{t+1}=1\|SE_t=1)$	(2) $Pr(SE_{t+1}=1\|SE_t=1)$	(3) $Pr(SE_{t+1}=1\|SE_t=1)$	(4) $Pr(SE_{t+1}=1\|SE_t=1)$
Exp	0.01687	0.01312	-0.006293	-0.009758
	(0.02226)	(0.02308)	(0.02979)	(0.03004)
Exp$\times P^M$			3.041**	3.246**
			(1.495)	(1.523)
Exp$\times U^M$			0.4239	0.5601
			(0.6376)	(0.6427)
Exp$\times P^E$			-1.054*	-1.085*
			(0.6340)	(0.6218)
Exp$\times U^E$			-0.3963	-0.4779
			(0.5019)	(0.5258)
SE past 10 years		0.2580***		0.2591***
		(0.08006)		(0.08008)
IHS 10-yr cumulative total earnings		-0.007582		-0.007426
		(0.007904)		(0.007913)
10-yr earnings volatility		-0.02666		-0.02607
		(0.01863)		(0.01870)
Dep. var. mean	0.8602	0.8602	0.8602	0.8602
R^2	0.1641	0.1956	0.1645	0.1961
N	25,500	25,500	25,500	25,500

Dependent variable is an indicator for dominant job SE in t and in $t+1$. All specifications include year and county fixed effects, a county-specific linear trend, and individual demographic characteristics. The specifications in even-numbered columns include prior labor market summary measures and their interaction with the full set of age group dummies. Sample: CPS-DER 2002-2015, individuals age 26-64 who are DER dominant job self-employed in the reference year, employed in $t+1$, and have no children in the household; excluding the six early expansion states. Standard errors clustered at county level. * p<0.10, ** p<0.05, *** p<0.01

Table 2.12: Triple difference estimates for persistence in SE

	(1) $Pr(SE_{t+1} = 1 \| SE_t = 1)$	(2) $Pr(SE_{t+1} = 1 \| SE_t = 1)$
Exp×Childless	-0.0006704	-0.0009748
	(0.01633)	(0.01588)
SE past 10 years		0.2739***
		(0.03683)
IHS 10-yr cumulative total earnings		-0.01520***
		(0.003890)
10-yr earnings volatility		-0.03015***
		(0.008212)
Dep. var. mean	0.8488	0.8488
R^2	0.04426	0.07586
N	56,500	56,500

Dependent variable is an indicator for dominant job SE in t and in $t+1$. All specifications include year and state fixed effects, their interaction, each set of fixed effects interacted with the childless indicator, and individual demographic characteristics. The specifications in even-numbered columns include prior labor market summary measures and their interaction with the full set of age group dummies. Sample: CPS-DER 2002-2015, individuals age 26-64 who are DER dominant job self-employed in the reference year and are employed in $t+1$; excluding the six early expansion states. Standard errors clustered at state level. * p<0.10, ** p<0.05, *** p<0.01

Chapter 3: Self-Employment in the Transition to Retirement (with Katharine Abraham, John Haltiwanger, Kristin Sandusky, and James Spletzer)

3.1 Introduction

As Baby Boomers enter their 60s and 70s, there has been a resurgence of interest in the question of how older Americans are transitioning from employment into retirement. There are many people for whom self-employment might be an attractive option along the path from a career job to retirement. It is well known that self-employment rates–the share of employed persons who are self-employed rather than working in a wage-and-salary job–are higher at older ages. These higher self-employment rates could be the result of individuals switching from wage and salary employment to self-employment at older ages, perhaps as a part of a transition to stopping work altogether. On the other hand, it could be that self-employed individuals are simply less likely to stop working. More generally, as we will show, the self-employment rate at any given age will reflect the full set of transition rates among wage and salary employment, self-employment and non-employment.

Our first objective in this paper is to document the increase in self-employment rates with age and show how the various transition rates associated with movements from one labor force status to another contribute to this increase. To do this, we make use of a unique linked data set that combines responses to the Annual Social and Economic Supplement to the Current Population Survey (the CPS-ASEC) with tax records that provide extensive history on the same individuals' earnings that we use to categorize individuals as wage and salary employed, self-employed or out of the labor force.[1] Our second objective is to identify the factors that, at any given age, are associated with higher or lower rates of transition across the three labor market states that are relevant to our analysis.

The unique integrated survey and administrative data set we have constructed has some significant advantages. Figure 3.1 displays self-employment rates by age group based, first, on administrative tax data and, second, on data from the CPS-ASEC.[2]. In each case, two self-employment rates are depicted: the share of employed persons for whom self-employment is the primary activity during a year and the analogous rate that counts as self-employed anyone who engaged in any self-employment activity during the year. Both sources show rising self-employment rates by age and, not surprisingly, for each source, the any self-employment rates are higher than the primary activity self-employment rates. The primary activity self-employment rate based on administrative data is 4.3% at age 23-28 and rises to 17.6% by age 65-70, an increase of 13.3 percentage points (pp). The same rate based on the CPS-ASEC rises from 2.3% at age 23-28 to 12.9% at age 65-70, an increase of 10.6 pp. However, two aspects of Figure 3.1 help to illustrate the advantages of using the administrative data to track employment activity. First, consistent with prior research showing that CPS data

[1]Labor earnings from incorporated self-employment are paid to business owners as wages or salaries, and therefore these payments cannot be distinguished from other wage and salary income in the tax data. Because our measurement of labor market status is based on the tax data, we focus on unincorporated self-employment.

[2]The administrative tax data used to construct Figure 3.1 are discussed in greater detail in Section 3.3.

miss a substantial fraction of self-employment activity (Abraham et al. 2021a,b), the self-employment rates (any and primary) are notably higher in the administrative data. Second, the gap between the tax-based and survey-based primary activity rates widens substantially for those in their late 60s and 70s. Use of the survey data alone would miss much of the rise with age in the primary activity self-employment rate. Additional advantages of the administrative data include that they permit us to compute reliable estimates of the full set of annual transition rates among wage and salary employment, self-employment and non-employment and to explore in a nuanced way the role of earnings histories in helping to explain patterns of self-employment by age.

3.2 Background and Literature Review

The higher rate of self-employment at older ages is a well-established if under-appreciated feature of the U.S. labor market (e.g., Fuchs (1982)). Studies that have examined changes in the pattern of self-employment with age commonly make use of data from household surveys such as the Current Population Survey (CPS), Retirement History Survey (RHS) or Health and Retirement Study (HRS). One caveat regarding such research is the growing evidence that self-employed household survey respondents frequently fail to report their self-employment activity or characterize their self-employment activity as working for an employer, leading to it being coded as a wage and salary job (Abraham et al. 2021a,b).

Several previous studies have sought to explain why the self-employment rate rises with age. In an early paper, Fuchs (1982) used RHS data for white urban men who were employed in 1969 or 1971 and interviewed again two years later to decompose the changes in self-employment rates for these short panels into the portion due to net switching from wage-and-salary employment into self-employment and the portion due to differential rates of labor force exit for wage-and-salary workers versus the self-employed. He found the increase in self-employment from ages 58 or 59 to ages 60 or 61 to be due entirely to net switching from wage and salary employment into self-employment, but the increase starting after age 60 to be due mainly to the self-employed being more likely to continue working. Zissimopoulos and Karoly (2007) use Fuchs' method to carry out a similar decomposition based on HRS data for individuals working full-time as of 1992, 1994, 1996 or 1998 and re-interviewed two years later. They find that net switching from wage-and-salary employment into self-employment explains about three-quarters of the modest increase in the self-employment rate over the following two years among men ages 51-55 who were employed full-time at the date of the baseline interview, but unimportant for explaining the larger increases at older ages. Both Fuchs (1982) and Zissimopoulos and Karoly (2007) study the change in self-employment among individuals who were working at the start of a two-year observation window, meaning that their analyses exclude self-employment among individuals who may have been out of the labor force at the time of the baseline interview but later returned to work.

These and subsequent studies exploring the life cycle dynamics of self-employment typically focus on a particular transition rate or set of transition rates across labor market statuses. Multiple studies have examined the factors that underlie transitions from wage-and-salary employment to self-employment. Among the factors found to raise the probability of making that transition are being more educated (e.g., Zissimopoulos and Karoly (2007)) and having prior self-employment experience (e.g., Fuchs (1982)). Prior occupation also has been identified as playing a role, with individuals in more highly skilled occupations generally being more likely to move to self-employment (e.g., Fuchs (1982), Zissimopoulos and Karoly (2007), and Cahill et al. (2013)). Other factors identified in past research as raising the probability of transitioning from wage-and-salary employment to self-employment are having greater accumulated wealth (e.g., Bruce et al. (2000), Zissimopoulos and Karoly (2007)) and receipt of Social Security benefits (Ramnath et al. 2020). Pension eligibility generally has been found to reduce this transition's probability (e.g., Fuchs (1982), Bruce et al. (2000), and Zissimopoulos and Karoly (2007)). Several studies have examined the role of health and health insurance coverage, but the findings regarding those variables are mixed.

Transitions from wage and salary employment and/or self-employment to non-employment have been another focus of interest (Fuchs 1982; Bruce et al. 2000; Ramnath et al. 2020). Ramnath et al. (2020) find that Social Security claiming at key eligibility ages increases the probability of both of these transitions, and that the association is stronger for wage-and-salary workers. The relatively small size of existing household panel surveys and the limited demographic information available in tax data have limited the possibilities for multivariate analysis of transitions out of self-employment. Rather less attention has been paid to transitions from non-employment back into employment, though both Bruce et al. (2000) and Zissimopoulos and Karoly (2009) have examined those transitions. Zissimopoulos and Karoly (2009) find that the factors associated with higher rates of transition from non-employment to self-employment include being more educated, having greater accumulated wealth and having prior self-employment experience.

Several of the cited studies report some variant of the three-by-three matrix of transition rates that fully describes movements among wage and salary employment, self-employment and non-employment either across repeated waves of the HRS (e.g., Bruce et al. (2000), Zissimopoulos and Karoly (2007), and Zissimopoulos and Karoly (2009)) or in tax data (Ramnath et al. 2020). These studies recognize that all of the transition rates in the three-by-three matrix may be relevant to understanding the trajectory of self-employment, but none has attempted to decompose how changes in the full set of transition rates individually and severally contribute to changes in the self-employment rate with age. To do this, we adapt the steady state decompositions similar to those used in Abraham and Shimer (2001) and Shimer (2012) to study how the full set of transitions among wage and salary employed (WS), self-employed (SE), and non-employed (NE) contribute to the increase in the self-employment rate with age, and begin to explore the factors that affect these transitions.

3.3 Data and Measurement

In our exploration of what lies behind the increase in the self-employment rate with age, we make use of Current Population Survey (CPS) data linked to administrative records containing 20-plus year earnings histories for the same individuals. The CPS data provide information on individuals' characteristics that is not available in the administrative records; the administrative records allow us to track individuals' career paths, and in particular their history of participation in self-employment and/or wage and salary employment, in a way that is not possible in the survey data. When combined, the linked data enable insights that could not be gleaned from either source of data if analyzed separately.

3.3.1 CPS-ASEC data

The Current Population Survey is a monthly household survey with a sample that represents the civilian population of the United States. The basic monthly CPS questionnaire collects relatively rich information on the characteristics of all members of selected households age 16 and older, including their age, gender, race, ethnicity, nativity, marital status, and education. The Annual Social and Economic Supplement (ASEC) is administered each spring to CPS households and collects information for the preceding calendar year for each adult in the household; the ASEC often is referred to as the "March supplement", though since 2002, about half of those in the ASEC sample have been interviewed in either February or April rather than in March.

We use CPS-ASEC data collected in 1996 through 2013. Since the ASEC questions refer to the previous calendar year, this means that we have CPS-ASEC information for the reference years 1995 through 2012. We restrict our sample to individuals reported in the CPS to be aged 15-79 for whom a Protected Identification Key (PIK)–an encrypted Social Security Number–is available. Two considerations motivate our exclusion of those age 80 and older. First, many of the CPS-ASEC cell sizes for those in their 80s and 90s are quite small and an analysis by age that included individuals age 80 and older would create significant disclosure problems. Second, in the CPS-ASEC microdata for most years after reference year 2002, anyone reported to be aged 80 or older is coded as either age 80 or age 85, rather than by specific year of age. Our analysis of self-employment at older ages thus focuses on persons in their 50s, 60s, and 70s.

To ensure that the information about age we have for our sample is accurate, we merge the CPS-ASEC microdata, by PIK, to the Census Bureau's Numident. The Numident is an administrative dataset containing date of birth and some additional information on all individuals who have been assigned a Social Security Number. Our CPS measure of a person's age, in years, is recorded as of the date of their CPS interview. Our Numident measure of a person's age is calculated as the reference year of the CPS-ASEC minus the birth year from the Numident. We drop from our sample anyone whose CPS age is inconsistent with their Numident age (i.e., whose CPS age is less than their Numident age or more than a year greater than their Numident age). Removing these cases results in a loss of approximately 4% of our sample.

Much of the analysis is based on transitions in labor market state from year t to year $t+1$. We define age as of January 1 of year t, the reference year for the CPS interview. This timing is important for the interpretation of our results. For example, if an individual transitions from full-time work to complete retirement on their 65th birthday, this will be observed in our data as an individual aged 64 as of January 1 of year t turning 65 later that same year. In data that capture annual earnings, such a person will be observed as employed in year t and as having transitioned to non-employment in year $t+1$. In this simple example, if there is a spike in retirements associated with turning age 65, we would expect to see higher rates of transition to non-employment among those who are age 64 at the start of year t.

Our final sample restriction is to exclude persons younger than 23 years or older than 77 years at the beginning of year t. Excluding those younger than age 23 allows us to avoid many of the transitions associated with the entrance of teenagers and young adults into the labor force. Excluding those older than age 77 at the beginning of year t is consistent with restricting age in the CPS data in both years t and $t+1$ to be less than 80 years.

3.3.2 CPS-DER data

For each reference year 1995-2012, the CPS-ASEC has been matched by PIK to the Social Security Administration's Master Earnings File (MEF). We refer to the resulting subset of the MEF, with information on CPS-ASEC respondents, as the CPS-DER, where DER is short for Detailed Earnings Record. The CPS-DER contains information from the MEF on each individual's self-employment earnings, derived from information reported on Schedule SE. Self-employment earnings is available for each year from 1978 to 2012 for every respondent in the CPS-ASEC who has a PIK, regardless of when the person's CPS-ASEC interview occurred. The CPS-DER also contains the Employer Identification Number (EIN) and the wage and salary earnings from that EIN for each Form W-2 that an individual receives in a given year. Similar to the information on self-employment, the CPS-DER contains wage and salary earnings for each year 1978 to 2012 for every CPS-ASEC respondent with a PIK, regardless of when the individual was surveyed in the CPS-ASEC. For analytical convenience, we sum the earnings from all wage and salary jobs a person may have held during a particular reference year to create a combined measure of annual wage and salary earnings for that year.

To summarize, for each CPS-ASEC respondent with a PIK, the CPS-DER contains annual self-employment earnings and wage and salary earnings for that respondent for each year from 1978 through 2012. For example, for an individual with CPS-ASEC reference year 1995, we have information on 17 years of historical self-employment earnings (1978-1994), contemporaneous self-employment earnings (1995), and 17 years of future self-employment earnings (1996-2012). For an individual with CPS-ASEC reference year 2012, we have information on 34 years of historical self-employment earnings (1978-2011) and contemporaneous self-employment earnings (2012). We use the GDP price deflator from the Bureau of Economic Analysis (Table 1.1.4, Line 1, 2012=100) to transform all reported earnings into 2012 dollars.

3.3.3 Analysis sample

We merge the demographic information from the CPS-ASEC and the administrative earnings data from the CPS-DER by PIK and CPS-ASEC reference year. As part of the merging process, we relabel the CPS-DER earnings data in relationship to the CPS-ASEC reference year. Specifically, we define 20 years of earnings history, current earnings and one year of future earnings for all individuals. The 20 years of earnings history is used to construct explanatory variables for our regression analysis. The current and one-year-ahead earnings information is used to define the year t to year $t+1$ employment status transitions that are the focus of our analysis. We have 20 years of earnings history only for individuals with CPS-ASEC reference years 1998 and later; accordingly, we have dropped those with reference years 1995-1997. Similarly, we cannot currently make use of data for individuals with CPS-ASEC reference year 2012, since we do not yet have information about these individuals' $t + 1$ employment status.

The final dataset for the present analysis covers CPS-ASEC reference years 1998 to 2011, and contains approximately 1,241,000 observations for individuals aged 23 to 77 as of January 1 of the applicable reference year.[3] All of the analyses we report make use of the CPS-ASEC estimation weight modified to account for missing PIKs. The PIK is missing for 20 to 30% of CPS-ASEC respondents, depending on the year. Following the procedure described by Abraham et al. (2021a), we use propensity score methods to re-weight the sample of persons for whom we have a PIK so that they represent the population as a whole.

In ongoing revisions, we have incorporated updated data through CPS-ASEC reference year 2015 and present analysis by single year age (and aggregated age groups at older ages in order to meet disclosure requirements). In this first draft using data through CPS-ASEC reference year 2012, to prevent problems with our being able to disclose future results, we have analyzed data for more aggregated age groupings (ages 23-28, 29-34, 35-40, 41-46, 47-52, 53-58, 59-64, 65-70, and 71-77).

3.3.4 Measurement of labor market states and transition rates

In the analysis that follows, we define three labor market states—not employed, dominant job wage and salary, and dominant job self-employed. Assignment to one of these labor

[3]Current Census Bureau disclosure rules require us to present sample sizes rounded to four significant digits.

force states is based on the earnings information contained in the DER.[4] An individual is classified as "dominant job wage and salary" if she has positive annual DER earnings and more than 50% of those earnings are derived from wage and salary work; the $\{0,1\}$ indicator "WS" is set to one if this condition is met. An individual is classified as "dominant job self-employed" if she has positive annual DER earnings and 50% or more of those earnings are derived from self-employment; the $\{0,1\}$ indicator "SE" is set to one if this condition is met. The third labor market state of "not employed" occurs when the individual has no DER earnings in a given year; the $\{0,1\}$ indicator "NE" is set to one for these individuals.[5]

We begin our analysis by constructing estimates of the share of people in our sample in each of the age groups we have defined in each of the three labor market states {NE, WS, SE}. We then construct three-by-three transition matrices showing how people move from one state to another from one year to the next, again separately by age group, shown in Table 3.1.[6] In each of these matrices, the transition rates in a given row sum to one. The

[4]With the exception of Figure 3.1, we do not use CPS-ASEC wage and salary or self-employment information in our analysis. Abraham et al. (2021a) compare labor force indicators based on the CPS-ASEC versus the DER labor force indicators and find substantial disagreements. Among those with self-employment income in the DER, 66.7% do not report any self-employment income in the CPS-ASEC; among those with self-employment income in the CPS-ASEC, 51.5% do not report any self-employment income in the DER. The disagreements in reports of wage and salary income are less pronounced, but it is nonetheless the case that 9.5% of those with DER wage-and-salary income had no reported CPS-ASEC wage-and-salary income for the same year and 12.4% of those with reported CPS-ASEC wage-and-salary income had no DER wage-and-salary income for that same year. We do not want to confound our analysis of self-employment in the transition to retirement with comparisons across data sources, and we rely on the longitudinal consistency of the administrative records to track individuals' labor market status across time.

[5]We do not use an earnings threshold in our analysis. As a means to remove "hobby jobs" from their analysis, Ramnath et al. (2020) use a $3,000 earnings threshold for activity to be classified as employment. We have done some preliminary analysis with earnings thresholds and found in unreported results that this does not make much of a difference for our core empirical results on transition dynamics.

[6]We also have carried out preliminary analyses with four labor market states in which we relax the dominant job concept and allow for a fourth state that is "both wage and salary and self-employment". These results are not reported here both because small cells for certain age groups would cause disclosure issues and because the steady state analysis that is described in the next section is more complicated with four labor market states than with three.

three transition rates along the diagonal of the matrix going from the top left to the bottom right represent the probability of remaining in the period t state in period $t+1$. Each of the six transition rates in the off-diagonals of the matrix represents a conditional probability of moving from one labor market state in period t to a different labor market state in period $t+1$. Since an individual will age by one year between t and $t+1$, these also will capture the rates of transition across labor market states for an individual between age A (the age in period t) and $A+1$ (the age in period $t+1$). In what follows, we make use of the average values of these transition rates by age group across our sample period.

There is a direct relationship between the labor market stocks and the labor market flows we have just described. More specifically, any given set of values for the three-by-three transition matrix implies a particular steady state value for the share of people in each of the three labor market states. We make use of steady state decompositions similar to those used by Abraham and Shimer (2001) and Shimer (2012) to characterize the way in which changes in transition rates over the life cycle affect the share of people who are not employed, wage and salary employed or self-employed.

3.4 Labor Market Transition Dynamics by Age

Figures 3.2 and 3.3 illustrate the dynamics of transitions among being dominant wage and salary (WS), dominant self-employed (SE) and not employed (NE) by worker age. The transition rates taken from the matrix diagonals depicted in Figure 3.2 quantify the likelihood of remaining in a particular labor market state from one year to the next. Persistence in WS is reasonably stable from age 23-28 to age 47-52 and then starts to decline at age 53-58.

Persistence in SE is lower than persistence in WS at all ages, but rises from age 23-28 through age 53-58 and then declines with age thereafter. A notable difference between the persistence profile for WS workers as compared to SE workers is that the drop off in persistence from age 53-58 to age 59-64 is much sharper for those in WS than for those in SE, though this is partially reversed after age 65. The probability that someone who is NE in one year will remain NE in the following year rises with age. Put slightly differently, at young ages, the probability that a non-worker will be working the following year is relatively high, but this becomes progressively less so at older ages.

Off-diagonal transition rates are depicted in Figure 3.3. These include all of the transitions from WS, SE and NE to other states. WS workers of all ages are more likely to transit to NE than to SE. Beginning at age 53-58, as individuals approach their retirement years, the WS-to-NE transition rate begins to rise sharply, though those still categorized as WS at age 71-77 are only a little more likely than those who are WS at age 65-70 to exit to NE. The WS-to-SE transition rate is relatively small (averaging only 1.3% on an annual basis across all age ranges) but rises slightly starting at age 53-58.

Among the self-employed, transitions to WS are more common than transitions to NE at younger ages, but the SE-to-NE transition rate becomes larger after age 47-52 and rises rapidly after age 53-58. Over the age 53-58 to age 65-70 interval, however, the SE-to-NE transition rate does not rise as rapidly as the WS-to-NE transition rate: The WS-to-NE transition rate more than doubles from age 53-58 to age 59-64, whereas the SE-to-NE transition rate increases by less than half. The behavior of these transition rates as individuals move into the 59-64 age range is notable since, given our timing conventions (age is measured at the beginning of year t for the t to $t+1$ transitions), the 59-64 age range includes the age at which individuals first become eligible to collect Social Security and the age at which they become eligible for Medicare. Figure 3.3 is consistent with reaching these critical thresholds disproportionately impacting the WS-to-NE transition.[7]

At young ages, the non-employed are much more likely to transition to WS than to SE, but this gap declines sharply with age as the NE-to-WS transition rate falls while the changes in the NE-to-SE transition rate are much smaller. These patterns suggest that the rise in the persistence of NE shown in Figure 3.2 can be attributed, in an accounting sense, mostly to the falling likelihood that a non-employed person will move into wage-and-salary employment.

Figures 3.2 and 3.3 together help to make the case that, to understand changes in the self-employment rate with age, it is important to examine the full set of transitions across states. To the extent that it has addressed the role of transitions in explaining self-employment rates, previous research generally has focused on the likelihood of switching between wage-and-salary employment and self-employment and on the relative persistence of the WS and SE in employment at older ages. In steady state, however, all of the transition rates in the three-by-three transition matrix can affect the shares of individuals in different states and thus potentially contribute to the increase in the self-employment rate with age.

[7]Transitions by single year age confirm this conjecture. For instance, focusing on the Medicare eligibility age threshold (we pool multiple years of the CPS-ASEC, which means that individuals of a given age can be from different birth cohorts and therefore can be subject to different age thresholds for full Social Security benefits), the rate of WS-to-NE transition increases by 40%, whereas the rate of SE-to-NE transition only increases by 14%.

As shown in Figure 3.3, the SE-to-WS transition declines monotonically with age, whereas the WS-to-SE transition is relatively stable with a slight increase after age 53-38, patterns that, all else the same, we would expect to be associated with a rising self-employment rate. In addition, while the WS-to-NE and SE-to-NE transitions both rise with age, the NE-to-WS transition declines much more rapidly than the NE-to-SE transition, a pattern that we also would expect to contribute to an increase in the self-employment rate with age.

To quantify the contribution of changes in the transition rates by age to the rise in self-employment, we borrow from the characterization of labor market flows developed in Abraham and Shimer (2001) and Shimer (2012) and modify that approach for our setting. In our context, we are interested in the evolution of the stocks of the three labor market states $\{\mathrm{WS}_t, \mathrm{SE}_t, \mathrm{NE}_t\}$. This evolution will be determined by the initial stocks and the full set of transition rates, as specified in the following equations:[8]

$$WS_{t+1} = \lambda^{NE,WS} NE_t + \lambda^{WS,WS} WS_t + \lambda^{SE,WS} SE_t \tag{3.1a}$$

$$SE_{t+1} = \lambda^{NE,SE} NE_t + \lambda^{WS,SE} WS_t + \lambda^{SE,SE} SE_t \tag{3.1b}$$

$$NE_{t+1} = \lambda^{NE,NE} NE_t + \lambda^{WS,NE} WS_t + \lambda^{SE,NE} SE_t \tag{3.1c}$$

The steady state solution of a 3-state labor market state model is given by:

[8]Transition rates may vary across both time and age. Whereas the cited analyses have been concerned with the variation in these rates over time, our analysis exploits the variation in transition rates with age. We are not explicit about this in equation (3.1) as we are simply trying to illustrate how the stocks are related to the flows. Note also that this characterization assumes that the system follows a Markov process.

$$WS = k[\lambda^{SE,NE}\lambda^{NE,WS} + \lambda^{NE,SE}\lambda^{SE,WS} + \lambda^{NE,WS}\lambda^{SE,WS}] \tag{3.2a}$$

$$SE = k[\lambda^{WS,NE}\lambda^{NE,SE} + \lambda^{NE,WS}\lambda^{WS,SE} + \lambda^{NE,SE}\lambda^{WS,SE}] \tag{3.2b}$$

$$NE = k[\lambda^{SE,WS}\lambda^{WS,NE} + \lambda^{WS,SE}\lambda^{SE,NE} + \lambda^{WS,NE}\lambda^{SE,NE}] \tag{3.2c}$$

where k is a proportionality constant set to ensure that the steady state shares of the three labor market states sum to one. The steady state solution represents the shares of people in each of the three labor market states to which outcomes will converge, given a set of transition rates that remain fixed over time. The steady state solution in equation (3.2) is derived by setting employment states in t and $t+1$ equal to each other and solving for the steady state shares. As shown in Figures 3.2 and 3.3, transition rates change with age, in some cases quite sharply, meaning that observed outcomes may differ from the steady state outcomes.

In what follows, we exploit this steady state solution to quantify the contribution of changes in transition rates to changes in the share of workers in each of the three labor market states as well as the self-employment rate (given by the ratio SE/(WS+SE)). We can compute this steady state solution for every age group depicted in Figures 3.2 and 3.3.

The actual and implied steady state shares of individuals in each of the three labor market states as well as the actual and implied steady state self-employment rate are depicted in Figure 3.4. The actual patterns in the data are denoted by the solid lines. The share of individuals who are WS begins to fall after age 41-46 with especially large declines after age

53-58. The share of individuals who are SE rises with age through 41-46 and then tails off after 59-64. The implication of the robust decline in WS and relative stability in SE after age 53-58 is that, consistent with what we saw earlier in Figure 3.1, the self-employment rate should rise with age, with especially large increases after age 53-58. The solid line in the panel at the bottom right of the figure confirms this. The share of individuals who are NE is initially quite flat, but begins to rise after age 41-46, with especially large increases after age 53-58.

The dashed lines in the figure show the implied steady state WS, SE and NE shares and implied steady state self-employment rate by age based on the transition rates we have estimated. The implied steady state shares capture the qualitative patterns of the actual shares relatively well.[9] Through age 41-46, the steady state shares match the actual rates quite closely; at older ages, there is a gap between the steady state and actual shares that increases with age before closing somewhat in the 71-77 age range. Even with these gaps at older ages, however, the implied steady state shares capture the patterns of the declining share of WS individuals, the hump-shaped pattern in the share who are SE, the rising share of NE individuals, and the rising self-employment rate with age. The difference between the implied steady state patterns and the actual patterns is that the steady state patterns overstate the actual changes. The reason for this is straightforward: especially when transition rates are changing rapidly with age, as is particularly the case among those older than age 41-46, it takes time to achieve convergence to the steady state solutions and

[9]Using initial stocks and transition rates by single year age, our implied steady state shares match the data very closely throughout the full age range 23-77 (see Appendix Figure B.2). A future draft will use these steady state shares by single year age.

therefore time for the actual data to catch up to the implied steady state.

We illustrate the relatively slow convergence to the steady state in Appendix Figure B.1. This figure is constructed as follows. Starting at the steady state shares for age 41-46 (which at that age closely approximate the actual shares), the transition equations specified in (3.1) are used with the transition rates for age 47-52. Figure B.1 shows that the shares who are WS, SE and NE eventually converge to the steady state shares implied by the age 47-52 transition rates, but that full convergence takes a considerable amount of time.[10]

Even though caution is appropriate given the gaps between the actual and steady state shares, the implied steady state changes with age capture the basic patterns in the actual changes quite well and, by construction, should be an accurate reflection of the impact of changing transition rates on the shares in each of the three labor market states by age. We use the steady state approximation as a vehicle for decomposing the contribution of different transition rates to the change in the distribution of the stock of people across labor market states with age.

To better understand the factors responsible for driving the changes with age in the steady state stocks and steady state self-employment rate, we conduct an exercise in which we allow certain sets of transition rates to vary according to their actual age-group-specific values while holding all of the other transition rates constant at their age 41-46 values, then trace out the implied steady state paths for the outcomes of interest under the specified counterfactual. The steady state equations are highly nonlinear, which means that the

[10] Our use of broad age groups yields slower implied annual convergence rates than would arise if we used detailed age groups. This is because there are much larger differences in transition rates and associated steady state values across broad age groups than for detailed age groups.

implied effects of allowing each of the individual transition rates or groups of transition rates to vary in turn will not necessarily add up to the implied effect of allowing all of the transition rates to vary simultaneously. Still, this exercise should give us a good sense of the relative importance of different transition rates or sets of transition rates as we seek to understand the changes with age in the distribution of individuals across labor market states and in the self-employment rate.

We begin by examining the implied steady state changes associated with allowing the WS-to-SE and SE-to-WS transition rates to vary with age, while holding constant the rates of transition into and out of NE at their age 41-46 values. The results of implementing this counterfactual are shown as the solid lines in Figure 3.5; for comparison, the dashed lines in the figure show the changes in the steady state implied when all of the transition rates in the three-by-three matrix are allowed to change with age, as already presented in Figure 3.4. Figure 3.6 presents the results of a similar counterfactual exercise, but with only the transitions to and from NE permitted to vary with age, holding the WS-to-SE and SE-to-WS transition rates constant at their age 41-46 values. Figures 3.7 and 3.8, respectively, present the results of counterfactual exercises in which only the NE-to-WS and NE-to-SE transition rates are allowed to vary (Figure 3.7) or only the WS-to-NE and SE-to-NE transition rates are allowed to vary (Figure 3.8).

The results of these exercises suggest that all of the various sets of transition rates play a role in accounting for the implied steady state changes with age in the distribution of individuals across labor market states and in the self-employment rate. Not surprisingly, allowing only the WS-to-SE and SE-to-WS transition rates to vary with age explains very little of the change in the share of individuals who are not employed with age, though there is a small effect that likely can be attributed to the changes in these transition rates moving more people into self-employment, a state from which the rate of exit to NE is higher than the rate of exit from WS to NE. This counterfactual does imply, however, a notable increase in the share of the population that is SE together with a modest decline in the share that is WS, leading to a rise in the self-employment rate especially after age 53-58. The implied rise at older ages in the share of the population that is self-employed in this counterfactual is at odds with the actual pattern, but this is not surprising since the counterfactual shuts down the increase in the rates of transition to NE with age that are proximately responsible for the declining share of the population that is SE in the baseline steady state numbers.

As a way to illustrate the magnitude of the contribution of changes in various combinations of transition rates to the overall changes in the steady state labor force status shares and steady state self-employment rate, we focus on the changes between age 41-46 and age 71-77. Table 3.2 reports the results of our illustrative calculations. The rows labeled "Long diff counterfactual" report how the steady state values for WS, SE, NE and the self-employment rate change when only the transition rates identified in a particular panel have been allowed to change from their age 41-46 values to their age 71-77 values; the rows labeled "Long diff steady state" show how the same values change when all of the transition rates are allowed to change as they actually did between age 41-46 and age 71-77; and the rows labeled "Percent (CF/SS)" show the percent of the overall changes in the steady state values explained by changes in the indicated transition rates. As can be seen in Panel A, about 38% of the rise in the steady state self-employment rate between age 41-46 and age 71-77 can be explained by changes in the WS-to-SE and SE-to-WS transition rates.

Figure 3.6 reports the results of a counterfactual in which all of the transitions to and from NE are turned on, but the WS-to-SE and SE-to-WS transitions are turned off. This counterfactual does a very good job of reproducing the steady state changes in the WS and NE shares with age; it also does a reasonably good job of explaining the decline in the steady state SE share with age, but undershoots the steady state change in the self-employment rate. According to the numbers in Panel B of Table 3.2, taken as a whole, changes in the rates of transition to and from NE explain about 60% of the rise in the steady state self-employment rate between age 41-46 and age 71-77. Changes with age in the WS-to-NE and SE-to-NE transitions (Figure 3.8 and panel C of Table 3.2) and in the NE-to-WS and NE-to-SE transitions (Figure 3.7 and panel D of Table 3.2) each make independent contributions, but the combined effect of these independent contributions is substantially smaller than the contribution of the full set of transitions into and out of non-employment considered simultaneously. According to Panel C, permitting only WS-to-NE and SE-to-NE to vary with age accounts for 20% of the increase in the steady state self-employment rate between age 41-46 and age 71-77; according to Panel D, permitting only the NE-to-WS and NE-to-SE transitions to vary with age accounts for 17% of the same increase; and the two contributions together sum to 37%. In contrast, letting all of the rates of transition into and out of NE simultaneously vary by age, as is done in Panel B, accounts for 60% of the entire steady state increase.

In short, changes both in the rate of switching between WS and SE and in the rates of movement out of and into the labor force (transitions to and from NE) are important in accounting for the rise in the self-employment rate by age. Switching rates play a role because, as they age, workers become more likely to make the switch from WS to SE as opposed to the switch from SE to WS. Transitions to NE play a role because the WS-to-NE transition rate rises disproportionately after age 53-58 relative to the SE-to-NE transition rate. Transitions from NE back to work also play a role because the NE-to-WS transition falls much more rapidly than the NE-to-SE transition, meaning that the relative size of the flow into SE from NE rises with age. According to our illustrative calculations, considered simultaneously, flows from work to NE and from NE to work explain about 60% of the rise in the steady state self-employment rate between age 41-46 and age 71-77, whereas changes in the rate at which workers switch between WS and SE account for an estimated 38%.

3.5 The Determinants of Transition Rates by Age

We turn next to an exploration of factors that help to explain the various labor market transitions we have been examining and, in particular, how the effects of those factors vary with age. The sample used for this portion of the analysis includes all 1998-2011 CPS-ASEC respondents who were age 41-77 at the start of their CPS-ASEC reference year for whom a PIK is available.

We estimate a separate regression for each of the six off-diagonal transitions–two for switching between WS and SE (WS-to-SE and SE-to-WS), two for transitions to non-employment (WS-to-NE and SE-to-NE), and two for transitions from non-employment back to work (NE-to-WS and NE-to-SE). The regression specification is

$$
\begin{aligned}
Y_{i,t+1} &= X_{it}'\beta_1 + \delta_a + \lambda_t + E_{it}\beta_2 + \sigma_{it}\beta_3 \\
&\quad + \sum_j S_{it} \times Age_{ijt}\beta_4 + \sum_j E_{it} \times Age_{ijt}\beta_5 + \sum_j \sigma_{it} \times Age_{ijt}\beta_6 + \epsilon_{it} \qquad (3.3)
\end{aligned}
$$

All of the regressions are conditional on a person's prior labor market state at t. In each case, the dependent variable $Y_{i,t+1} = 1$ if individual i is in a given labor market state (wage and salary (WS), self-employed (SE), or non-employment (NE)) in year $t+1$, and 0 otherwise. So, for example, if $Y_{i,t+1}$ is an indicator for self-employed at $t+1$ and the regression is conditional on wage and salary work at t, the regression is concerned with the factors that affect the transition from WS to SE. As before, labor market state is identified based on a dominant job concept: if an individual has both wage and salary and self-employment earnings in a given year, her labor market state is the type of work that accounts for the greater share of her earnings in that year.

On the right hand side of each regression, X_{it} is a vector of demographic characteristics for individual i in year t that includes dummies for gender, race (white, black, or other), marital status, educational attainment (less than high school, high school, some college, and college or above), and foreign-born status; δ_a is a set of detailed age fixed effects; and λ_t is a set of year fixed effects.

The variable E_{it} is a measure of real discounted earnings over the previous 20 years and the variable σ_{it} is a measure of earnings volatility over the previous 20 years, for individual i as of year t. Both variables are defined as in Chapter 2, Section 2.6.1; note that the summation for E_{it} is over the previous 20 years instead of over the previous 10 years.

The final three terms interact dummy variables for broad age groups with education dummies, cumulative earnings, and earnings volatility.[11] Age_{ijt} is a vector that contains a dummy for age 53-64 and a dummy for age 65 and older, with age 41-52 as the omitted group. S_{it} are education dummies for having less than a high school education, some college or a Bachelor's degree or higher, with exactly a high school education as the omitted category. We also include an indicator for having less than 2 years with positive total earnings (in which case earnings volatility is not defined) and interact this indicator with the broad age dummies. All regressions are weighted using the CPS-ASEC estimation weights adjusted using propensity score methods to account for observations lost due to missing PIKs.

The choice of the particular variables for which we have examined age interaction effects was motivated in part by previous findings in the literature. Several previous studies have found that more advantaged older workers–i.e., those who are more educated or have higher net worth–are more likely to transition from wage and salary work to self-employment and (in one case) also to transition from non-employment to self-employment. Accordingly, we examine how the effects of education on transition probabilities change with age. We do not observe individuals' accumulated wealth, but we do know the cumulative value of their real earnings over the previous 20 years, which we treat as a proxy for wealth. In addition, we include a measure of the volatility of earnings to capture potentially unanticipated fluctuations in earnings that could have been associated with economic setbacks or other

[11] For robustness, we have tried specifications that use narrower age groups or detailed age dummies for the interactions. We have chosen to report the specification using broad age groups because it yields more precise estimates, but using narrower age ranges does not substantially alter the overall patterns in the results.

difficulties.

Selected coefficients from the regressions just described are reported in Table 3.3. These coefficients include the main effect of having different levels of education in comparison to exactly a high school education, the main effect for the cumulative earnings measure and the main effect for the earnings volatility measure (corresponding in each case to the effect of the variable for someone age 41-52) and the coefficients on these variables interacted with the age 53-64 and age 65+ dummy variables. Although we do not focus on these coefficients since they are difficult to interpret in isolation, their associated t-statistics provide some useful guidance on the statistical significance of the various effects of interest.

The coefficients shown in Table 3.3 can be used to calculate the effects of various characteristics on the probability that an individual will remain in her initial state and on the probability that the individual will transition to each of the other two states.[12] Table 3.4 reports the implied marginal effects of education, cumulative real earnings, and earnings volatility at different ages on the likelihood of persisting in a particular initial state (WS, SE or NE). For this purpose, the marginal effect of education is the effect of having the indicated education level rather than exactly a high school education. The marginal effects of cumulative earnings and earnings volatility are the effects of having a value for the variable that is one standard deviation higher than the age-group-specific mean, with the standard deviation used for this purpose the standard deviation for the age group in question.[13]

[12]The model coefficients can be used directly to calculate the effects on the probability of transitioning to an alternative state; the corresponding effects on the probability of persisting in the initial state then can be calculated as the negative of the sum of the two transition probabilities.

[13]The mean (standard deviation) of cumulative earnings is 13.24 (2.695) for those age 41-52, 12.79 (3.624) for those age 53-64, and 10.84 (5.124) for those age 65 and older. The corresponding figures for the mean (standard deviation) of the earnings volatility measure are 0.711 (0.552) for those age 41-52, 0.673 (0.587)

Table 3.5 reports the implied marginal effects of these same variables on each of the six off-diagonal transition rates. We have organized the discussion of these tables according to individuals' initial state (WS, SE or NE).

For those who are employed and receiving more than half of their earnings from wage and salary work (WS) in year t, persistence in WS to year $t+1$ increases with both education and past earnings. The positive effect of being more educated on continuing in WS is greatest for those age 53-64; for example, having a Bachelor's degree or higher relative to exactly a high school education increases the probability of remaining in WS in year $t+1$ by 1.07 pp for ages 53-64, but by just 0.41 pp for ages 65 and up. The positive effect of cumulative past earnings on persistence in WS declines monotonically with age.

With respect to effects of the same variables on transitions out of WS, higher cumulative past earnings increase the probability of entering self-employment from WS and reduce the probability of exiting to non-employment from WS. These effects are larger after turning 65 than at younger ages. Above age 65, a 1-sd increase in cumulative earnings increases the probability of transitioning from WS to SE by 2.15 pp, a 47% increase relative to the mean WS-to-SE transition rate, and reduces the probability of transitioning from WS to NE by almost 4 pp, a 68% decrease relative to the mean WS-to-NE transition rate. Being more educated has qualitatively similar effects on transitions to self-employment and non-employment, but with a somewhat different age pattern: the largest (positive) effects on transitions to SE are observed for those age 65 and older, but the largest (negative) effects on transitions to NE are observed for those age 53-64.

for those age 53-64 and 0.786 (0.624) for those age 65 and older.

Wage and salary workers with higher historical earnings volatility are less likely to remain in WS from t to $t + 1$, and this becomes more pronounced with age. The lower persistence in WS for those with more volatile earnings reflects higher probabilities of transitioning both to SE and to NE. The marginal effect of a 1-sd increase in earnings volatility on transitions to SE is modest and fairly constant across age groups. The marginal effect on exiting to NE is larger in magnitude and grows with age: a 1-sd increase in earnings volatility increases the probability of becoming non-employed by 3 pp (52%) for the youngest age group, rising to almost 4 pp (69%) for age 65 and older.

Conditional on being employed and receiving at least half of earnings from self-employment (SE) in year t, through age 53-64, those with higher past cumulative earnings are more likely to remain SE in year $t+1$, but this is reversed among those age 65 and older. As can be seen in Table 3.4, at age 65 and older, having cumulative earnings that are one standard deviation higher reduces the likelihood of remaining self-employed by about 3.5 pp. This is different than the pattern for the wage and salary employed, for whom having cumulative earnings that are one standard deviation higher increases the likelihood of persisting by about 1.8 pp. Similarly, a working person age 65 plus who has a college degree is less likely to persist in the same state if self-employed but more likely if wage and salary employed.

The patterns of exit from SE to WS and SE to NE, shown in Table 3.5, are the opposite side of the coin and examining them can help us to understand the patterns of persistence in SE by education and cumulative earnings. Among those younger than age 65, having a college education or higher past earnings increases the probability of transitioning from SE to

WS and reduces the probability of transitioning from SE to NE, with the latter effect being the dominant influence. Among those age 65 and older, however, relative to being a high school graduate, having a Bachelor's degree or higher increases the probability of moving from SE to WS by a modest 0.26 pp and also increases the probability of exiting from SE to NE by 1.14 pp, so that both transitions contribute to lower persistence in self-employment for college-educated individuals age 65 and older. Similarly, again among those age 65 and older, higher earnings substantially increase transitions from SE to WS (by 2.72 pp or 31% relative to the mean rate for a one standard deviation increase in cumulative earnings) and also slightly increase transitions from SE to NE (by 0.78 pp or 5.3% relative to the mean rate), with both effects again contributing to lower persistence in SE. Consistent with the finding that higher education generally increases the SE-to-WS transition rate, those 65 and older with less than a high school education are 1.6 pp (18% of the mean rate) less likely than high school graduates to make that transition. Somewhat at odds with the general pattern, however, relative to a high school graduate the same age, those age 65 and older with less than a high school education are 6.7 pp (46% of the mean) more likely to transition from SE to NE.

Similar to the marginal effects found when conditioning on being WS in the prior year, among those younger than 65, higher earnings volatility increases transitions from SE to both WS and, especially, NE. After age 65, the effect of higher earnings volatility on transitions to NE remains positive, though much dampened, whereas the effect on transitions to WS becomes negative.

Among those not employed (NE) in year t, higher past cumulative earnings lower the probability of remaining NE and increase the probability of transitioning to both WS and SE. The positive effects of earnings on transitions to employment are smaller at older ages. A 1-sd increase in cumulative earnings raises the probability of transitioning to either WS or SE by about 1.4 pp at ages 41-52, but at age 65 and older, this effect drops to 0.9 pp for transitions to WS and 0.4 pp for transitions to SE. Being more educated increases both transitions to WS and, especially, transitions to SE at age 53-64, but the effects of education on the NE-to-WS and NE-to-SE transitions are smaller (and more similar to each other) after age 65. Earnings volatility affects transitions to the two employment states differently. At 53 and older, higher earnings volatility reduces the propensity to transition from NE to WS, but increases the propensity to transition from NE to SE. For ages 53-64, a 1-sd increase in earnings volatility reduces the probability of transitioning to wage and salary employment by 0.6 pp (12.6% of the mean transition rate) and increases the probability of transitioning to self-employment from non-employment by 0.7 pp (32% of the mean transition rate). The magnitudes of these effects are considerably smaller for those age 65 and older.

To summarize, education and past cumulative earnings have broadly similar effects on the off-diagonal transition propensities we have examined, increasing the likelihood of moving either from WS to SE or from SE to WS, and reducing the likelihood of moving from employment to non-employment. Similarly, education and past cumulative earnings both also increase the probability of moving from non-employment to employment (both WS and SE). Perhaps not surprisingly, the positive marginal effects of education and earnings on moving from non-employment back into employment are substantially dampened after age 65, an age at which many people have already retired. This dampening is captured even with the broad age bins used in our current regression specification. Interestingly, at older ages, the effects of both education and past earnings on persistence in employment depend on whether one was wage and salary employed or self-employed. Past age 65, both having a college degree and higher past earnings reduce persistence if starting from self-employment, but increase persistence if starting from wage and salary employment. The marginal effects of earnings volatility also depend on the prior state.

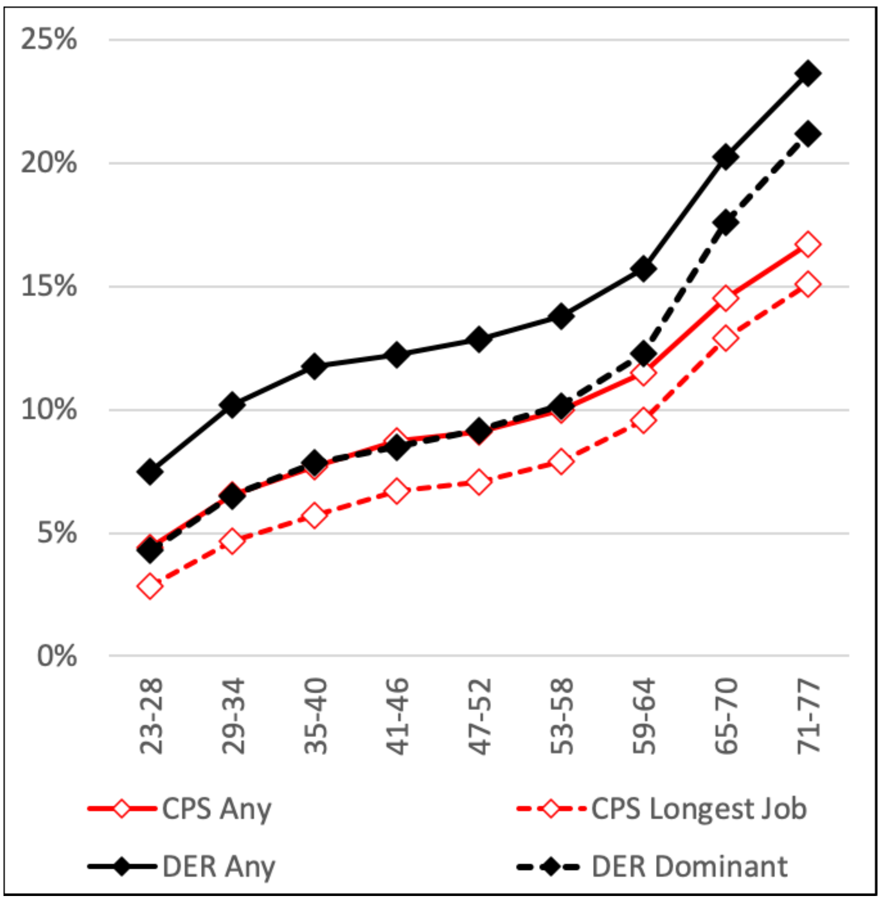
3.6 Conclusion

It is well known that self-employment rates are higher at older ages. Our measure of dominant self-employment based on the DER is 8.5% for those aged 41-46, increases to 10.1% for those aged 47-52, and is 17.6% for those aged 65-70. Our methodology for understanding the sources of this increase in the self-employment rate with age is based on the observation that the steady state self-employment rate at any given age reflects the full set of transition rates among the three labor market states of wage and salary employment, self-employment, and non-employment. Using household survey data (the CPS-ASEC) linked to administrative records (the DER) that we use to assign labor force state, we show that about 38% of the rise in the steady state self-employment rate between age 41-46 and age 71-77 can be explained by changes in the rates of switching between wage and salary employment and self-employment, and about 60% can be explained by the rates of transition to and from non-employment.

In ongoing revisions, we have incorporated CPS-ASEC data through reference year 2015 (and DER earnings information through 2016) and have estimated labor market shares and transition rates by single year age (and aggregated age groups in the 60s and 70s). This will allow us, for example, to examine more directly the effects of factors such as becoming eligible to collect Social Security or becoming eligible for Medicare on the labor market transitions that affect self-employment. Working with these single-year transition rates substantially reduces the size of the discrepancies between the actual and steady state paths of the WS, SE and NE shares and the self-employment rate (see Figure B.2). We also document differences in the rise in the self-employment rate with age across educational attainment, gender, and race. The self-employment rate increases more with age among workers with a college degree (compared to workers with lower levels of education), who are male (compared to female workers), and who are White (compared to non-White workers). We will examine, for each group, the contributions of various transitions to the growth in the self-employment rate with age, and how this differs across groups. Finally, we will use regressions to explore how much of the differences in self-employment rates across groups can be explained by demographics and earnings histories, such as prior self-employment experience and whether an individual's prior work experience has been primarily in an industry with a high rate of self-employment.

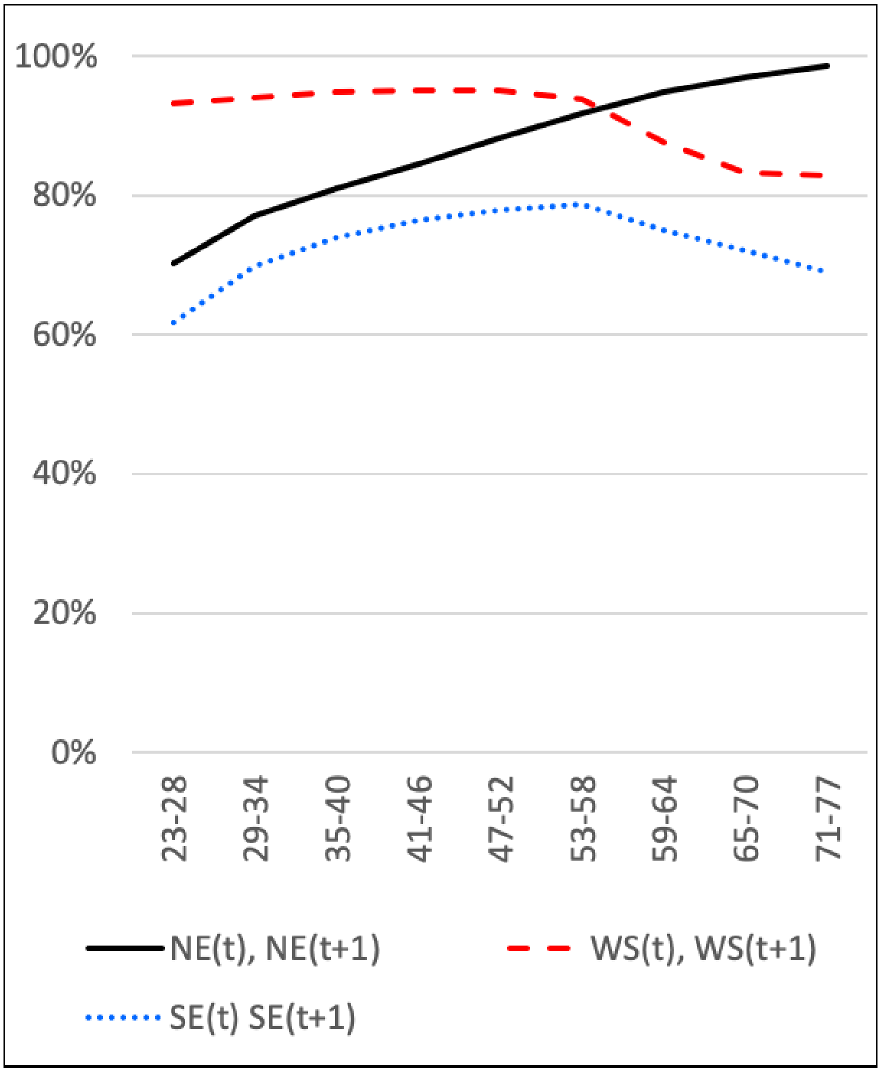
3.7 Figures

Figure 3.1: Self-employment rates by age, Current Population Survey (CPS) and Detailed Earnings Record (DER) administrative data, ages 23-28 to 71-77



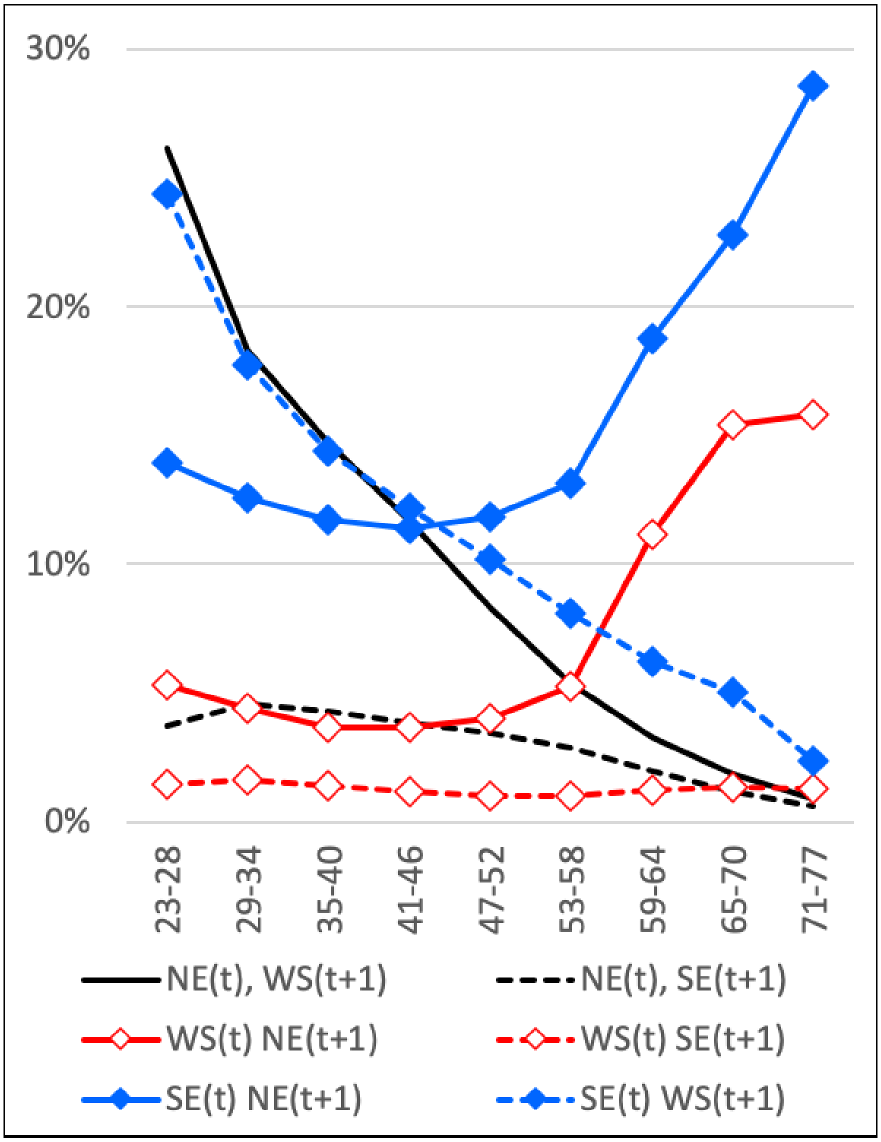
The self-employment rate is the share of employed persons who have any self-employment income or whose longest or dominant job is self-employment. Reported estimates are based on pooled annual data for the period 1998 through 2011.

Figure 3.2: Rates of persistence in the states WS, SE and NE



Data: CPS-DER, 1998 to 2011.

Figure 3.3: Rates of transition across the states WS, SE and NE



Data: CPS-DER, 1998 to 2011.

Figure 3.4: Actual and steady state values of WS, SE, NE and self-employment rate by age

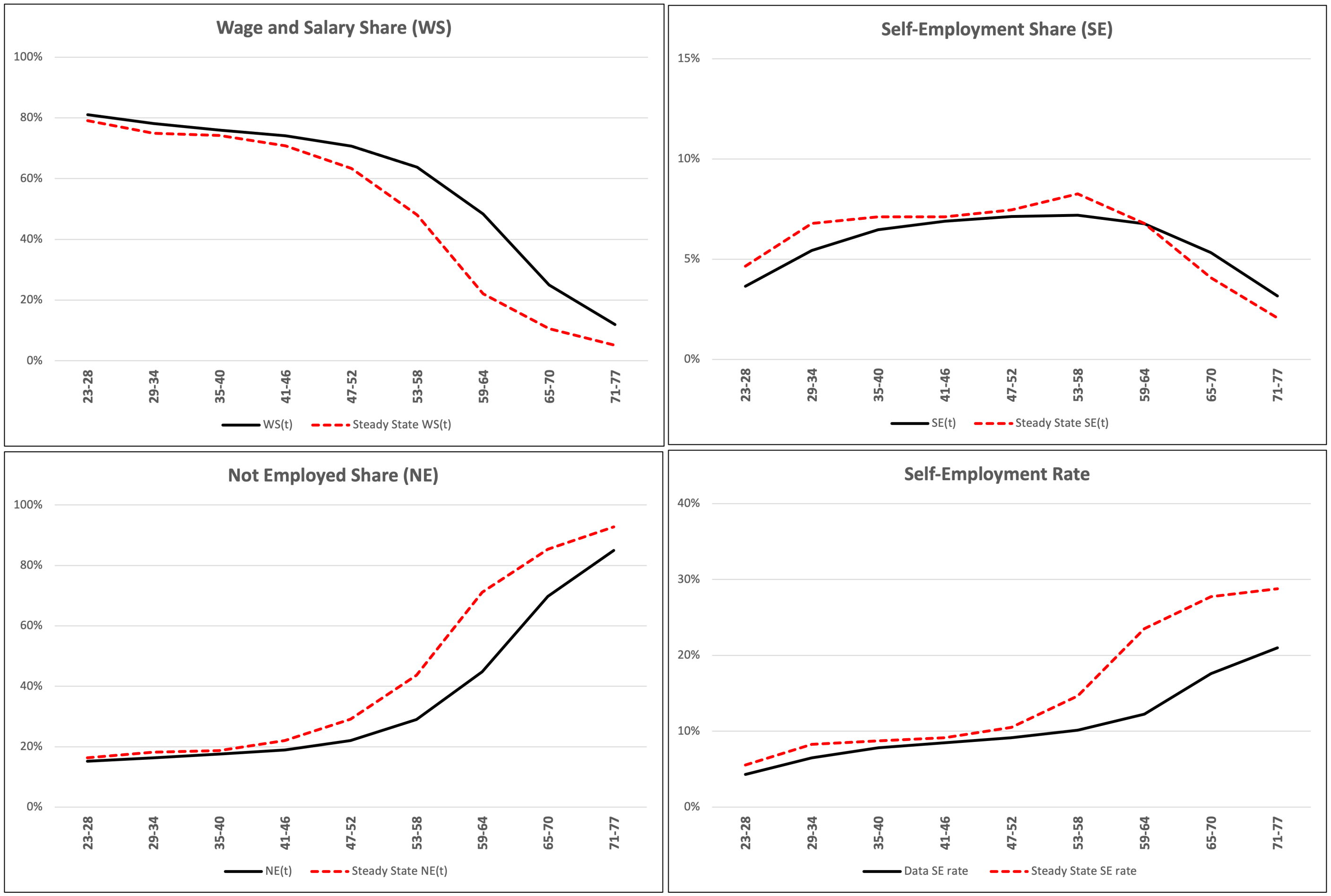


Figure 3.5: Counterfactual values of WS, SE, NE and self-employment rate when only WS-SE and SE-WS rates allowed to vary with age

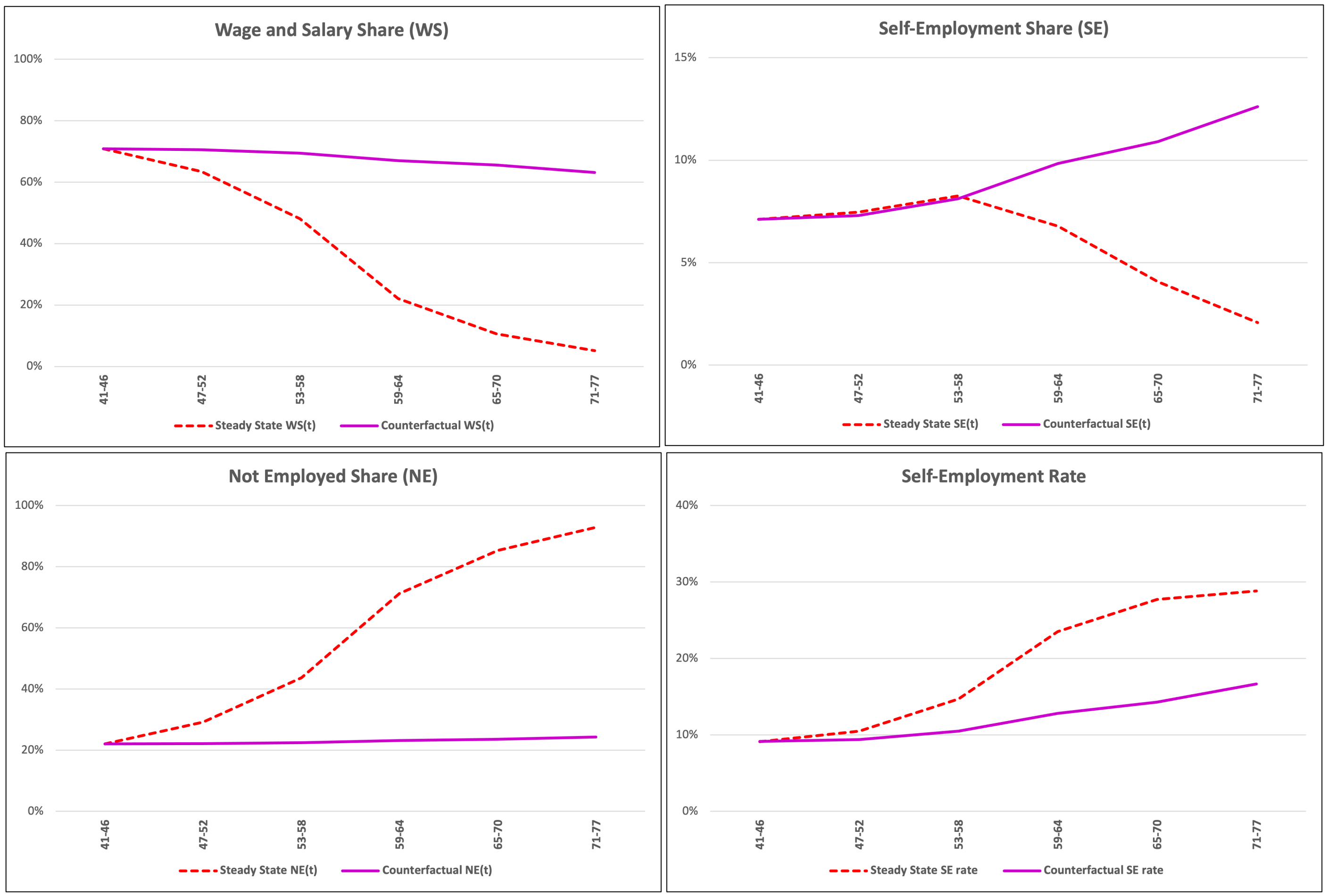


Figure 3.6: Counterfactual values of WS, SE, NE and self-employment rate when only flows into and out of NE allowed to vary with age

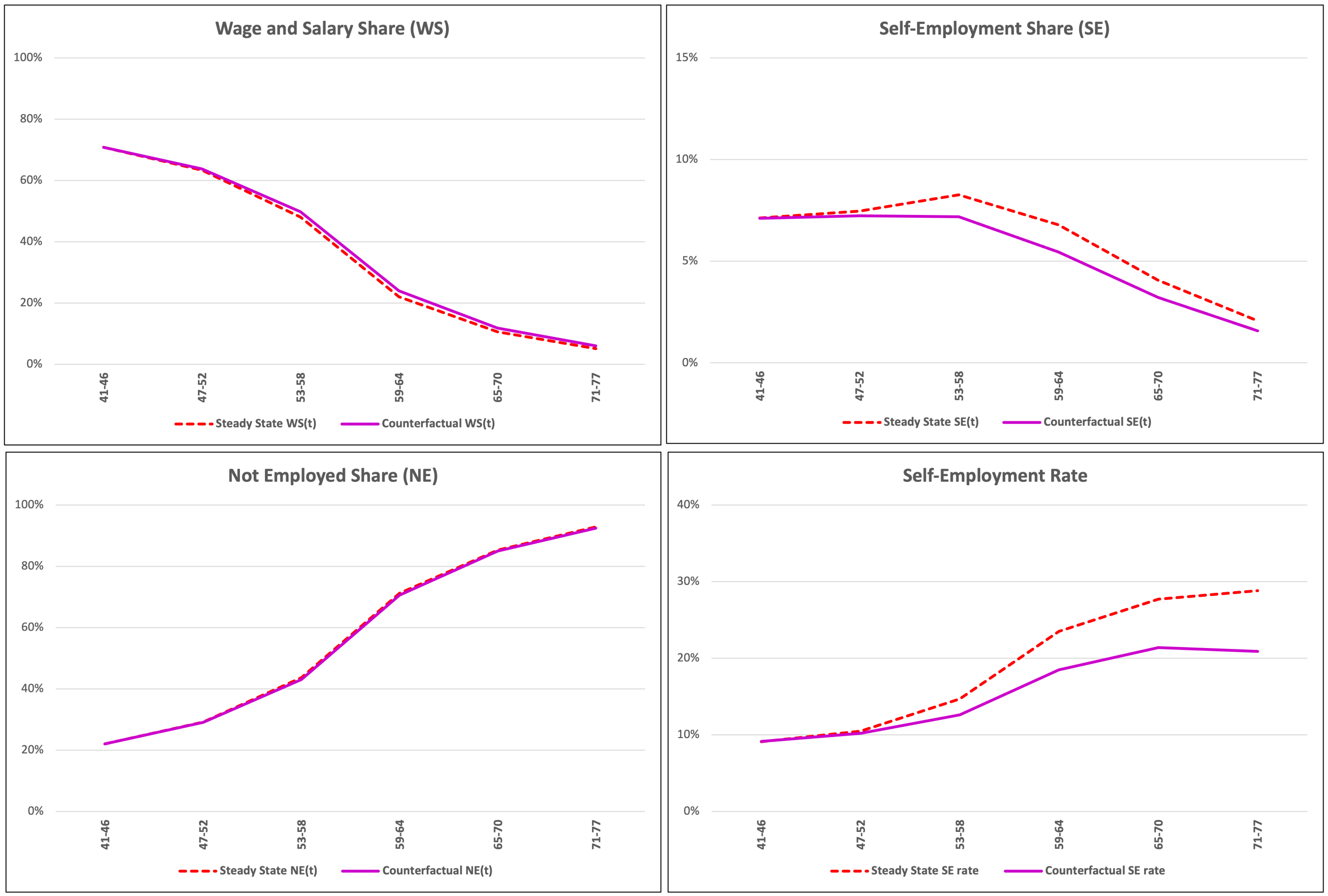


Figure 3.7: Counterfactual values of WS, SE, NE and self-employment rate when only NE-WS and NE-SE allowed to vary with age

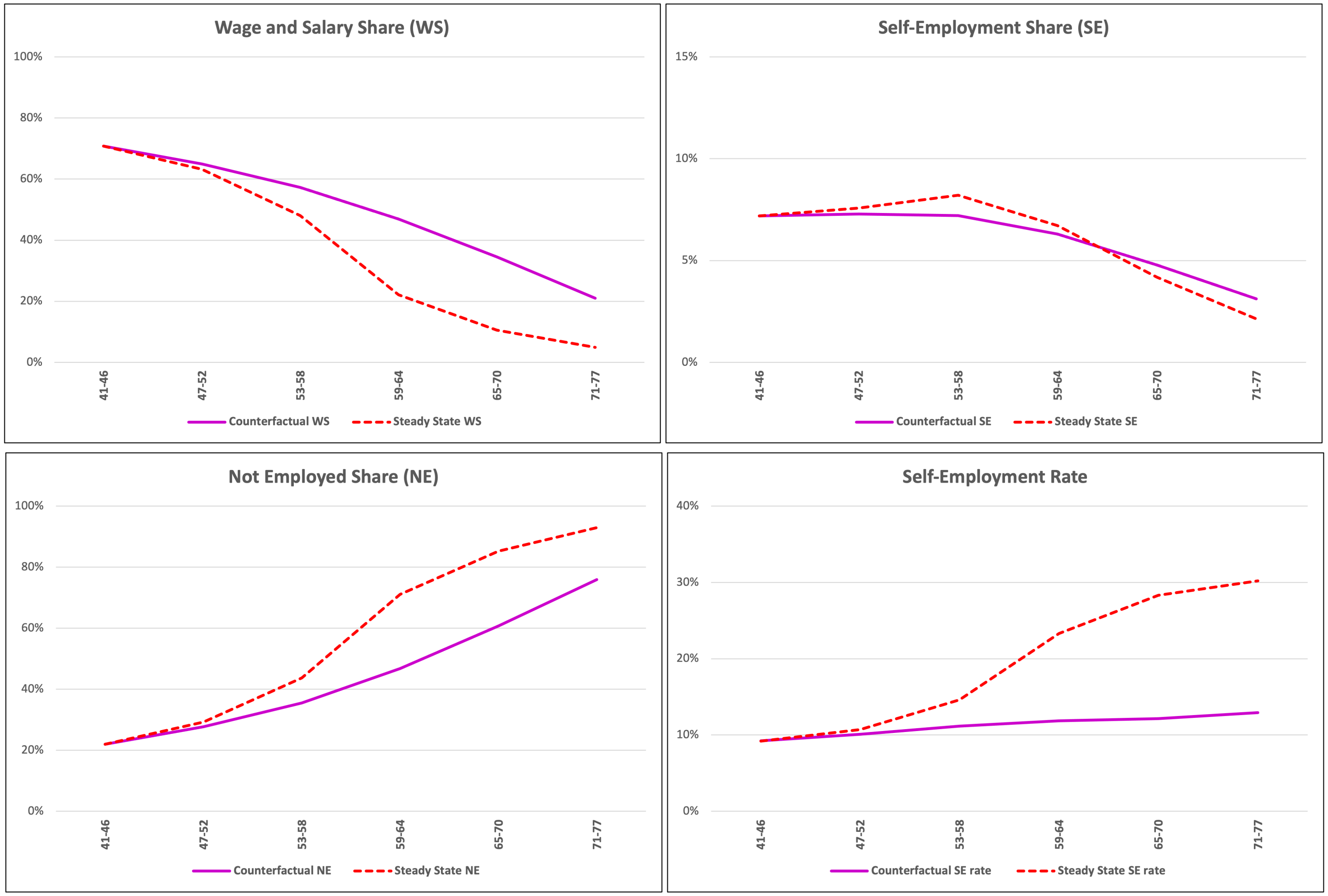
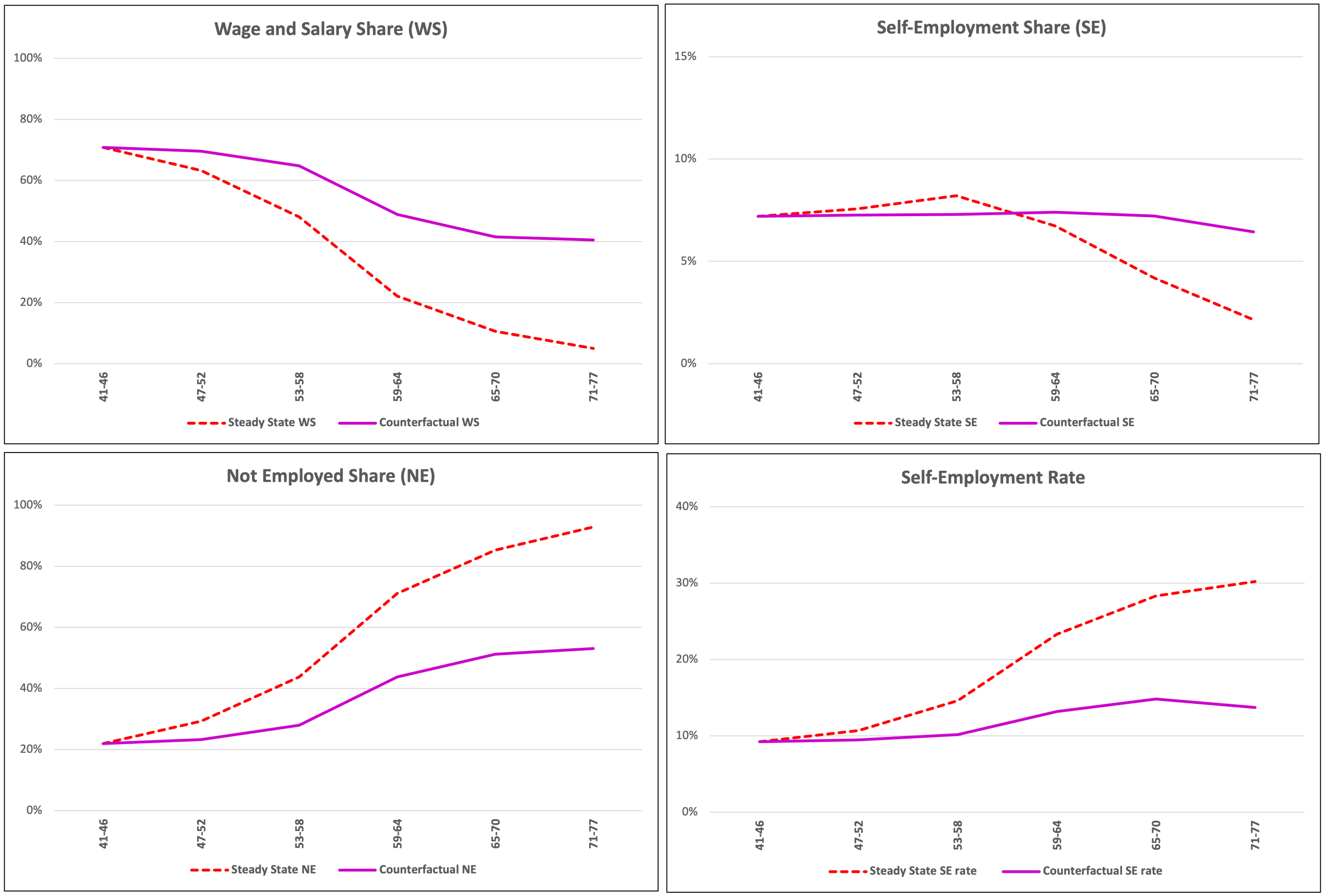


Figure 3.8: Counterfactual values of WS, SE, NE and self-employment rate when only WS-NE and SE-NE allowed to vary with age



3.8 Tables

Table 3.1: 3x3 labor market state transitions

	$WS_{t+1} = 1$	$SE_{t+1} = 1$	$NE_{t+1} = 1$
$WS_t = 1$	$\lambda^{\text{WS,WS}}$	$\lambda^{\text{WS,SE}}$	$\lambda^{\text{WS,NE}}$
$SE_t = 1$	$\lambda^{\text{SE,WS}}$	$\lambda^{\text{SE,SE}}$	$\lambda^{\text{SE,NE}}$
$NE_t = 1$	$\lambda^{\text{NE,WS}}$	$\lambda^{\text{NE,SE}}$	$\lambda^{\text{NE,NE}}$

Table 3.2: Counterfactual decompositions

A. Only Switchers (WS-SE and SE-WS) allowed to vary after age 41				
Age 41+	WS(t)	SE(t)	NE(t)	SE Rate
Long Diff Counterfactual	-0.077	0.055	0.022	0.075
Long Diff Steady State	-0.657	-0.051	0.707	0.197
Percent (CF/SS)	0.117	-1.089	0.031	0.382
B. Only NE "Ins and Outs" allowed to vary after age 41				
Age 41+	WS(t)	SE(t)	NE(t)	SE Rate
Long Diff Counterfactual	-0.648	-0.055	0.704	0.118
Long Diff Steady State	-0.657	-0.051	0.707	0.197
Percent (CF/SS)	0.987	1.097	0.995	0.597
C. Only Transitions to NE allowed to vary after age 41				
Age 41+	WS(t)	SE(t)	NE(t)	SE Rate
Long Diff Counterfactual	-0.299	-0.009	0.309	0.04
Long Diff Steady State	-0.657	-0.051	0.707	0.197
Percent (CF/SS)	0.456	0.187	0.437	0.203
D. Only Transitions from NE allowed to vary after age 41				
Age 41+	WS(t)	SE(t)	NE(t)	SE Rate
Long Diff Counterfactual	-0.494	-0.041	0.534	0.033
Long Diff Steady State	-0.657	-0.051	0.707	0.197
Percent (CF/SS)	0.752	0.806	0.755	0.168

Table 3.3: Regression coefficients on interaction terms in labor market state transition equations

			WS(t) to SE(t+1)	NE(t) to SE(t+1)	SE(t) to WS(t+1)	NE(t) to WS(t+1)	WS(t) to NE(t+1)	SE(t) to NE(t+1)
Education								
	Less than HS							
		Main Effect	0.1561	-0.4947	-0.2049	-1.2290	1.7610	-1.5220
			[1.919]	[-3.005]	[-0.3216]	[-5.217]	[9.972]	[-1.962]
		Interacted with 53-64	-0.0456	0.0842	0.4480	0.2029	0.1505	3.6690
			[-0.3551]	[0.3906]	[0.4477]	[0.6576]	[0.5395]	[3.01]
		Interacted with 65+	0.1131	0.2111	-1.3460	0.7914	-1.8590	3.0390
			[0.5508]	[1.053]	[-0.9999]	[2.757]	[-4.165]	[1.855]
	Some College							
		Main Effect	0.0147	0.1680	0.9775	0.2184	-0.1175	-1.1540
			[0.2768]	[1.128]	[2.013]	[1.025]	[-1.015]	[-1.953]
		Interacted with 53-64	0.1492	0.5326	-0.6405	0.5619	-0.1941	0.1647
			[1.661]	[2.653]	[-0.8323]	[1.955]	[-0.9944]	[0.1759]
		Interacted with 65+	0.2107	0.0429	-0.9056	0.1006	0.3055	3.6230
			[1.180]	[0.2195]	[-0.7925]	[0.3592]	[0.7876]	[2.605]
	College+							
		Main Effect	0.2899	1.2610	1.0470	0.0366	-0.3250	-1.8740
			[5.404]	[8.365]	[2.307]	[0.1696]	[-2.789]	[-3.394]
		Interacted with 53-64	0.4367	0.3770	-1.0450	0.7864	-1.4720	-0.2183
			[4.909]	[1.848]	[-1.477]	[2.693]	[-7.617]	[-0.2534]
		Interacted with 65+	0.8699	-0.7715	-0.7847	0.3494	-1.2420	3.0160
			[4.934]	[-3.849]	[-0.7757]	[1.218]	[-3.243]	[2.449]
Cumulative earnings								
	Main Effect		0.0313	0.5107	0.4806	0.5190	-1.2930	-0.8472
			[1.063]	[12.81]	[2.562]	[9.098]	[-20.2]	[-3.711]
	Interacted with 53-64		0.1379	-0.0507	0.6978	-0.1773	0.4703	-0.5097
			[2.933]	[-0.9365]	[2.535]	[-2.284]	[4.604]	[-1.521]
	Interacted with 65+		0.3873	-0.4326	0.0500	-0.3347	0.5216	0.6617
			[5.251]	[-8.459]	[0.1336]	[-4.573]	[3.255]	[1.452]
Earnings volatility								
	Main Effect		1.5260	-0.1784	0.7328	-3.4770	5.4270	12.3900
			[0.0349]	[-0.0047]	[1.414]	[-15.99]	[38.59]	[19.65]
	Interacted with 53-64		-0.2384	1.3720	1.2330	2.4930	-0.1744	-0.8529
			[-2.188]	[6.786]	[1.593]	[8.618]	[-0.7368]	[-0.9056]
	Interacted with 65+		-0.2677	0.6665	-1.3530	3.3600	0.9500	3.2810
			[-1.374]	[3.406]	[-1.231]	[12]	[2.244]	[2.453]

All regression coefficients are multiplied by 100 so that they can be interpreted as percents. All models also include dummies for detailed age, marital status, race, gender, high school, and year, as well as a dummy for having only one year of earnings and the interactions of that variable with age. Main effects are for individuals age 41-52. Numbers in brackets underneath the coefficient estimates are t-statistics.

Table 3.4: Marginal effects of education, cumulative earnings, and earnings volatility on labor market persistence by age

			WS(t) to WS(t+1)	SE(t) to SE(t+1)	NE(t) to NE(t+1)
Education					
	Less than high school				
		Age 41-52	-1.917	1.727	1.724
		Age 53-64	-2.023	-2.390	1.437
		Age 65+	-0.171	-5.157	0.722
	Some college				
		Age 41-52	0.103	0.176	-0.386
		Age 53-64	0.148	0.652	-1.481
		Age 65+	-0.413	-3.860	-0.530
	Bachelors or higher				
		Age 41-52	0.035	0.827	-1.298
		Age 53-64	1.070	2.090	-2.461
		Age 65+	0.407	-1.404	-0.876
Cumulative earnings					
		Age 41-52	3.401	0.988	-2.775
		Age 53-64	2.368	0.646	-2.905
		Age 65+	1.808	-3.498	-1.344
Earnings volatility					
		Age 41-52	-3.838	-7.244	2.017
		Age 53-64	-3.839	-7.926	-0.123
		Age 65+	-4.764	-1.128	-0.232
Dependent variable mean			89.6	76.5	93.2

Marginal effects for cumulative earnings and earnings volatility are evaluated for an age group-specific one standard deviation change. Marginal effects for the three educational attainment categories are evaluated relative to the outcome for an otherwise similar high school graduate.

Table 3.5: Marginal effects of education, cumulative earnings, and earnings volatility on labor market transitions by age

			WS(t) to SE(t+1)	WS(t) to NE(t+1)	SE(t) to WS(t+1)	SE(t) to NE(t+1)	NE(t) to WS(t+1)	NE(t) to SE(t+1)
Education								
	Less than high school							
		Age 41-52	0.156	1.761	-0.205	-1.522	-1.229	-0.495
		Age 53-64	0.111	1.912	0.243	2.147	-1.026	-0.411
		Age 65+	0.269	-0.098	-1.551	6.708	-0.438	-0.284
	Some college							
		Age 41-52	0.015	-0.118	0.978	-1.154	0.218	0.168
		Age 53-64	0.164	-0.312	0.337	-0.989	0.780	0.701
		Age 65+	0.225	0.188	0.072	3.788	0.319	0.211
	Bachelors or higher							
		Age 41-52	0.290	-0.325	1.047	-1.874	0.037	1.261
		Age 53-64	0.727	-1.797	0.002	-2.092	0.823	1.638
		Age 65+	1.160	-1.567	0.262	1.142	0.386	0.490
Cumulative earnings								
		Age 41-52	0.084	-3.485	1.295	-2.283	1.399	1.376
		Age 53-64	0.613	-2.981	4.271	-4.917	1.238	1.667
		Age 65+	2.145	-3.953	2.719	0.779	0.944	0.400
Earnings volatility								
		Age 41-52	0.842	2.996	0.405	6.839	-1.919	-0.098
		Age 53-64	0.756	3.083	1.154	6.772	-0.578	0.701
		Age 65+	0.785	3.979	-0.387	1.515	-0.073	0.305
Dependent variable mean			4.6	5.8	8.8	14.7	4.6	2.2

Marginal effects for cumulative earnings and earnings volatility are evaluated for an age group-specific one standard deviation change. Marginal effects for the three educational attainment categories are evaluated relative to the outcome for an otherwise similar high school graduate.

Appendix A: Appendix to Chapter 1

A.1 Additional Data Sources

County and state level covariates are constructed using the following data sources:

- Population shares by age, sex, and race are calculated from the County Characteristics Resident Population Estimates file produced by the U.S. Census Bureau.
- Child poverty rates are produced by the Small Area Income and Poverty Estimates program at the Census Bureau.
- Unemployment rates and labor force participation rates are provided by the Local Area Unemployment Statistics program at the U.S. Bureau of Labor Statistics.
- The share of adults with own children under age 18 in the state who are below twice the poverty threshold and who are single parents is calculated using American Community Survey single year microdata. This is a state-level variable.
- The teenage birth rate, a state-level variable, is defined as births to mothers ages 15-19 per 1,000 women ages 15-19. Number of births by mother's age are from NCHS Vital Statistics Natality public-use data.
- The county rate of alcohol deaths among ages 18-57 is calculated from the MCD microdata. A death is coded as an alcohol death if the underlying cause of death is a 100% alcohol-attributable chronic or acute cause as defined by the CDC, excluding fetal alcohol syndrome and "Fetus and newborn affected by maternal use of alcohol".[1]
- The number of substance abuse treatment facilities per 100,000 population is calculated using County Business Patterns data from the Census Bureau. I sum the number of establishments in a county with the following 6-digit NAICS codes: 621420 - Outpatient mental health and substance abuse centers, and 623220 - Residential mental health and substance abuse facilities. From the same data source, I also create the number of child and youth services establishments (624110 - Child and Youth Services, e.g., adoption agencies, foster care placement services) and the number of community food and housing services establishments (6242 - Community Food and Housing, and Emergency and Other Relief Services, e.g., temporary shelters) per 100,000 population.
- The share of state and local public expenditures on various categories are state-level variables constructed from the Census Bureau's Annual Survey of State and Local Government Finances. The shares of expenditures on the following three categories

[1]Alcohol-Related Disease Impact online application defines a list of alcohol-related ICD codes.

are included as covariates in some regression specifications: public welfare (cash assistance payments; vendor payments for medical care, including Medicaid, and other purposes; other public welfare), housing and community development (e.g. spending on public housing projects and rent subsidies), and correctional institutions for adults and juveniles.

A.2 Effects of opioid-related policies on opioid overdose death rates

To understand some of the policy mechanisms driving the correlation between the initial exposure measures in 2000 and post-2010 changes in opioid deaths, this section estimates the effects of (i) the OxyContin reformulation and (ii) state implementation of PDMPs on prescription and illicit opioid death rates. For the reformulation, differences across counties in the two initial PO supply measures are used to identify the effects of the one-time national shock. The estimated effects therefore serve as a test of whether the instruments, i.e., the initial PO supply measures, are correlated with changes in opioid death rates after 2010, the year of the reformulation. For the state PDMPs, the identifying variation occurs at the state-year level. The policy indicator is interacted with each baseline supply measure. Suppose, for example, that the coefficient on the interaction term when the outcome is the PO death rate is negative and significant. This would indicate that PDMPs reduced PO deaths more in counties with higher initial PO density/oxycodone share, consistent with the instruments measuring exposure to PDMPs. In short, the following two specifications aim to validate the hypothesized mechanisms underlying the first stage.

The effects of the OxyContin reformulation on illicit and prescription opioid death rates should differ across counties based on initial measures of exposure to the supply shock.

Following Alpert et al. (2018), I estimate the effect of the reformulation using an event study specification:

$$Y_{ct} = (\delta_t \times \text{BLMeasure}_{c,2000}) + X'_{ct}\gamma + \theta_c + \lambda_t + \eta_{ct} \tag{A.1}$$

Y_{ct} is an opioid type-specific overdose death rate in county c and year t. $\text{BLMeasure}_{c,2000}$ is either the oxycodone share or the per-capita PO supply in 2000, and is interacted with a full set of year fixed effects δ_t. θ_c and λ_t are county and year fixed effects; X'_{ct} is the full set of covariates, in levels, included in the IV model. The year effects trace out the difference over time in illicit and prescription opioid death rates between counties with relatively higher and lower levels of the initial supply measures. The estimation sample spans 2010-18 and is restricted to the set of counties used for the foster care analysis. I set the 2010 coefficient to zero, so that differences in opioid death rates are relative to the difference in 2010, and show the results graphically.

In estimating the effect of state PDMPs on opioid death rates, I follow a recent literature that distinguishes between voluntary (or operational) PDMPs and mandatory-access PDMPs (Buchmueller and Carey 2018; Grecu et al. 2019), and focus on measuring the effect of mandatory-access PDMPs. The specification interacts a state-year-specific mandatory PDMP policy indicator with a county baseline supply measure, and also includes as an independent variable an operational PDMP policy indicator interacted with the baseline supply measure:

$$Y_{ct} = \alpha_1(\text{PDMPMandate}_{st} \times \text{BLMeasure}_{c,2000}) + \alpha_2(\text{PDMPOperational}_{st} \times \text{BLMeasure}_{c,2000})$$
$$+ X'_{ct}\gamma + \theta_c + \lambda_t + \epsilon_{ct} \tag{A.2}$$

PDMPMandate_{st} is an indicator for whether a mandatory-access PDMP was in place in state s in year t, and $\text{PDMPOperational}_{st}$ is an analogous indicator for the presence of a voluntary PDMP. To be consistent with the first stage, the estimation sample is limited to years 2010-2018 and the set of identified counties used in the foster care analysis. α_1 measures the change in the average opioid death rate before and after a mandatory PDMP is implemented, differenced between states that did and did not implement a mandatory PDMP during the sample period[2], and allows the effect to differ across counties within a state depending on the level of the baseline exposure measure. To control for preexisting trends in opioid deaths in counties with different levels of the initial exposure measure, I additionally show estimates from a version of this specification that includes a county-specific linear trend term. In the estimation of equations (A.1) and (A.2), standard errors are clustered at the county level, and each observation is weighted by the county population ages 18-57 in that year.

OxyContin reformulation event study results. The year effect estimates for the illicit opioid death rate, estimated according to equation (A.1), are plotted in Fig A.1; the red lines plot the year effect coefficients for each initial PO supply measure from an event study

[2]Only two states, Nevada and Louisiana, passed must-access PDMP laws prior to 2010; Georgia and Indiana passed must-access PDMP laws after the first half of 2018 and are coded as not having a mandatory PDMP during the sample period. The sample period of 2010-2018 captures the majority of mandatory PDMP implementations.

specification that includes both measures–i.e., year effects interacted with the oxycodone share and year effects interacted with PO density. Consistent with the first-stage estimates for the post-2010 change in the illicit opioid death rate, the black lines in both panels indicate that counties with relatively higher initial oxycodone share or PO density had larger increases in the illicit opioid death rate, and this difference grew rapidly between 2010 and 2016. When both measures are included in the event study regressions, effect magnitudes are reduced for each initial exposure measure, but the fact that the coefficients for 2014 and later remain significant indicates that both exposure measures are explaining cross-county variation in the post-2010 increase in illicit opioid deaths. As suggested by (Alpert et al. 2018), places with higher rates of oxycodone misuse (proxied here by the supply share of oxycodone) prior to the reformulation are likely to exhibit stronger consumer substitution toward illicit opioids in response to a sudden and substantial reduction in the supply of abusable oxycodone. To the extent that overall PO density contains a signal about oxycodone density, places with higher initial PO density should similarly see larger increases in illicit opioid deaths.

Year effect estimates for the PO-only death rate are plotted in Fig A.2. The right panel shows the coefficients on initial PO density: counties with higher initial PO density have a lower PO-only death rate on average after 2011, and this difference increases over time. This is consistent with greater substitution away from PO misuse in initially more opioid-dense counties after the reformulation reduced the supply of oxycodone. The left panel of this figure plots the year effect coefficients on the initial oxycodone share. Comparing the black line (corresponding to a specification where only the initial oxycodone share is interacted with year indicators) to the red line (corresponding to a specification where initial oxycodone share and initial PO density are each interacted with year indicators), counties with higher oxycodone share have a significantly higher PO-only death rate on average after 2012 once we control for initial PO density. This implies that the post-2010 reduction in PO-only deaths in higher-PO density counties observed in the right panel are concentrated in high-PO density but low-oxycodone share counties.

PDMP difference-in-difference results. Table A.2 presents DiD estimates from equation (A.2) of the average effect of PDMPs on opioid death rates, allowing the effects to differ by initial oxycodone share (top panel) or initial PO density (bottom panel). Illicit opioid death rate is the outcome in columns 3 and 4. In column 3, illicit opioid deaths increased more in counties with higher initial oxycodone share after either a mandatory or a voluntary PDMP took effect, in treated relative to comparison states. PDMPs have been shown to reduce the prescribing of opioids (Alpert et al. 2020; Sacks et al. 2021), which should reduce the availability of substitute prescription opioids in this post-oxycodone supply shock era; counties with greater reliance on oxycodone within a PDMP state experienced a relatively larger reduction in the supply of abusable POs as a result of the reformulation, and therefore could exhibit stronger substitution to illicit opioids when the supply of non-oxycodone POs is further reduced by a PDMP. Similarly, counties with higher initial PO density saw larger increases in illicit opioid deaths after a PDMP took effect. Although the inclusion of a county-specific linear trend in column 4 reverses coefficient signs, caution is warranted in interpreting these estimates: given that illicit opioid deaths were rapidly increasing–and doing so differentially across counties–during the sample period, a county-specific linear trend may reflect unmodeled dynamics in the dependent variable when a single policy indicator (interacted with an exposure measure) is used to estimate the average difference in illicit opioid death rates across counties with differing levels of policy exposure after PDMP implementation in treated relative to comparison states.

Columns 1 and 2 in Table A.2 show the DiD estimates for the PO-only death rate. The effect of operational PDMPs on PO-only deaths did not differ by either initial exposure measure. The bottom panel of Column 1 suggests that mandatory PDMPs reduced the PO-only death rate more in counties with higher initial PO density, but this effect disappears once a county-specific linear trend is added. The county linear trend also reverses the sign of the coefficient on the interaction between mandatory PDMPs and the initial oxycodone share. Although the direction of the effects of PDMPs on opioid overdose deaths are less conclusive than the OxyContin reformulation results, the main takeaway of this exercise is that the effect of PDMPs on both illicit opioid and PO-only death rates differs across counties based on both initial PO supply measures, consistent with the idea that these measures reflect exposure to supply-side opioid policies.

A.3 Foster care entry by reasons for removal

The AFCARS case-level data provide reasons and conditions associated with the decision to remove the child from home; a record may have multiple or no reasons for removal. Consistent with the fact that child neglect is the most common form of child maltreatment (Berger and Slack 2020), neglect is cited in 60% of foster care entries, making it the most commonly cited reason for removal. Parental drug use is also prevalent, being cited in 31% of entries. Meinhofer and Angleró-Díaz (2019) calculate that the number and proportion of entries citing parental drug use steadily increased from 39,130 (14.5% of total removals) in 2000 to 96,672 (36.3% of total removals) in 2017, and also note that this trend has coincided with trends in increased opioid use and overdose deaths. While consistent with the main 2SLS estimates of the relationship between total entries and illicit opioid death, the rise in entries involving parental drug use likely reflects both improved reporting reliability and a true increase in admissions involving parental drug use. Ghertner et al. (2018) note that historically, child welfare caseworkers have not always reliably reported parental substance use as a reason for removal even when it is present, and that counties differed in reporting practices. Efforts undertaken by the Administration for Children and Families and state agencies to improve reporting reliability may explain part of the dramatic increase in admissions citing parental drug use. Given these reporting issues (which potentially apply to other reasons for removal as well), I consider total admissions as the main measure of entries into foster care. Equation (1.3) is estimated by 2SLS for entry by reason for removal in order to explore the types of maltreatment, if any, that are driving the effect of parental illicit opioid use on total entry.

Appendix Table A.5 shows the 2SLS estimates for four categories of reasons associated with entry: neglect, physical abuse, parental substance abuse (includes both drug abuse and alcohol abuse), and parental incarceration/death. All columns include the full set of long-differenced covariates as well as their initial levels in 2000. In columns 2 and 4, I find no significant effects of either type of parental opioid abuse on entries citing physical abuse or parental incarceration/death. In column 3, the change in illicit opioid deaths is negatively related to the change in entry citing parental substance abuse, which results from a strong negative relationship between the initial oxycodone share and changes in entry citing parental substance abuse (see reduced-form estimates in Appendix Table A.6). I also re-estimate the IV model for entries due to parental substance abuse, dropping the counties of one state at a time from the sample. The 2SLS coefficient on ΔIllicit op mort is plotted in Appendix Fig A.8, which shows that while estimates fluctuate slightly when large states such as California and Illinois are excluded, the negative IV estimate is not driven by any one state. A potential explanation for the negative reduced-form estimates is if the initial oxycodone share–and thus the post-2010 growth in illicit opioid use and overdose deaths–is systematically related to the reliability of reports of parental substance abuse associated with the decision to remove a child from home.

In the first column across all three panels, the estimated coefficients on the change in the PO-only death rate indicate a consistently negative relationship between changes in parental PO misuse and foster care entry. Mechanically, this is a combination of signs of coefficients on the instruments in the reduced-form and signs on the first stage coefficients. When I estimate the reduced-form regression of the change in entry citing neglect on the instruments and the full set of controls (see column 6 of Appendix Table A.7), I find that initial PO density is positively related to changes in entry due to neglect, while initial oxycodone share is negatively associated with the change in entry due to neglect. In addition, recall that in the first stage, higher initial PO density predicts larger reductions in PO-only deaths after 2010, while higher initial oxycodone share predicts larger increases in PO-only deaths. While the 2SLS estimate for the effect of parental PO misuse cannot be obtained as the reduced-form scaled by the first-stage in this multiple endogenous regressors model, we can still use this intuition to see that signs of the RF coefficients and first-stage coefficients result in a negative 2SLS coefficient on PO-only deaths. As before, I also re-estimate the IV model for entries citing neglect dropping one state at a time from the sample. The 2SLS coefficient on the change in PO-only deaths from this exercise is plotted in Appendix Fig A.9. The magnitude of the negative estimate is reduced when New York state is excluded from the estimation sample, but the negative IV estimate does not appear to be driven by the inclusion of any one state.

A.4 Robustness and placebo tests

A.4.1 Zero-first-stage test: triplicate states sample

I implement the zero-first-stage test in a sample of states that, due to a prescription policy implemented decades prior to the launch of OxyContin, adopted OxyContin at much lower rates and were initially less exposed to oxycodone prescriptions than states without the policy. When Purdue Pharma launched OxyContin in 1996, California, Idaho, Illinois, New York, and Texas had active triplicate prescription programs,[3] which were laws mandating the use of triplicate prescription forms for prescribing Schedule II controlled substances,[4] a DEA

[3]These five states adopted triplicate prescription programs between 1939 and 1982.

[4]One copy of the prescription was kept by the physician, and the two others were to be given to the pharmacy that fills the prescription, which turns one copy over to the state drug monitoring agency.

classification that includes oxycodone and many other opioids. Alpert et al. (2019) show that this arguably exogenous geographic variation in initial exposure to OxyContin[5] led to significantly higher prescription opioid deaths in non-triplicate states until the OxyContin reformulation in 2010, and after 2010, higher illicit opioid deaths in non-triplicate states. This finding suggests that within the triplicate states, lower OxyContin exposure across the board and the initial deterrence effect of triplicate programs should break the correlation between initial OxyContin exposure and subsequent paths of PO and illicit opioid deaths. Initial OxyContin exposure maps well onto the main instruments in my IV model, since $\text{OxyShare}_{c,2000}$ measures the initial disproportionate prescribing of oxycodone, and $\text{RxOpSupply}_{c,2000}$ proxies the initial per-capital PO supply, which is partly driven by the supply of oxycodone. In other words, among counties in triplicate states, we would expect a weak first-stage relationship between the initial exposure measures and post-2010 changes in opioid deaths, providing a plausible sample for the zero-first-stage test.

I first compare the distribution of each initial exposure measure in triplicate and non-triplicate states in Appendix Figure A.5. The triplicate sample has 214 counties, and the non-triplicate sample has 979 counties. Triplicate states (in red) indeed had lower oxycodone share and per-capita PO supply in 2000 than non-triplicate states (in grey). In the left panel, the median oxycodone share in the non-triplicate distribution is above the 90th percentile of the triplicate distribution. In the right panel, the median PO density in non-triplicate

[5]Alpert et al. (2019) argue that triplicate states were less exposed to the introduction of OxyContin for two reasons. Because of lower expected returns to marketing efforts in triplicate states, Purdue Pharma targeted its marketing and promotion campaigns for the drug in non-triplicate states. Additionally, the triplicate programs on their own hindered the take-up of OxyContin through the salience of government scrutiny and increased hassle costs for physicians.

states was 109 mg/resident, compared with 85 mg/resident in triplicate states. There is still a substantial amount of variation across counties in triplicate states in the two initial supply measures. The standard deviation of initial oxycodone share in the triplicate distribution is 0.11, not much smaller than the corresponding SD of 0.14 in the non-triplicate distribution. The standard deviation of initial PO density in the triplicate distribution is 41.6 mg/resident, compared with 58.3 mg/resident in the non-triplicate distribution. This observation helps to rule out the possibility that zero first-stage and RF estimates in the triplicate sample are simply due to a lack of cross-county variation in the instruments.

Appendix Table A.12 compares the first-stage estimates for the post-2010 change in the illicit opioid death rate in triplicate and non-triplicate samples in columns 1 and 2, and similarly for the post-2010 change in the PO-only death rate in columns 3 and 4. Estimates in columns 1 and 3 verify that the first stage is indeed zero in the triplicate sample. Comparing across columns 1 and 2, the positive relationship between initial oxycodone share and growth in illicit opioid deaths after 2010 is 7 to 22 times larger and highly significant in the non-triplicate states. The magnitude of the positive coefficient on initial PO density is 2 to 8 times larger in the non-triplicate states. Comparing across columns 3 and 4, the first stage estimates for the change in PO-only deaths are also zero in the triplicate sample, whereas both initial exposure measures are significantly related to the change in PO-only deaths in the non-triplicate sample.

Having verified a zero first stage in triplicate states, we now check the reduced-form estimates in this sample. If the initial exposure measures are related to post-2010 changes in foster care outcomes only through changes in illicit opioid use and PO misuse, the RF estimates in the triplicate sample should be zero since the first-stage relationship is zero. Appendix Tables A.13 and A.14 show the RF estimates in the triplicate, non-triplicate, and full samples for foster care entry and caseload. Column 1 of Appendix Table A.13 shows that across the three long difference intervals, the RF entry estimates in triplicate states is not different from zero. In contrast, column 2 shows a significant and positive reduced-form relationship in non-triplicate states between at least one of the two initial exposure measures and the post-2010 change in foster care admissions. The non-triplicate and full sample RF estimates are very similar, indicating that the IV estimates are driven by cross-county variation in initial oxycodone and PO exposure within non-triplicate states that led to different time paths of illicit opioid and PO deaths and, consequently, different trends in foster care entry. Appendix Table A.14 tells a similar story for foster care caseload, although a significant RF relationship in the non-triplicate and full samples is found only with the initial oxycodone share.

Passing the zero-first-stage test using the triplicate sample bolsters confidence in instrument exogeneity by showing that in at least one instance where a first stage does not exist, we do not observe a direct relationship between the instruments and foster care outcomes. In addition to testing for a potential violation of the exclusion restriction, these results also reveal that the estimated increases in foster care entry and caseload due to an increase in parental illicit opioid use after 2010 are occurring in non-triplicate states. Consistent with this, the average change in foster care entry between 2010-11 and 2017-18 in non-triplicate states was an increase of 3.9%, compared with a 5.9% decline in triplicate states. As in Alpert et al. (2019), this heterogeneity highlights the importance of initial conditions for present-day geographic variation in foster care trends, and in particular the persistent protective effect of a supply-side policy that reduced initial exposure to prescription opioids.

A.4.2 Robustness checks

I assess the robustness of the main IV estimates to (1) using the foster care entry or caseload rate per 1,000 children as the dependent variable instead of IHS entry or caseload, (2) using the long difference of annual levels instead of two-year averaged levels, (3) alternative sets of instruments, (4) omitting the counties of one state at a time from the analysis sample, and (5) not weighting the observations by the 2018 county child population.

1. Instead of Δ IHS of foster care entry or caseload count, I use Δ entry or caseload rate per 1,000 children as the dependent variable of the IV model. Results are shown in Appendix Tables A.20 and A.21. Similar to the main results, the illicit opioid death coefficients in all three panels of Appendix Table A.20 indicate that parental illicit opioid use significantly increased the foster care entry rate. Column 6 in the third panel indicates that between 2010-11 and 2017-18, moving from the 10th to the 90th percentile of the change in the illicit opioid death rate is associated with a change in the foster care entry rate that is larger by 2.2 admissions per 1,000 children. The 10th-to-90th percentile move is associated with a change in the caseload rate that is larger by 7.2 cases per 1,000 children. The magnitudes of these increases are similar to the increase in entry and caseload rates implied by the main IV estimates (where the

outcome variable approximates the percent change in entry/caseload) for the county with median entry/caseload rate in 2010.

2. The main IV model is specified in long differences between the average over 2010-11 and two-year averages over consecutive years at the end of the sample period, in order to reduce the amount of uncertainty in the realization of the effect of ongoing parental opioid abuse on entry into foster care. To assess robustness to how the long difference is defined, I estimate entry and caseload IV models where the long difference is defined using annual levels relative to 2010 (2010-2016, 2017, and 2018) or to 2011 (2011-2016, 2017, and 2018). The results in Appendix Table A.22 indicate that using annual levels relative to 2010 yields similar IV estimates as using two-year averages, and the IV estimates are similar across the inclusion of controls. Appendix Table A.23 shows that using annual levels relative to 2011, the estimated effect of illicit opioid use is only positive and statistically significant when the long difference ends in 2016. Estimates are close to zero using the 2011-17 and 2011-18 difference. While this suggests that the IV estimates are somewhat dependent on the exact specification of the long difference, the preponderance of the evidence is still consistent with the conclusion that parental illicit opioid use significantly increased foster care admissions after 2010. The caseload estimates are shown in Appendix Tables A.24 and A.25; the estimated effects of illicit opioid use on caseload are not sensitive to using annual levels in the long difference.

3. To check that the estimates are not being driven by reporting peculiarities in the 2000 ARCOS opioid retail supply data used for the main instruments, I estimate the IV

models for foster care entry and caseload using (i) the same instruments constructed using 2001 data, and (ii) the broader set of opioids reported in 2001 to measure the per-capita total supply of prescription opioids, instead of only oxycodone and hydrocodone. As an alternative measure of a county's exposure to oxycodone prescriptions, I also use (iii) the per-capita supply of oxycodone in lieu of the oxycodone share in 2000. Results are reported in Appendix Tables A.26 and A.27. For comparison, the first column of each table replicates the main estimates, which use the oxycodone share and per-capita supply of oxycodone plus hydrocodone in 2000 as instruments. In models in which the instruments are measures from the year 2001 (columns 2 and 3), initial levels of the controls are also from 2001. IV estimates for entry and caseload using the three alternative instrument sets are quite similar to the main estimates, indicating that the effects of parental illicit opioid use and PO misuse on the foster care system are robust to different ways of measuring initial prescription opioid density and oxycodone exposure.

4. I show that estimates are not sensitive to the set of states included in the sample by omitting the counties of one state at a time from the sample and re-estimating the IV models. These estimates are plotted in Appendix Figures A.6 and A.7. Each point is a 2SLS estimate (using the 2010/11-2017/18 LD) from a sample excluding the state indicated on the x-axis; dashed lines represent the distortion-adjusted 95% CIs, and the color red indicates the IV coefficient and 95% CIs using the full sample. IV estimates of the effect of parental illicit opioid use on foster care entry are stable when any one

state is omitted, with only small fluctuations around the full-sample estimate. The effect of illicit opioid use on caseload displays somewhat larger fluctuations, but the 95% CIs when any one state is omitted still exclude zero. The effect on caseload is reduced when Texas is excluded, which seems reasonable given that Texas accounts for the largest share of counties in the sample (84 counties or 7% of the sample).

5. The main IV regressions are weighted by the county child population in 2018 to correct for potential heteroskedasticity related to population size in the county error term. Appendix Tables A.28 and A.29 present the IV entry and caseload estimates where every observation is given equal weight. Unweighted IV estimates of the effect of parental illicit opioid use on entry and caseload are slightly smaller, but still positive and significantly different from zero. The stability of the Δ Illicit opioid death rate coefficient across the inclusion of controls remains unchanged.

A.5 Additional Figures

Figure A.1: OxyContin reformulation event study estimates

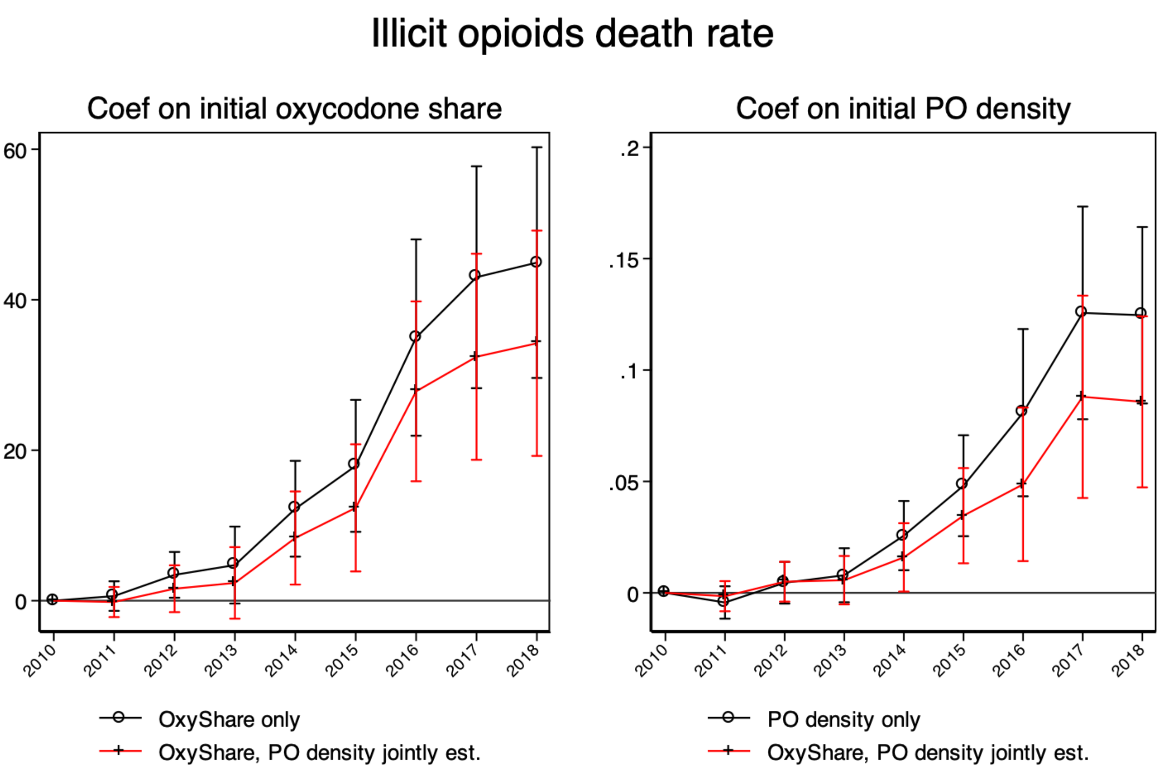


Figure A.2: OxyContin reformulation event study estimates

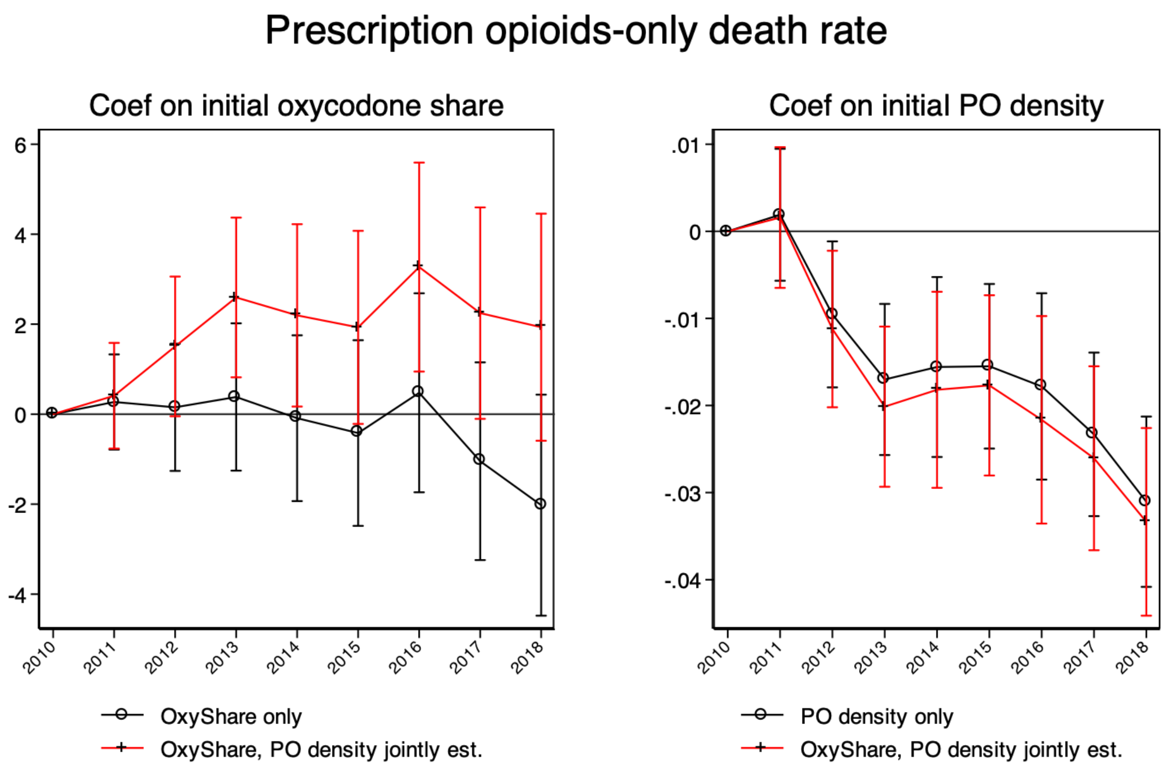
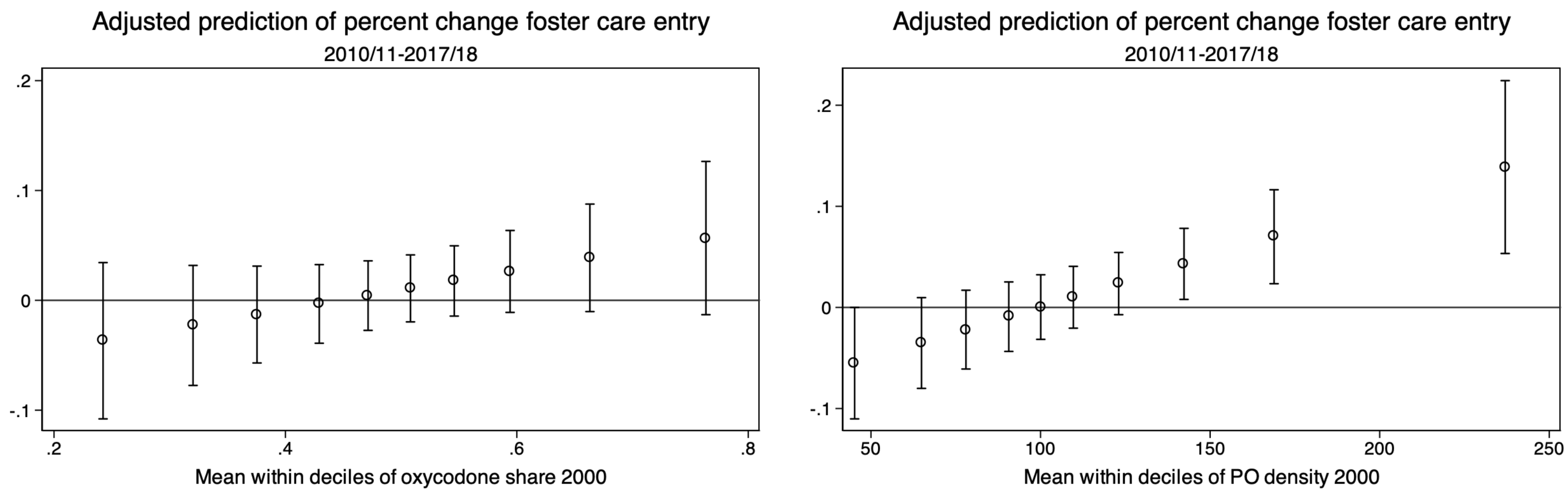
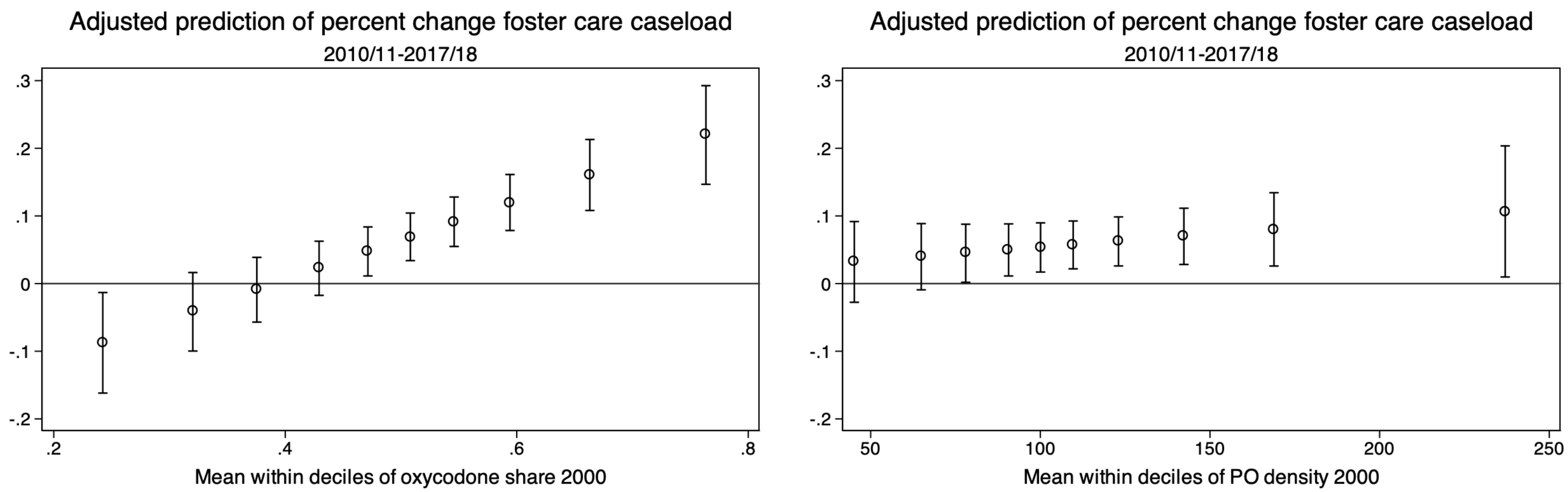


Figure A.3: Reduced-form estimates: marginal effects of initial exposure measures on percent change FC entry



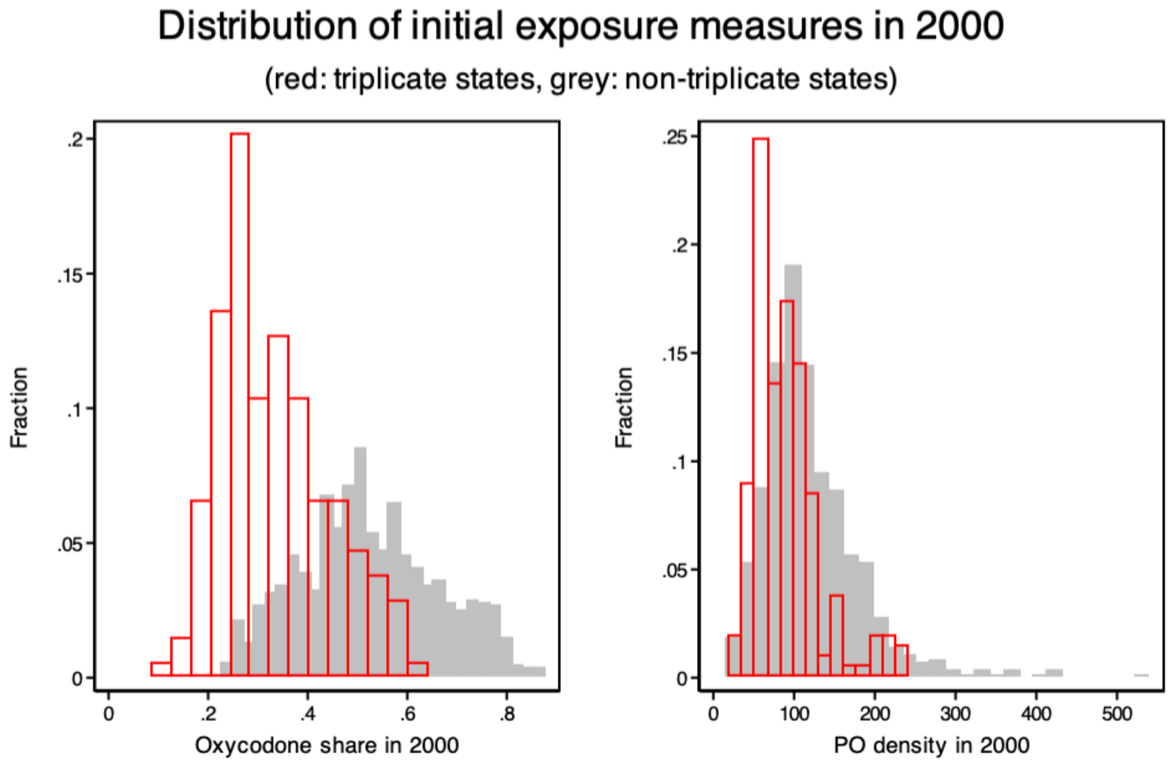
x-axis is the mean value within each decile of oxycodone share 2000 (L) or PO density 2000 (R). Each point is the averaged predicted response, over all observations in the sample, of the percent change in foster care entry when the initial exposure measure is held constant at the mean within-decile value and all other covariates take their actual values. Coefficients used in the prediction are from the reduced-form model regressing ΔIHS Entry on the initial exposure measures and the full set of controls. Standard errors are estimated using the delta method; bars are the 95% confidence intervals.

Figure A.4: Reduced-form estimates: marginal effects of initial exposure measures on percent change FC caseload



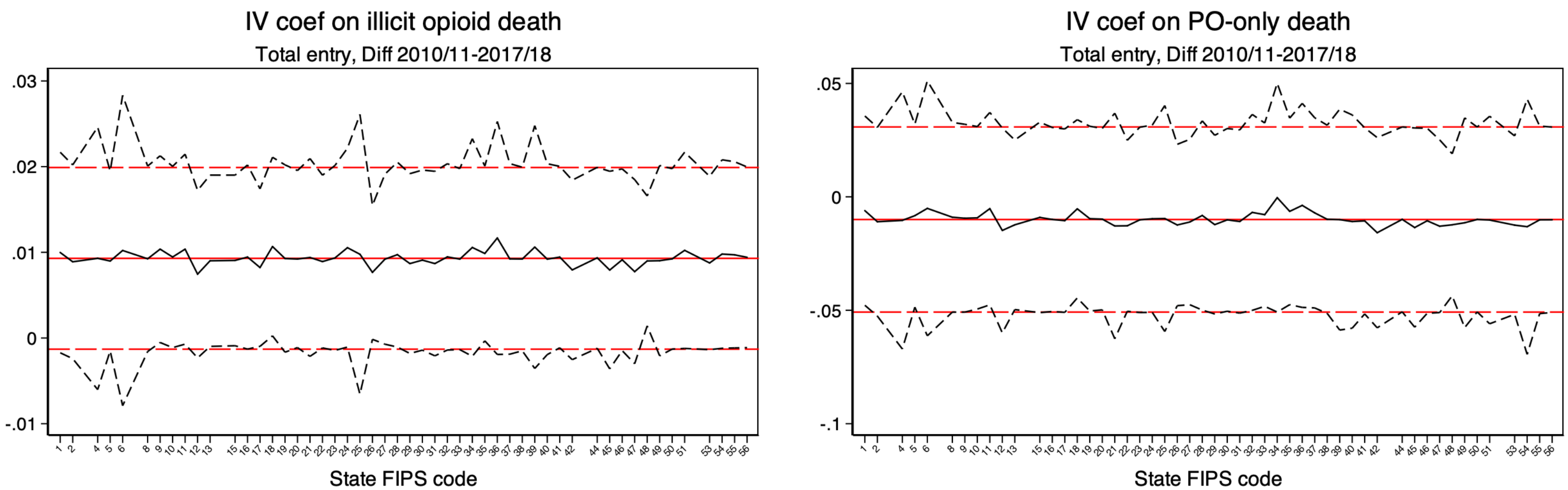
x-axis is the mean value within each decile of oxycodone share 2000 (L) or PO density 2000 (R). Each point is the averaged predicted response, over all observations in the sample, of the percent change in foster care caseload when the initial exposure measure is held constant at the mean within-decile value and all other covariates take their actual values. Coefficients used in the prediction are from the reduced-form model regressing ΔIHS Caseload on the initial exposure measures and the full set of controls. Standard errors are estimated using the delta method; bars are the 95% confidence intervals.

Figure A.5: Variation in initial exposure measures in triplicate and non-triplicate states



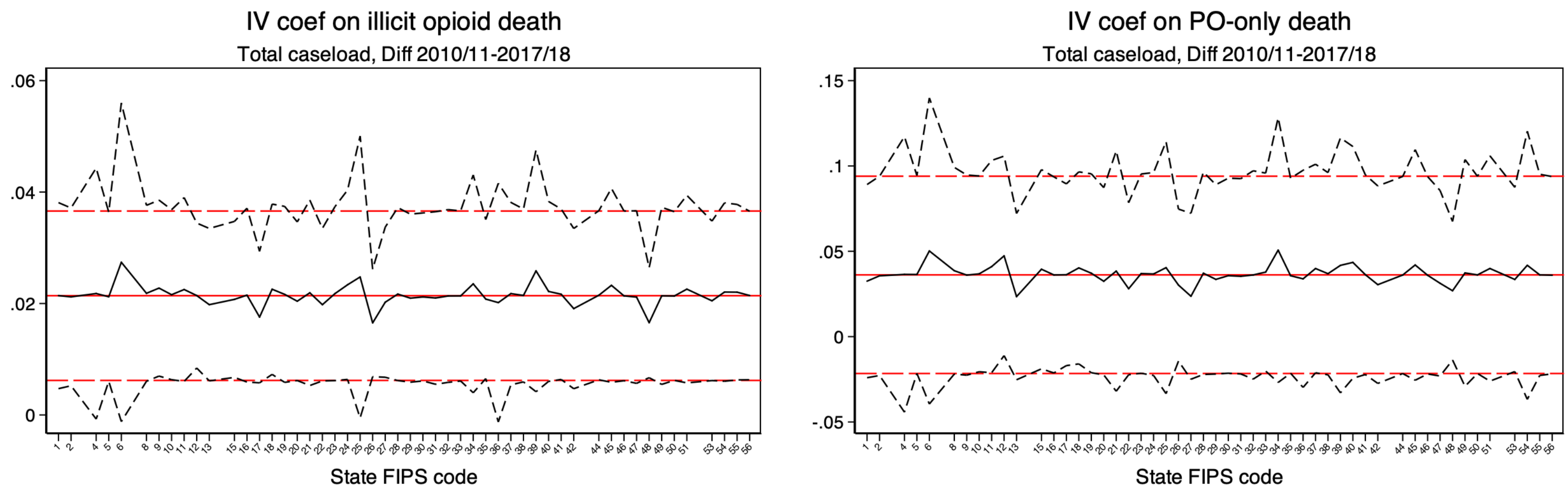
Triplicate states: CA, ID, IL, NY, TX. Triplicate sample has 214 counties; non-triplicate sample has 979 counties.

Figure A.6: Sensitivity to excluding a single state from estimation sample, foster care entry



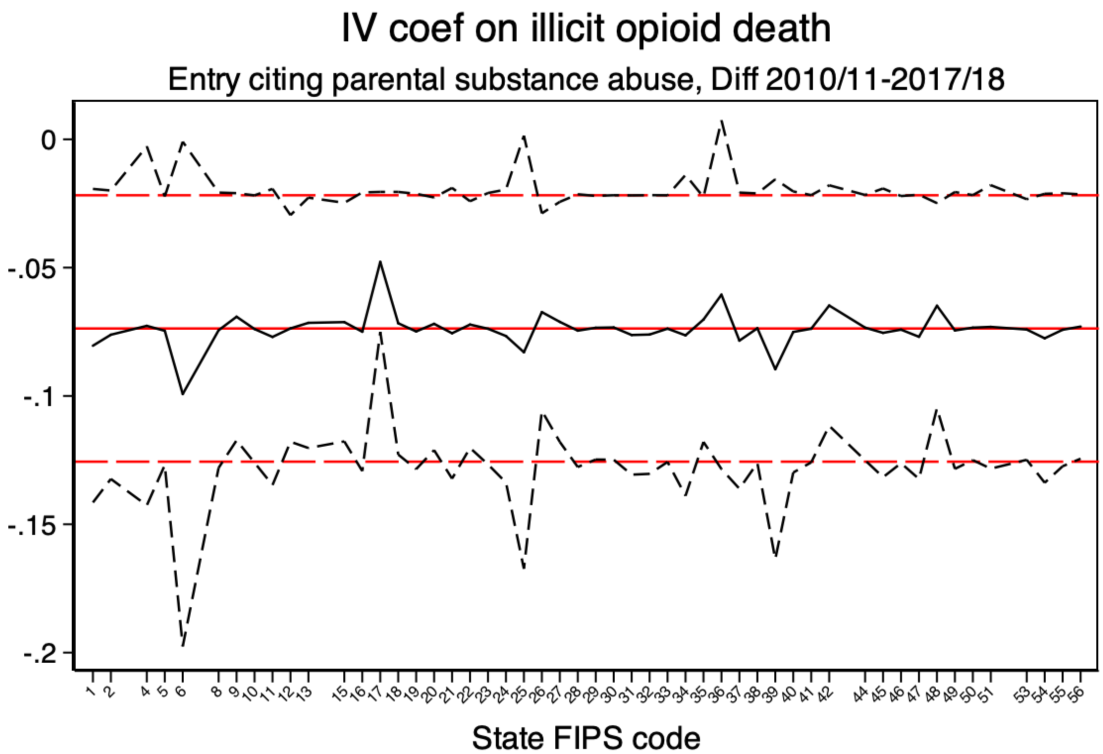
This figure shows the sensitivity of the IV coefficient on ΔIllicit opioid death rate and ΔPO-only death rate for the total entry model including all covariates to dropping the counties of one state at a time from the estimation sample. The dependent variable is the 2010/11-2017/18 change in IHS entry. Red solid line is the actual IV coefficient, and red dashed lines are the corresponding tF 95% CI. Black solid line is the IV coefficient when the indicated state on the x-axis is dropped from the sample, and black dashed lines are the tF 95% CI.

Figure A.7: IV estimates for foster care caseload, dropping one state at a time from estimation sample



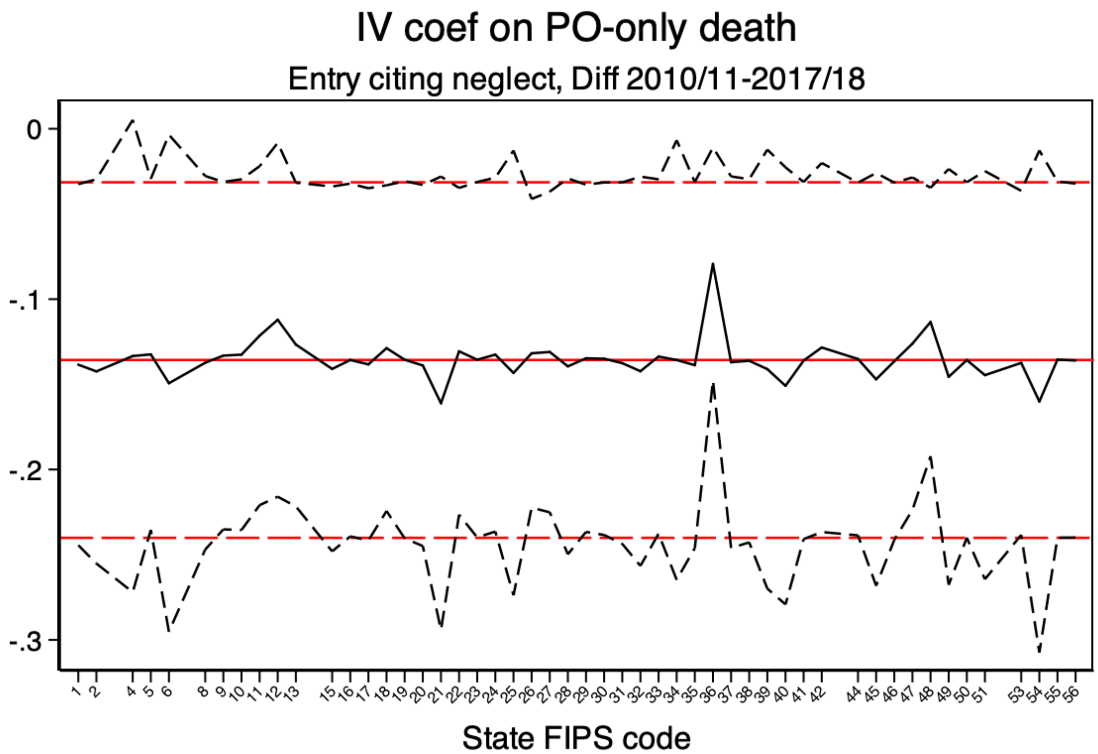
This figure shows the sensitivity of the IV coefficient on ΔIllicit opioid death rate and ΔPO-only death rate for the total caseload model including all covariates to dropping the counties of one state at a time from the estimation sample. The dependent variable is the 2010/11-2017/18 change in IHS caseload. Red solid line is the actual IV coefficient, and red dashed lines are the corresponding tF 95% CI. Black solid line is the IV coefficient when the indicated state on the x-axis is dropped from the sample, and black dashed lines are the tF 95% CI.

Figure A.8: Sensitivity to excluding a single state from sample, entry citing parental substance abuse



This figure shows the sensitivity of the IV coefficient on ΔIllicit opioid death rate for the entry citing parental substance abuse model including all covariates to dropping one state at a time from the estimation sample. The dependent variable is the 2010/11-2017/18 change in IHS entry citing parental substance abuse. Black solid line is the IV coefficient when the indicated state on the x-axis is dropped from the sample, and black dashed lines are the tF 95% CI.

Figure A.9: Sensitivity to excluding a single state from sample, entry citing neglect



This figure shows the sensitivity of the IV coefficient on ΔPO-only death rate for the entry citing neglect model including all covariates to dropping one state at a time from the estimation sample. The dependent variable is the 2010/11-2017/18 change in IHS entry citing neglect. Black solid line is the IV coefficient when the indicated state on the x-axis is dropped from the sample, and black dashed lines are the tF 95% CI.

A.6 Additional Tables

Table A.1: OLS estimates of the relationship between opioid overdose rates and foster care admissions, consistent sample

Estimation Method	Illicit opioid OD death rate	PO-only OD death rate	N
County fixed effects	0.0002	-0.0038*	3,579
	(0.0007)	(0.0020)	
First differences	0.0005	-0.0014	3,579
	(0.0006)	(0.0014)	
2-year Long differences	0.0007	-0.0009	3,579
	(0.0006)	(0.0017)	
3-year Long differences	0.0008	-0.0004	3,579
	(0.0007)	(0.0019)	
4-year Long differences	0.0010	-0.0008	3,579
	(0.0007)	(0.0019)	
5-year Long differences	0.0011	-0.0002	2,386
	(0.0007)	(0.0020)	
6-year Long differences	0.0015*	-0.0008	1,193
	(0.0008)	(0.0023)	

Sample is observations only from 2016-18. Outcome variable is IHS foster care admissions count. Levels of all variables are averages over two consecutive years, resulting in smaller samples sizes for the 5-year and 6-year long difference specifications. All regressions are weighted by county population under age 18. Standard errors clustered at county level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.2: DiD estimates of effects of PDMPs on opioid overdose death rates

	(1) Rx op. only	(2) Rx op. only	(3) Illicit op.	(4) Illicit op.
Baseline measure: oxycodone share in 2000				
PDMP Mandatory $\times$OxyShare$_c$	2.8966*	-3.2108*	21.4534***	-7.8714
	(1.5805)	(1.9328)	(6.0296)	(6.8862)
PDMP Operational $\times$OxyShare$_c$	-0.8784	5.1077	48.7836***	-36.3085**
	(2.1568)	(3.4235)	(14.1251)	(15.6818)
Covariates	y	y	y	y
County linear trend	n	y	n	y
R^2	0.648	0.765	0.704	0.890
Baseline measure: PO density in 2000				
PDMP Mandatory $\times$PODensity$_c$	-0.0254***	-0.0033	0.0876***	-0.0318*
	(0.0085)	(0.0075)	(0.0232)	(0.0181)
PDMP Operational $\times$PODensity$_c$	-0.0036	0.0166	0.1251**	-0.1255**
	(0.0087)	(0.0135)	(0.0628)	(0.0588)
Covariates	y	y	y	y
County linear trend	n	y	n	y
R^2	0.652	0.765	0.703	0.890
Dep. var. mean	4.155	4.155	10.564	10.564
N	11,898	11,898	11,898	11,898

Data span years 2010-18. All columns include county and year fixed effects. All regressions are weighted by the county's population ages 18-57. Standard errors clustered at county level. * p<0.10, ** p<0.05, *** p<0.01

Table A.3: Relationship between 2010/11-2015/16 change in opioid death rates and each baseline exposure measure

	(1) Δ Illicit op mort	(2) Δ Illicit op mort	(3) Δ Illicit op mort	(4) Δ PO-only mort	(5) Δ PO-only mort	(6) Δ PO-only mort
OxyShare 2000	18.0386***		14.9441***	2.2391**		4.3662***
	(4.5017)		(4.3187)	(0.9913)		(1.0811)
PO density 2000		0.0484***	0.0315***		-0.0167***	-0.0216***
		(0.0125)	(0.0118)		(0.0043)	(0.0047)
Dep. var. mean	9.567	9.567	9.567	-0.611	-0.611	-0.611
R^2	0.394	0.384	0.401	0.152	0.168	0.183

N=1,193 counties. All regressions are weighted by the county's 2018 population under age 18, and include the full set of controls in long differences relative to 2010-11 average and their initial levels in 2000. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.4: Relationship between 2010/11-2017/18 change in opioid death rates and each baseline exposure measure

	(1) Δ Illicit op mort	(2) Δ Illicit op mort	(3) Δ Illicit op mort	(4) Δ PO-only mort	(5) Δ PO-only mort	(6) Δ PO-only mort
OxyShare 2000	29.8888***		22.5237***	-0.3196		2.9645**
	(5.9919)		(6.1842)	(1.1830)		(1.2348)
PO density 2000		0.0976***	0.0736***		-0.0296***	-0.0328***
		(0.0186)	(0.0197)		(0.0051)	(0.0054)
Dep. var. mean	15.971	15.971	15.971	-1.473	-1.473	-1.473
R^2	0.459	0.459	0.475	0.126	0.179	0.184

N=1,193 counties. All regressions are weighted by the county's 2018 population under age 18, and include the full set of controls in long differences relative to 2010-11 average and their initial levels in 2000. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.5: IV estimates for foster care entry, by reasons for removal

	(1) Neglect	(2) Physical abuse	(3) Parental substance abuse	(4) Parental incarceration/death
LD 2010/11-2015/16				
Δ Illicit op mort	-0.0071	-0.0050	-0.0738***	-0.0244
	(0.0097)	(0.0137)	(0.0239)	(0.0169)
tF 95% CI	(-.0332, .0191)	(-.0422, .0322)	(-.1387, -.0089)	(-.0702, .0215)
Δ PO-only mort	-0.0702**	0.0362	-0.0660	0.0706
	(0.0305)	(0.0401)	(0.0451)	(0.0495)
tF 95% CI	(-.1442, .0039)	(-.0613, .1336)	(-.1755, .0435)	(-.0497, .1909)
Dep. var. mean	0.249	-0.074	0.363	0.122
Cragg-Donald F	23.902	23.902	23.902	23.902
Illicit op mort SW F	17.823	17.823	17.823	17.823
PO-only mort SW F	25.798	25.798	25.798	25.798
LD 2010/11-2016/17				
Δ Illicit op mort	-0.0173*	-0.0146	-0.0700***	-0.0236*
	(0.0089)	(0.0134)	(0.0240)	(0.0140)
tF 95% CI	(-.0422, .0077)	(-.0521, .0229)	(-.1372, -.0028)	(-.0629, .0157)
Δ PO-only mort	-0.0960***	-0.0236	-0.1200**	0.0058
	(0.0304)	(0.0421)	(0.0528)	(0.0461)
tF 95% CI	(-.1688, -.0233)	(-.1241, .077)	(-.2463, .0062)	(-.1043, .116)
Dep. var. mean	0.262	-0.117	0.397	0.101
Cragg-Donald F	21.371	21.371	21.371	21.371
Illicit op mort SW F	15.995	15.995	15.995	15.995
PO-only mort SW F	28.025	28.025	28.025	28.025
LD 2010/11-2017/18				
Δ Illicit op mort	-0.0311***	-0.0205	-0.0737***	-0.0196
	(0.0106)	(0.0141)	(0.0202)	(0.0132)
tF 95% CI	(-.0584, -.0038)	(-.0566, .0157)	(-.1256, -.0218)	(-.0535, .0144)
Δ PO-only mort	-0.1357***	-0.0614	-0.1283**	-0.0101
	(0.0411)	(0.0483)	(0.0622)	(0.0464)
tF 95% CI	(-.2401, -.0314)	(-.1842, .0613)	(-.2864, .0299)	(-.1281, .1079)
Dep. var. mean	0.246	-0.148	0.409	0.068
Cragg-Donald F	18.826	18.826	18.826	18.826
Illicit op mort SW F	20.845	20.845	20.845	20.845
PO-only mort SW F	21.993	21.993	21.993	21.993

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18; all columns include the full set of covariates. Parental substance abuse includes parental drug abuse and alcohol abuse. Parental incarceration/death includes "temporary or permanent placement of a parent or caretaker in jail that adversely affects care for the child" and death of a parent. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.6: Reduced-form estimates for foster care entry citing parental substance abuse

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry	(4) Δ IHS Entry	(5) Δ IHS Entry	(6) Δ IHS Entry
LD 2010/11-2015/16						
OxyShare 2000	-1.0076***	-1.0620***	-1.2791***	-1.2688***	-1.3876***	-1.3911***
	(0.2553)	(0.2539)	(0.2438)	(0.2420)	(0.2541)	(0.2548)
PO density 2000	-0.0010	-0.0009	-0.0009	-0.0011	-0.0009	-0.0009
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Dep. var. mean	0.363	0.363	0.363	0.363	0.363	0.363
R^2	0.167	0.177	0.213	0.216	0.221	0.222
LD 2010/11-2016/17						
OxyShare 2000	-1.0909***	-1.2266***	-1.6392***	-1.6282***	-1.8110***	-1.8130***
	(0.2692)	(0.2660)	(0.2778)	(0.2740)	(0.2825)	(0.2830)
PO density 2000	-0.0012	-0.0009	-0.0010	-0.0011	-0.0007	-0.0007
	(0.0009)	(0.0009)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Dep. var. mean	0.397	0.397	0.397	0.397	0.397	0.397
R^2	0.176	0.202	0.248	0.251	0.263	0.263
LD 2010/11-2017/18						
OxyShare 2000	-1.3034***	-1.4533***	-1.9801***	-1.9660***	-2.0388***	-2.0398***
	(0.2990)	(0.2928)	(0.3255)	(0.3197)	(0.2995)	(0.2996)
PO density 2000	-0.0013	-0.0008	-0.0009	-0.0011	-0.0012	-0.0012
	(0.0010)	(0.0009)	(0.0009)	(0.0009)	(0.0010)	(0.0010)
Dep. var. mean	0.409	0.409	0.409	0.409	0.409	0.409
R^2	0.197	0.231	0.266	0.270	0.293	0.293
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Estimates from regressing the change (long difference between average 2010-11 and average 2015-16, 2016-17, and 2017-18) in IHS entry citing parental substance abuse on changes in the illicit opioid death rate, the PO-only death rate, and the same set of covariates as in the IV model. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.7: Reduced-form estimates for foster care entry citing neglect

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry	(4) Δ IHS Entry	(5) Δ IHS Entry	(6) Δ IHS Entry
LD 2010/11-2015/16						
OxyShare 2000	-0.0911	-0.1718	-0.3460*	-0.3426*	-0.4181**	-0.4118**
	(0.1826)	(0.1900)	(0.1886)	(0.1883)	(0.2015)	(0.2013)
PO density 2000	0.0013**	0.0015***	0.0012**	0.0012**	0.0013**	0.0013**
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Dep. var. mean	0.249	0.249	0.249	0.249	0.249	0.249
R^2	0.246	0.266	0.311	0.312	0.315	0.317
LD 2010/11-2016/17						
OxyShare 2000	-0.2563	-0.3672**	-0.6756***	-0.6661***	-0.7569***	-0.7546***
	(0.1718)	(0.1721)	(0.1912)	(0.1891)	(0.1970)	(0.1976)
PO density 2000	0.0014**	0.0018***	0.0015***	0.0015***	0.0017***	0.0017***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Dep. var. mean	0.262	0.262	0.262	0.262	0.262	0.262
R^2	0.239	0.259	0.293	0.296	0.307	0.308
LD 2010/11-2017/18						
OxyShare 2000	-0.4620***	-0.5886***	-1.0040***	-0.9878***	-1.0988***	-1.1029***
	(0.1728)	(0.1714)	(0.2017)	(0.1989)	(0.2074)	(0.2064)
PO density 2000	0.0016**	0.0021***	0.0020***	0.0020***	0.0022***	0.0022***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Dep. var. mean	0.246	0.246	0.246	0.246	0.246	0.246
R^2	0.235	0.255	0.286	0.290	0.305	0.307
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Estimates from regressing the change (long difference between average 2010-11 and average 2015-16, 2016-17, and 2017-18) in IHS entry citing neglect on changes in the illicit opioid death rate, the PO-only death rate, and the same set of covariates as in the IV model. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.8: IV estimates for foster care caseload, average of two endpoint years

	(1) Δ IHS Caseload	(2) Δ IHS Caseload	(3) Δ IHS Caseload	(4) Δ IHS Caseload	(5) Δ IHS Caseload	(6) Δ IHS Caseload
LD 2010/11-2015/16						
Δ Illicit op mort	0.0303***	0.0268***	0.0295***	0.0301***	0.0312***	0.0312***
	(0.0113)	(0.0096)	(0.0098)	(0.0101)	(0.0105)	(0.0104)
tF 95% CI						(.0029, .0594)
Δ PO-only mort	0.0652*	0.0593*	0.0575*	0.0544*	0.0563**	0.0562**
	(0.0368)	(0.0331)	(0.0297)	(0.0290)	(0.0268)	(0.0269)
tF 95% CI						(-.0092, .1216)
Dep. var. mean	0.058	0.058	0.058	0.058	0.058	0.058
Cragg-Donald F	15.023	17.157	20.432	20.799	24.236	23.902
Illicit op mort SW F	19.122	21.778	20.679	20.257	17.608	17.823
PO-only mort SW F	17.537	19.747	22.399	22.307	26.235	25.798
LD 2010/11-2016/17						
Δ Illicit op mort	0.0227***	0.0204***	0.0230***	0.0241***	0.0240***	0.0240***
	(0.0072)	(0.0064)	(0.0075)	(0.0079)	(0.0079)	(0.0078)
tF 95% CI						(.002, .046)
Δ PO-only mort	0.0507*	0.0453*	0.0539**	0.0513**	0.0502**	0.0502**
	(0.0294)	(0.0274)	(0.0260)	(0.0257)	(0.0227)	(0.0227)
tF 95% CI						(-.0041, .1045)
Dep. var. mean	0.064	0.064	0.064	0.064	0.064	0.064
Cragg-Donald F	16.563	18.104	19.024	19.165	21.492	21.371
Illicit op mort SW F	22.700	23.026	18.222	17.768	15.899	15.995
PO-only mort SW F	20.919	22.431	24.478	24.522	28.229	28.025
LD 2010/11-2017/18						
Δ Illicit op mort	0.0190***	0.0170***	0.0206***	0.0215***	0.0214***	0.0214***
	(0.0055)	(0.0052)	(0.0060)	(0.0062)	(0.0059)	(0.0059)
tF 95% CI						(.0062, .0366)
Δ PO-only mort	0.0324	0.0249	0.0373	0.0371	0.0361	0.0362
	(0.0236)	(0.0233)	(0.0250)	(0.0253)	(0.0227)	(0.0228)
tF 95% CI						(-.0216, .094)
Dep. var. mean	0.056	0.056	0.056	0.056	0.056	0.056
Cragg-Donald F	18.022	18.881	15.909	15.569	18.870	18.826
Illicit op mort SW F	25.045	23.735	20.010	19.710	20.750	20.845
PO-only mort SW F	22.787	22.689	19.441	18.995	22.102	21.993
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.9: OLS estimates for foster care caseload, average of two endpoint years

	(1) Δ IHS Caseload	(2) Δ IHS Caseload	(3) Δ IHS Caseload	(4) Δ IHS Caseload	(5) Δ IHS Caseload	(6) Δ IHS Caseload
LD 2010/11-2015/16						
Δ Illicit op mort	0.0002	0.0001	-0.0001	-0.0002	-0.0004	-0.0003
	(0.0014)	(0.0013)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
Δ PO-only mort	-0.0060**	-0.0050**	-0.0040*	-0.0039	-0.0031	-0.0032
	(0.0025)	(0.0025)	(0.0024)	(0.0024)	(0.0025)	(0.0025)
Dep. var. mean	0.058	0.058	0.058	0.058	0.058	0.058
R^2	0.105	0.121	0.145	0.146	0.152	0.153
LD 2010/11-2016/17						
Δ Illicit op mort	0.0006	0.0006	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Δ PO-only mort	-0.0060**	-0.0052**	-0.0048**	-0.0047**	-0.0041*	-0.0041*
	(0.0023)	(0.0023)	(0.0023)	(0.0023)	(0.0024)	(0.0023)
Dep. var. mean	0.064	0.064	0.064	0.064	0.064	0.064
R^2	0.123	0.143	0.172	0.174	0.176	0.176
LD 2010/11-2017/18						
Δ Illicit op mort	0.0016	0.0015	0.0007	0.0008	0.0006	0.0005
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Δ PO-only mort	-0.0050**	-0.0044*	-0.0044*	-0.0043*	-0.0038	-0.0038
	(0.0024)	(0.0024)	(0.0023)	(0.0023)	(0.0023)	(0.0023)
Dep. var. mean	0.056	0.056	0.056	0.056	0.056	0.056
R^2	0.143	0.163	0.191	0.192	0.198	0.198
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.10: IV estimates for $t-1$ prior FY continuation rate

	(1) Δ $t-1$ Cont. Rate	(2) Δ $t-1$ Cont. Rate	(3) Δ $t-1$ Cont. Rate	(4) Δ $t-1$ Cont. Rate	(5) Δ $t-1$ Cont. Rate	(6) Δ $t-1$ Cont. Rate
LD 2011/12-2015/16						
Δ Illicit op mort	-0.0018	-0.0023	-0.0014	-0.0014	-0.0017	-0.0017
	(0.0021)	(0.0018)	(0.0020)	(0.0021)	(0.0020)	(0.0020)
tF 95% CI						(-.0086, .0053)
Δ PO-only mort	0.0017	0.0014	0.0044	0.0043	0.0030	0.0029
	(0.0075)	(0.0077)	(0.0067)	(0.0064)	(0.0058)	(0.0058)
tF 95% CI						(-.0118, .0176)
Dep. var. mean	0.041	0.041	0.041	0.041	0.041	0.041
N	1,184	1,184	1,184	1,184	1,184	1,184
Cragg-Donald F	9.719	9.368	11.860	12.457	16.318	16.263
Illicit op mort SW F	11.013	11.766	10.908	10.425	10.296	10.328
PO-only mort SW F	13.886	13.589	17.461	18.585	22.106	22.300
LD 2011/12-2016/17						
Δ Illicit op mort	-0.0008	-0.0009	-0.0005	-0.0005	-0.0004	-0.0004
	(0.0015)	(0.0014)	(0.0018)	(0.0019)	(0.0017)	(0.0017)
tF 95% CI						(-.007, .0063)
Δ PO-only mort	-0.0018	-0.0018	0.0008	0.0006	0.0017	0.0017
	(0.0067)	(0.0073)	(0.0078)	(0.0075)	(0.0058)	(0.0058)
tF 95% CI						(-.0156, .019)
Dep. var. mean	0.041	0.041	0.041	0.041	0.041	0.041
N	1,179	1,179	1,179	1,179	1,179	1,179
Cragg-Donald F	10.870	9.423	7.004	7.180	11.419	11.442
Illicit op mort SW F	12.872	11.126	6.753	6.637	7.846	7.866
PO-only mort SW F	15.618	13.447	9.592	10.215	13.867	13.949
LD 2011/12-2017/18						
Δ Illicit op mort	-0.0007	-0.0005	0.0004	0.0004	0.0002	0.0002
	(0.0015)	(0.0015)	(0.0022)	(0.0022)	(0.0017)	(0.0017)
tF 95% CI						(-.0061, .0065)
Δ PO-only mort	-0.0031	-0.0020	0.0021	0.0020	0.0024	0.0024
	(0.0065)	(0.0072)	(0.0090)	(0.0087)	(0.0061)	(0.0061)
tF 95% CI						(-.0176, .0224)
Dep. var. mean	0.048	0.048	0.048	0.048	0.048	0.048
N	1,178	1,178	1,178	1,178	1,178	1,178
Cragg-Donald F	11.627	9.892	5.855	5.963	10.653	10.681
Illicit op mort SW F	12.592	10.651	6.047	6.153	8.985	9.155
PO-only mort SW F	15.025	12.530	6.824	7.105	10.624	10.732
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

The $t-1$ continuation rate is defined as the share of children entering foster care in FY $t-1$ who remain in care at the end of FY t. Long difference is taken between averaged levels 2011-12 and 2015-16, 2016-17, or 2017-18. LD is relative to averaged levels over 2011-12 because prior FY continuation rate is missing for the first year of the sample period, 2010. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.11: IV estimates for $t-2$ FY continuation rate

	(1) Δ $t-2$ Cont. Rate	(2) Δ $t-2$ Cont. Rate	(3) Δ $t-2$ Cont. Rate	(4) Δ $t-2$ Cont. Rate	(5) Δ $t-2$ Cont. Rate	(6) Δ $t-2$ Cont. Rate
LD 2012/13-2015/16						
Δ Illicit op mort	-0.0008	-0.0011	-0.0003	-0.0002	-0.0005	-0.0005
	(0.0018)	(0.0016)	(0.0020)	(0.0020)	(0.0018)	(0.0018)
tF 95% CI						(-.0055, .0045)
Δ PO-only mort	0.0226	0.0212	0.0284	0.0266	0.0216	0.0216
	(0.0175)	(0.0160)	(0.0195)	(0.0186)	(0.0142)	(0.0142)
tF 95% CI						$(-\infty, \infty)$
Dep. var. mean	0.025	0.025	0.025	0.025	0.025	0.025
Cragg-Donald F	2.241	2.535	2.630	2.632	3.616	3.576
Illicit op mort SW F	9.185	17.508	14.376	14.730	17.021	16.782
PO-only mort SW F	2.937	3.533	3.390	3.403	3.752	3.742
LD 2012/13-2016/17						
Δ Illicit op mort	-0.0001	-0.0001	0.0003	0.0003	-0.0003	-0.0003
	(0.0011)	(0.0012)	(0.0018)	(0.0018)	(0.0013)	(0.0013)
tF 95% CI						(-.0051, .0045)
Δ PO-only mort	0.0101	0.0143	0.0173	0.0160	0.0119	0.0119
	(0.0097)	(0.0113)	(0.0148)	(0.0139)	(0.0084)	(0.0086)
tF 95% CI						(-.0302, .054)
Dep. var. mean	0.026	0.026	0.026	0.026	0.026	0.026
Cragg-Donald F	4.475	3.679	2.459	2.547	5.917	5.730
Illicit op mort SW F	7.753	6.772	3.489	3.736	8.547	8.297
PO-only mort SW F	6.108	5.190	3.298	3.491	6.695	6.487
LD 2012/13-2017/18						
Δ Illicit op mort	0.0001	0.0006	0.0013	0.0012	0.0006	0.0006
	(0.0012)	(0.0014)	(0.0023)	(0.0022)	(0.0014)	(0.0015)
tF 95% CI						(-.0066, .0077)
Δ PO-only mort	0.0025	0.0095	0.0132	0.0126	0.0092	0.0091
	(0.0080)	(0.0101)	(0.0143)	(0.0137)	(0.0078)	(0.0079)
tF 95% CI						(-.0299, .0482)
Dep. var. mean	0.027	0.027	0.027	0.027	0.027	0.027
Cragg-Donald F	5.494	4.209	2.414	2.497	5.896	5.720
Illicit op mort SW F	6.260	5.115	2.878	3.010	6.115	6.020
PO-only mort SW F	6.778	5.435	3.004	3.159	6.369	6.242
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,190 counties. The $t-2$ continuation rate is defined as the share of children entering foster care in FY $t-2$ who remain in care at the end of FY t. Long difference is taken between averaged levels 2012-13 and 2015-16, 2016-17, or 2017-18. LD is relative to averaged levels over 2012-13 because $t-2$ FY continuation rate is missing for the first 2 years of the sample period, 2010-11. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.12: First stage estimates in triplicate vs. non-triplicate states

	(1) Δ Illicit op mort	(2) Δ Illicit op mort	(3) Δ PO-only mort	(4) Δ PO-only mort
Sample	**Triplicate**	**Non-Triplicate**	**Triplicate**	**Non-Triplicate**
LD 2010/11-2015/16				
OxyShare 2000	1.1460	16.1817**	0.2967	4.0488**
	(11.2060)	(6.8288)	(3.3998)	(1.6820)
PO density 2000	0.0177	0.0329**	-0.0015	-0.0222***
	(0.0241)	(0.0130)	(0.0099)	(0.0052)
Dep. var. mean	4.398	12.124	-0.603	-0.615
N	214	979	214	979
R^2	0.739	0.384	0.226	0.209
LD 2010/11-2016/17				
OxyShare 2000	0.9422	22.3444**	-1.3391	4.9004***
	(12.1732)	(9.1965)	(3.3195)	(1.7296)
PO density 2000	0.0152	0.0627***	-0.0057	-0.0277***
	(0.0290)	(0.0206)	(0.0106)	(0.0057)
Dep. var. mean	6.116	17.656	-0.627	-0.888
N	214	979	214	979
R^2	0.758	0.398	0.220	0.187
LD 2010/11-2017/18				
OxyShare 2000	4.1623	28.5548***	-3.4487	4.7475***
	(10.3158)	(9.3094)	(3.3545)	(1.8371)
PO density 2000	0.0111	0.0839***	-0.0047	-0.0345***
	(0.0303)	(0.0227)	(0.0095)	(0.0058)
Dep. var. mean	7.037	20.391	-0.864	-1.775
N	214	979	214	979
R^2	0.747	0.450	0.251	0.208
OxyShare 2000 mean (SD)	0.334	0.525	0.334	0.525
	(0.1079)	(0.1382)	(0.1079)	(0.1382)
PO density 2000 mean (SD)	91.021	121.335	91.021	121.335
	(41.602)	(58.257)	(41.602)	(58.257)

Triplicate states are CA, ID, IL, NY, TX. LD using 2010-11 average and 2015-16, 2016-17, or 2017-18 average. All regressions are weighted by the county's 2018 population under age 18, and include the full set of controls in long differences relative to 2010-11 average and their initial levels in 2000. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.13: Triplicate states sample: reduced-form estimates for foster care entry

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry
Sample	**Triplicate**	**Non-Triplicate**	**Full**
LD 2010/11-2015/16			
OxyShare 2000	-0.0065	0.5841***	0.5634***
	(0.5272)	(0.1485)	(0.1359)
PO density 2000	-0.0011	0.0003	0.0003
	(0.0012)	(0.0003)	(0.0003)
Dep. var. mean	-0.034	0.086	0.046
N	214	979	1,193
R^2	0.341	0.198	0.184
LD 2010/11-2016/17			
OxyShare 2000	-0.2574	0.3474**	0.3279***
	(0.5821)	(0.1448)	(0.1234)
PO density 2000	0.0004	0.0006*	0.0007**
	(0.0013)	(0.0003)	(0.0003)
Dep. var. mean	-0.042	0.077	0.038
N	214	979	1,193
R^2	0.347	0.178	0.184
LD 2010/11-2017/18			
OxyShare 2000	-0.0054	0.2187	0.1794
	(0.5826)	(0.1448)	(0.1244)
PO density 2000	-0.0007	0.0011***	0.0010***
	(0.0014)	(0.0003)	(0.0003)
Dep. var. mean	-0.059	0.039	0.007
N	214	979	1,193
R^2	0.396	0.181	0.198

LD using 2010-11 average and 2015-16, 2016-17, or 2017-18 average. All regressions are weighted by the county's 2018 population under age 18, and include the full set of controls in long differences relative to 2010-11 average and their initial levels in 2000. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.14: Triplicate states sample: reduced-form estimates for foster care caseload

	(1) Δ IHS Caseload	(2) Δ IHS Caseload	(3) Δ IHS Caseload
Sample	**Triplicate**	**Non-Triplicate**	**Full**
LD 2010/11-2015/16			
OxyShare 2000	-0.1770	0.6766***	0.7110***
	(0.4193)	(0.1682)	(0.1313)
PO density 2000	-0.0019*	-0.0002	-0.0002
	(0.0010)	(0.0004)	(0.0004)
Dep. var. mean	-0.032	0.103	0.058
N	214	979	1,193
R^2	0.358	0.243	0.213
LD 2010/11-2016/17			
OxyShare 2000	-0.3626	0.5534***	0.6634***
	(0.5004)	(0.1641)	(0.1171)
PO density 2000	-0.0006	-0.0001	-0.0000
	(0.0012)	(0.0004)	(0.0004)
Dep. var. mean	-0.059	0.124	0.064
N	214	979	1,193
R^2	0.373	0.220	0.221
LD 2010/11-2017/18			
OxyShare 2000	-0.3954	0.3951**	0.5895***
	(0.5614)	(0.1653)	(0.1267)
PO density 2000	-0.0006	0.0002	0.0004
	(0.0014)	(0.0004)	(0.0004)
Dep. var. mean	-0.081	0.123	0.056
N	214	979	1,193
R^2	0.375	0.221	0.232

LD using 2010-11 average and 2015-16, 2016-17, or 2017-18 average. All regressions are weighted by the county's 2018 population under age 18, and include the full set of controls in long differences relative to 2010-11 average and their initial levels in 2000. Robust standard errors. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.15: IV estimates for foster care caseload, by placement setting

	(1) ΔIHS Caseload, Non-relative foster home	(2) ΔIHS Caseload, Relative foster home	(3) ΔIHS Caseload, Group home/institution
LD 2010/11-2015/16			
Δ Illicit op mort	0.0301***	0.0498***	0.0350***
	(0.0093)	(0.0167)	(0.0131)
tF 95% CI	(.0049, .0552)	(.0044, .0952)	(-.0004, .0705)
Δ PO-only mort	0.0330	0.1338***	0.0247
	(0.0236)	(0.0457)	(0.0300)
tF 95% CI	(-.0243, .0903)	(.0226, .2449)	(-.0482, .0977)
Dep. var. mean	0.018	0.201	-0.027
Cragg-Donald F	23.902	23.902	23.902
Illicit op mort SW F	17.823	17.823	17.823
PO-only mort SW F	25.798	25.798	25.798
LD 2010/11-2016/17			
Δ Illicit op mort	0.0191***	0.0377***	0.0276***
	(0.0068)	(0.0135)	(0.0096)
tF 95% CI	(.0002, .038)	(-.0002, .0756)	(.0007, .0546)
Δ PO-only mort	0.0268	0.1217***	0.0359
	(0.0197)	(0.0422)	(0.0282)
tF 95% CI	(-.0202, .0738)	(.0208, .2225)	(-.0314, .1032)
Dep. var. mean	0.033	0.242	-0.095
Cragg-Donald F	21.371	21.371	21.371
Illicit op mort SW F	15.995	15.995	15.995
PO-only mort SW F	28.025	28.025	28.025
LD 2010/11-2017/18			
Δ Illicit op mort	0.0167***	0.0302***	0.0258***
	(0.0055)	(0.0113)	(0.0080)
tF 95% CI	(.0024, .0309)	(.0012, .0592)	(.0053, .0463)
Δ PO-only mort	0.0144	0.0910**	0.0286
	(0.0204)	(0.0435)	(0.0291)
tF 95% CI	(-.0375, .0663)	(-.0195, .2014)	(-.0453, .1026)
Dep. var. mean	0.027	0.261	-0.154
Cragg-Donald F	18.826	18.826	18.826
Illicit op mort SW F	20.845	20.845	20.845
PO-only mort SW F	21.993	21.993	21.993

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Endogenous regressors in both columns are Δ Illicit opioid OD death rate and Δ PO-only OD death rate. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions include the full set of covariates, and are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.16: Characteristics of those who misused pain relievers vs. used heroin, 2012-13

	Misused pain relievers in past year (column %)	Used heroin in past year (column %)	Difference
Education			
< High school	14.9	23.5	8.6***
High school graduate	28.3	35.2	6.9*
Some college	28.5	32.7	4.2
College graduate	17.8	4.0	-13.8***
Total family income			
<$20,000	24.3	41.0	16.7***
$20,000-$49,999	34.9	27.2	-7.7**
$50,000-$74,999	15.2	12.0	-3.2
≥$75,000	25.5	19.8	-5.7*
Age			
12-17	10.5	4.6	-5.9***
18-25	27.7	37.9	10.2***
26-34	23.0	35.4	12.4***
35-49	22.0	13.7	-8.3**
50+	16.7	8.4	-8.3***
Health insurance and household structure			
Any health insurance coverage	75.3	55.3	-20.0***
Other family members 18+ in HH	79.8	73.2	-6.6
Child <18 in HH	47.1	34.5	-12.6***
Married	29.2	9.7	-19.5***
Weighted count	11,777,000	666,000	

National Survey on Drug Use and Health, 2-Year Restricted-use Data Analysis System, 2012-13. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.17: IV estimates for foster care caseload: gender-specific opioid overdose death rates

Regressors:	(1) ΔIHS Caseload Female deaths	(2) ΔIHS Caseload Male deaths
LD 2010/11-2015/16		
Δ Illicit op mort	0.0549***	0.0216***
	(0.0206)	(0.0071)
tF 95% CI	(-.0001, .1098)	(.0021, .0411)
Δ PO-only mort	0.0956**	0.0399**
	(0.0475)	(0.0199)
tF 95% CI	(-.0454, .2366)	(-.0085, .0882)
Dep. var. mean	0.058	0.058
Cragg-Donald F	9.568	24.026
Illicit op mort SW F	18.147	17.096
PO-only mort SW F	13.325	26.192
LD 2010/11-2016/17		
Δ Illicit op mort	0.0496***	0.0157***
	(0.0190)	(0.0049)
tF 95% CI	(-.0131, .1122)	(.0023, .0291)
Δ PO-only mort	0.0918**	0.0351**
	(0.0442)	(0.0162)
tF 95% CI	(-.0396, .2232)	(-.0028, .073)
Dep. var. mean	0.064	0.064
Cragg-Donald F	8.810	24.154
Illicit op mort SW F	10.601	17.522
PO-only mort SW F	13.557	31.324
LD 2010/11-2017/18		
Δ Illicit op mort	0.0491***	0.0136***
	(0.0180)	(0.0035)
tF 95% CI	(-.0196, .1177)	(.0051, .0221)
Δ PO-only mort	0.0886	0.0224
	(0.0541)	(0.0153)
tF 95% CI	(-.1282, .3054)	(-.0143, .059)
Dep. var. mean	0.056	0.056
Cragg-Donald F	5.703	24.497
Illicit op mort SW F	8.170	26.942
PO-only mort SW F	7.952	28.320

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. In column 1 (2), the endogenous regressors are female (male) Δ Illicit opioid OD death rate and Δ PO-only OD death rate. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions include the full set of covariates, and are weighted by the county's 2018 population under age 18. Robust standard errors. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.18: IV estimates for foster care caseload, by child age

	(1) Δ IHS Caseload, Age <1	(2) Δ IHS Caseload, Age 1-5	(3) Δ IHS Caseload, Age 6-12	(4) Δ IHS Caseload, Age 13-17
LD 2010/11-2015/16				
Δ Illicit op mort	0.0328***	0.0351***	0.0340***	0.0259***
	(0.0113)	(0.0124)	(0.0116)	(0.0095)
tF 95% CI	(-.0001, .0657)	(-.0009, .0711)	(.0036, .0645)	(.0001, .0518)
Δ PO-only mort	0.0483*	0.0637**	0.0692**	0.0480*
	(0.0293)	(0.0297)	(0.0330)	(0.0247)
tF 95% CI	(-.026, .1227)	(-.0108, .1382)	(-.0111, .1494)	(-.0128, .1089)
Dep. var. mean	0.200	0.120	0.159	-0.121
Cragg-Donald F	19.967	20.964	23.970	22.328
Illicit op mort SW F	14.187	14.730	19.317	17.703
PO-only mort SW F	22.305	23.025	25.859	24.589
LD 2010/11-2016/17				
Δ Illicit op mort	0.0165**	0.0268***	0.0259***	0.0231***
	(0.0073)	(0.0098)	(0.0087)	(0.0075)
tF 95% CI	(-.0066, .0397)	(-.003, .0565)	(.0023, .0495)	(.002, .0442)
Δ PO-only mort	0.0174	0.0575**	0.0661**	0.0468**
	(0.0238)	(0.0265)	(0.0273)	(0.0231)
tF 95% CI	(-.0444, .0792)	(-.0091, .1241)	(.0014, .1308)	(-.0101, .1036)
Dep. var. mean	0.230	0.142	0.175	-0.148
Cragg-Donald F	15.622	17.693	22.906	19.413
Illicit op mort SW F	11.590	12.803	17.426	15.697
PO-only mort SW F	20.195	23.012	29.640	25.246
LD 2010/11-2017/18				
Δ Illicit op mort	0.0099	0.0225***	0.0235***	0.0221***
	(0.0062)	(0.0076)	(0.0064)	(0.0060)
tF 95% CI	(-.0078, .0275)	(.0015, .0435)	(.0077, .0394)	(.0062, .038)
Δ PO-only mort	-0.0113	0.0412	0.0492*	0.0363
	(0.0231)	(0.0275)	(0.0263)	(0.0232)
tF 95% CI	(-.0749, .0523)	(-.0323, .1146)	(-.0159, .1144)	(-.0248, .0974)
Dep. var. mean	0.230	0.141	0.177	-0.177
Cragg-Donald F	14.383	16.234	20.354	16.826
Illicit op mort SW F	15.141	16.818	23.626	18.842
PO-only mort SW F	16.589	18.453	23.874	19.517

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 child population corresponding to the age range of the outcome variable in each column; all columns include the full set of covariates. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.19: IV estimates for foster care caseload, interaction with state policies

	(1) Δ IHS Caseload	(2) Δ IHS Caseload	(3) Δ IHS Caseload
Policy	**State-admin. CWS**	**UMR**	**Maltrt defn incl parental drug use**
Policy × Δ Illicit op mort	0.0196	-0.0058	0.0700
	(0.0200)	(0.0092)	(0.0791)
tF 95% CI	(-.0711, .1103)	(-.0261, .0146)	$(-\infty,\infty)$
Policy × Δ PO-only mort	-0.0561	-0.1222**	0.1383
	(0.0649)	(0.0485)	(0.2326)
tF 95% CI	(-.2451, .1329)	(-.2425, -.002)	(-4.2027, 4.4793)
Δ Illicit op mort	0.0115	0.0268***	0.0258
	(0.0089)	(0.0082)	(0.0191)
tF 95% CI	(-.0104, .0335)	(.0037, .0498)	$(-\infty,\infty)$
Δ PO-only mort	0.0869**	0.1033**	0.0266
	(0.0440)	(0.0406)	(0.1012)
tF 95% CI	(-.0289, .2027)	(-.0149, .2215)	(-.3598, .4131)
Dep. var. mean	0.056	0.056	0.082
N	1,193	1,193	1,014
Cragg-Donald F	4.252	6.922	0.779
Policy × Illicit op mort SW F	6.839	41.510	1.175
Policy × PO-only mort SW F	14.269	24.470	3.817
Illicit op mort SW F	24.830	15.615	3.226
PO-only mort SW F	19.303	14.164	8.511

Long difference is taken between averaged levels 2010-11 and 2017-18. Instruments: oxycodone share in 2000, per-capita PO supply in 2000, and each interacted with the corresponding state policy indicator for the policy in each column. All regressions include the full set of covariates and are weighted by the county's 2018 population under age 18. Robust standard errors. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.20: IV estimates for foster care entry rate

	(1) Δ Entry rate	(2) Δ Entry rate	(3) Δ Entry rate	(4) Δ Entry rate	(5) Δ Entry rate	(6) Δ Entry rate
LD 2010/11-2015/16						
Δ Illicit op mort	0.1138***	0.1058***	0.1206***	0.1219***	0.1342***	0.1342***
	(0.0386)	(0.0327)	(0.0343)	(0.0352)	(0.0390)	(0.0388)
tF 95% CI						(.0213, .2472)
Δ PO-only mort	0.0759	0.0901	0.0792	0.0711	0.1117	0.1125
	(0.1236)	(0.1211)	(0.1075)	(0.1051)	(0.1001)	(0.1008)
tF 95% CI						(-.1406, .3655)
Dep. var. mean	0.229	0.229	0.229	0.229	0.229	0.229
Cragg-Donald F	14.548	15.574	18.917	19.367	21.581	21.318
Illicit op mort SW F	13.419	17.202	16.737	16.458	14.225	14.399
PO-only mort SW F	16.338	17.185	20.321	20.463	23.218	22.811
LD 2010/11-2016/17						
Δ Illicit op mort	0.0593***	0.0534***	0.0693***	0.0716***	0.0775***	0.0778***
	(0.0189)	(0.0171)	(0.0213)	(0.0220)	(0.0233)	(0.0234)
tF 95% CI						(.0051, .1504)
Δ PO-only mort	-0.0051	-0.0137	0.0252	0.0169	0.0386	0.0393
	(0.0804)	(0.0823)	(0.0820)	(0.0816)	(0.0766)	(0.0770)
tF 95% CI						(-.1562, .2349)
Dep. var. mean	0.231	0.231	0.231	0.231	0.231	0.231
Cragg-Donald F	15.419	15.617	15.720	15.919	17.323	17.282
Illicit op mort SW F	15.909	16.669	13.243	13.044	11.932	11.983
PO-only mort SW F	18.976	18.782	19.691	19.981	22.227	22.084
LD 2010/11-2017/18						
Δ Illicit op mort	0.0351**	0.0273*	0.0429**	0.0441**	0.0516**	0.0519***
	(0.0154)	(0.0146)	(0.0193)	(0.0196)	(0.0201)	(0.0201)
tF 95% CI						(-.0043, .1082)
Δ PO-only mort	-0.0458	-0.0834	-0.0374	-0.0417	-0.0102	-0.0097
	(0.0656)	(0.0671)	(0.0749)	(0.0748)	(0.0727)	(0.0729)
tF 95% CI						(-.2073, .188)
Dep. var. mean	0.146	0.146	0.146	0.146	0.146	0.146
Cragg-Donald F	16.791	16.598	12.973	12.863	15.509	15.557
Illicit op mort SW F	17.891	17.569	14.110	13.976	16.054	16.243
PO-only mort SW F	20.375	19.147	15.127	14.861	17.710	17.719
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Foster care entry rate is the number of foster care entries per 1,000 children age 0-17. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.21: IV estimates for foster care caseload rate

	(1) Δ Caseload rate	(2) Δ Caseload rate	(3) Δ Caseload rate	(4) Δ Caseload rate	(5) Δ Caseload rate	(6) Δ Caseload rate
LD 2010/11-2015/16						
Δ Illicit op mort	0.2038**	0.1827**	0.2133***	0.2172***	0.2447***	0.2444***
	(0.0954)	(0.0763)	(0.0796)	(0.0820)	(0.0879)	(0.0873)
tF 95% CI						(-.0095, .4983)
Δ PO-only mort	0.2046	0.2407	0.2171	0.1902	0.2747	0.2742
	(0.2389)	(0.2298)	(0.2036)	(0.1973)	(0.1882)	(0.1889)
tF 95% CI						(-.2, .7485)
Dep. var. mean	0.428	0.428	0.428	0.428	0.428	0.428
Cragg-Donald F	14.548	15.574	18.917	19.367	21.581	21.318
Illicit op mort SW F	13.419	17.202	16.737	16.458	14.225	14.399
PO-only mort SW F	16.338	17.185	20.321	20.463	23.218	22.811
LD 2010/11-2016/17						
Δ Illicit op mort	0.1433**	0.1312**	0.1720***	0.1796***	0.1957***	0.1957***
	(0.0609)	(0.0527)	(0.0664)	(0.0692)	(0.0725)	(0.0725)
tF 95% CI						(-.0298, .4212)
Δ PO-only mort	0.1053	0.1418	0.2450	0.2091	0.2698	0.2699
	(0.1867)	(0.1924)	(0.1948)	(0.1899)	(0.1788)	(0.1795)
tF 95% CI						(-.186, .7258)
Dep. var. mean	0.509	0.509	0.509	0.509	0.509	0.509
Cragg-Donald F	15.419	15.617	15.720	15.919	17.323	17.282
Illicit op mort SW F	15.909	16.669	13.243	13.044	11.932	11.983
PO-only mort SW F	18.976	18.782	19.691	19.981	22.227	22.084
LD 2010/11-2017/18						
Δ Illicit op mort	0.1070**	0.0982**	0.1503***	0.1560***	0.1718***	0.1718***
	(0.0464)	(0.0427)	(0.0561)	(0.0571)	(0.0576)	(0.0575)
tF 95% CI						(.0107, .3329)
Δ PO-only mort	0.0255	0.0307	0.1833	0.1578	0.2128	0.2127
	(0.1562)	(0.1666)	(0.1993)	(0.1970)	(0.1916)	(0.1916)
tF 95% CI						(-.3064, .7319)
Dep. var. mean	0.520	0.520	0.520	0.520	0.520	0.520
Cragg-Donald F	16.791	16.598	12.973	12.863	15.509	15.557
Illicit op mort SW F	17.891	17.569	14.110	13.976	16.054	16.243
PO-only mort SW F	20.375	19.147	15.127	14.861	17.710	17.719
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Foster care caseload rate is the number of children who entered care prior to the end of the current FY and have not been discharged by the end of the FY per 1,000 children age 0-17. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.22: IV estimates for foster care entry, difference relative to 2010

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry	(4) Δ IHS Entry	(5) Δ IHS Entry	(6) Δ IHS Entry
LD 2010-16						
Δ Illicit op mort	0.0247***	0.0234***	0.0254***	0.0259***	0.0269***	0.0267***
	(0.0069)	(0.0063)	(0.0070)	(0.0072)	(0.0079)	(0.0078)
tF 95% CI						(.0062, .0472)
Δ PO-only mort	0.0160	0.0132	0.0102	0.0051	0.0100	0.0092
	(0.0338)	(0.0308)	(0.0301)	(0.0304)	(0.0277)	(0.0277)
tF 95% CI						(-.0714, .0899)
Dep. var. mean	0.047	0.047	0.047	0.047	0.047	0.047
Cragg-Donald F	10.598	11.498	12.425	11.915	15.292	15.012
Illicit op mort SW F	24.047	27.716	22.560	22.489	19.040	19.351
PO-only mort SW F	11.494	11.919	12.020	11.164	15.087	14.688
LD 2010-17						
Δ Illicit op mort	0.0157***	0.0142***	0.0145***	0.0152***	0.0151***	0.0150***
	(0.0051)	(0.0046)	(0.0049)	(0.0051)	(0.0052)	(0.0051)
tF 95% CI						(.0008, .0291)
Δ PO-only mort	0.0045	-0.0064	-0.0067	-0.0080	-0.0087	-0.0087
	(0.0269)	(0.0245)	(0.0232)	(0.0231)	(0.0222)	(0.0221)
tF 95% CI						(-.0677, .0504)
Dep. var. mean	0.019	0.019	0.019	0.019	0.019	0.019
Cragg-Donald F	12.965	14.483	14.937	14.745	16.523	16.448
Illicit op mort SW F	20.179	21.461	18.301	18.091	16.874	16.859
PO-only mort SW F	15.144	16.133	17.047	16.782	18.849	18.854
LD 2010-18						
Δ Illicit op mort	0.0117***	0.0096**	0.0119**	0.0121**	0.0128**	0.0128**
	(0.0044)	(0.0041)	(0.0049)	(0.0049)	(0.0052)	(0.0051)
tF 95% CI						(-.0013, .0199)
Δ PO-only mort	-0.0132	-0.0261	-0.0197	-0.0205	-0.0152	-0.0152
	(0.0218)	(0.0208)	(0.0220)	(0.0223)	(0.0222)	(0.0220)
tF 95% CI						(-.0508, .0308)
Dep. var. mean	-0.014	-0.014	-0.014	-0.014	-0.014	-0.014
Cragg-Donald F	15.269	15.591	14.426	14.064	15.224	15.313
Illicit op mort SW F	23.297	21.513	18.712	18.399	18.097	18.705
PO-only mort SW F	18.533	16.978	17.173	16.394	17.764	17.835
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between annual levels in 2010 and 2016, 2017, or 2018. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.23: IV estimates for foster care entry, difference relative to 2011

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry	(4) Δ IHS Entry	(5) Δ IHS Entry	(6) Δ IHS Entry
LD 2011-16						
Δ Illicit op mort	0.0145***	0.0134***	0.0171***	0.0172***	0.0169***	0.0170***
	(0.0050)	(0.0048)	(0.0058)	(0.0060)	(0.0060)	(0.0060)
tF 95% CI						(.0005, .0335)
Δ PO-only mort	0.0100	0.0154	0.0225	0.0222	0.0224	0.0225
	(0.0198)	(0.0194)	(0.0190)	(0.0188)	(0.0164)	(0.0165)
tF 95% CI						(-.0173, .0623)
Dep. var. mean	0.056	0.056	0.056	0.056	0.056	0.056
Cragg-Donald F	12.461	12.952	14.426	14.663	18.234	17.936
Illicit op mort SW F	20.851	21.372	16.560	16.153	16.533	16.633
PO-only mort SW F	19.674	20.322	22.938	22.955	27.119	26.746
LD 2011-17						
Δ Illicit op mort	0.0065*	0.0058	0.0059	0.0061	0.0053	0.0052
	(0.0038)	(0.0036)	(0.0044)	(0.0046)	(0.0044)	(0.0044)
tF 95% CI						(-.0085, .019)
Δ PO-only mort	-0.0080	-0.0088	-0.0060	-0.0056	-0.0037	-0.0040
	(0.0162)	(0.0159)	(0.0157)	(0.0157)	(0.0134)	(0.0135)
tF 95% CI						(-.0394, .0315)
Dep. var. mean	0.028	0.028	0.028	0.028	0.028	0.028
Cragg-Donald F	12.447	12.921	10.344	9.978	13.207	13.044
Illicit op mort SW F	18.358	17.596	12.233	11.546	12.481	12.280
PO-only mort SW F	19.130	19.880	15.605	15.414	19.893	19.649
LD 2011-18						
Δ Illicit op mort	0.0036	0.0025	0.0018	0.0017	0.0022	0.0022
	(0.0035)	(0.0035)	(0.0048)	(0.0049)	(0.0047)	(0.0047)
tF 95% CI						(-.0133, .0176)
Δ PO-only mort	-0.0197	-0.0242*	-0.0224	-0.0214	-0.0146	-0.0145
	(0.0140)	(0.0143)	(0.0164)	(0.0164)	(0.0143)	(0.0142)
tF 95% CI						(-.0567, .0277)
Dep. var. mean	-0.005	-0.005	-0.005	-0.005	-0.005	-0.005
Cragg-Donald F	14.759	14.026	9.918	9.822	13.075	13.090
Illicit op mort SW F	18.682	15.172	9.911	9.685	10.957	11.081
PO-only mort SW F	20.333	17.710	11.370	11.332	13.362	13.419
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between annual levels in 2011 and 2016, 2017, or 2018. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Standard errors clustered at county level. * p<0.10, ** p<0.05, *** p<0.01

Table A.24: IV estimates for foster care caseload, difference relative to 2010

	(1) Δ IHS Caseload	(2) Δ IHS Caseload	(3) Δ IHS Caseload	(4) Δ IHS Caseload	(5) Δ IHS Caseload	(6) Δ IHS Caseload
LD 2010-16						
Δ Illicit op mort	0.0245***	0.0216***	0.0219***	0.0224***	0.0228***	0.0227***
	(0.0084)	(0.0072)	(0.0078)	(0.0079)	(0.0084)	(0.0084)
tF 95% CI						(.0008, .0447)
Δ PO-only mort	0.0636	0.0563	0.0593*	0.0555	0.0536*	0.0532*
	(0.0409)	(0.0366)	(0.0353)	(0.0356)	(0.0307)	(0.0308)
tF 95% CI						(-.0365, .1428)
Dep. var. mean	0.054	0.054	0.054	0.054	0.054	0.054
Cragg-Donald F	10.598	11.498	12.425	11.915	15.292	15.012
Illicit op mort SW F	24.047	27.716	22.560	22.489	19.040	19.351
PO-only mort SW F	11.494	11.919	12.020	11.164	15.087	14.688
LD 2010-17						
Δ Illicit op mort	0.0221***	0.0195***	0.0201***	0.0212***	0.0208***	0.0208***
	(0.0070)	(0.0062)	(0.0066)	(0.0070)	(0.0069)	(0.0068)
tF 95% CI						(.002, .0397)
Δ PO-only mort	0.0534	0.0422	0.0473	0.0451	0.0417	0.0419
	(0.0347)	(0.0312)	(0.0291)	(0.0292)	(0.0268)	(0.0267)
tF 95% CI						(-.0295, .1133)
Dep. var. mean	0.054	0.054	0.054	0.054	0.054	0.054
Cragg-Donald F	12.965	14.483	14.937	14.745	16.523	16.448
Illicit op mort SW F	20.179	21.461	18.301	18.091	16.874	16.859
PO-only mort SW F	15.144	16.133	17.047	16.782	18.849	18.854
LD 2010-18						
Δ Illicit op mort	0.0178***	0.0152***	0.0191***	0.0197***	0.0200***	0.0200***
	(0.0053)	(0.0049)	(0.0059)	(0.0060)	(0.0060)	(0.0059)
tF 95% CI						(.0043, .0357)
Δ PO-only mort	0.0252	0.0123	0.0258	0.0243	0.0249	0.0249
	(0.0252)	(0.0241)	(0.0269)	(0.0273)	(0.0260)	(0.0260)
tF 95% CI						(-.0456, .0953)
Dep. var. mean	0.039	0.039	0.039	0.039	0.039	0.039
Cragg-Donald F	15.269	15.591	14.426	14.064	15.224	15.313
Illicit op mort SW F	23.297	21.513	18.712	18.399	18.097	18.705
PO-only mort SW F	18.533	16.978	17.173	16.394	17.764	17.835
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between annual levels in 2010 and 2016, 2017, or 2018. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.25: IV estimates for foster care caseload, difference relative to 2011

	(1) Δ IHS Caseload	(2) Δ IHS Caseload	(3) Δ IHS Caseload	(4) Δ IHS Caseload	(5) Δ IHS Caseload	(6) Δ IHS Caseload
LD 2011-16						
Δ Illicit op mort	0.0236***	0.0212***	0.0276***	0.0282***	0.0270***	0.0271***
	(0.0081)	(0.0073)	(0.0094)	(0.0097)	(0.0091)	(0.0091)
tF 95% CI						(.002, .0522)
Δ PO-only mort	0.0477*	0.0496*	0.0592**	0.0571**	0.0548**	0.0551**
	(0.0270)	(0.0258)	(0.0267)	(0.0263)	(0.0223)	(0.0225)
tF 95% CI						(.0009, .1093)
Dep. var. mean	0.073	0.073	0.073	0.073	0.073	0.073
Cragg-Donald F	12.461	12.952	14.426	14.663	18.234	17.936
Illicit op mort SW F	20.851	21.372	16.560	16.153	16.533	16.633
PO-only mort SW F	19.674	20.322	22.938	22.955	27.119	26.746
LD 2011-17						
Δ Illicit op mort	0.0204***	0.0189***	0.0231***	0.0245***	0.0229***	0.0230***
	(0.0065)	(0.0062)	(0.0083)	(0.0089)	(0.0081)	(0.0082)
tF 95% CI						(-.0024, .0483)
Δ PO-only mort	0.0399	0.0391	0.0525*	0.0532*	0.0488**	0.0489**
	(0.0248)	(0.0247)	(0.0269)	(0.0272)	(0.0221)	(0.0223)
tF 95% CI						(-.0097, .1075)
Dep. var. mean	0.073	0.073	0.073	0.073	0.073	0.073
Cragg-Donald F	12.447	12.921	10.344	9.978	13.207	13.044
Illicit op mort SW F	18.358	17.596	12.233	11.546	12.481	12.280
PO-only mort SW F	19.130	19.880	15.605	15.414	19.893	19.649
LD 2011-18						
Δ Illicit op mort	0.0156***	0.0143***	0.0198***	0.0204***	0.0197***	0.0196***
	(0.0051)	(0.0051)	(0.0071)	(0.0073)	(0.0066)	(0.0066)
tF 95% CI						(-.0021, .0413)
Δ PO-only mort	0.0159	0.0135	0.0283	0.0284	0.0264	0.0264
	(0.0192)	(0.0207)	(0.0259)	(0.0260)	(0.0219)	(0.0218)
tF 95% CI						(-.0384, .0912)
Dep. var. mean	0.058	0.058	0.058	0.058	0.058	0.058
Cragg-Donald F	14.759	14.026	9.918	9.822	13.075	13.090
Illicit op mort SW F	18.682	15.172	9.911	9.685	10.957	11.081
PO-only mort SW F	20.333	17.710	11.370	11.332	13.362	13.419
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between annual levels in 2011 and 2016, 2017, or 2018. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are weighted by the county's 2018 population under age 18. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.26: IV estimates for foster care entry, alternative instruments

	(1) ΔIHS Entry	(2) ΔIHS Entry	(3) ΔIHS Entry	(4) ΔIHS Entry
LD 2010/11-2015/16				
Δ Illicit op mort	0.0297***	0.0333***	0.0357***	0.0248***
	(0.0086)	(0.0097)	(0.0107)	(0.0072)
tF 95% CI	(.0062, .0604)	(.0062, .0604)	(.0045, .067)	(.0053, .0443)
Δ PO-only mort	0.0275	0.0113	-0.0060	0.0134
	(0.0231)	(0.0215)	(0.0254)	(0.0204)
tF 95% CI	(-.0409, .0635)	(-.0409, .0635)	(-.0729, .0608)	(-.0397, .0665)
Dep. var. mean	0.046	0.046	0.046	0.046
Cragg-Donald F	23.902	27.232	17.891	23.997
Illicit op mort SW F	17.823	16.169	14.202	18.059
PO-only mort SW F	25.798	26.221	19.743	20.637
LD 2010/11-2016/17				
Δ Illicit op mort	0.0163***	0.0180***	0.0196***	0.0126***
	(0.0051)	(0.0056)	(0.0061)	(0.0041)
tF 95% CI	(.0019, .0308)	(.0021, .0339)	(.0014, .0378)	(.0012, .024)
Δ PO-only mort	0.0075	-0.0029	-0.0189	-0.0055
	(0.0174)	(0.0162)	(0.0193)	(0.0149)
tF 95% CI	(-.0342, .0491)	(-.0404, .0347)	(-.0659, .0281)	(-.0424, .0314)
Dep. var. mean	0.038	0.038	0.038	0.038
Cragg-Donald F	21.371	24.242	23.696	25.818
Illicit op mort SW F	15.995	15.400	13.800	16.930
PO-only mort SW F	28.025	32.798	26.509	24.331
LD 2010/11-2017/18				
Δ Illicit op mort	0.0093**	0.0097**	0.0097**	0.0073**
	(0.0041)	(0.0042)	(0.0043)	(0.0031)
tF 95% CI	(-.0013, .0199)	(-.0011, .0205)	(-.0014, .0207)	(0, .0146)
Δ PO-only mort	-0.0100	-0.0140	-0.0218	-0.0166
	(0.0161)	(0.0151)	(0.0174)	(0.0131)
tF 95% CI	(-.0508, .0308)	(-.0507, .0228)	(-.0661, .0225)	(-.0483, .0151)
Dep. var. mean	0.007	0.007	0.007	0.007
Cragg-Donald F	18.826	22.324	19.950	28.227
Illicit op mort SW F	20.845	21.981	22.319	29.544
PO-only mort SW F	21.993	25.906	21.737	25.853

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Each column uses a different set of initial exposure measures to instrument for Δ Illicit op mort and Δ PO-only mort. The first column replicates the main IV estimates where the instruments are the oxycodone share and per-capita PO supply (sum of oxycodone and hydrocodone) in 2000. Column 2 instruments: oxycodone share and per-capita PO supply (sum of oxycodone and hydrocodone) in 2001. Column 3 instruments: oxycodone share and per-capita PO supply (expanded set of POs) in 2001. Column 4 instruments: per-capita oxycodone supply and per-capita PO supply (sum of oxycodone and hydrocodone) in 2000. All regressions are weighted by the county's 2018 population under age 18; all columns include the full set of covariates. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.27: IV estimates for foster care caseload, alternative instruments

	(1) Δ IHS Caseload	(2) Δ IHS Caseload	(3) Δ IHS Caseload	(4) Δ IHS Caseload
LD 2010/11-2015/16				
Δ Illicit op mort	0.0312***	0.0348***	0.0374***	0.0276***
	(0.0104)	(0.0115)	(0.0130)	(0.0090)
tF 95% CI	(.0029, .0594)	(.0026, .0669)	(-.0003, .0751)	(.0033, .0518)
Δ PO-only mort	0.0562**	0.0375	0.0185	0.0458*
	(0.0269)	(0.0243)	(0.0296)	(0.0246)
tF 95% CI	(-.0092, .1216)	(-.0216, .0965)	(-.0594, .0964)	(-.0182, .1098)
Dep. var. mean	0.058	0.058	0.058	0.058
Cragg-Donald F	23.902	27.232	17.891	23.997
Illicit op mort SW F	17.823	16.169	14.202	18.059
PO-only mort SW F	25.798	26.221	19.743	20.637
LD 2010/11-2016/17				
Δ Illicit op mort	0.0240***	0.0254***	0.0268***	0.0193***
	(0.0078)	(0.0082)	(0.0091)	(0.0063)
tF 95% CI	(.002, .046)	(.0021, .0487)	(-.0003, .0538)	(.002, .0366)
Δ PO-only mort	0.0502**	0.0321	0.0178	0.0340*
	(0.0227)	(0.0199)	(0.0243)	(0.0193)
tF 95% CI	(-.0041, .1045)	(-.0141, .0782)	(-.0412, .0769)	(-.0139, .0818)
Dep. var. mean	0.064	0.064	0.064	0.064
Cragg-Donald F	21.371	24.242	23.696	25.818
Illicit op mort SW F	15.995	15.400	13.800	16.930
PO-only mort SW F	28.025	32.798	26.509	24.331
LD 2010/11-2017/18				
Δ Illicit op mort	0.0214***	0.0209***	0.0208***	0.0153***
	(0.0059)	(0.0057)	(0.0057)	(0.0039)
tF 95% CI	(.0062, .0366)	(.0063, .0354)	(.0063, .0354)	(.0061, .0245)
Δ PO-only mort	0.0362	0.0220	0.0166	0.0160
	(0.0228)	(0.0197)	(0.0227)	(0.0161)
tF 95% CI	(-.0216, .094)	(-.0259, .0698)	(-.041, .0742)	(-.023, .055)
Dep. var. mean	0.056	0.056	0.056	0.056
Cragg-Donald F	18.826	22.324	19.950	28.227
Illicit op mort SW F	20.845	21.981	22.319	29.544
PO-only mort SW F	21.993	25.906	21.737	25.853

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Each column uses a different set of initial exposure measures to instrument for Δ Illicit op mort and Δ PO-only mort. The first column replicates the main IV estimates where the instruments are the oxycodone share and per-capita PO supply (sum of oxycodone and hydrocodone) in 2000. Column 2 instruments: oxycodone share and per-capita PO supply (sum of oxycodone and hydrocodone) in 2001. Column 3 instruments: oxycodone share and per-capita PO supply (expanded set of POs) in 2001. Column 4 instruments: per-capita oxycodone supply and per-capita PO supply (sum of oxycodone and hydrocodone) in 2000. All regressions are weighted by the county's 2018 population under age 18; all columns include the full set of covariates. Robust standard errors. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.28: IV estimates for foster care entry, unweighted observations

	(1) Δ IHS Entry	(2) Δ IHS Entry	(3) Δ IHS Entry	(4) Δ IHS Entry	(5) Δ IHS Entry	(6) Δ IHS Entry
LD 2010/11-2015/16						
Δ Illicit op mort	0.0241***	0.0215***	0.0224***	0.0226***	0.0207***	0.0207***
	(0.0059)	(0.0052)	(0.0052)	(0.0053)	(0.0051)	(0.0051)
tF 95% CI						(.0101, .0313)
Δ PO-only mort	0.0090	0.0058	0.0054	0.0052	0.0013	0.0013
	(0.0116)	(0.0102)	(0.0098)	(0.0098)	(0.0090)	(0.0090)
tF 95% CI						(-.0274, .03)
Dep. var. mean	0.113	0.113	0.113	0.113	0.113	0.113
Cragg-Donald F	12.873	15.756	17.756	17.880	20.711	20.543
Illicit op mort SW F	47.967	58.873	59.867	58.213	57.506	57.357
PO-only mort SW F	8.949	9.619	11.099	11.100	11.297	11.232
LD 2010/11-2016/17						
Δ Illicit op mort	0.0152***	0.0132***	0.0132***	0.0136***	0.0125***	0.0125***
	(0.0040)	(0.0035)	(0.0035)	(0.0036)	(0.0035)	(0.0035)
tF 95% CI						(.0053, .0198)
Δ PO-only mort	0.0085	0.0048	0.0041	0.0045	0.0011	0.0012
	(0.0104)	(0.0089)	(0.0082)	(0.0082)	(0.0073)	(0.0073)
tF 95% CI						(-.02, .0225)
Dep. var. mean	0.126	0.126	0.126	0.126	0.126	0.126
Cragg-Donald F	12.872	16.050	18.538	18.237	21.984	21.916
Illicit op mort SW F	45.619	58.483	56.244	54.339	57.057	56.934
PO-only mort SW F	10.278	11.581	13.491	13.223	14.123	14.133
LD 2010/11-2017/18						
Δ Illicit op mort	0.0106***	0.0087***	0.0096***	0.0100***	0.0094***	0.0095***
	(0.0034)	(0.0031)	(0.0033)	(0.0034)	(0.0032)	(0.0032)
tF 95% CI						(.0027, .0162)
Δ PO-only mort	0.0016	-0.0020	-0.0001	0.0002	0.0006	0.0008
	(0.0082)	(0.0078)	(0.0078)	(0.0078)	(0.0072)	(0.0072)
tF 95% CI						(-.0193, .021)
Dep. var. mean	0.112	0.112	0.112	0.112	0.112	0.112
Cragg-Donald F	16.076	17.878	17.541	17.249	20.356	20.335
Illicit op mort SW F	49.817	59.670	51.455	47.191	55.305	55.293
PO-only mort SW F	12.642	13.383	14.505	14.185	15.610	15.850
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are unweighted. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Table A.29: IV estimates for foster care caseload, unweighted observations

	(1) Δ IHS Caseload	(2) Δ IHS Caseload	(3) Δ IHS Caseload	(4) Δ IHS Caseload	(5) Δ IHS Caseload	(6) Δ IHS Caseload
LD 2010/11-2015/16						
Δ Illicit op mort	0.0238***	0.0211***	0.0225***	0.0225***	0.0208***	0.0208***
	(0.0062)	(0.0054)	(0.0054)	(0.0055)	(0.0052)	(0.0052)
tF 95% CI						(.0098, .0318)
Δ PO-only mort	0.0197	0.0143	0.0142	0.0142	0.0110	0.0110
	(0.0129)	(0.0106)	(0.0102)	(0.0102)	(0.0091)	(0.0092)
tF 95% CI						(-.0184, .0404)
Dep. var. mean	0.150	0.150	0.150	0.150	0.150	0.150
Cragg-Donald F	12.873	15.756	17.756	17.880	20.711	20.543
Illicit op mort SW F	47.967	58.873	59.867	58.213	57.506	57.357
PO-only mort SW F	8.949	9.619	11.099	11.100	11.297	11.232
LD 2010/11-2016/17						
Δ Illicit op mort	0.0204***	0.0173***	0.0176***	0.0178***	0.0170***	0.0170***
	(0.0048)	(0.0040)	(0.0041)	(0.0042)	(0.0040)	(0.0040)
tF 95% CI						(.0086, .0255)
Δ PO-only mort	0.0239*	0.0170	0.0176*	0.0183*	0.0152	0.0152
	(0.0143)	(0.0111)	(0.0105)	(0.0106)	(0.0093)	(0.0093)
tF 95% CI						(-.0118, .0421)
Dep. var. mean	0.172	0.172	0.172	0.172	0.172	0.172
Cragg-Donald F	12.872	16.050	18.538	18.237	21.984	21.916
Illicit op mort SW F	45.619	58.483	56.244	54.339	57.057	56.934
PO-only mort SW F	10.278	11.581	13.491	13.223	14.123	14.133
LD 2010/11-2017/18						
Δ Illicit op mort	0.0187***	0.0157***	0.0175***	0.0176***	0.0171***	0.0172***
	(0.0041)	(0.0035)	(0.0039)	(0.0039)	(0.0037)	(0.0037)
tF 95% CI						(.0093, .0251)
Δ PO-only mort	0.0191*	0.0130	0.0178*	0.0186*	0.0183*	0.0184*
	(0.0115)	(0.0098)	(0.0106)	(0.0108)	(0.0098)	(0.0099)
tF 95% CI						(-.0092, .046)
Dep. var. mean	0.179	0.179	0.179	0.179	0.179	0.179
Cragg-Donald F	16.076	17.878	17.541	17.249	20.356	20.335
Illicit op mort SW F	49.817	59.670	51.455	47.191	55.305	55.293
PO-only mort SW F	12.642	13.383	14.505	14.185	15.610	15.850
Initial cond. 2000	y	y	y	y	y	y
Demographics	n	y	y	y	y	y
Socioeconomic	n	n	y	y	y	y
Other substance abuse	n	n	n	y	y	y
Public expenditures	n	n	n	n	y	y
Supply social svcs	n	n	n	n	n	y

N=1,193 counties. Long difference is taken between averaged levels 2010-11 and 2015-16, 2016-17, or 2017-18. Instruments: oxycodone share and per-capita PO supply in 2000. All regressions are unweighted. Robust standard errors. * p<0.10, ** p<0.05, *** p<0.01

Appendix B: Appendix to Chapter 3

B.1 Additional Figures

Figure B.1: Convergence to the steady state (47-52), starting at steady state (41-46)

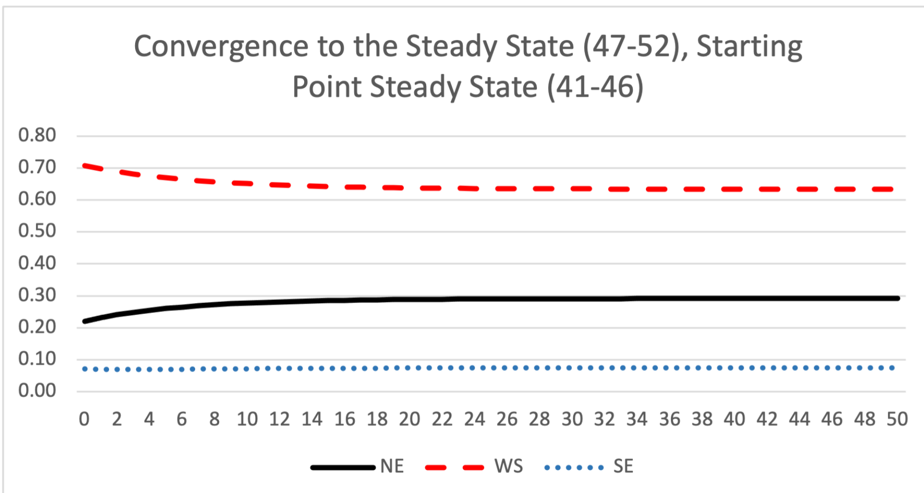
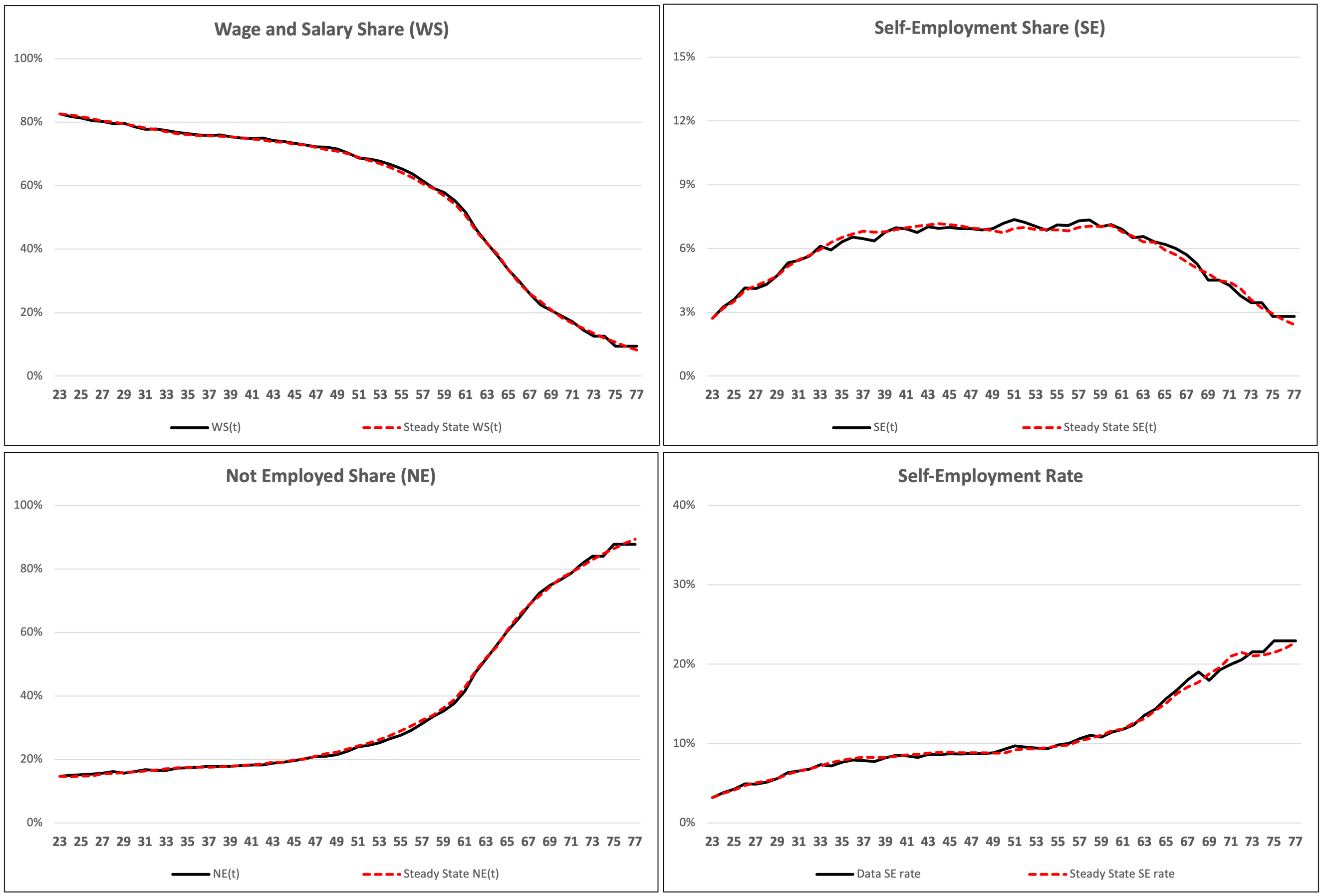


Figure B.2: Actual and steady state values of WS, SE, NE and self-employment rate by single year age



Data: CPS-DER, 1998 to 2015.

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