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Essays on the Effects of Medicaid Eligibility Policies on Outcomes Related to Substance Use Between 1996 and 2010

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Essays on the Effects of Medicaid Eligibility Policies on Outcomes Related to Substance Use Between 1996 and 2010

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy
in Economics

by

Benjamin Nathaniel Shumway Smith

2026

ABSTRACT OF THE DISSERTATION

Essays on the Effects of Medicaid Eligibility Policies on Outcomes Related to Substance Use Between 1996 and 2010

by

Benjamin Nathaniel Shumway Smith

Doctor of Philosophy in Economics

University of California, Los Angeles, 2026

Professor Dora Luisa Costa, Chair

In this dissertation, I build a novel collection of income thresholds used by all states to determine Medicaid eligibility for non-elderly, non-disabled adults, for all years between 1996 and 2010. I use this collection of income thresholds to explore same-year impacts of access to Medicaid, using a simulated instrumental variables approach. First, I explore effects of Medicaid access on opioid use, finding heterogeneous effects. For small states, opioid sales and opioid use disorder treatment episodes increased. However, for larger states, and for the country on average, effects on these outcomes are muted. I find no evidence that opioid overdose deaths increased, and I interpret this heterogeneity as evidence that prescription regulations in larger states were effective in preventing increasing access to Medicaid from leading to increased use of opioids. I also explore effects of Medicaid access on substance-use disorder (SUD) treatments. I find that while SUD treatment episodes increased substantially after increases in income thresholds for Medicaid eligibility, daily client loads and overall facility counts did not increase. This implies that SUD treatment episodes must have become faster following these Medicaid expansions. This change may be explained by another set

of findings: hospital-based SUD treatment facilities increased their client counts, with a particularly large increase within relatively quick detoxification treatments. Comparing my results to prior research, I note that the 2010 Affordable Care Act (ACA) significantly broadened Medicaid's coverage of SUD treatments, which may explain past findings that when focusing on the period following 2010, Medicaid expansions led to an outsize increase in outpatient rehabilitation services, rather than hospital-based detoxification services. In the final chapter of this dissertation, I provide a detailed discussion of my sources and methods for building the novel collection of income thresholds for Medicaid eligibility, including how I decide what values to use when existing data are missing or conflicting.

The dissertation of Benjamin Nathaniel Shumway Smith is approved.

Felipe M. Goncalves

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2026

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LIST OF SYMBOLS

$Info_{it}$	Individual i's household size, parental status, and employment in year t
$Policy_{ist}(\cdot)$	Maximum annual income that i's household would have to earn in year t to qualify for Medicaid in state s, given $Info_{it}$
$elig_{ist}(\cdot,\cdot,\cdot)$	Individual i's "simulated" eligibility if i were to live in state s in year t
$\mathbb{1}\{\cdot\}$	Indicator function, 1 if expression within brackets is true, 0 if false
μ_{it}	The fraction of states in which i would be eligible for Medicaid at time t, given i's household income and $Info_{it}$
IV_{it}	The Instrumental Variable (IV) designed by either Borusyak and Hull (2023) or Currie and Gruber (1996), using information on individual i in year t
$elig_{st}$	The average eligibility rate for state s in year t
IV_{st}	The average IV value for individuals in state s in year t
α_0, γ_0	Constant terms within my models
δ_s, ρ_s	State fixed effects
δ_t, ρ_t	Year fixed effects
α_1, γ_1	Coefficient of interest in first and second stage regressions, respectively
β, ξ	Coefficients on control variables
X_{st}	State-by-year control variables
ϵ_{st}, ν_{st}	Error terms
Y_{st}	Outcome of interest for state s in year t
$\widehat{elig}_{st}$	Predicted eligibility rate, using first-stage regression

LIST OF ACRONYMS

ACA	Affordable Care Act
AFDC	Aid to Families with Dependent Children
ARCOS	Automation of Reports and Consolidated Orders System
ASEC	Annual Social and Economic Supplement
BH	Borusyak and Hull (2023)
CBPP	Center on Budget and Policy Priorities
CDC	Centers for Disease Control and Prevention
CG	Currie and Gruber (1996)
CMS	Centers for Medicare and Medicaid Services
CPS	Current Population Survey
DEA	Drug Enforcement Administration
FDA	Food and Drug Administration
FPG	Federal Poverty Guideline
HEAL	Helping to End Addiction Long-term
HHS	Department of Health and Human Services
HSGAC	Homeland Security and Governmental Affairs Committee
H13	Hamersma (2013)
ICD-9	International Classification of Diseases, Ninth Revision
ICD-10	International Classification of Diseases, Tenth Revision
IPUMS	Integrated Public Use Microdata Series
IV	Instrumental Variable
KFF	Kaiser Family Foundation
MAT	Medication-Assisted Treatment
MCOD	Multiple Cause of Death
MME	Morphine Milligram Equivalent

MMWD	Modified Medicaid Waiver Database
MWD	Medicaid Waiver Database
NIH	National Institutes of Health
N-SSATS	National Survey of Substance Abuse Treatment Services
OLS	Ordinary Least Squares
OUD	Opioid Use Disorder
PDMP	Prescription Drug Monitoring Program
PRWORA	Personal Responsibility and Work Opportunity Reconciliation Act
SAMHSA	Substance Abuse and Mental Health Services Administration
SD	Standard Deviation
SEER	Survey of Epidemiology and End Result
SIPP	Survey of Income and Program Participation
SNAP	Supplemental Nutrition Assistance Program
SUD	Substance Use Disorder
TANF	Temporary Assistance for Needy Families
TEDS-A	Treatment Episode Data Set – Admissions
TEDS-D	Treatment Episode Data Set – Discharges
2SLS	Two-Stage Least Squares

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Chapter 1

Did Medicaid Fuel the Early Opioid Epidemic? An Investigation Using State Policies

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Abstract

I examine the role of Medicaid in fueling the early opioid epidemic, between 1996 and 2010. To do so, I construct a novel database of Medicaid policy reforms in the late 1990s and 2000s, a period marked by substantial changes to program eligibility, and I link these reforms to a range of data on opioid sales, abuse treatment, and use-related mortality. To address endogeneity of program eligibility, my empirical strategy uses two simulated instrumental variables that isolate quasi-exogenous variation in eligibility within and across states. Regression estimates indicate that Medicaid expansions contributed to significantly higher opioid sales, though effect sizes are relatively small and explain only a fraction of the documented rise in sales over this period. I then show that these expansions also resulted in a significant increase in opioid abuse treatments, an effect I argue is more consistent with greater abuse than improved treatment access. Estimates of program impacts appear to be smaller in states that more tightly regulated prescription access. Perhaps surprisingly, these policy reforms do not seem to have significantly increased opioid-related mortality. These findings provide novel evidence on the causal drivers of the opioid epidemic, and they offer insights for the broader debate about tradeoffs in the design of health insurance policy.

1.1 Introduction

Over the past three decades, the United States has endured an "epidemic" of opioid overdoses that has taken over 800,000 lives since 1999 (CDC, 2025). Beneath the death toll is a crisis of widespread opioid addiction, with survey responses suggesting that over 9 million people abused opioids in 2023 alone (SAMHSA, 2024). Even for those fortunate enough not to suffer a fatal overdose, addiction can decimate one's finances, health, and relationships. The suffering caused by addiction at this scale has hurt entire communities, through pathways such as increases in shoplifting, health issues, and preventable death (see, e.g. Quinones, 2015).

Despite such enormous costs, there are still open questions about this epidemic's origins. It is fairly well-established that beginning in the 1990's, physicians began to face pressure from pharmaceutical companies (Arteaga and Barone, 2023; Quinones, 2015; Van Zee, 2009), and even regulators and powerful entities like the Joint Commission (an accrediting organization; Jones et al., 2018), to prescribe more opioids, and to prescribe them to a wider population. Prescription opioid use skyrocketed thereafter, but an enduring question is why it did not do so evenly across states. Differences between states were driven at least in part by state-level policies, such as "triplicate" prescription policies (Alpert et al., 2022; Arteaga and Barone, 2023), prescription drug monitoring programs (PDMP; Buchmueller and Carey, 2018; Rutkow et al., 2015), and pain clinic laws (Finkelstein, Gentzkow, and Li, 2025; Lyapustina et al., 2016; Mizushima et al., 2025; Rutkow et al., 2015). Other state-level characteristics may have played a role too, including pre-existing relationships between opioid producers and physicians (Arteaga and Barone, 2023), and regional variation in economic hardship[1]. One potential factor in driving the early opioid epidemic in some places more

[1] Case and Deaton (2015, 2017, 2020) have explored the role of reduced economic opportunities and social cohesion behind increased rates of "deaths of despair" among non-Hispanic White individuals (see also Carpenter, McClellan, and Rees, 2017; Giles, Hungerman, and Oostrom, 2023; Pierce and Schott, 2020; Venkataramani et al., 2020). Evidence is mixed on whether these factors actually played a meaningful role

than others, and has not received much attention in the literature, is access to subsidized opioid pills through health insurance plans.

In this paper, I investigate the role played by Medicaid, a public health insurance program intended to offer poor individuals low-cost coverage, in fueling the early opioid epidemic. I focus on Medicaid because it has been accused, using anecdotal evidence, of having fueled the opioid epidemic (Eberstadt, 2017; U.S. Senate HSGAC, 2018), which if true, would be particularly tragic due to the vulnerabilities already faced by its target population. Medicaid also underwent a wide variety of state-level changes in eligibility programs, following the passage of a 1996 welfare reform law, setting up a natural experiment with rich, state-level policy variation. One issue that I face, however, when attempting to estimate the effects of Medicaid is that Medicaid eligibility is endogenous: for example, if one individual makes less money than another, she might both be more likely to be eligible for Medicaid and more susceptible to an opioid addiction, all else equal. To get around this problem, I follow Borusyak and Hull (2023) in constructing a simulated instrumental variable (IV), using individual-level CPS data and a novel collection of state-by-year Medicaid eligibility thresholds for non-elderly adults without disabilities, based on Burns, Dague, and Kasper's 2018a; 2018b Medicaid Waiver Database.

I then estimate effects using a two-stage least squares model, using data from 1996 through 2010[2], finding that increases in state Medicaid eligibility rates during this time did increase same-year prescription opioid sales[3], but with several caveats. First, this increase

in opioid abuse trends, however (Currie and Schwandt, 2020; Ruhm, 2019).

[2]I define the early opioid epidemic as the "First Wave" of the epidemic, which began in the 1990's and ended in 2010, when black-market opioids took over growth in the opioid market (CDC, 2025). In this paper, I set my starting year as 1996, which is the year when OxyContin was first released

[3]This outcome appears to be neither inherently good nor inherently bad. Opioid pills serve a legitimate purpose of treating otherwise untreatable pain, and many people strongly believed that pain was under-treated during the early-1990's. However, the downside to treatment using opioids is that they are highly addictive, and this downside was poorly understood throughout much of my sample period (I talk more about this in Section 1.2.2). Since any individual who received an opioid prescription, especially during the earliest years, was likely to be under-informed about the risk of addiction, any increase in individuals receiving opioid prescriptions would be expected to lead to new opioid addictions among some fraction of

was too small (point estimate: 0.717% for a 1 p.p. increase in the state eligibility rate) to account for more than a small fraction of opioid sales during the time period. Second, I cannot rule out null effects for opioid overdoses. Third, opioid abuse treatments increased significantly (point estimate: 3.21%) when Medicaid eligibility rates increased. This effect, and possibly other benefits of Medicaid, likely offset at least part of the mortality impacts of the increase in opioid abuse that likely resulted from the increase in sales. It is unclear why abuse treatments rose by so much, but increasing abuse appears to drive much of this increase, and I am unable to find evidence of a major role for alternative mechanisms. Finally, I find evidence that all estimates are more negative in larger states, which I interpret[4] as suggestive evidence that Medicaid expansions increased opioid sales by less in states with more effective prescription regulations.

The effect of Medicaid on opioid abuse is ex ante ambiguous[5]. On the one hand, commentators have accused Medicaid of fueling the opioid epidemic, and Powell, Pacula, and Taylor (2020) indeed found that the 2006 launch of Medicare Part D, an elderly-focused health insurance plan that only covers prescriptions, did indeed increase opioid overdoses among the populations for whom they estimate effects[6]. This result does not necessarily mean that Medicaid also had such an effect, however[7], and research thus far has unanimously shown either null or even beneficial effects (e.g. increased use of opioid abuse treatments, reduced opioid-related hospitalizations, and/or reduced overdose deaths) of Medicaid eligibility policies. While this literature is fairly conclusive for expansions in states' Medicaid eligibility

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the new recipients.

[4]In light of Alpert et al.'s 2022 findings that five states with triplicate prescription policies (including four of the largest states in the country: CA, IL, NY, and TX) had less aggressive increases in opioid overdoses.

[5]For example, it could have a negative effect through preventive care and substance abuse treatment, or it could have a positive effect through increased access to pain treatment and prescriptions

[6]The under-65-year-old population, who were affected through diversion of pills.

[7]Baker, Bundorf, and Kessler (2020) find that Medicare Part D has a particularly large effect on how many prescriptions beneficiaries obtain, compared to Medicare Advantage. Medicare Advantage is a comprehensive health plan like Medicaid, meaning that prescriptions, physician visits, and other benefits are all paid for by the same insurance program.

policies that took place since the 2010 Affordable Care Act (ACA) was passed (Averett, Smith, and Wang, 2019; Goodman-Bacon and Sandoe, 2017; Kravitz-Wirtz et al., 2020; Maclean and Saloner, 2019; Meinhofer and Witman, 2018; Sharp et al., 2018; Wen et al., 2017), such a result might not be so surprising for that time period, given how different the opioid market was then, compared to the previous 15 years: by 2010, physicians and patients had a better understanding of the risks of addiction from these pills; an effective, abuse-deterrent form of OxyContin was launched (Alpert, Powell, and Pacula, 2018; Evans, Lieber, and Power, 2019); prescription regulations had begun to tighten (see, e.g. Mulligan, 2024); and the use of illicit opioids (such as heroin and later, fentanyl) surged to compensate for the increasing difficulty to obtain and abuse prescription opioids. In contrast, the role of Medicaid closer to the beginning of the epidemic is still poorly understood. While some papers have explored the effects of different sets of expansions on certain outcomes during the 2000's[8], the comprehensive effect of all observable Medicaid eligibility policies from 1996 to 2010 remains to be precisely estimated for overdoses, and nothing is known about effects on opioid sales, opioid abuse treatments, and different types of overdoses, or whether effects are heterogeneous. This paper attempts to fill many of these gaps in the literature.

My findings largely support and add detail to the previous literature, although the effect I find for opioid sales offers some contrast to past findings. Like Snider et al. (2019) and Venkataramani and Chatterjee (2019), as well as papers that look at post-2010 expansions, I find a negative, though in my case, statistically insignificant, effect on my measure of overdoses, when I weight my regressions by population. Similarly, my finding of large, positive effects on opioid abuse treatments is consistent with Baicker et al.'s 2017 statistically insignificant, but positive estimates from the Oregon Health Insurance Experiment and pos-

[8] Baicker et al. (2017) find that the Oregon Health Insurance Experiment, which randomized access to Medicaid, had positive, but small and statistically insignificant effects on possession of opioid prescriptions and large and positive, but statistically insignificant, effects for opioid treatment prescriptions. Venkataramani and Chatterjee (2019) find that three Medicaid expansions in 2001 (AZ, ME, NY) reduced overall overdoses in the treated states. Snider et al. (2019) and Wen et al. (2020) find negative effects on relatively severe outcomes (substance-abuse-related deaths and opioid-related hospitalizations, respectively), using Medicaid eligibility policies between 2002-2015 and 2005-2013, respectively.

itive estimates for post-2010 Medicaid expansions (e.g. by Meinhofer and Witman, 2018). These findings suggest that there is no time period in which Medicaid expansions have been a major contributor to rising opioid overdoses throughout the opioid epidemic, and Medicaid has, both before 2010 and after, helped fight the opioid epidemic by increasing use of opioid abuse treatments. That said, I find that unlike the post-2010 expansions (Meinhofer and Witman, 2018), the effects of pre-2010 expansions on abuse treatments were not driven by new Medicaid enrollees, which may be explained by an increase in opioid abuse due to increasing prescription opioid sales. The estimated positive effect that I find on prescription opioid sales, especially in smaller states, also diverges from the literature (Sharp et al., 2018) by showing that the early Medicaid expansions did lead to increased access to opioid pills, although this effect was relatively small. The heterogeneity in effects that I find between states suggests that state-level regulations may have meaningfully affected Medicaid's ability to either mitigate or exacerbate the opioid epidemic within the given states before 2010, highlighting the under-researched possibility for regulators, or even insurers themselves, to design policies in a way that minimizes overuse of medical services but without meaningfully reducing the insurer's ability to provide access to needed healthcare[9].

The remainder of this paper is constructed as follows. Section 1.2 discusses the context for this study. Section 1.3 introduces the data I use. Section 1.4 explains the methodology. Section 1.5 presents results, and Section 1.6 explores possible mechanisms underlying results. I discuss takeaways in Section 1.7, and I conclude in Section 1.8.

[9] Medicaid, in particular, has been estimated to have various benefits to health and other forms of wellness (see, e.g. Brevoort, Grodzicki, and Hackmann, 2020; Finkelstein et al., 2012; Goodman-Bacon, 2021; Jacome, 2022; Miller, Johnson, and Wherry, 2021), and a policy aimed at reducing overuse of services must be careful to not throw the baby out with the bathwater by reducing access for those who genuinely need the given services.

1.2 Background

1.2.1 Medicaid

Each state sets its own eligibility requirements for individuals to be allowed to enroll in Medicaid. As Medicaid is intended to be targeted mainly to the poor, household income plays a role as a key determining factor towards eligibility (Currie and Gruber, 1994). For most non-elderly adults[10], eligibility depends on whether their household income falls below the applicable income threshold set by their state of residence. This is normally set as a percentage of the Federal Poverty Guideline (FPG) for an individual's household size, and it may depend on whether they are employed and often depends on whether they have dependent children.

Medicaid offers coverage to beneficiaries at low (and often no) cost for a wide range of medical services, including primary, urgent, and emergency care, and surgeries. As discussed by Gruber (2000), states have far less leeway over which services are covered by Medicaid, compared to their ability to choose eligibility criteria. As such, the range of healthcare options that an enrolled individual has access to in one state should be largely similar to the options they would have if enrolled in another state.

This paper focuses on the time frame between 1996 and 2010, during which time period, eligibility was most generous for employed parents, and generosity for this group increased in many states over time. For childless and/or unemployed adults, eligibility varied much more widely across states and over time, with many states simply not having any enrollment options for these individuals (especially childless adults, either employed or unemployed) for most or all of the sample period.

[10] There are eligibility categories that are particularly generous for certain populations, such as pregnant individuals (Gruber, 2000) and children (defined as younger than 19 years old; Dubay, Hill, and Kenney, 2002). There are also special sets of criteria that do not focus so strongly on income, which can apply to certain populations, such as disabled and/or elderly individuals (Gruber, 2000; Rudowitz et al., 2024).

1.2.2 Opioids

In the early 1990's, under-treatment of chronic pain was receiving increasing attention (see, e.g. Melzack, 1990), and Purdue Pharma capitalized on these concerns when they launched OxyContin in 1996 (Gourd, 2019; Jones et al., 2018). Part of the novelty with OxyContin was its potency: OxyContin was sold in a pill form that contained pure oxycodone (an opioid), which unlike previous oxycodone-ibuprofen or -acetaminophen (Tylenol) combination pills, had no dosage limit[11]. While previous opioid pills like Percocet were most often sold with 2.5mg of oxycodone each, OxyContin pills were regularly sold in 20 or 40mg oxycodone tablets, with tablets with as much as 160mg being available.

Another factor that quickly made OxyContin a major player in the pharmaceutical industry was advertising. Purdue Pharma launched a massive marketing campaign that was successful in expanding the market for opioids beyond the traditional domain of cancer patients, and into the population of patients with chronic pain. Their sales pitches were massively successful at convincing physicians to prescribe more OxyContin, but they were not all factually correct. One oft-repeated, but erroneous, marketing claim was that the risk of addiction was less than one percent Van Zee (2009). In addition, while advertisers pushed doctors to prescribe the pills to be taken only twice per day, the pills' effects often did not last the advertised 12 hours, in which cases patients reported experiencing intense cravings while they watched the clock until they could take their next dose[12] (Ryan, Girion, and Glover, 2016).

By 2000, concerns were already reaching the top levels of government about the addictive nature of OxyContin (U.S. Government Accountability Office, 2003), although meaningful responses came slowly. As recognition of the problem grew, states passed various regulatory

[11]Ibuprofen and acetaminophen are both toxic at high-enough doses.

[12]Many patients and other users discovered that instead of taking the pills every 12 hours, they could crush them for a more rapid, more intense high that made the pill even more addictive.

actions, such as Prescription Drug Monitoring Programs (PDMPs) and pain clinic laws, to try to reduce opioid overprescription, but their effectiveness was varied and often limited (see, e.g. Alpert et al., 2022; Alpert, Powell, and Pacula, 2018; Arteaga and Barone, 2023; Buchmueller and Carey, 2018; Mallatt, 2022). Even when state-level systems did work in a given state, they did not erase spillovers from other states – Florida, in particular, has been identified as a "pill mill" hub responsible for substantial interstate spillovers, in the years leading up to its implementation of pill mill laws beginning in 2010 (Rutkow et al., 2015; Beall, 2018).

The 2002 FDA approval of buprenorphine as an opioid abuse treatment option marked an important step towards addressing rising opioid addiction, but medication-assisted abuse treatments (buprenorphine, naltrexone, and methadone) continued to be severely underutilized (Heidbreder, Fudala, and Greenwald, 2023). Similarly, naloxone, a medication that can reverse an opioid overdose, was available during this time period, but it was not nearly as easy to obtain or to use as it is today (Freeman et al., 2018).

In 2010, the FDA approved an abuse-deterrent formulation of OxyContin, which was found to be effective (Alpert, Powell, and Pacula, 2018; Evans, Lieber, and Power, 2019). More broadly, Mulligan (2024) finds that there was a turning point in federal policy at around the same time, concluding that overall federal policies "subsidized and facilitated opioid prescriptions from the year 2000 until about 2010," after which point, prescription regulations began to strengthen. Subsequent increases in opioid abuse began to come from black market products, such as heroin, instead of prescription opioids, although annual prescription opioid overdose rates still have not returned to the level they were at in 1999 (CDC, 2025).

Having described how increased prescribing of OxyContin and other opioids laid the foundations for an epidemic, it is important to note that these medications do also serve a legitimate purpose for pain treatment. When used correctly, and they often are, they can make life tolerable for many users who would otherwise be in agony. However, they are addictive, much more so than was widely understood in the 1990's and even the 2000's, and this unappreciated addictive power means that for any effect I estimate on opioid sales from this time period, some fraction of those sales may reasonably be expected to create new addictions and feed existing ones, both among the intended recipients of the pills and among others in the community.

1.3 Data

I now discuss the data that I use. For this project, the key data challenge is collecting annual, state income thresholds for Medicaid eligibility. Once I have these data, I use data from the March Supplement of the Current Population Survey (CPS) to estimate surveyed individuals' eligibility for Medicaid and observe their reported enrollment. I also generate state-level aggregates of prescription opioid sales, opioid abuse treatments, and opioid overdoses, and I collect data from various other sources for use as control variables. For the scope of this paper, I focus on the years 1996 through 2010 and use all 51 "states," which include District of Columbia as its own "state."

1.3.1 Medicaid Eligibility Thresholds

Collecting data from multiple sources, I create what I believe to be the most complete set of state-by-year income thresholds for Medicaid eligibility from 1996 through 2010, for non-elderly adults without disabilities. This is not trivial, as these policies are determined at the state level and there is no federal database on all Medicaid eligibility requirements in all states, over the time period of interest.

Fortunately, researchers have managed to collect income thresholds going back to 1996, relying on contemporaneous reports from the Kaiser Family Foundation (KFF) and the

Center on Budget and Policy Priorities, among other sources of documentation. My dataset of income thresholds relies most heavily on one of these collections of thresholds, the Medicaid Waiver Database (MWD)[13], which was constructed by Burns, Dague, and Kasper (2018a,b). MWD includes employed and unemployed income eligibility thresholds[14] for childless adults and parents in households of three people, with documentation on important details behind most meaningful eligibility changes. There are also flags for years in which premiums or enrollment caps were used for a given state policy.

This database is not entirely complete, however, especially in the earlier years. In an attempt to make this set of thresholds even more complete and accurate than it already appears to be, I modify[15] the data using information from various other sources[16]. I refer to the finished dataset as the Modified MWD (MMWD), in order to reflect the outsize role the MWD plays within my dataset.

The state-level thresholds in 1996 and 2010 are recorded in Table 1.A1, and the annual path each state took specifically for employed parents is shown in Figure 1.A1. Many, though not all, states increased their thresholds for some or all categories[17] over time, and states varied widely in their annual thresholds and in the trajectories they followed between 1996 and 2010.

I use CPS data to estimate individuals' eligibility in the year preceding each survey

[13] Accessible at https://oaktrust.library.tamu.edu/items/e946d181-73a3-4d4f-91b8-9f7e463832b8, last accessed on May 19, 2025.

[14] These are given as a percent of the FPG, against which an individual's family income would be compared to determine eligibility. FPG values are obtained at https://aspe.hhs.gov/topics/poverty-economic-mobility/poverty-guidelines/prior-hhs-poverty-guidelines-federal-register-references.

[15] I explain the details of my procedure for modifying this dataset in Section 3.4 in the third chapter of this dissertation. One significant gap in the data that I am unable to remedy is that thresholds are missing for unemployed parents in most states until 2001. When thresholds are 0 or missing, I assume all individuals in the affected group are ineligible for Medicaid.

[16] These sources include Broaddus et al., 2002; Guyer and Mann, 1998b,a, 1999b; Hamersma, 2013; Kaiser Family Foundation, 2025; McMorrow et al., 2016, and State Policy Documentation Project, 2000.

[17] Note, however, that thresholds are missing for unemployed parents until around 2001 in most states.

year. To summarize my procedure[18], I compare each individual's income with the income threshold that applies to them given their state of residence, family size, employment status, and whether or not they have dependent children in their household. If their income is higher than the threshold, or if the threshold is 0% of the applicable FPG, then I say they are not eligible. Otherwise, I say they are eligible.

Table 1.1 provides summary statistics for calculated state-by-year eligibility rates for the 19-to-64-year-old population in the first row: the eligibility rate ranges from 0% to 24%, with a mean of 4.6% and median of 2.5%. This variable is calculated for the entire sample that I focus on: all 51 states in all years between 1996 and 2010 (inclusive).

1.3.2 CPS Survey Data: Enrollment

My data on Medicaid enrollment comes from the individual-level, nationally-representative samples from the CPS, which I obtain from IPUMS CPS (Flood et al., 2024). This and other variables that I draw from the CPS are answered in March of a given year, in reference to the previous year, so I use the surveys from 1997 through 2011 to infer values from 1996 through 2010.

One concern about these data is that survey answers in general may be less reliable. It is, therefore, comforting that the path this variable takes tracks fairly similarly to the aggregated administrative enrollment data provided by Centers for Medicare and Medicaid Services (CMS)[19]. In fact, Currie and Gruber (1996) suggest that CPS's Medicaid enrollment variables "may actually be superior" to administrative enrollment data because they reflect

[18] I discuss my methods in more detail in Sections 1.A1.1 and 3.5.

[19] See Figure 1.A2, which compares the data from both sources on adults aged 19 and older. The comparison is imperfect, however, because the CMS data on enrollment available through CMS's website (`https://www.cms.gov/data-research/computer-data-systems/medicaid-data-sources-general-information/msis-tables`) is in fiscal-year frequency, while the CPS data are annual. As such, the path the CMS time series takes in the figure appears shifted to the right relative to the one from CPS data. The CPS levels are also somewhat higher than the CMS levels, presumably due to imperfect reporting or knowledge of enrollment in the CPS survey, although the difference is fairly small, relative to the size of annual enrollment according to each dataset.

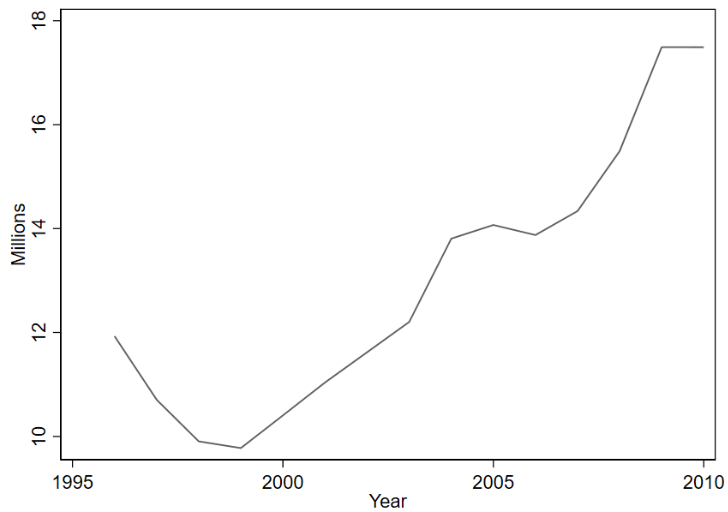
knowledge of eligibility for coverage. Hospitals have the incentive to sign patients up for Medicaid if they are eligible, so that the hospitals can guarantee reimbursement for the patients' care. Given that incentive, the logic is that even if an individual was not literally enrolled in Medicaid last year, they might still say they were enrolled if they knew that they would be covered in the event that they needed medical care. This conjecture is consistent with CPS's levels being somewhat higher than CMS's levels in Figure 1.A2.

Annual total Medicaid enrollment for the age group of interest[20], multiplied by CPS population weights (ASEC weights), is shown in Figure 1.1. The trajectory is non-monotonic but increases for most of the time frame, due at least in large part to state Medicaid expansions and reductions in employer-sponsored insurance (Holahan, Cohen, and Rousseau, 2007). Medicaid enrollment fell after the 1996 welfare reform was passed, presumably due to state administrative frictions during the next few years (Ku and Bruen, 1999). Enrollment fell again in 2006, this time at least in part due to the creation of Medicare Part D, through which elderly (not included in the figure) and disabled individuals could access low-cost prescriptions (Holahan, Cohen, and Rousseau, 2007).

Summary statistics for state-by-year enrollment rates for the 19-to-64-year-old population are displayed in the second row of Table 1.1: enrollment rates range from 1.6% to 18.8%, with a mean of 6.6% and a median of 5.9%. The fact that this variable is larger than the eligibility rate reflects that the eligibility thresholds I have only reflect one pathway to eligibility, whereas my enrollment measure includes individuals who are eligible for Medicaid by other means, such as disability. As with eligibility, this variable is present for the entire sample time frame.

[20]Since I do not have eligibility thresholds that apply to children and seniors, I remove them from the main sample of CPS data when observing enrollment and calculating eligibility.

Figure 1.1: Adult Medicaid Enrollment



Notes: Medicaid enrollment among individuals of ages 19-64. Aggregated sum for each year estimated using weighted responses for the given year, using the himcaidly variable in the CPS March Supplement from the following year. Source: CPS

1.3.3 Opioid Sales

The first outcome variable related to opioid use that I collect is opioid sales from pharmaceutical distributors to retail pharmacies. These data are collected by the US Drug Enforcement Administration (DEA) and compiled into a dataset called ARCOS. The version I use was generously shared by Arteaga and Barone (2023) and covers 3-digit zip code-by-year total grams sold, for each of various opioid medications.

I use these data to generate an overall "opioid" sales category[21]. Three drugs that play particularly important roles in this aggregate are oxycodone (such as OxyContin), hydrocodone (such as Vicodin), and methadone (an opioid abuse treatment medication that is itself a potentially addictive opioid). I also convert each value into 60 morphine milligram equivalent (MME) units[22] (for oxycodone, this is equivalent to one 40mg pill, which was a

[21] The opioids recorded are codeine, dihydrocodeine, oxycodone, hydromorphone, hydrocodone, levorphanol, meperidine, methadone, morphine, oxymorphone, and fentanyl.

[22] For more details and sources on where my MME conversion factors are from, see 1.A1.2. Note that

commonly prescribed dosage to be taken twice per day). The paths of these variables at the national level are shown in Figure 1.2a. Each variable follows an increasing path over the sample period, with a notable jump in overall opioid sales in 2006 that may be related to the launch of Medicare Part D (Powell, Pacula, and Taylor, 2020).

I drop the year 2000 for methadone and overall opioid sales, because, as is visible in Figure 1.2a, the ARCOS data are missing for a number of types of opioids, including methadone, in that year.that year for value for the "opioids" variable falls sharply in the year 2000. This appears to stem from the official 2000 ARCOS report[23], as opposed to having been lost in posterior data cleaning.

Summary statistics at the state-by-year level for some of these variables are shown in rows 3 through 5 in Table 1.1: overall opioid sales range from 1.52 to 29.36 MME doses per capita, with a mean of 9.25 and a median of 8.29. Summary statistics for oxycodone and methadone are shown in the fourth and fifth rows, respectively. These variables are available for all 51 states from 1997 through 2010, although the methadone and overall opioid sales variables are defined as missing in 2000, as previously mentioned.

1.3.4 Substance Abuse Treatment Episodes

Another outcome variable that I use is the public-access data on substance abuse treatment episode admissions (or more simply, "admissions" or "treatments") offered by Substance Abuse and Mental Health Services Administration (SAMHSA) through the TEDS-A dataset. This dataset contains most substance abuse treatment admissions that take place in facilities that receive any kind of government funding, including grants or even public insurance

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these units are not perfect representations of true comparability between morphine and other opioids, but I take them as the most reasonable approximations that I could find.

[23] https://www.deadiversion.usdoj.gov/arcos/retail_drug_summary/archive/report_yr_2000.pdf

payments from, e.g. Medicaid[24]. The data are given at the individual treatment level, with state- and year-level identifiers, and information on up to three substances for which they reported abuse upon admission, together with demographic variables, information on their education level, and what type of person or organization referred them.

One potential problem with this dataset is that reporting standards vary from state to state[25]. Although any persistent reporting differences between states should be absorbed by state fixed effects, I also explore, in Section 1.5.6, whether results appear to be driven by these reporting differences in reporting.

I create two new variables from this dataset. The first is for prescription opioids, which I define as a substance abuse problem with either "non-prescription methadone"[26] or "other opioids or synthetics"[27]. The second new variable is for opioids more broadly. This is my chosen measure of opioid treatment admissions, and I define it as individuals obtaining treatment, who are flagged as abusing the same substances as for prescription opioids, in addition to heroin, or have a diagnosis with "opioid dependence" or "opioid abuse." As diversion may be common (Powell, Pacula, and Taylor, 2020), I do not attempt to restrict opioid treatment admissions (or overdoses) by age like I do with eligibility and enrollment.

The variation over time in these variables can be seen in Figure 1.2b. It is noteworthy that the line representing prescription opioid treatments in this figure appears to parallel the line for prescription opioid sales in Figure 1.2a, suggesting a relationship between the two.

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[24]One possible concern is that any estimates for an outcome in these data might be driven by previously unobserved, private facilities entering the TEDS-A sample by beginning to accept Medicaid. This appears not to play a major role, however, as I do not find any evidence of an increase in facilities accepting Medicaid within the N-SSATS (a SAMHSA survey of substance abuse facilities, which includes private facilities) survey data.

[25]For example, some states may report treatments received in corrections facilities while others may not, according to the TEDS-A documentation.

[26]Referred to as such to distinguish it from methadone prescribed as part of the opioid abuse treatment.

[27]According to the data documentation, this category includes "buprenorphine, codeine, hydrocodone, hydromorphone, meperidine, morphine, opium, oxycodone, pentazocine, propoxyphene, tramadol, and any other drug with morphine-like effects"

Table 1.1 shows summary statistics for overall opioid abuse treatment admissions and prescription opioid abuse treatment admissions at the state-by-year level. Overall opioid admissions range (row 6) from 2.5 to 790.8 admissions per 100 thousand residents, with a mean of 140.3. Admissions for prescription opioid treatments (row 7) make up a sizable portion of the overall counts, but one that is less than half on average (as defined either by the mean or the median). These variables are available from 1996 through 2010, for all 51 states except a total of 20 missing state-year observations due to missed reporting years.

1.3.5 Opioid Overdose deaths

I draw data on opioid overdose deaths from restricted-level, multiple-cause-of-death (MCOD) files maintained by National Center for Health Statistics (2025). This dataset provides information on each death from across the country, with demographic information, up to 20 causes of death per individual, and county, state, month, and year identifiers.

I create two overdose variables: prescription opioid overdoses and the variable I focus on, overall opioid overdoses – similar to what I do for the TEDS-A data. Since the MCOD files transitioned in 1999 from using ICD-9 to ICD-10 codes to record causes of death, I opt to ensure internal consistency by restricting my sample to the time frame beginning in 1999. I also restrict my sample to residents of the state where they died, in order to limit the role of cross-state spillovers within my estimated effects. I construct the two variables by following the procedure described in Arteaga and Barone (2023)[28]. Figure 1.2c shows that between 1999 and 2010, prescription opioid overdoses explain the majority of the increasing annual opioid overdose counts, and that their paths diverge significantly a few years after 2010.

In Table 1.1, row 8 provides summary statistics at the state-by-year level for opioid over-

[28] I define prescription opioid overdoses as deaths with ICD-10 cause of death codes (used in 1999 onwards) X40–X44, X60–64, X85, or Y10–Y14 with contributing causes T40.2 and T40.3. I define broader "opioid overdoses" as deaths with ICD-10 codes X40–X44, X60–64, X85, or Y10–Y14 with contributing causes T40.0-T40.4

dose deaths. This variable ranges from 0.16 to 22.97 overdoses per 100 thousand residents, with a mean of 4.35 and a median of 3.76. Prescription opioid overdoses, shown in row 9, account for the majority of opioid overdoses during this time period, on average. These variables are present for all states and all years in the sample, except that I drop their values from before 1999. Note that the mean and median opioid overdose values are each more than 15 times smaller than the corresponding values for opioid treatment admissions, which underscores the relative rarity of overdose deaths.

1.3.6 Other Data

Survey of Substance Abuse Treatment Facilities

I obtain data from the National Survey of Substance Abuse Treatment Services (N-SSATS), using the concatenated file for 1997-2011 provided by SAMHSA[29]. This survey allows me to calculate annual estimates for the number and type of facilities offering substance abuse treatment in each state, with the most detailed information about the facilities being available in the later iterations of the survey. I use the data from the surveys from 1998, 2000, and every year from 2002 through 2010[30].

Demographic Variables

I draw state-by-year and county-by-year population data from the Survey of Epidemiology and End Results (SEER). These datasets also allow me to calculate annual (for each state and county) population and population shares of different age ranges[31], races, and Hispanic

[29]Obtained at https://www.samhsa.gov/data/data-we-collect/n-sumhss-national-substance-use-and-mental-health-services-survey/datafiles/n-ssats-1997-2011. Last accessed on September 18, 2025.

[30]The omitted years have no surveys, except for 1997, which reports a significantly smaller sample than in the later years.

[31]Powell, Pacula, and Taylor (2020)

ethnicity.

Political Variables

I bring in county-by-year data on county-level party vote shares in general elections for president, governor, and senators. These data were created and generously made public by Algara and Amlani (2021).

From these data, I calculate state-level vote shares in favor of the Democratic-party candidate, for each type of election. For years in which an election did not occur, I impute the most recent value for the given state. In the rare case when a state holds two senate elections in the same year, I take the average vote share over the two elections.

These data do not include Alaska, however, so for Alaska, I create the same variables at the state level, using data from Alaska's Division of Elections[32].

Other Control Variables

I bring in annual dummy variables on the existence of relevant policies from Alpert, Powell, and Pacula (2018) and Table A.1 in Powell, Pacula, and Taylor (2020), for whether a given state had each of the following policies or features: any existing PDMP, a "must-access" PDMP, medical marijuana laws, active and legal marijuana dispensaries, and pill-mill laws[33].

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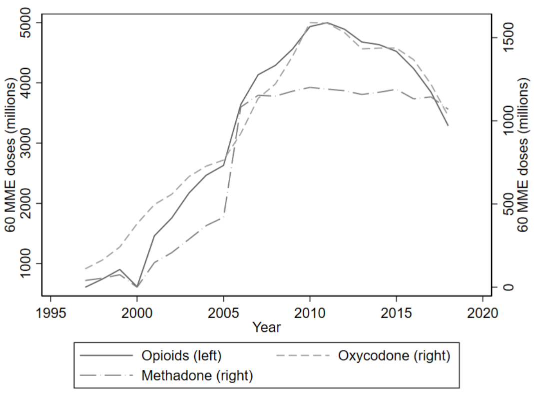
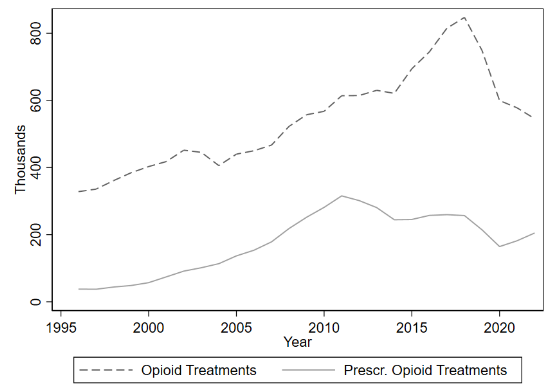
[32] `https://www.elections.alaska.gov/election-results/`

[33] Specifically, Alpert, Powell, and Pacula's 2018 values begin in 1999, so I use Powell, Pacula, and Taylor (2020) to extend the dummy for the medical marijuana law back to 1996. The two articles disagree substantially on the start years for "any PDMP," so I default to using the values from Powell, Pacula, and Taylor (2020), as it is the article published most recently, and it has the most comprehensive values during my sample period (rather than truncating the creation year at 1999). The other three variables are all 0 in 1999 in Alpert, Powell, and Pacula (2018), so I simply use their values and impute 0's for the 3 preceding years.

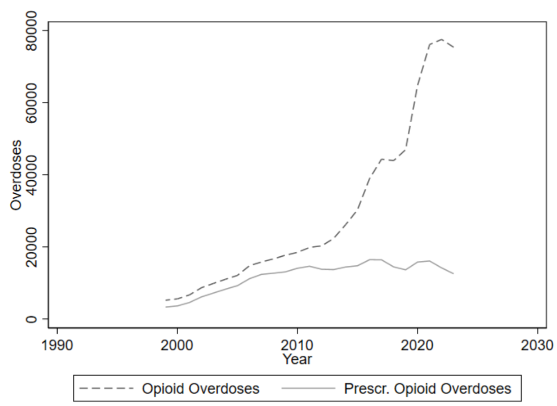
Figure 1.2: Main Outcomes



(a) Pharmaceutical Opioid Sales



(b) Opioid Treatment Admissions



(c) Opioid Overdose Deaths

Notes: "MME" stands for "morphine milligram equivalent." The measure of sales for "Opioids" includes codeine, dihydrocodeine, oxycodone, hydromorphone, hydrocodone, levorphanol, meperidine, methadone, morphine, oxymorphone, and fentanyl. Prescription opioid abuse treatment admissions are defined as admissions for a substance abuse problem with either "non-prescription methadone" or "other opioids or synthetics". Opioid abuse treatment admissions are defined as admissions for one of the prescription opioid abuse categories or substance abuse involving heroin, or having a diagnosis with "opioid dependence" or "opioid abuse." Prescription opioid overdoses are defined using ICD-10 codes X40–X44, X60–64, X85, or Y10–Y14 with contributing codes T40.2 and T40.3. Broader opioid overdoses are defined using ICD-10 codes X40–X44, X60–64, X85, or Y10–Y14 with contributing codes T40.0-T40.4. Sources: ARCOS, TEDS-A, MCOD.

Table 1.1: Summary Statistics (by State and Year)

	Mean	Median	SD	Min	Max	N
Eligibility (per cap.)	0.046	0.025	0.050	0.000	0.240	765
Enrollment (per cap.)	0.073	0.066	0.030	0.018	0.195	765
Opioid Sales (doses per cap.)	9.25	8.29	5.65	1.52	29.36	663
Oxycodone	2.68	2.30	1.95	0.09	16.56	714
Methadone	2.00	1.17	2.10	0.00	11.36	663
Opioid treat. (per 100k)	140.3	70.1	162.4	2.5	790.8	745
Prescr.	51.4	30.9	64.5	0.0	503.2	745
Opioid overdoses (per 100k)	4.35	3.76	3.12	0.16	22.97	612
Prescr.	3.35	2.72	2.68	0.00	19.63	612

Notes: Outcomes are, respectively: Medicaid eligibility per capita (among 19- through 64-year-olds for whom I am able to measure eligibility), enrollment per capita (among the 19-64-year-old population), per-capita sales of opioids, oxycodone and methadone (60MME doses, beginning in 1997), opioid treatment admissions and prescription opioid treatment admissions (per 100 thousand state residents, with 20 state-year observations missing), and opioid overdoses and prescription opioid overdoses (per 100 thousand state residents, excluding before 1999). Sources: ARCOS, MCOD, TEDS-A, Modified MWD, CPS.

1.4 Methodology

One obstacle towards estimating the effects of Medicaid eligibility is that eligibility is endogenous: an income shock might simultaneously affect an individual's Medicaid eligibility and their vulnerability to opioid addiction (e.g. if they become depressed because of a loss of income). If this is the case, then a naive regression of opioid overdoses on Medicaid eligibility could estimate a spurious positive effect driven not by Medicaid eligibility but by income. The solution that Currie and Gruber (CG, 1996) propose, which Borusyak and Hull (BH, 2023) build upon, is to construct an IV of "simulated" eligibility that is meant to be exogenous to variation in economic and demographic variables, isolating instead the policy generosity in the individual's state, compared to the policy generosity they would experience anywhere else in the country, given their income level. While I focus on BH's IV in this paper, I describe and use both IVs below, for comparison. After generating the IVs, I use them within Two-Stage Least Squares (2SLS) regressions to estimate the effects of state-level eligibility rates.

1.4.1 Instrumental Variables

I begin to build BH's IV by first defining the following variables for individual i at time t:

$Info_{it}$: i's family size, parental status, and employment in year t,

$Policy_{ist}(Info_{it})$ (eligibility threshold): maximum annual income that i would have to earn in year t to qualify for Medicaid in state s, given $Info_{it}$, and

$$elig_{ist}(Policy, Info, Income) := \mathbb{1}\{Income_{it} \leq Policy_{ist}(Info_{it})\}, \tag{1.1}$$

which is i's "simulated" eligibility if i were to live in state s at time t. Note that the above formula gives i's estimated actual eligibility if s is the state i actually lives in. I then define

$$\mu_{it} := E_{it}[elig_{ist}(Policy, Info, Income)|Income_{it}, Info_{it}], \tag{1.2}$$

which is simply the fraction of states in which i would be eligible for Medicaid at time t, given i's income and $Info$ values.

The "recentered" IV that BH propose is then calculated by subtracting μ_{it} from i's eligibility in i's state of residence, $elig_{it}$:

$$IV_{it} := elig_{it} - \mu_{it}. \tag{1.3}$$

This nets out the effect of an income change for i, because if i's income increases, then $elig_{it}$ will be less likely to be 1 and μ_{it} will also likely become smaller. In expectation, the two effects should cancel out across the full sample, such that the only way for IV_{it}

to predictably increase or decrease would be for i's state of residence to change its income eligibility threshold, or alternatively, for it to not change its threshold to keep up with other states' threshold changes.

When aggregating this IV to the state level, I take the weighted (with CPS weights) average value of the IV across all individuals in each state and year. I aggregate this IV to the state level for practicality: the state geographic level is the most granular geographic level of aggregation that is available both in the CPS data and for each of the outcome variables.

For a discussion of possible measurement error concerns, see Section 1.A1.3.

IV from Currie and Gruber (1996)

CG's IV is constructed, for state s and year t, by simply taking the whole CPS sample[34] from year t, and calculating the (weighted, using CPS weights) fraction of them who would be eligible for Medicaid if they lived in state s, given $Income_{it}$ and $Info_{it}$ for each individual i in year t.

Both IVs are conceptually similar, and since my regressions in this paper are run at the state level, I cannot utilize the increased precision that individual-level BH IVs could give me. Still, I choose to use the BH IV as my IV of choice because it is most directly interpretable as a shock to state residents' eligibility. I include main results using the CG IV[35] as a robustness check.

[34]This is the main difference between what I do versus what CG do – they instead use a national, randomized subsample of 3,000 individuals for each year in their sample

[35]In practice, the CG IV has slightly more variation (standard deviation of 0.056 vs. 0.050)

1.4.2 Two-Stage Least Squares

Having built the IVs, I estimate effects of eligibility policies using a Two-Stage Least Squares (2SLS). This strategy depends on the assumption that the chosen IV isolates random fluctuations in state Medicaid eligibility policies. While the IV allows me to eliminate the mechanical role that differences in family size and income distributions might have on a state's overall Medicaid eligibility, the IV may still be vulnerable to other political forces that could drive both states' Medicaid eligibility policies and their rates of opioid use. In order to attempt to both account for political factors and investigate their salience, I control for demographic variables and unemployment rates, which may influence political outcomes, as well as vote shares in major elections[36]. Additionally, I control for policies that might both be correlated with Medicaid eligibility policies (if their presence reflects, for example, a more proactive policy atmosphere in the state) and directly affect opioid use[37].

I also control for state and year fixed effects. State fixed effects are necessary to control for unobserved state-specific factors (inasmuch as they are constant over time) that influenced Medicaid policy and/or opioid use. Year fixed effects control for national trends influencing Medicaid enrollment and/or opioid use across the country from year to year. To account for correlation of standard errors within states over time, I additionally cluster standard errors at the state level.

In Section 1.5.6, I evaluate how much estimates change if I leave out fixed effects or control

[36]However, the unemployment rate and, according to Arteaga and Barone (2025), vote shares in presidential and other elections, may be affected by opioid abuse, and these variables may therefore be poor controls. Omitting these control variables from my main analysis does not meaningfully change results.

[37]My main set of controls are the following: population, share White, share Black, share Hispanic, share female, shares of six age groups (0–11, 12–17, 18–24, 25–44, 45–64, 65 and older), annual average unemployment rate, share Democratic vote in most recent general elections for president (In Section 1.5.6, I add general vote shares for senator and governor in a sample that omits Washington, DC, which holds neither type of election), and state-level shares (using CPS data) of the population of age 25 and older with no college, some (i.e. less than 4 years) college, and at least 4 years of college, as well as indicators for whether a state has an existing PDMP, a "must-access" PDMP, medical marijuana laws, active and legal marijuana dispensaries, and pill-mill laws.

variables, in order to understand which estimates are most sensitive to their omission and thus may also be more sensitive to any remaining omitted variable bias, even after including controls and fixed effects.

For my preferred regression specification, I weight each state-by-year observation equally, reflecting an interpretation of each state as its own "laboratory of democracy" with an often distinct policy trajectory. That said, I also separately estimate effects on my main outcomes using population-weighted regressions, which are more common in the literature. A comparison of both sets of results offers insights on heterogeneity in effects between large versus small states.

First Stage

For the first-stage regression, I use a simple OLS regression of the eligibility rate on the eligibility IV, for each state s and year t:

$$elig_{st} = \alpha_0 + \delta_s + \delta_t + \alpha_1 IV_{st} + \beta X_{st} + \varepsilon_{st}, \tag{1.4}$$

where $elig_{st}$ denotes the average[38] eligibility rate for s and t; IV_{st} is one of the two IVs, X_{st} contains control variables; δ_s and δ_t are fixed effects for state and year, respectively; α_0, α_1, β are coefficients; and ε_{st} is the residual term.

Second Stage

For the second stage, I regress opioid-related outcomes on the predicted values for eligibility for each state s and year t, using predictions generated from the first-stage regression. This regression follows the equation

[38] Weighted using CPS weights

$$Y_{st} = \gamma_0 + \rho_s + \rho_t + \gamma_1 \widehat{elig_{st}} + \xi X_{st} + \nu_{st}, \tag{1.5}$$

where Y_{st} is an opioid-related outcome (or enrollment)[39]; $\widehat{elig_{st}}$ is the predicted eligibility rate; γ_1 is the coefficient of interest that denotes the estimated effects of eligibility rates on outcome Y; X_{st} contains the same control variables as in the first stage; ρ_s and ρ_t are fixed effects for state and year, respectively; γ_0 and ξ are coefficients, and ν_{st} is a residual term.

1.5 Results

In this section, I present my results and discuss heterogeneous effects and robustness. My main results come from regressions that weight each state-by-year observation equally[40], but I also include results of population-weighted regressions for each of the main outcomes, which are more commonly used in the literature. Comparing these two weighting schemes offers a simple way to explore heterogeneity between states, with larger states playing a more dominant role in population-weighted regressions than in equal-weighted ones.

1.5.1 First Stage

First stage results are reported in Table 1.2, Panel A, Columns (2) and (3). Intuitively, the coefficients should be close to 1 if the IVs properly represent shocks to eligibility, and they are indeed statistically equivalent to 1. The BH IV (Column 2) has a particularly tightly estimated effect close to 1, which is perhaps to be expected, as individuals' own eligibility is part of the formula for constructing the BH IV. Both IVs are strong: the BH IV has an F

[39] In many regressions, I use log outcomes, which I modify by first adding 0.0000001 to the given variables' values in order to avoid taking the log of zero, which is undefined. One might suspect that effects on each of the outcomes may take more time to materialize, but I leave estimation of dynamic effects to future research.

[40] This allows me to treat each state equally as its own laboratory of democracy, in terms of the unique path that its Medicaid policies follow.

statistic of 5,336, and the CG IV has an F statistic of 172.

Panel B shows an effect on enrollment rates[41] of somewhat larger than 0.2, regardless of which model is used (Column 1: OLS; Columns 2-3: 2SLS). The estimate in Column 2 (using the BH IV) suggests that for every 100 individuals who are newly eligible in a state, about 20.3 are predicted to enroll[42]. Panel C is similar to Panel B but uses log enrollment counts instead of enrollment rates as the outcome. Effects on enrollment are reduced somewhat but still highly significant when weighting regressions by population instead of weighting each state-by-year observation equally (see Table 1.A3).

1.5.2 Opioid Sales

Table 1.3, Panel A, Column (2) reports that a 1 p.p. (about 22%, relative to the sample mean) increase in a state's Medicaid eligibility rate leads to an approximately 0.717% (sample mean: 9.25 60-MME doses per capita) increase in overall opioid sales in the same year. Dividing this effect by 0.203 suggests about a 3.5% effect for every 1 p.p. (about 13.7%, relative to the sample mean) increase in the state's enrollment rate.

I find similar effects when using the CG IV (Column 3) or an OLS regression of opioid sales on eligibility (Column 1). Effects are also similar, though less precise, if control variables are omitted (Column 3 of Table 1.A4 in the Appendix). The similar effect size suggests that remaining omitted variable bias may not be expected to meaningfully affect results for opioid sales. Effects disappear when omitting fixed effects (Columns 2 and 4), reinforcing their importance in the main specification.

If I weight the regressions by population (Table 1.A5, Panel A) instead of weighting each state-by-year observation equally, estimated effects are still positive but smaller and

[41] Recall that both eligibility rates and enrollment rates are estimated for states' populations aged 19 to 64 years old.

[42] As such, for the 2SLS effects that follow, each effect can be divided by 0.203 to approximate the effect of a commensurate increase in the state's enrollment rate (as opposed to the eligibility rate).

Table 1.2: Effects on Eligibility and Enrollment

	(1)	(2)	(3)
VARIABLES	Elig.	BH IV	CG IV
Panel A: First Stage (Effects of IVs on Eligibility)			
IV	–	1.015***	0.957***
		(0.014)	(0.073)
R^2	–	0.989	0.972
Outcome mean	–	0.046	0.046
Observations	–	765	765
Panel B: Effects on Enrollment Rate			
Eligibility	0.225***	0.203***	0.216***
	(0.056)	(0.056)	(0.064)
R^2	0.834	0.238	0.239
Outcome mean	0.073	0.073	0.073
Observations	765	765	765
Model	OLS	2SLS	2SLS
Panel C: Effects on log Enrollment			
Eligibility	2.82***	2.55***	2.78***
	(0.49)	(0.48)	(0.51)
R^2	0.979	0.178	0.179
Outcome mean	0.073	0.073	0.073
Observations	765	765	765
Model	OLS	2SLS	2SLS

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: The IVs used in Columns (2) and (3) are generated following Borusyak and Hull's (2023) and Currie and Gruber's (1996) methodology, respectively. Panel A shows the effects of each IV on state-by-year eligibility rates, using an OLS regression. Panel B shows the effects of either the eligibility rate (Column 1) or the eligibility rate, as proxied by an IV (Columns 2-3), on the state-by-year enrollment rate. Panel C is the same as Panel B, except that the outcome is state-by-year log enrollment. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Outcome means are in per-capita units. All 51 states and all years between 1996 and 2010 are included in these regressions. Sources: Modified MWD, CPS.

statistically insignificant. The differential results between weighting schemes suggest that Medicaid expansions in smaller states had a more positive effect on opioid sales than in larger states.

Looking at tighter categories within opioid sales (Table 1.4, Columns 1-3), I estimate positive effects on all three categories of opioid sales (oxycodone, hydrocodone, and methadone, although statistical significance is weak at best), suggesting that the estimated increase in overall pharmaceutical opioid sales is driven not by one type of opioid but by increased sales within various opioid categories.

When weighted by population (Table 1.A8, Columns 1-3), the estimated magnitudes change somewhat, with hydrocodone notably playing a larger and statistically significant role. The broad conclusion that the (in this case statistically insignificant) increase in overall opioid sales was spread across a variety of opioid types appears to hold when weighting regressions by population.

1.5.3 Opioid Treatment Admissions

Table 1.3, Panel B, Column (2) shows that a 1 p.p. increase in a state's Medicaid eligibility rate is estimated to lead to an approximately 3.21% (sample mean: 140 per 100 thousand) increase in overall opioid treatment admissions[43]. Dividing this effect by 0.203 suggests about a 16% effect for a 1p.p. (∼13.7%) increase in the state's enrollment rate.

As with opioid sales, estimated effects using the CG IV (Column 3) or an OLS regression (Column 1) are similar. Estimates are statistically insignificant and weaker when controls or fixed effects are omitted (Columns 3 and 4 of Table 1.A6, respectively), suggesting that their inclusion is important. The sensitivity to omitting control variables suggests that this outcome may be more sensitive to omitted variable bias than opioid sales.

[43]Note that this is number of admissions, not number of individuals treated. It is not uncommon for the same individual to obtain multiple treatments over time.

Table 1.3: Main Results

VARIABLES	(1) OLS	(2) 2SLS: BH	(3) 2SLS: CG
Panel A: Log Opioid Sales			
Eligibility	0.638**	0.717***	0.734**
	(0.273)	(0.253)	(0.317)
Observations	663	663	663
R^2	0.995	0.275	0.275
Outcome mean	9.25	9.25	9.25
Panel B: Log Opioid Treatment Admissions			
Eligibility	3.48**	3.21**	3.69***
	(1.33)	(1.26)	(1.39)
Observations	745	745	745
R^2	0.954	0.373	0.373
Outcome mean	140	140	140
Panel C: Log Opioid Overdoses			
Eligibility	1.070	0.800	0.382
	(0.948)	(0.885)	(0.902)
Observations	612	612	612
R^2	0.936	0.178	0.177
Outcome mean	4.35	4.35	4.35

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: Column (1) lists OLS effects of the state-by-year eligibility rate on the respective outcomes. Columns (2) and (3) list 2SLS effects using IVs to proxy for state eligibility rates. The IVs used in Columns (2) and (3) are generated following Borusyak and Hull's (2023) and Currie and Gruber's (1996) methodology, respectively. Panels A through C show the effects estimated by each model on state-by-year log opioid sales, log opioid treatment admissions, and log opioid overdose deaths, respectively. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The years 1996 and 2000 are omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. The years 1996 through 1998 are omitted in MCOD data. Units of outcome means are 60-MME doses per capita (Panel A), opioid abuse treatment admissions per 100 thousand residents (Panel B), and opioid overdose deaths per 100 thousand residents (Panel C). Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

When weighting these regressions by population (Table 1.A5, Panel B), estimated effects are still positive but fall meaningfully in magnitude and, when using the BH IV, become statistically insignificant. Similarly to the results for opioid sales, the differential results between weighting schemes suggest that Medicaid expansions in smaller states had a more positive effect on admissions than in larger states.

In Column (4) of Table 1.4, I estimate that the effect on prescription (instead of overall) opioid treatment admissions is positive and larger than the effect for overall admissions, albeit less precisely estimated. As prescription opioid treatment admissions account for less than half of all opioid treatment admissions, I interpret this finding as suggesting that there is a broad-based increase in opioid abuse treatment admissions from individuals dependent on prescription and/or illicit opioids. As with overall opioid admissions, this effect becomes smaller and statistically insignificant if I weight the regression by population (Table 1.A8, Column 4).

1.5.4 Opioid Overdose deaths

While the coefficient on opioid overdose deaths (using the preferred specification, shown in Column 2 of Panel C in Table 1.3) is also positive and of similar magnitude (0.800) to the coefficient for opioid sales, the standard error is too large for me to reject null effects. Similarly, effects are insignificant when using other specifications (see Columns 1 and 3 in Panel C and Columns 2 through 4 in Table 1.A7).

The effect on prescription opioid overdose deaths (Column 5 in Table 1.4) is nearly three times as large, in absolute terms, as the effect on overall opioid overdose deaths and is, in this case, negative, though it is again statistically insignificant. Combined, these two results offer suggestive evidence of divergent impacts on opioid overdoses, with a negative effect on prescription opioid overdose deaths that is somewhat outweighed by positive effects on other opioid overdose deaths[44].

If I weight these regressions by population (Table 1.A5, Panel C), I find that estimated effects on overall opioid overdose deaths are still statistically insignificant but are now negative with a similarly large absolute magnitude to the one estimated with equal regression weights. The effect (Table 1.A8, Column 5) on prescription opioid overdose deaths becomes even more negative, and is now statistically significant, suggesting that at the population level, these Medicaid expansions actually led to an estimable, and large, decrease in prescription opioid overdoses, although this effect appears to have still been tempered by a much less negative effect on illicit opioid overdose deaths. Comparing these results to results from equal-weighted regressions suggests that effects on opioid overdoses were more positive (or less negative), both overall and within each subcategory), in smaller states.

1.5.5 Heterogeneity

Each of the main results is more negative (or less positive) when regressions are weighted by population than when observations are weighted equally. This difference in effects suggests that effects are heterogeneous between states, with smaller states having more positive (or less negative) effects for each of the main outcomes than larger states[45].

Beyond state-level heterogeneity, I explore whether effects are heterogeneous along other dimensions (including race, time frame, and initial eligibility threshold), and while I find some differences, none of them hold statistical significance. This is likely in large part due to a lack of power within the state-by-year sample. For more details, see Section 1.A1.4.

[44] I call these other deaths “illicit” opioid overdose deaths, in the sense that they stem from opioids of black market origin

[45] Note that while effects on enrollment are somewhat smaller when weighting regressions by population, this alone does not explain the differential effects among the other outcome variables – if it did, then effects on opioid overdose deaths would be expected to become closer to zero when weighting by population, rather than changing signs (overall opioids) or becoming more negative (prescription opioids)

Table 1.4: 2SLS Effects Using BH IV (Subcategories: log Outcomes)

	Opioid Sales			Opioid Admissions	Opioid Overdoses
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Oxycodone	Hydrocodone	Methadone	Prescription	Prescription
Eligibility	0.581*	0.492*	0.941	5.26	-2.19
	(0.326)	(0.295)	(0.763)	(3.57)	(2.33)
Constant	-5.49	16.52	-9.99	16.48	-146.5*
	(12.28)	(11.67)	(23.65)	(42.78)	(81.2)
Observations	714	714	663	745	612
R^2	0.256	0.240	0.141	0.194	0.110
Outcome mean	2.68	1.30	2.00	51.35	3.35
Controls	X	X	X	X	X
Fixed effects	X	X	X	X	X
Time coverage	1997-2010	1997-2010	1997-2010	1996-2010	1999-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The first three columns show log effects on major categories of opioid sales: oxycodone, hydrocodone, and methadone. The second set of columns (4) splits log effects on opioid abuse treatment admissions into a category in which abuse of a prescription opioid was identified. Column (5) splits log effects on opioid overdose deaths similarly into cases in which a prescription opioid was identified as a cause of death. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The year 2000 is omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. Units of outcome means are 60-MME doses per capita (Columns 1-3), opioid abuse treatment admissions per 100 thousand residents (Column 4), and opioid overdose deaths per 100 thousand residents (Column 5). Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

1.5.6 Robustness

For my model to be valid, the IVs must be independent of any unobserved variable that affects one or more of the outcomes. This assumption could fail, for example, if other federal or state policy changes were correlated both with the IVs and with the outcomes.

It is unlikely that my results are driven by changes to federal programs other than Medicaid. For this to be the case, my variation in eligibility thresholds would have to correlate strongly enough with the between-state differences in and/or within-state changes to a separate program's characteristics over the course of the sample. As a test of whether this might be the case for one program affected by the 1996 welfare reforms, I repeat the main analysis, this time including a control for the annual share (calculated from CPS data) of households in each state with someone enrolled in SNAP (also known as food stamps). Including this control changes my results very little (see Table 1.A9 in the Appendix), suggesting that SNAP is not driving results.

Additionally, it does not appear that contemporaneous changes to alternative pathways to Medicaid are driving results. To check for this possibility, I estimate 2SLS effects (see Table 1.A12) on enrollment rates, restricting the sample for enrollment rates to individuals who report unemployment due to disability or retirement. Estimated effects for this sample are statistically insignificant, with somewhat smaller magnitudes to those estimated in Table 1.2. This finding suggests that my main results are not being driven by confounding effects from disabled individuals.

As one might be concerned that politics may influence both Medicaid policies and opioid use, I default to including controls for Democratic vote shares in general presidential elections. If I also include Democratic vote shares in general elections for senator and governor, estimated effects (columns 4-6 in Table 1.A10) are not statistically different than effects (columns 4-6 in the same table) estimated from a similar (i.e. excluding Washington, DC) regression in which these two variables are omitted (columns 1-3). Including these controls does reduce the effect on opioid sales modestly, but it is still highly statistically significant, while the effect on opioid treatment admissions changes little and becomes even more precisely estimated.

Another concern is measurement error. To test the sensitivity of my analysis to measurement error, I create a smaller sample that omits a subset of states that may be particularly susceptible to having either poorly measured (in some sense) eligibility thresholds or interstate opioid spillovers. To create this sample, I follow Hamersma (2013) in dropping Minnesota and Oregon from my analysis due to their large state health programs, and I also drop Vermont, because (as discussed in Section 3.4) the eligibility thresholds I use for that state may be more generous than the criteria that were actually applied to most Medicaid applicants. One more reason to drop Oregon is that it capped enrollment every year beginning in 2004, so the income thresholds I use were not as meaningful for determining enrollment during the affected years. Additionally, there may be spillovers in opioid access from Florida, so I also drop Florida and its adjacent neighbors (South Carolina, Alabama, and Georgia) to reduce the role that interstate spillovers might play in affecting results. Table 1.5 shows that when applying the main 2SLS estimation strategy to this sample, effects on all outcomes are similar to effects across the full sample. This suggests that measurement error and cross-state opioid spillovers do not drive results.

The data on treatment admissions has reporting differences between states and is not balanced across years, with ten different states missing data over a combined 20 years. Table 1.6 shows that when I omit these ten states from the sample, the effect on opioid treatment admissions falls and also becomes less precise, though still positive and significant with $p < 0.1$. The effect on opioid sales likewise falls and becomes less precise, suggesting that the smaller effect on opioid treatment admissions within this sample is driven by real heterogeneity in effects on opioid outcomes more generally between subsets of states, and not by differential TEDS-A reporting practices among the unbalanced states.

Table 1.5: 2SLS Estimates (Within Subset of States: log Outcomes)

	(1)	(2)	(3)
VARIABLES	Sales	Admissions	Overdoses
Eligibility	0.822***	3.48**	0.779
	(0.269)	(1.37)	(0.937)
Constant	5.30	55.1*	-0.789
	(8.36)	(28.3)	(39.42)
Observations	572	641	528
R^2	0.279	0.369	0.188
Outcome mean	9.16	143	4.42
Controls	X	X	X
Fixed effects	X	X	X
State coverage	44 states	44 states	44 states
Time coverage	1997-2010	1996-2010	1999-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates, within a restricted sample of states: I remove states (OR, MN, VT) with thresholds that may be especially poor reflections of true access to subsidized healthcare through Medicaid or a similar program, and FL (a pill-mill hub leading up to 2010) and its adjacent neighbors, AL, SC, and GA. Outcomes are, respectively, log opioid sales, log opioid abuse treatment admissions and log opioid overdoses. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The year 2000 is omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. Units of outcome means are 60-MME doses per capita (Column 1), opioid abuse treatment admissions per 100 thousand residents (Column 2), and opioid overdose deaths per 100 thousand residents (Column 3). Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

Table 1.6: 2SLS Estimates (Within Balanced TEDS-A Sample: log Outcomes)

	(1)	(2)	(3)
VARIABLES	Sales	Admissions	Overdoses
Eligibility	0.646*	2.10*	0.823
	(0.349)	(1.22)	(0.976)
Constant	10.4	81.6**	-31.3
	(9.7)	(32.8)	(52.8)
Observations	533	615	492
R^2	0.292	0.387	0.144
Outcome mean	9.15	153	4.43
Controls	X	X	X
Fixed effects	X	X	X
State coverage	41 states	41 states	41 states
Time coverage	1997-2010	1996-2010	1999-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates, within a restricted sample of states. I remove states that have at least one year missing in TEDS-A data: AK, AL, AR, AZ, DC, IN, KY, MS, WV, and WY. Outcomes are, respectively, log opioid sales, log opioid abuse treatment admissions and log opioid overdoses. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The year 2000 is omitted for opioid sales due to incomplete data. Units of outcome means are 60-MME doses per capita (Column 1), opioid abuse treatment admissions per 100 thousand residents (Column 2), and opioid overdose deaths per 100 thousand residents (Column 3). Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

1.6 Mechanisms

I have shown that increasing Medicaid eligibility for able-bodied, non-elderly adults led, during the time period between 1996 and 2010, to an increase in pharmaceutical opioid sales and an increase in opioid treatment episodes, with knock-on effects reducing prescription opioid overdoses but possibly increasing other opioid overdoses. These results appear to be more positive in smaller states, but otherwise broad-based across subgroups of the sample population and across types of opioids.

In this section, I discuss possible explanations for the following questions: Why did opioid sales increase? Did opioid abuse increase? Why did opioid abuse treatments increase? And, what explains the apparent divergent results in overdose deaths? I propose hypotheses that might explain the results, and I explore each proposed hypothesis to the extent possible.

1.6.1 Why Did Opioid Sales Increase?

An increase in opioid sales would be expected due to an increase in access to a generous health plan like Medicaid, if the dominant effect of this health plan is to dramatically reduce prescription prices for new enrollees. Other possible effects of Medicaid might counteract this effect, however, including preventive care or opioid abuse treatments, which might help reduce patients' future need for opioid prescriptions. Prescribing restrictions[46] might also dampen any effect on access to prescriptions. My main results are consistent with the effect of reduced prescription prices dominating opposite-direction effects, resulting in a net positive effect of increased access to Medicaid on state-level opioid sales.

The dampening effect of prescribing restrictions on these effects appears to be visible when comparing the equal-weighted regression results to the population-weighted regression

[46] By regulators, or alternatively, coverage restrictions within the insurance plan

results. I find that effects on opioid sales are less positive in larger states, which runs parallel with Alpert et al.'s 2022 findings that five states with triplicate states (four of which are among the largest states in the US: CA, IL, NY, and TX) in 1996 had relatively smaller increases in opioid abuse and overdoses in the following years, and that this effect appears to be driven by reduced pharmaceutical advertising to physicians in the treated states. The fact that I find smaller effects of expanded Medicaid programs on opioid sales in the largest states suggest that Medicaid expansions interacted with state regulations, such that in states with regulations that effectively restricted opioid prescriptions, the net effect of Medicaid expansions on these prescriptions was more muted.

1.6.2 Did Opioid Abuse Increase?

The positive effects that I find on opioid sales would be expected to have caused increased opioid abuse, since these pills are highly addictive, and the addiction risk was poorly understood at the time. Moreover, an increase in prescription opioid abuse would be expected to have further spilled over into an increase in illicit opioid abuse, since for every new cohort of abusers, some subset of users would be expected to become so dependent that they begin using heroin, which was cheaper than prescription opioids in terms of street prices (Quinones, 2015).

In an attempt to empirically explore whether there was an increase in destructive opioid abuse, I split the sample of treatment admissions by whether clients were referred to treatment by one of the following sources: judicial (i.e. from a criminal-justice related origin), health (i.e. from a healthcare provider, excluding drug abuse treatment providers), or individual (i.e. self or another individual) referrals. I estimate (in Table 1.7) that effects on treatment admissions are particularly pronounced among treatments that come from health and individual referrals, while the point estimate for treatments from judicial referrals is only about half as large.

The differential results from different referral sources suggest that there was an increase in opioid abuse, and that this increase fed into the increased opioid abuse treatment admissions that I estimate among my main results. Most importantly, I can reject the hypothesis that the effects on treatments are driven entirely by health providers catching opioid abuse among new enrollees and sending them to treatment. If anything, treatment admissions rise even more for individual referrals, suggesting a rise in clients with opioid abuse that had become severe enough that either they or a close contact determined an intervention was necessary. The much smaller estimate for judicial referrals also suggests that there was not some other effect of Medicaid that simply caused treatments from all referral sources to increase similarly, although I cannot statistically reject the null hypothesis that effects are equal between all three referral types.

1.6.3 Why Did Opioid Abuse Treatments Increase?

While I have suggested that at least part of the increase in opioid abuse treatments could be explained by an increase in opioid abuse, an alternative possibility is that the increase in opioid abuse treatments is driven by new Medicaid enrollees with prior addictions, who can now access treatment that they could not previously access. If I split the sample of treatment episodes between clients with reported Medicaid coverage and clients who report having no insurance[47], I estimate large and positive, though not statistically significant, effects for each group (shown in Table 1.8)[48], suggesting that the increase in opioid abuse treatments is likely spread out beyond just being driven by Medicaid beneficiaries. As such, differential access

[47] Which is perhaps the most likely alternative for many new enrollees.

[48] I again find similar effects between the two groups, even when further restricting the data to specific referral sources. These results are shown in Table 1.A11. Note, however, that the results from these tables are only suggestive rather than definitive, as they could be heavily biased by sample selection: about 53.4% of treatment admissions are missing a value for insurance coverage, and the estimated effect on treatment admissions on this population is a statistically insignificant -3.76 (std. err.: 4.47). How much of this negative effect is distributed among individuals who are enrolled in Medicaid vs. not is unknown.

Table 1.7: 2SLS Effects on Admissions (Split by Referral Source: log Outcomes)

VARIABLES	(1) Admissions	(2) Admissions	(3) Admissions
Eligibility	1.84	3.67*	4.30**
	(1.97)	(1.88)	(1.95)
Constant	214*	285**	147*
	(110)	(132)	(80)
Observations	745	745	745
R^2	0.090	0.114	0.089
Outcome mean	24.4	10.4	73.6
Controls	X	X	X
Fixed effects	X	X	X
Time coverage	1996-2010	1996-2010	1996-2010
Sample	Jud. Ref.	Health Ref.	Indiv. Ref.

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome in these regressions is log opioid abuse treatment admissions, restricted to admissions stemming from different referral sources: Column (1) restricts the sample to judicial (i.e. criminal justice) referrals, Column (2) restricts the sample to healthcare referrals, and Column (3) restricts the sample to individual referrals. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. Units of outcome means are opioid abuse treatment admissions per 100 thousand residents. Sources: TEDS-A, Modified MWD, CPS.

to treatment options appears not to drive effects on treatment admissions[49].

Another alternative explanation for the increase in opioid abuse treatments, however, is supply-side effects. For example, there could be something about an influx of Medicaid dollars in a state that spurs the supply curve for substance abuse services to shift outward. I explore[50] this with data from the N-SSATS survey of substance-abuse treatment facilities, and while I find some evidence of increasing market share from hospital-based providers, I do not find any indication of increased capacity among the N-SSATS samples[51].

Taken together, the hypothesis that appears most consistent with my results is that the driving factor behind the increase in opioid abuse treatment episodes is an increase in opioid abuse, rather than increasing access to treatment.

1.6.4 What Explains the Apparent Divergent Results for Overdose Deaths?

One possible explanation for why I find that prescription opioid overdoses, but not illicit opioid overdoses, fall significantly, is that users of illicit opioids are likely more heavily addicted. As such, these individuals may both be more likely to suffer a fatal overdose in their lifetimes and, even though abuse treatment abuse admissions increase similarly for both types of opioids, these treatments may be less effective in ending addiction for the more heavily addicted population[52].

[49] This may not be so surprising, as it is fairly common in the TEDS-A dataset for clients not to be charged for the services they receive.

[50] I discuss this further in the second chapter of this dissertation.

[51] This does not necessarily rule out an increase in capacity among facilities missing from the N-SSATS survey, however (Meinhofer and Witman, 2018).

[52] This might be especially true during the time period of focus, before medically-assisted opioid treatment was as widely used as it is now.

Table 1.8: 2SLS Effects on Admissions (Split by Insurance Coverage: log Outcomes)

	(1)	(2)
VARIABLES	Admissions	Admissions
Eligibility	14.5	19.8
	(13.6)	(14.0)
Constant	215	418
	(314)	(350)
Observations	745	745
R^2	0.202	0.178
Outcome mean	18.8	48.3
Controls	X	X
Fixed effects	X	X
Time coverage	1996-2010	1996-2010
Sample	Medicaid	No Insurance

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome in this regression is log opioid abuse treatment admissions, restricted to admissions for clients who are either covered by Medicaid (Column 1) or have no insurance (Column 2). Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The year 2000 is omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. Units of outcome means are opioid abuse treatment admissions per 100 thousand residents. Sources: TEDS-A, Modified MWD, CPS.

1.7 Discussion

In this section, I attempt to interpret the effects I have estimated, in terms of how much of the opioid epidemic these effects can explain, where my estimates fit within the broader literature, and what we might learn more broadly about other insurance programs.

1.7.1 How Much of the First Wave is Explained by Estimated Effects?

The accusations that helped motivate this paper were that Medicaid has a history of "financing" or "fueling" the opioid epidemic (Eberstadt, 2017; U.S. Senate HSGAC, 2018). To get a rough estimate for how much of the First Wave is explained by my estimated effects of Medicaid expansions, I do the following exercise.

First, I assume eligibility affects opioid use only through its effect on Medicaid enrollment[53]. This could include effects on new enrollees, as well as on their neighbors and broader communities, but all of these effects are assumed to be mediated by the effects of increased eligibility on increased enrollment within each given state.

With this assumption, I can estimate the fraction of the increase in each outcome that can be explained by eligibility's effect on enrollment, by comparing the 2SLS effects[54] of eligibility on log enrollment (β_e) to the 2SLS effects of eligibility on a log outcomes, and then multiplying the ratio of the two effects by the ratio of the two variables' percent increase over the time period in which both variables are present.

Mathematically, if I call the effect on log opioid sales β_s and signify the percent change in enrollment and opioid sales from 1997 to 2010 as $\%\Delta e$ and $\%\Delta s$, and if I also call the

[53]Specifically, I assume that it only operates through enrollment for adults of ages 19-64, and not through changes in enrollment for other age groups.

[54]I use population-weighted estimates for this calculation, in order to better reflect the overall role of Medicaid on national trends.

estimated fraction F_{es}, then this gives me the following equation:

$$F_{es} = \frac{\%\Delta e}{\%\Delta s} * \frac{\beta_s}{\beta_e}. \tag{1.6}$$

Plugging in the values for these numbers for the samples from 1997 through 2010[55], I find

$$F_{es} = \frac{0.634}{7.10} * \frac{0.438}{2.07} = 0.0189. \tag{1.7}$$

Similarly, I obtain the following estimate for opioid abuse treatment admissions from 1996 through 2010:

$$F_{et} = \frac{0.466}{0.729} * \frac{1.70}{2.14} = 0.508. \tag{1.8}$$

These estimates suggest that policy-induced increases in Medicaid enrollment caused 1.89% of the total growth of prescription opioid sales from 1997 through 2001 and 50.8% of the total growth of opioid abuse treatments. Notably, these numbers are only rough estimates, and the larger estimate for treatments is particularly sensitive to my choice of what time frames to focus on[56]. Regardless, the broad takeaway is that these Medicaid expansions appear to have played a very small role, at most, in the massive increase in opioid sales during the First Wave, but that they played a more important role in increasing opioid abuse treatments during the same time period.

[55]Note that for this calculation, I re-estimate effects on log enrollment, using only data from 1997 through 2010, to obtain an coefficient of 2.07

[56]For example, my estimate for F_{et} would be larger if I use values from 1997-2010 instead of 1996-2010, due to the declining Medicaid enrollment that occurred between 1996 and 1999.

1.7.2 How Do My Results Compare to Other Estimates in the Literature?

Enrollment

I find that in the sample time period, for every 100 newly eligible individuals, about 20.3 are estimated to enroll in Medicaid if I weight each state and year equally. This number falls modestly to 15.8 (std. err.: 3.70) for every 100 if I weight each state-year observation by population, which is more comparable to what is done in other papers. This latter effect is similar to those found in the literature, although estimates vary somewhat based on methodology, sample selection, and context[57].

Opioid Sales

My findings for opioid sales imply a break from the literature. While Sharp et al. (2018) finds statistically insignificant (and negative) effects from post-ACA expansions, and Baicker et al. (2017) find what appear to be null effects[58] from the Oregon Health Insurance Experiment in 2008, I find positive effects on prescription opioid sales, both overall and within major types of opioids. The effects I find are small, relative to the overall increase in opioid sales, but they do suggest that pre-2010 Medicaid expansions played a role in increasing access to opioids that post-2010 expansions appear not to have played.

[57] Hamersma and Kim (2013) and Busch and Duchovny (2005) both find a 14.8% take-up rate for low-income parents, and Busch and Duchovny (2005) also estimates a take-up rate of 11.5% for their full sample. Busch and Duchovny (2005) use the same dataset and a similar estimation strategy to this paper, and I find that my estimates approach their full-sample estimate if I restrict my sample to 1996-2001 (theirs is 1995-2001). The estimated effect of the 2008 Oregon Health Insurance Experiment was about 25% – a larger effect that could be explained by information effects, since individuals made eligible by the lottery (which they had actively chosen to enter) were likely more aware of their eligibility than they might be in a typical expansion. Post-2010 expansions are estimated to have take-up effects within a similar range (Frean, Gruber, and Sommers, 2017). Expansions for more vulnerable populations (pregnant women: Currie and Gruber, 1996; children Cutler and Gruber, 1996; uninsured or poorer individuals: Busch and Duchovny, 2005) tend to have larger marginal take-up effects.

[58] Note, however, that Baicker et al. (2017) are unable to capture the full community effects that I am estimating, since diversion of opioids may play an important role and would bias experimental treatment effects toward zero.

Powell, Pacula, and Taylor (2020) find much larger effects from Medicare Part D: about an 8-10% increase (relative to the sample mean) in 60MME doses of opioids sold per capita, for every extra 1 p.p. (mean: 12.4%) in the underlying elderly share of a state's population (equivalent to a 1p.p. increase in the eligibility rate), which suggests an effect that is several times larger than my estimate for each 1 p.p. difference in a state's Medicaid eligibility rate. This difference in magnitudes is consistent with the greater exposure to opioid prescriptions that elderly patients have, as well as Baker, Bundorf, and Kessler's 2020 findings that Medicare Part D induces more prescriptions than a comparable comprehensive plan (Medicare Advantage) like Medicaid.

Opioid Treatment Admissions

The positive effect I find on treatment admissions is consistent with findings based on Medicaid expansions that occurred after the ACA – suggesting a decades-long trend of Medicaid expansions leading to increased opioid abuse treatment utilization. The magnitude that I estimate is large but appears to be within the ranges that have been estimated for post-ACA expansions (Meinhofer and Witman, 2018; Sharp et al., 2018; Wen et al., 2017), as well as the imprecise estimate found from the 2008 Oregon Health Insurance Experiment (Baicker et al., 2017). One difference is that the effect of post-ACA expansions appears to have been driven by access among new Medicaid enrollees (Meinhofer and Witman, 2018), whereas this I find no evidence that this is the case with the pre-ACA expansions that I focus on. If taken at face value, this difference could be explained by the differing effects of each set of expansions on opioid sales.

Meinhofer and Witman (2018) also find that there was a supply-side response in the opioid abuse treatment services industry after post-ACA expansions. They find that licensed opioid treatment programs increased their coverage of Medicaid beneficiaries and that the total number of facilities increased. This increase in the number of facilities was not, however, visible in the N-SSATS data on licensed opioid treatment programs and was only observable in a database for specialized clinics focused on treating opioid dependency by prescribing and dispensing buprenorphine. As buprenorphine was not approved by the FDA until 2002, and its early use faced administrative hurdles that were only slowly reduced (Jones et al., 2018), I do not expect buprenorphine to have played a major role in most of the time period I look at, but there may have been other facilities not captured in N-SSATS that could have had important supply-side changes between 1996 and 2010, just like Meinhofer and Witman (2018) find for later expansions.

Opioid Overdose Deaths

My finding that opioid overdose deaths may have fallen (when using population weights, as the other papers do), because of increasing Medicaid access, is in line with findings from the literature. Venkataramani and Chatterjee (2019) and Snider et al. (2019) each find that a broader category of overdose deaths (not just from opioids) fell after the expansions that they focus on from this same time period, and Wen et al. (2020) have a similar finding for opioid-related inpatient hospitalizations from 2005-2013. However, I provide the first suggestive evidence from this time period that this effect is dominated by prescription opioid overdoses, and that effects on illicit opioid overdoses were much less negative, to the point that even effects on overall opioid overdose may have been positive in smaller states. As with opioid sales, my results for opioid overdoses imply that Medicaid expansions had a much more benign role in the opioid epidemic than Powell, Pacula, and Taylor (2020) estimate for Medicare Part D.

1.7.3 External Validity

As made clear by Powell, Pacula, and Taylor (2020), the increase in opioid sales that I estimate is not unique to Medicaid. Other comprehensive insurance programs (including private programs) may also be posited to have had similar effects to what I estimate for Medicaid, which would likely depend on the following program characteristics: how much they subsidized prices for opioid prescriptions; whether they had a mechanism for limiting access to pills, at least for individuals at most risk of opioid abuse; to what extent they covered and/or encouraged preventive care, alternatives to opioid prescriptions, and opioid abuse treatments; whether physicians were incentivized to keep visits short, allowing less time to provide a personalized treatment plan; and how vulnerable their beneficiary populations were to needing and abusing opioid prescriptions. Future research on other health insurance plans during the First Wave would be helpful toward better understanding the role of each of these factors in both facilitating access to needed care and preventing overuse of medical services.

1.8 Conclusion

My estimates confirm parts of both types of arguments that have previously been made about the role of Medicaid in the opioid epidemic. U.S. Senate HSGAC (2018) was correct to posit that Medicaid may have helped finance increasing prescription opioid sales during the opioid epidemic. This boost to sales appears to have led to increased abuse and, in some states, possibly even an increase in opioid overdose deaths. At the same time, the estimated effect on sales is very small and comes nowhere near explaining the overall rise in prescription opioid sales during the First Wave. Moreover, increasing use of opioid admissions in affected states appears to have significantly counteracted the effects of increases in opioid abuse on overdoses.

I also find evidence that Medicaid's effects varied according to the regulatory and/or pharmaceutical advertising environments in different states, which implies that effective policies could potentially be implemented over, or even within, Medicaid programs to help them reduce insurance payments for unnecessary medical care. One such policy, which has been widely implemented since 2010[59], and yet has been poorly researched, is Medicaid Lock-In Programs (MLIPs), which can restrict individuals' coverage for scheduled prescriptions if they are deemed to be at high risk of abusing themThe Pew Charitable Trusts, 2015; Roberts, Gellad, and Skinner, 2016; Roberts and Skinner, 2014. More research on this and other policy ideas (such as Prior Authorization and ideas discussed in Laverdiere et al., 2016) may prove useful toward helping policymakers design Medicaid and other health insurance programs with more effective mechanisms to limit overprovision of medical services without meaningfully compromising access to needed care.

[59] And existed to some degree even before 2010

1.A1 Appendix

1.A1.1 Constructing Eligibility Measures

When constructing individuals' eligibility for Medicaid using their information in the CPS, I restrict the sample to each household head and their spouse or unmarried partner[60], and I then define the following variables for this restricted sample.

Family size: A sum of one (for self), plus one if married and spouse[61] lives in same household, plus the number of the household head's children, step children, and foster children who are under 19 years old and living in the home.

Family income: the sum of own income, plus married spouse's income, plus income for any of the household head's children, step children, and foster children who are under 19 years old and living in the home, minus any income that each of these individuals receives from welfare sources.

Parenthood: 1 if the household head has any children, step children, and foster children who are under 19 years old and living in the home; 0 otherwise.

Employment: 1 if worked more than 0 weeks last year, 0 otherwise. This is interpreted as a flag for whether there was at least some time last year in which the individual could have qualified for Medicaid under the (often higher) threshold for employed individuals.

I then define an individual's eligibility as 1 if their family income lies at or below their state's income threshold for their family size, parental status, and employment status, and

[60] Defining these variables for other individuals in the household is difficult, if not impossible, because the relationship variable is given only with respect to the household head. Also note that the CPS data excludes individuals "living in institutions (for example, a correctional institution or a residential nursing or mental health care facility) and those on active duty in the Armed Forces" (U.S. Bureau of Labor Statistics, 2025). The size of these demographics is small compared to the total non-elderly adult population in the US, but their omission from the CPS data may still induce some level of bias into results.

[61] i.e. an unmarried partner is not counted as a family member in this definition, as they are less likely to count as part of the family for Medicaid eligibility purposes.

0 otherwise. I also define eligibility as 0 if the applicable income threshold is recorded as 0% of the applicable FPG.

1.A1.2 Determining Appropriate MME Conversion Factors

For most opioids in the ARCOS sample that I use, I use MME conversion factors from the 2022 CDC table referenced in Yessaillian et al. (2024), Table 2, which is currently unavailable at its original location on the CDC's website. I supplement this for remaining opioids with information in NIH HEAL Initiative (2024).

The one opioid in the ARCOS sample with the least overall guidance that I could find on what MME factor to use in this context is also the strongest of the opioids in the sample: fentanyl. The ARCOS data that I use does not distinguish between types of fentanyl that are sold, and estimated MME factors range widely depending on the method of use (NIH HEAL Initiative, 2024; Utah Department of Health and Human Services, n.d.; Yessaillian et al., 2024). I choose to use an MME conversion factor of 100 for fentanyl, which is within the range of values and is consistent with the potency of fentanyl patches (Utah Department of Health and Human Services, n.d.; Stanley, 2005).

1.A1.3 Measurement error

While the CPS data are rich, there are still important details that I do not know about respondents. These details include whether an individual's household size or state of residence has changed meaningfully in the last year, and if so, when it happened. I could build a proxy for whether a birth might have happened last year, using the age of the youngest child, but this proxy still would not tell me in what month or even what year they were born. I also do not know if there was a death or a marriage (or new unmarried partnership) in the family, or if a child or unmarried partner left home last year. There is a variable for the individual's year of their most recent divorce, but this variable is only available once every two years instead of annually, so I ignore this variable.

There is also a variable for whether the individual moved from another house within the past year (i.e. since the beginning of last March), and whether this move crossed state or country boundaries. However, this does not tell me if they spent any time living in the current state last year, and if so, how much time. For example, they could have moved last month and not lived in the current state at all last year – or they could have moved last April and spent eight months living in the current state last year. As such, I make no use of this variable.

In addition, for those individuals who had a spell of unemployment last year, it is generally hard to tell exactly what their families' monthly salary was during each given spell of each family member's employment or unemployment, so it is possible that one or more adult family members were (in)eligible for Medicaid even if the yearly total income makes it look like the opposite was the case. The ideal scenario would be to know for each month, whether an individual was employed in the eyes of Medicaid during that month. But that is impossible within these data.

As a result of the imprecise knowledge I have about all of these variables from last year, some individuals may report that they were enrolled in Medicaid last year, but were actually enrolled only in another state, and I would have no way of knowing that. And the opposite may also occur: some individuals may appear ineligible for Medicaid given their current family size, but they may have had an unobserved change to their family structure last year, before which they were eligible for Medicaid.

Since eligibility thresholds, family size, family income, parenthood, and employment status (not to mention that I currently ignore possible eligibility through alternative routes, such as disability and pregnancy) are all observed with some noise, there is inherent measurement error in the eligibility data. Any unobserved mismeasurement in family size, income, parenthood, and employment may plausibly be assumed to be uncorrelated with the eligibility IV, and may make results less precise but should not bias them. Mismeasurement in eligibility thresholds, however, could bias the IV, so in Section 1.5.6, I explore whether results might change when using time frames for which the thresholds I use might plausibly be more trustworthy.

For eligibility thresholds specifically, one source of measurement error is that my eligibility thresholds only reflect one pathway into eligibility. There are other pathways that I am not capturing, including through being a child, being elderly, or being disabled or pregnant, as well as through idiosyncratic state health programs that provide separate pathways into health insurance that often bear similarities to Medicaid. I exclude children and the elderly for this reason, and as long as other pathways do not experience changes that interact with changes in my IV, then the existence of other pathways does not pose an identification problem. To explore whether changes to the disability pathway drives results, I estimate 2SLS effects (see Table 1.A12) on enrollment rates, restricting the sample for enrollment rates to individuals who report unemployment due to disability or retirement. The estimates I find are statistically insignificant, but their magnitudes are fairly similar to the magnitudes estimated in Table 1.2. This result suggests that my main results are not being driven by confounding effects from alternative pathways from disabled individuals, although the standard errors are wide enough that I cannot rule out large differences in effects within this population compared to my main results[62].

There is also reporting error in the enrollment data, which makes the second-stage results for that variable less precise, but it should not induce bias into these results as long as the measurement error in enrollment is independent from the IV.

[62] As an aside, note that the exclusion restriction would not be violated if some fraction of already-eligible, disabled individuals were induced into enrollment by eligibility expansions for non-disabled adults, e.g. by increased awareness about their own eligibility (these have been referred to as woodwork effects).

1.A1.4 Heterogeneity: Details

Heterogeneity by Initial Eligibility Threshold

Currie and Gruber (1996) find more pronounced effects from "targeted" (i.e. more restricted in terms of who the expansion applies to) Medicaid expansions than "broad" expansions in their analysis on childbirth-related outcomes. One way to use this insight in this context is to simply compare effects in states where Medicaid eligibility for employed parents was initially restricted to lower percentages of the FPL, versus higher percentages. To do this, I split the sample in about half at 50% of the FPL: for the 26 states with a 1996 eligibility threshold of less than 50% of the FPL, I call them the "Low" states, and the remaining 25 states are called the "High" states. Conceptually, any expansions among the "Low" group are bringing in a proportionally more high-poverty population than expansions among the "High" group, so expansions among the "Low" group are relatively targeted towards lower-income individuals.

The results, shown in Table 1.A13, suggest somewhat larger effects for the states that started with "High" thresholds, although this difference is not statistically significant. As such, effects do not appear to be meaningfully driven by either group of states.

Heterogeneity by Race

Table 1.A14 shows that effects on opioid abuse treatment admissions and opioid overdoses are larger for Black individuals (Columns 2 and 4), compared to White individuals (Columns 1 and 3), although the differences are not statistically significant. This suggests that the main results are not driven by one race or another.

Heterogeneity by Time

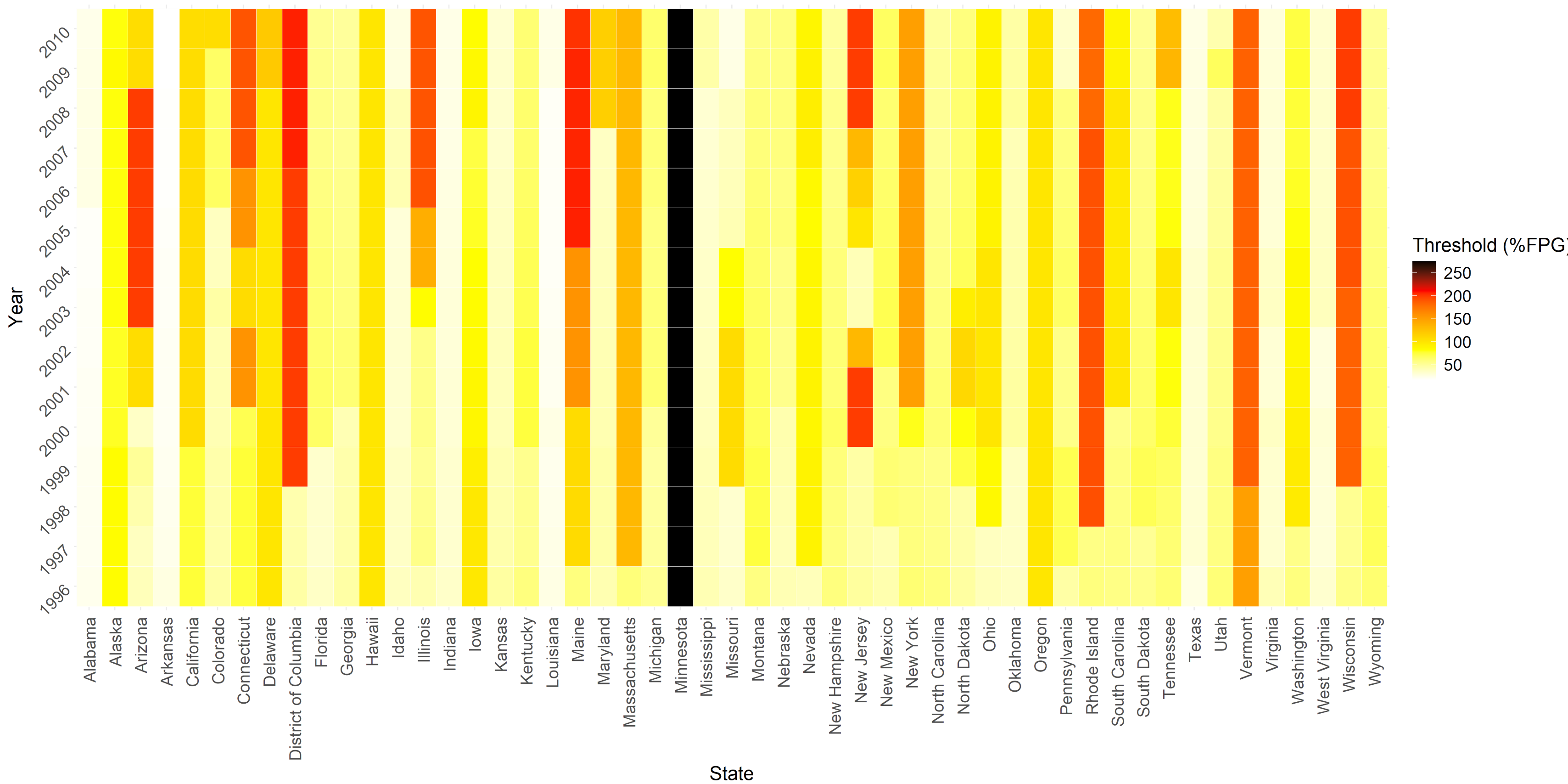
I evaluate heterogeneity over time by splitting the sample into two overlapping ten-year windows: 1996 to 2005 and 2001 to 2010.

First-stage effects using the preferred IV (BH) are close to one and statistically significant in both time periods (see Column 2 in Panel A of Table 1.A15). This helps assuage any concerns of the IV's relevance either during the earlier period, due to possible measurement error and (for unemployed parents) missing values in the eligibility thresholds. During the later period, 2SLS estimates for enrollment rates (comparing Columns 5 and 2 in Panel B) are somewhat smaller, possibly due to a deceleration in new Medicaid expansion policies and confounding effects from the launch of Medicare Part D, but they are still large enough to imply meaningful Medicaid takeup on the margin.

Second-stage effects on opioid variables are imprecisely estimated during these shorter time windows, so I do not attempt to compare these effects between both time windows.

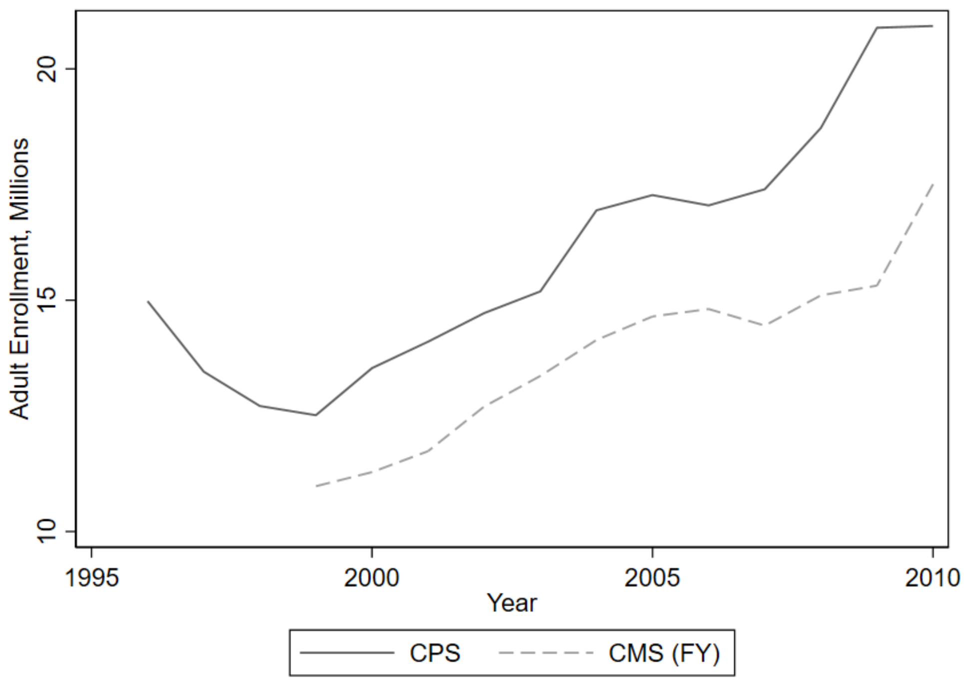
1.A1.5 Figures

Figure 1.A1: Medicaid Income Eligibility Thresholds for Employed Parents



Notes: FPG stands for "Federal Poverty Guideline." Thresholds based on data for family sizes of three. Source: Modified MWD.

Figure 1.A2: Comparison of CPS to CMS Enrollment Measures



Notes: Total enrollment measure per year, for adults aged 19 years old and older, from either CMS or CPS data. CPS total enrollment values are calculated using ASEC weights. CMS data are in fiscal-year frequency, while CPS data are annual.

1.A1.6 Tables

Table 1.A1: State Eligibility Thresholds (%FPG) for adults

	Parents				Childless			
	Employed		Unemployed		Employed		Unemployed	
State	1996	2010	1996	2010	1996	2010	1996	2010
AK	83	81	–	78.5	0	0	0	0
AL	23	24	–	11	0	0	0	0
AR	27	17	–	12	0	0	0	0

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Table 1.A1 (continued)

	Parents				Childless			
	Employed		Unemployed		Employed		Unemployed	
State	1996	2010	1996	2010	1996	2010	1996	2010
AZ	40	106	–	100	0	100	0	100
CA	76	106	–	100	0	0	0	0
CO	47	106	–	100	0	0	0	0
CT	75	191	–	185	0	56	0	56
DC	47	207	–	200	0	133	0	133
DE	100	120.5	100	100	100	100	100	100
FL	36	53	–	20.5	0	0	0	0
GA	47	50	–	24.5	0	0	0	0
HI	100	100	100	100	100	100	100	100
IA	98	83	–	28.5	0	0	0	0
ID	38	27	–	21.5	0	0	0	0
IL	43	191	–	191	0	0	0	0
IN	35	25	–	19.5	0	0	0	0
KS	48	32	–	26.5	0	0	0	0
KY	59	62	–	35	0	0	0	0
LA	26	25	–	11.5	0	0	0	0
MA	60	133	–	133	0	0	0	0
MD	43	116	–	116	0	0	0	0
ME	59	203	–	200	0	100	0	100
MI	53	64	–	38	0	0	0	0
MN	275	275	275	275	75	75	75	75

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Table 1.A1 (continued)

	Parents				Childless			
	Employed		Unemployed		Employed		Unemployed	
State	1996	2010	1996	2010	1996	2010	1996	2010
MO	35	25	–	19.5	0	0	0	0
MS	42	46	–	25	0	0	0	0
MT	58	56	–	32.5	0	0	0	0
NC	59	49	–	36.5	0	0	0	0
ND	48	59	–	39.5	0	0	0	0
NE	42	58	–	46.5	0	0	0	0
NH	59	49	–	40	0	0	0	0
NJ	49	200	–	200	0	0	0	0
NM	44	67	–	29.5	0	0	0	0
NV	40	88	–	25.5	0	0	0	0
NY	62	150	–	150	78	100	78	100
OH	40	90	–	90	0	0	0	0
OK	37	47	–	34.5	0	0	0	0
OR	100	100	100	100	100	100	100	100
PA	47	34	–	26.5	0	0	0	0
RI	59	181	–	175	0	0	0	0
SC	57	89	–	49.5	0	0	0	0
SD	55	52	–	52	0	0	0	0
TN	62	129	–	71.5	0	0	0	0
TX	26	26	–	12.5	0	0	0	0
UT	61	44	–	39	0	0	0	0

Continued on next page

Table 1.A1 (continued)

	Parents				Childless			
	Employed		Unemployed		Employed		Unemployed	
State	1996	2010	1996	2010	1996	2010	1996	2010
VA	41	29	–	24.5	0	0	0	0
VT	150	185	150	185	150	150	150	150
WA	59	74	–	37.5	0	0	0	0
WI	56	200	–	200	0	0	0	0
WV	33	33	–	17	0	0	0	0
WY	63	52	–	39.5	0	0	0	0

Source: Modified MWD.

Table 1.A2: Data Sources

Variable	Source	Time Frame	Balanced Panel?
Eligibility	CPS, MMWD	1996-2010	Yes
Enrollment	CPS	1996-2010	Yes
Opioid Sales	ARCOS	1997-2010*	Yes
Oxycodone	ARCOS	1997-2010	Yes
Methadone	ARCOS	1997-2010*	Yes
Opioid treatments	TEDS-A	1996-2010	No**
Prescr. opioid treatments	TEDS-A	1996-2010	No**
Opioid overdoses	MCOD	1999-2010	Yes
Prescr. opioid overdoses	MCOD	1999-2010	Yes

*No data in 2000.
**Not all states report every year.

Table 1.A3: Effects on Eligibility and Enrollment (Weighted by Population)

	(1)	(2)	(3)
VARIABLES	Elig.	BH IV	CG IV
Panel A: First Stage (Effects of IVs on Eligibility)			
IV	–	1.014***	0.973***
		(0.013)	(0.081)
R^2	–	0.992	0.979
Outcome mean	–	0.046	0.046
Observations	–	765	765
Panel B: Effects on Enrollment Rate			
Eligibility	0.179***	0.158***	0.164***
	(0.035)	(0.037)	(0.037)
R^2	0.862	0.244	0.244
Outcome mean	0.073	0.073	0.073
Observations	765	765	765
Model	OLS	2SLS	2SLS
Panel C: Effects on log Enrollment			
Eligibility	2.38***	2.14***	2.31***
	(0.38)	(0.41)	(0.39)
R^2	0.983	0.169	0.170
Outcome mean	0.073	0.073	0.073
Observations	765	765	765
Model	OLS	2SLS	2SLS

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: The IVs used in Columns (2) and (3) are generated following Borusyak and Hull's (2023) and Currie and Gruber's (1996) methodology, respectively. Panel A shows the effects of each IV on state-by-year eligibility rates, using an OLS regression. Panel B shows the effects of either the eligibility rate (Column 1) or the eligibility rate, as proxied by an IV (Columns 2-3), on the state-by-year enrollment rate. Panel C is the same as Panel B, except that the outcome is state-by-year log enrollment. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Outcome means are in per-capita units. Regressions are weighted by population. All states and all years between 1996 and 2010 are included in these regressions. Sources: Modified MWD, CPS.

Table 1.A4: 2SLS Effects on log Opioid Sales

VARIABLES	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Eligibility	0.638**	-2.85	0.635*	-0.172	0.717***
	(0.273)	(2.96)	(0.366)	(2.060)	(0.253)
Constant	4.05	17.2***	17.0***	-15.5	4.71
	(8.53)	(0.19)	(0.018)	(16.9)	(8.11)
Observations	663	663	663	663	663
R^2	0.995	–	0.023	0.796	0.275
Outcome mean	9.25	9.25	9.25	9.25	9.25
Controls	X			X	X
Fixed effects	X		X		X
Time coverage	1997-2010	1997-2010	1997-2010	1997-2010	1997-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: Column (1) lists OLS effects of the state-by-year eligibility rate on the respective outcomes. Columns (2) through (5) list 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome is state-by-year log opioid sales. Columns (1), (3), and (5) control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Columns (1), (3), and (5) control for state and year fixed effects. The year 2000 is omitted for opioid sales due to incomplete data. Units of outcome means are 60-MME doses per capita. Sources: ARCOS, Modified MWD, CPS.

Table 1.A5: Main Results (Weighted by Population)

VARIABLES	(1) OLS	(2) 2SLS: BH	(3) 2SLS: CG
Panel A: Log Opioid Sales			
Eligibility	0.355	0.438	0.379
	(0.356)	(0.347)	(0.368)
Observations	663	663	663
R^2	0.996	0.315	0.315
Outcome mean	9.25	9.25	9.25
Panel B: Log Opioid Treatment Admissions			
Eligibility	2.11**	1.70	2.25*
	(1.03)	(1.05)	(1.22)
Observations	745	745	745
R^2	0.958	0.494	0.494
Outcome mean	140	140	140
Panel C: Log Opioid Overdoses			
Eligibility	-0.432	-0.742	-0.769
	(1.273)	(1.291)	(1.301)
Observations	612	612	612
R^2	0.924	0.216	0.216
Outcome mean	4.35	4.35	4.35

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: Column (1) lists OLS effects of the state-by-year eligibility rate on the respective outcomes. Columns (2) and (3) list 2SLS effects using IVs to proxy for state eligibility rates. The IVs used in Columns (2) and (3) are generated following Borusyak and Hull's (2023) and Currie and Gruber's (1996) methodology, respectively. Panels A through C show the effects estimated by each model on state-by-year log opioid sales, log opioid treatment admissions, and log opioid overdose deaths, respectively. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The years 1996 and 2000 are omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. The years 1996 through 1998 are omitted in MCOD data. Units of outcome means are 60-MME doses per capita (Panel A), opioid abuse treatment admissions per 100 thousand residents (Panel B), and opioid overdose deaths per 100 thousand residents (Panel C). Regressions are weighted by population. Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

Table 1.A6: 2SLS Effects on log Opioid Abuse Treatment Admissions

VARIABLES	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Eligibility	3.48**	5.05	0.52	1.71	3.21**
	(1.33)	(3.50)	(1.47)	(1.60)	(1.26)
Constant	71.3**	7.69***	7.89***	-39.5**	75.4***
	(29.4)	(0.25)	(0.068)	(19.3)	(28.6)
Observations	745	745	745	745	745
R^2	0.954	0.036	0.001	0.797	0.373
Outcome mean	140.3	140.3	140.3	140.3	140.3
Controls	X			X	X
Fixed effects	X		X		X
Time coverage	1996-2010	1996-2010	1996-2010	1996-2010	1996-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: Column (1) lists OLS effects of the state-by-year eligibility rate on the respective outcomes. Columns (2) through (5) list 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome is state-by-year log opioid abuse treatment admissions. Columns (1), (3), and (5) control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Columns (1), (3), and (5) control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. Units of outcome means are opioid abuse treatment admissions per 100 thousand residents. Sources: TEDS-A, Modified MWD, CPS.

Table 1.A7: 2SLS Effects on log Opioid Overdose Deaths

VARIABLES	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Eligibility	1.07	-2.40	-0.665	-0.742	0.800
	(0.95)	(2.71)	(0.878)	(1.291)	(0.885)
Constant	2.59	4.87***	4.78***	-50.9	6.21
	(34.37)	(0.22)	(0.044)	(42.9)	(34.72)
Observations	612	612	612	612	612
R^2	0.936	–	–	0.216	0.178
Outcome mean	4.35	4.35	4.35	4.35	4.35
Controls	X			X	X
Fixed effects	X		X		X
Time coverage	1999-2010	1999-2010	1999-2010	1999-2010	1999-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: Column (1) lists OLS effects of the state-by-year eligibility rate on the respective outcomes. Columns (2) through (5) list 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome is state-by-year log opioid overdose deaths. Columns (1), (3), and (5) control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Columns (1), (3), and (5) control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. Units of outcome means are opioid overdose deaths per 100 thousand residents. Sources: MCOD, Modified MWD, CPS.

Table 1.A8: 2SLS Effects Using BH IV (Subcategories, Using Population Weights: log Outcomes)

	Opioid Sales			Opioid Admissions	Opioid Overdoses
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Oxycodone	Hydrocodone	Methadone	Prescription	Prescription
Eligibility	0.430	0.861***	0.689	1.42	-3.23**
	(0.465)	(0.309)	(0.999)	(0.99)	(1.61)
Constant	15.8	11.8	-13.2	-1.4	-122***
	(16.0)	(11.0)	(23.7)	(20.8)	(44)
Observations	714	714	663	745	612
R^2	0.263	0.303	0.153	0.185	0.174
Outcome mean	2.68	1.30	2.00	51.35	3.35
Controls	X	X	X	X	X
Fixed effects	X	X	X	X	X
Time coverage	1997-2010	1997-2010	1997-2010	1996-2010	1999-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The first three columns show log effects on major categories of opioid sales: oxycodone, hydrocodone, and methadone. The second set of columns (4) splits log effects on opioid abuse treatment admissions into a category in which abuse of a prescription opioid was identified. Column (5) splits log effects on opioid overdose deaths similarly into cases in which a prescription opioid was identified as a cause of death. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The year 2000 is omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. Units of outcome means are 60-MME doses per capita (Columns 1-3), opioid abuse treatment admissions per 100 thousand residents (Column 4), and opioid overdose deaths per 100 thousand residents (Column 5). Regressions are weighted by population. Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

Table 1.A9: 2SLS Effects (Add SNAP: log Outcomes)

VARIABLES	(1) Sales	(2) Admissions	(3) Overdoses
Eligibility	0.716***	3.22**	0.818
	(0.256)	(1.26)	(0.910)
Constant	4.74	77.5***	6.69
	(8.23)	(28.8)	(34.54)
Observations	663	745	612
R^2	0.275	0.374	0.184
Outcome mean	9.25	140	4.35
Controls	X	X	X
SNAP Control	X	X	X
Fixed effects	X	X	X
Time coverage	1997-2010	1996-2010	1999-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. Outcomes are state-by-year log opioid sales, opioid abuse treatment admissions, and opioid overdose deaths, respectively. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; unemployment rate; and share enrolled in SNAP. The year 2000 is omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. Units of outcome means are 60-MME doses per capita (Column 1), opioid abuse treatment admissions per 100 thousand residents (Column 2), and opioid overdose deaths per 100 thousand residents (Column 3). Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

Table 1.A10: 2SLS Effects (Add Senatorial and Gubernatorial Vote Shares: log Outcomes)

VARIABLES	(1) Sales	(2) Admissions	(3) Overdoses	(4) Sales	(5) Admissions	(6) Overdoses
Eligibility	0.811***	2.25**	0.226	0.642***	2.20***	-0.290
	(0.217)	(0.90)	(0.734)	(0.207)	(0.84)	(0.830)
Constant	6.05	88.6***	-3.21	6.25	89.1***	-5.67
	(7.95)	(29.7)	(40.79)	(7.75)	(30.1)	(40.94)
Observations	650	734	600	650	734	600
R^2	0.288	0.367	0.153	0.326	0.368	0.172
Outcome mean	9.28	138	4.39	9.28	138	4.39
Controls	X	X	X	X	X	X
All Poli. Controls				X	X	X
Fixed effects	X	X	X	X	X	X
State coverage	50 states	50 states	50 states	50 states	50 states	50 states
Time coverage	1997-2010	1996-2010	1999-2010	1997-2010	1996-2010	1999-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. Outcomes are state-by-year log opioid sales, opioid abuse treatment admissions, and opioid overdose deaths, respectively. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections and, in Columns 4-6, gubernatorial, and senatorial elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. The year 2000 is omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. Washington, DC is dropped from this sample, as it does not hold gubernatorial or senatorial elections. Units of outcome means are 60-MME doses per capita (Columns 1 and 4), opioid abuse treatment admissions per 100 thousand residents (Columns 2 and 5), and opioid overdose deaths per 100 thousand residents (Columns 3 and 6). Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

Table 1.A11: 2SLS Effects on Treatment Admissions (Split by Referral Source and Insurance Coverage: log Outcomes)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Admissions	Admissions	Admissions	Admissions	Admissions	Admissions
Eligibility	26.9	31.2	23.4*	22.3	15.5	18.34
	(21.5)	(20.5)	(13.9)	(13.8)	(12.6)	(13.2)
Constant	453	773**	487	805**	106	544
	(279)	(383)	(298)	(360)	(305)	(356)
Observations	745	745	745	745	745	745
R^2	0.169	0.205	0.189	0.193	0.197	0.177
Outcome mean	2.00	11.2	1.92	2.73	10.7	25.1
Controls	X	X	X	X	X	X
Fixed effects	X	X	X	X	X	X
Time coverage	1996-2010	1996-2010	1996-2010	1996-2010	1996-2010	1996-2010
Sample	Jud. x Med.	Jud. x None	Health x Med.	Health x None	Indiv. x Med.	Indiv. x None

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome in these regressions is log opioid abuse treatment admissions, restricted to admissions stemming from different combinations of referral sources and insurance coverage: Columns (1) and (2) restrict the sample to clients coming through judicial (i.e. criminal justice) referrals, Columns (3) and (4) restrict the sample to healthcare referrals, Columns (5) and (6) restrict the sample to individual referrals, and each pair of columns is split into samples of clients who either have Medicaid (denoted by "Med.") enrollment or have no insurance, respectively. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. Units of outcome means are opioid abuse treatment admissions per 100 thousand residents. Sources: TEDS-A, Modified MWD, CPS.

Table 1.A12: Effects on Enrollment Rate among Disabled/Retired

VARIABLES	(1) OLS	(2) 2SLS: BH	(3) 2SLS: CG
Eligibility	0.171	0.164	0.136
	(0.106)	(0.104)	(0.121)
Constant	-1.71	-1.78	-1.81
	(1.57)	(1.45)	(1.46)
Observations	765	765	765
R^2	0.768	0.066	0.065
Outcome mean	0.173	0.173	0.173
Controls	X	X	X
Fixed effects	State; Year	State; Year	State; Year
Time coverage	1996-2010	1996-2010	1996-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: The IVs used in Columns (2) and (3) are generated following Borusyak and Hull's (2023) and Currie and Gruber's (1996) methodology, respectively. This table shows the effects of either the eligibility rate (Column 1) or the eligibility rate, as proxied by an IV (Columns 2-3), on the state-by-year enrollment rate calculated among the population in the CPS who report unemployment due to disability or retirement. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Outcome means are in per-capita units. Sources: Modified MWD, CPS.

Table 1.A13: 2SLS Results (High vs. Low Initial Eligibility Threshold: log Outcomes)

	Low Initial Threshold			High Initial Threshold		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Sales	Admissions	Overdoses	Sales	Admissions	Overdoses
Eligibility	0.683*	2.81	0.61	0.997***	3.28**	-1.28
	(0.371)	(2.02)	(1.72)	(0.317)	(1.39)	(1.87)
Constant	11.6	74.4	-96.7**	7.74	41.3	21.3
	(14.9)	(53.3)	(46.3)	(11.89)	(38.1)	(35.9)
Observations	338	374	312	325	371	300
R^2	0.272	0.318	0.328	0.441	0.578	0.229
Outcome mean	9.02	98.6	4.25	9.49	182	4.45
Controls	X	X	X	X	X	X
Fixed effects	X	X	X	X	X	X
State coverage	26 states	26 states	26 states	25 states	25 states	25 states
Time coverage	1997-2010	1996-2010	1999-2010	1997-2010	1996-2010	1999-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. Outcomes are, respectively, log opioid sales, log opioid abuse treatment admissions and log opioid overdoses, for states with either "Low" (< 50%) or "High" (≥ 50%) initial Medicaid eligibility thresholds for employed parents. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The year 2000 is omitted for opioid sales due to incomplete data. Some states are missing for one or more years in TEDS-A data. Units of outcome means are 60-MME doses per capita (Columns 1 and 4), opioid abuse treatment admissions per 100 thousand residents (Columns 2 and 5), and opioid overdose deaths per 100 thousand residents (Columns 3 and 6). Sources: ARCOS, TEDS-A, MCOD, Modified MWD, CPS.

Table 1.A14: 2SLS Effects (Split by Race: log Outcomes)

VARIABLES	(1) Admissions	(2) Admissions	(3) Overdoses	(4) Overdoses
Eligibility	3.79***	6.08**	4.00	6.78
	(1.13)	(2.94)	(3.52)	(8.41)
Constant	68.7**	74.2	39.9	137
	(32.4)	(118.9)	(54.3)	(312)
Observations	745	745	612	612
R^2	0.328	0.097	0.304	0.265
Outcome mean	97.4	25.0	3.97	0.25
Controls	X	X	X	X
Fixed effects	X	X	X	X
Time coverage	1996-2010	1996-2010	1999-2010	1999-2010
Sample	White	Black	White	Black

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. Outcomes are, respectively, log opioid abuse treatment admissions and log opioid overdoses, for either White or Black individuals in the respective outcome sample. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. Units of outcome means are opioid abuse treatment admissions per 100 thousand residents (Columns 1-2) and opioid overdose deaths per 100 thousand residents (Columns 3-4). Sources: TEDS-A, MCOD, Modified MWD, CPS.

Table 1.A15: 1st Stage Effects (Split by Time Window)

	Early			Late		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Elig.	BH	CG	Elig.	BH	CG
Panel A: First Stage (Effects on Eligibility)						
Eligibility	–	1.025***	0.988***	–	1.003***	0.899***
		(0.0120)	(0.0917)		(0.0277)	(0.0753)
Observations	–	510	510	–	510	510
R^2	–	0.989	0.970	–	0.991	0.980
Outcome mean	–	0.040	0.040	–	0.054	0.054
Panel B: 2SLS (Effects on Enrollment Rate)						
Eligibility	0.216***	0.191***	0.189***	0.171***	0.132***	0.148***
	(0.053)	(0.050)	(0.053)	(0.043)	(0.044)	(0.056)
Observations	510	510	510	510	510	510
R^2	0.842	0.248	0.248	0.888	0.192	0.193
Outcome mean	0.067	0.067	0.067	0.078	0.078	0.078
Model	OLS	2SLS	2SLS	OLS	2SLS	2SLS
Time coverage	1996-2005	1996-2005	1996-2005	2001-2010	2001-2010	2001-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: The IVs used in Columns (2) and (5) are generated following Borusyak and Hull's (2023) methodology, and the IVs used in Columns (3) and (6) use Currie and Gruber's (1996) methodology. Panel A shows the effects of each IV on state-by-year eligibility rates, using an OLS regression. Panel B shows the effects of either the eligibility rate (Columns 1 and 4) or the eligibility rate, as proxied by an IV (Columns 2-3 and 5-6), on the state-by-year enrollment rate. Columns (1)-(3) show effects on the first ten years of the full sample, 1996-2005, while Columns (4)-(6) show effects on the last ten years, 2001-2010. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Outcome means are in per-capita units. Sources: Modified MWD, CPS.

Chapter 2

The Role of Pre-ACA Medicaid Expansions in Access to Substance Use Disorder Treatments

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Abstract

I explore the role of Medicaid expansions in the provision of substance use disorder (SUD) treatment services between 1996 and 2010, using a novel set of income thresholds for Medicaid eligibility. I find that while increasing access to Medicaid led to a dramatic increase in SUD treatment episodes, there was no increase in numbers of SUD treatment facilities or client loads (as given on a specific reference date) at those facilities. This discrepancy is explained by a sharp increase in client loads at hospital-based inpatient facilities, especially for relatively rapid detoxification services, which appear to have led the average SUD treatment episode to shorten dramatically. Comparing my results to those of Meinhofer and Witman (2018) is insightful, as they find that within their sample from 2007 through 2016, Medicaid expansions actually had the largest effects on clients' utilization of outpatient rehabilitation services, which might be thought to be more effective for long-term health improvements. I propose that these considerably different sets of results can be explained by major changes in Medicaid coverage of SUD treatment after the 2010 Affordable Care Act (ACA), although further research is needed to confirm this.

2.1 Introduction

In the first chapter of this dissertation (see Section 1.5), I find that opioid use disorder (OUD) treatment episodes increased in response to increasing access to Medicaid between 1996 and 2010. Not only were OUD treatment episodes positively affected, but so were overall substance use disorder (SUD) treatment episodes.

This chapter aims to understand the supply-side story behind this increase on OUD and SUD treatment episodes. Increasing access to Medicaid means the marginal SUD patient poses less financial risk to hospital-based inpatient facilities than previously, which could allow hospitals to increase their inpatient capacity to treat SUD. In this chapter, I find that this is exactly what happened during this time period: overall number of SUD facilities did not increase in the same year as a Medicaid expansion, and overall SUD treatment client loads on specific reference dates also did not increase, but inpatient client loads increased dramatically, especially for relatively short-term detoxification treatments. Given that I find that SUD treatment episodes increased but daily client loads at SUD treatment facilities did not, the large increase in detoxification services at inpatient facilities appears to have led SUD treatments overall to have shorter durations overall.

I also find a positive effect on client loads for longer-term inpatient rehabilitation treatments, which are meant to be a next step after detoxification in order to help improve clients' long-term outcomes. This suggests that some portion of clients receiving increased detoxification services are receiving much-needed, long-term care, which could help explain why I find in Section 1.5 that Medicaid expansions reduce opioid overdose deaths. However, the effect on client loads receiving rehabilitation services at inpatient facilities is smaller than the effect for detoxification client loads at the same, suggesting that many people who are induced into detoxification do not receive rehabilitation services immediately thereafter. Moreover, I find a large, though imprecise, effect on SUD clients' average number of prior visits. which suggests that some people may be churning through more treatments than they otherwise would have, following a Medicaid expansion. This would be beneficial if it meant more people entering rehabilitation after detoxification, but that does not appear to be the whole story.

To execute my analysis, I leverage the Two-Stage Least Squares (2SLS) model developed in Section 1.4, using simulated IVs developed by Borusyak and Hull (2023) and Currie and Gruber (1996). I combine Medicaid eligibility thresholds described in Section 1.3 with income and demographic data collected by the Census Bureau in the Current Population Survey (CPS), together with the Substance Abuse and Mental Health Services Administration's (SAMHSA) data on SUD treatment episodes and SUD treatment facilities.

To my knowledge, this is the first paper to explicitly explore whether SUD treatment episodes are affected differently by Medicaid expansions than client loads at SUD treatment facilities, and why such a disconnect might exist. Most of the literature (e.g. Clemans-Cope, Epstein, and Kenney, 2017; Maclean and Saloner, 2019; Wen et al., 2017) on this topic has focused on expansions that occurred following the 2010 Affordable Care Act (ACA) – a law that simultaneously increased coverage of SUD treatment options under Medicaid, meaning that the effects of Medicaid expansion on SUD Treatment provision may be quite different now, compared to before 2010, which is the focus of this paper. The closest paper to this one is Meinhofer and Witman (2018), which finds that following Medicaid expansions between 2007 and 2016, OUD treatment episodes increased and the number of OUD treatment providers also increased. In contrast to my findings, they also find that effects on SUD treatment utilization were concentrated in outpatient rehabilitation treatments, rather than inpatient detoxification treatments, as in my findings. I suggest that this discrepancy may be linked to broadening Medicaid coverage of SUD treatment under the Affordable Care Act (ACA) in 2010. I compare my paper to these and other findings from Meinhofer and Witman (2018) in more depth in Section 2.5.6.

The remainder of this paper is constructed as follows. Section 2.2 discusses the context for this study. Section 2.3 introduces the data I use. Section 2.4 explains the methodology, and Section 2.5 presents and discusses results. I conclude in Section 2.6.

2.2 Background

2.2.1 Medicaid

Medicaid is a healthcare program targeted mainly to the poor in the United States, providing beneficiaries with a wide array of cheap (often free) healthcare services, including primary and emergency care, and surgeries. This program is administered jointly by the federal government and each state.

States have relatively little control over what services their Medicaid programs cover (Gruber, 2000), but each state does have substantial leverage to set its own eligibility requirements for Medicaid within its jurisdiction. These requirements are typically changed by submitting a Section 1115 waiver to the federal government. A key component of these eligibility requirements is whether applicants' household income falls below a certain threshold (Currie and Gruber, 1994), which is normally based on the Federal Poverty Guideline (FPG) for applicants' household size and state of residence, as well as whether or not they have children. Between 1996 and 2010, which is the period of focus for this paper, many states allowed substantial growth in coverage for adult parents, and to a lesser extent, childless adults[1].

[1]In contrast, children (defined as younger than 19 years old) and pregnant individuals already had relatively generous coverage across much of the country, even at the beginning of the sample period (Gruber, 2000). Certain other populations, such as disabled and elderly individuals, have also had special criteria that focused less on income, through which they could access Medicaid (Gruber, 2000; Rudowitz et al., 2024).

2.2.2 SUD Treatment

SUD treatment in the United States has evolved dramatically over the past century, and efforts now often focus on treating SUD as a disease rather than a moral failing (Henninger and Sung, 2014). This paper covers SUD treatment overall, which includes treatment for addiction to alcohol, as well as illegal drugs such as cocaine and opioids.

The time period of focus for this paper, 1996 through 2010, was marked by a particularly large increase in OUD, which spurred changes in how OUD was treated. While medication-assisted treatment (MAT) for OUD, often in the form of outpatient clinics that provided methadone, existed prior to 1996, growing addiction to opioids spurred more demand for effective OUD treatments (Henninger and Sung, 2014; Quinones, 2015; Jones et al., 2018). One major legal change that addressed this increasing need for OUD treatment was the U.S. Food and Drug Administration's (FDA) 2002 approval of buprenorphine as another MAT option. Legal barriers and social stigma associated with MAT and other forms of SUD treatment continued to fall over the course of the 2000's (Jones et al., 2018), allowing for people to more easily access SUD treatment.

Besides outpatient clinics, SUD treatment services have historically been provided in a variety of settings, including hospitals, jails, and residential facilities. Services offered include the following options. The first step is typically rapid, intensive detoxification ("detox"), which help the client[2] manage withdrawal symptoms as the addictive substance dissipates from their system (Substance Abuse and Mental Health Services Administration, 2006). Detoxification is generally intended to be followed by maintenance or rehabilitation, which provide longer-term treatment.

[2] Patients may be called "clients" in SUD treatment contexts.

2.3 Data

In this paper, I draw on a subset of the data described in Section 1.3, in the first chapter of this dissertation. As in that chapter, I combine annual, state income thresholds for Medicaid with individual-level survey data from the March Supplement of the Current Population Survey (CPS) in order to estimate respondents' eligibility for Medicaid, and I also use CPS to observe respondents' reported enrollment. The control variables and population estimates that I use in this paper are also the same as in Section 1.3. The outcome variables I use in this paper are somewhat different than those described in Section 1.3, so I focus my attention on them, while briefly describing the other variables I use. I again focus on adults, aged 19 through 64, using data between the years 1996 through 2010 and from all 51 "states," which include the District of Columbia.

2.3.1 Medicaid Eligibility Thresholds

Collecting data from multiple sources, I construct what I believe to be the most complete set of state-by-year income thresholds for Medicaid eligibility from 1996 through 2010, for non-elderly adults without disabilities. I describe the process for collecting these data in Section 1.3.1. This is not trivial, as these policies are determined at the state level and there is no federal database on all Medicaid eligibility requirements in all states, over the time period of interest. I call the final dataset the Modified Medicaid Waiver Database (Modified MWD), in reference to Burns, Dague, and Kasper; Burns, Dague, and Kasper's 2018a; 2018b Medicaid Waiver Database, which forms the backbone of these data.

There is one change that I make to the income thresholds for Medicaid eligibility in this paper, relative to the thresholds described in Section 1.3.1: I now stop distinguishing "employed" versus "unemployed" thresholds, opting to use only the "employed" thresholds. I do this because, after a more thorough review of the origin of these thresholds, I discovered that the distinction between the two is based only on whether a household's income counts as earned versus unearned income, which I am unable to distinguish within CPS data. Using the new definition for thresholds does not meaningfully change results in Section 1.5, but I nonetheless plan to use the new definition in future drafts of that paper, as it simplifies any analysis that I do and appears to better align with the variables given in the CPS data.

The state-level thresholds in 1996 and 2010 are recorded in Table 2.A1, and the annual path each state took specifically for parents is shown in Figure 2.A1. Many, though not all, states increased their thresholds for some or all categories over time, and states varied widely in their annual thresholds and in the trajectories they followed between 1996 and 2010.

I use CPS data to estimate individuals' eligibility in the year preceding each survey year. To summarize my procedure[3], I compare each individual's income with the income threshold that applies to them given their state of residence, family size, and whether or not they have dependent children in their household. If their income is higher than the threshold, or if the threshold is 0% of the applicable FPG, then I say they are not eligible. Otherwise, I say they are eligible.

Table 2.1 provides summary statistics for calculated state-by-year eligibility rates for the 19-to-64-year-old population in the first row: the eligibility rate ranges from 0.5% to 24%, with a mean of 5.1% and median of 3.1%. This variable is calculated for the entire sample that I focus on: all 51 states in all years between 1996 and 2010 (inclusive).

2.3.2 CPS Survey Data: Enrollment

My data on Medicaid enrollment comes from individual responses in the CPS data, which I obtain from IPUMS CPS (Flood et al., 2024). This and other variables that I draw from the CPS are answered in March of a given year, in reference to the previous year, so I use the surveys from 1997 through 2011 to infer values from 1996 through 2010. Whenever I

[3] I discuss my methods in more detail in Section 3.5.

use CPS data in this paper, I focus on data from respondents aged 19 through 64 years old, which is the population I have Medicaid income thresholds for. As with the income thresholds, these data are discussed in detail in Section 1.3.

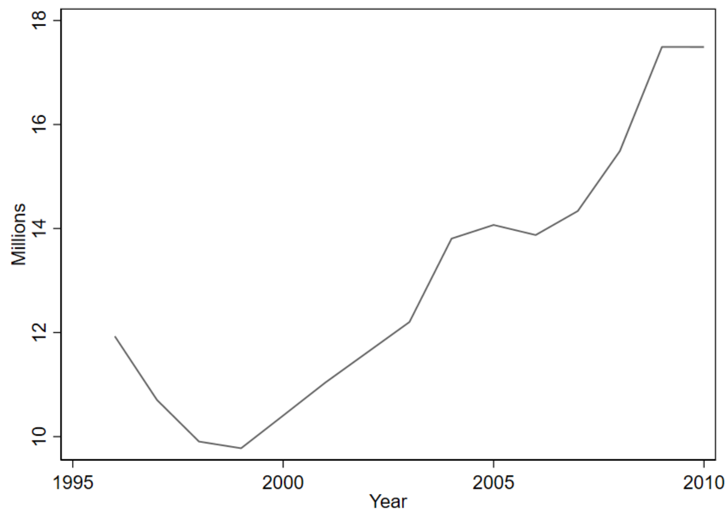
Annual total Medicaid enrollment for the age group of interest, multiplied by CPS population weights (ASEC weights), is shown in Figure 2.1. The trajectory is non-monotonic but increases for most of the time frame, due at least in large part to state Medicaid expansions and reductions in employer-sponsored insurance (Holahan, Cohen, and Rousseau, 2007). Medicaid enrollment fell after the 1996 welfare reform was passed, presumably due to state administrative frictions during the next few years (Ku and Bruen, 1999). Enrollment fell again in 2006, this time at least in part due to the creation of Medicare Part D, through which elderly (not included in the figure) and disabled individuals could access low-cost prescriptions (Holahan, Cohen, and Rousseau, 2007).

Summary statistics for state-by-year enrollment rates for the 19-to-64-year-old population are displayed in the second row of Table 2.1: enrollment rates range from 1.8% to 19.5%, with a mean of 7.3% and a median of 6.6%. The fact that this variable is generally larger than the eligibility rate reflects that the eligibility thresholds I use are an imperfect measure of true eligibility – I can only calculate one pathway to eligibility, whereas my enrollment measure includes individuals who are eligible for Medicaid by other means, such as disability. That said, the maximum eligibility rate is higher than the maximum enrollment rate, which reflects possible imperfections in how I measure eligibility, which I explore in more depth in Section 1.5.6. As with eligibility, this variable is present for the entire sample time frame.

2.3.3 SUD Treatment Episodes: Admissions

As in Section 1.3, I use public-access data on SUD treatment episode admissions (or more simply, "admissions" or "treatments") offered by Substance Abuse and Mental Health Services Administration (SAMHSA) through the TEDS-A dataset. The outcome variable of

Figure 2.1: Adult Medicaid Enrollment



Notes: Medicaid enrollment among individuals of ages 19-64. Aggregated sum for each year estimated using weighted responses for the given year, using the himcaidly variable in the CPS March Supplement from the following year. Source: CPS

focus is overall SUD treatment admissions, as opposed to Section 1.3, where the focus is on OUD treatment admissions. I construct the state-by-year variable for SUD treatment admissions by simply adding together all treatment episodes that appear in a state and year for adults in TEDS-A. As shown in Figure 2.2a, total SUD treatment admissions rose on net from 1996 through 2010, with OUD treatment admissions accounting for much of this increase.

One concern with this dataset is that while states typically report the required data, different states have different standards for what data to report. For example, some may include data from correctional facilities, while others do not. Moreover, there are 20 state-year observations that are missing from the sample. In Section 2.5.4, I find that my main results are not meaningfully affected if I drop states with imperfect reporting in TEDS-A.

Table 2.1 shows summary statistics for overall SUD treatment admissions at the state-by-year level. SUD admissions (row 3) range from 468 to 314,591 admissions per state and year, with a mean of 37,087. Corresponding numbers for OUD treatment admissions are displayed in row 4. These variables are available from 1996 through 2010, for all 51 states, except for 20 missing state-year observations due to missed reporting years.

2.3.4 Survey of Substance Use Disorder Treatment Facilities

In this paper, some of the most important outcome variables come from the National Survey of Substance Abuse Treatment Services (N-SSATS), using the concatenated file for 1997-2011 provided by SAMHSA[4]. This survey allows me to calculate annual estimates for the number and type of facilities offering SUD treatment in each state, with the most detailed information about the facilities being available in the later iterations of the survey. I use the data from the surveys from 1998, 2000, and every year from 2002 through 2010[5].

Using these data, I construct the following outcome variables for each state and year in the N-SSATS sample: number of SUD facilities and number of clients[6] on a given reference date (I refer to this variable as facilities' client load), and number of clients per year. I also construct variations of these variables, breaking up numbers of facilities by types of services [7] offered and facility type[8], and breaking up numbers of clients by types of services received and facility type. Figure 2.2a shows that while client loads at SUD facilities increased on average between 1996 and 2010[9], the overall number of SUD facilities actually fell over the

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[4]Obtained at `https://www.samhsa.gov/data/data-we-collect/n-sumhss-national-substance-use-and-mental-health-services-survey/datafiles/n-ssats-1997-2011`. Last accessed on September 18, 2025.

[5]The omitted years have no surveys, except for 1997, which reports a significantly smaller sample than in the later years.

[6]Some of the values for client counts are estimates rather than precisely recorded numbers, so these variables may be somewhat noisier than other variables constructed using these data. As another aside, some survey respondents report that they are responding for more than one facility. My facility count variables are constructed as a count based on how many facilities each respondent says they are answering the survey for.

[7]e.g. detoxification or residential treatment

[8]Inpatient, residential, or outpatient. Note that N-SSATS surveys omit certain types of facilities, including jails, halfway houses, and solo practitioners.

[9]Note that this lines up fairly well with the data on SUD treatment admissions, as shown in Figure 2.2a

same time period. Part of the purpose of this paper is to explore this disconnect.

Table 2.1 shows summary statistics for variables derived from N-SSATS at the state-by-year level. The total number of SUD facilities per year and state (row 5) ranges from 34 to 1,979, with a mean of 288. The total client load at these facilities (row 9) on the respective reference dates ranges from 1,401 to over 158,653, with a mean of 21,563. Summary statistics for inpatient facilities (row 6), residential facilities (row 7), outpatient facilities (row 8), and clients receiving detoxification treatment on the reference date (row 10) are also listed in Table 2.1. These variables are available for 1998 (with the exception of facility counts by type), 2000, and 2002 through 2010, for all 51 states.

2.3.5 Other Data

Demographic Variables

I draw state-by-year and county-by-year population data from the Survey of Epidemiology and End Results (SEER). These datasets also allow me to calculate annual (for each state and county) population and population shares of different age ranges[10], races, and Hispanic ethnicity.

Political Variables

I bring in county-by-year[11] data on party vote shares in general elections for president, governor, and senators, using data from Algara and Amlani (2021) and Alaska's Division of Elections[12].

From these data, I calculate state-level vote shares in favor of the Democratic-party

[10] Powell, Pacula, and Taylor (2020)

[11] Except for Alaska, which I only obtain data for at the state-by-year level.

[12] https://www.elections.alaska.gov/election-results/

Table 2.1: Summary Statistics (by State and Year)

	Mean	Median	SD	Min	Max	N
Eligibility (per cap.)	0.051	0.031	0.048	0.005	0.240	765
Enrollment (per cap.)	0.073	0.066	0.030	0.018	0.195	765
SUD treatment episodes	37,087	20,707	49,236	468	314,591	745
OUD	8,775	2,634	15,701	48	102,980	745
SUD Facilities	288	216	305	34	1,979	561
Inpatient	20	14	21	0	142	510
Residential	77	52	113	5	835	510
Outpatient	236	184	229	26	1,400	510
SUD Clients on Ref. Date	21,563	13,327	25,710	1401	158,653	561
Detox	543	282	838	11	7,439	561

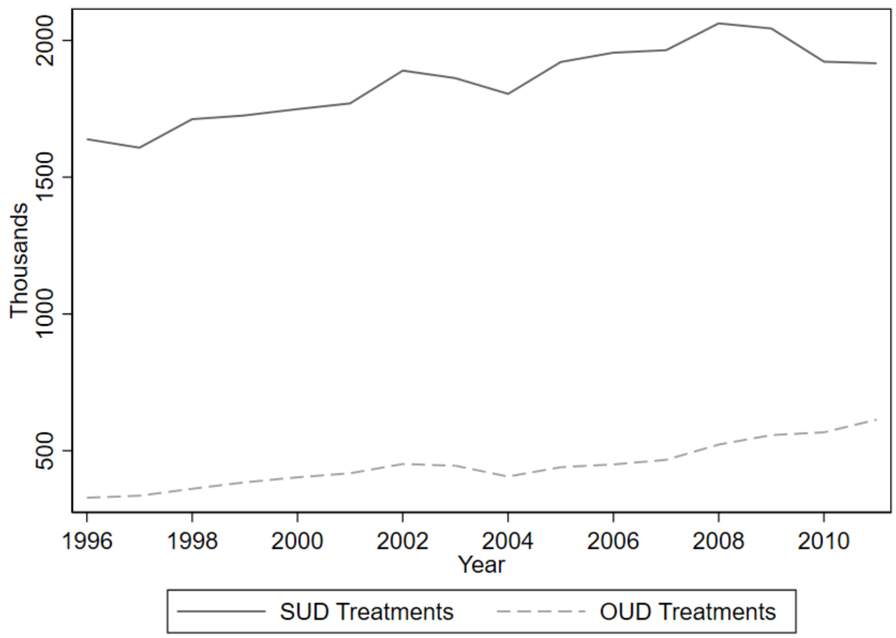
Notes: Outcomes are, respectively: Medicaid eligibility per capita (among 19- through 64-year-olds for whom I am able to measure eligibility), enrollment per capita (among the 19-64-year-old population), SUD and OUD treatment admissions (with 20 state-year observations missing), number of SUD facilities, hospital-based inpatient SUD facilities, residential SUD facilities, outpatient SUD facilities, clients at SUD facilities on a given reference date, and clients receiving detoxification treatment at SUD facilities on a given reference date. The data on SUD facilities and their clients covers all 51 states but is only present in 1998, 2000, and 2002 through 2010, with the 1998 survey missing data on facility type (inpatient, residential, or outpatient). Sources: TEDS-A, N-SSATS Modified MWD, CPS.

candidate, for each type of election. For years in which an election did not occur, I impute the most recent values for the given state. In the rare case when a state holds two senate elections in the same year, I take the average vote shares over the two elections.

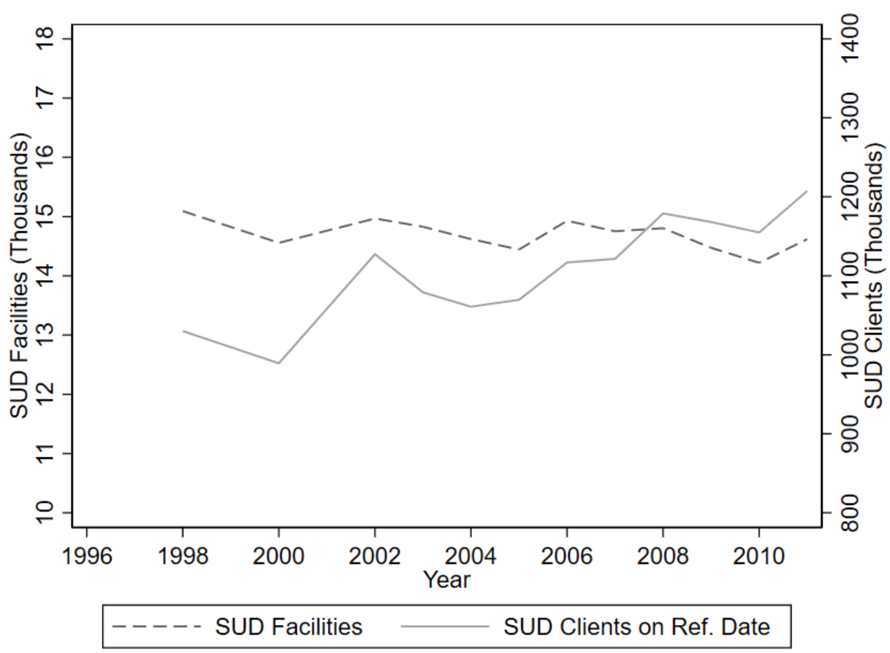
Other Control Variables

I obtain annual dummy variables on the existence of relevant policies from Alpert, Powell, and Pacula (2018) and Table A.1 in Powell, Pacula, and Taylor (2020), for whether a given state had each of the following policies or features: any existing PDMP, a "must-access" PDMP, medical marijuana laws, active and legal marijuana dispensaries, and pill-mill laws. I discuss these variables in slightly more detail in Section 1.3.

Figure 2.2: Main Outcomes



(a) Treatment Admissions



(b) Counts of SUD Treatment Facilities and Clients

Notes: SUD treatment admissions are all SUD treatment episodes in TEDS-A for adults. Opioid abuse treatment admissions are defined as admissions for adults with admissions for a SUD involving "non-prescription methadone," "other opioids or synthetics," or heroin, or who have a diagnosis of "opioid dependence" or "opioid abuse." SUD facilities are facilities that respond to N-SSATS surveys, and the variable for "SUD clients on Ref. Date" denotes the total number of clients that facilities reported were present on a specified reference date. Sources: TEDS-A, N-SSATS.

2.4 Methodology

As discussed in Section 1.4, identification of effects is complicated by the fact that Medicaid eligibility is endogenous: an income shock might simultaneously affect both an individual's Medicaid eligibility and their vulnerability to opioid addiction. To address this concern, I use the same method I use in Section 1.4: a simulated Instrumental Variable (IV) of Medicaid eligibility that is meant to isolate the relative policy generosity in individuals' states of residence, ignoring changes in income and demographics. As in Section 1.4, I focus on the IV proposed by Borusyak and Hull (BH, 2023), which I compare to the IV from Currie and Gruber (CG, 1996) for robustness. After generating the IVs, I estimate state-by-year level effects using Two-Stage Least Squares (2SLS) regressions, as defined in Section 1.4.

In summary, the BH IV is constructed as follows. For a given individual i in year t, their BH IV is calculated as $elig_{it} - \mu_{it}$, where $elig_{it}$ represents a dummy for whether they are eligible in their state of residence and μ_{it} represents the fraction of states in which, given i's household income, family size, and parental status, i would be eligible if i lived there. To generate a state-level aggregate of this IV, I simple take the mean value of all individuals i who live in each state s.

The CG IV is conceptually similar: for a given state s in year t, the CG IV is simply the (weighted, using CPS's ASEC weights) fraction of the total sample population surveyed across the country in year t, who would be eligible if they lived in state s during that year, given their stated income, family size, and parental status.

For each regression, I follow my methodology from Section 1.4 by controlling for the same variables[13], using state and year fixed effects, and clustering standard errors at the

[13]The control variables are population, share White, share Black, share Hispanic, share female, shares of six age groups (0–11, 12–17, 18–24, 25–44, 45–64, 65 and older), annual average unemployment rate, share Democratic vote in most recent general elections for president (and in samples that omit Washington, DC, I may also include general elections for senator and governor), and state-level shares (using CPS data) of the population of age 25 and older with no college, some (i.e. less than 4 years) college, and at least 4 years

state level. Unlike in Section 1.4, I weight each state-by-year observation by population in my preferred regression specification.

The 2SLS regressions are designed as follows.

2.4.1 First Stage

For the first-stage regression, I use a simple OLS regression of the eligibility rate on the eligibility IV, for each state s and year t:

$$elig_{st} = \alpha_0 + \delta_s + \delta_t + \alpha_1 IV_{st} + \beta X_{st} + \varepsilon_{st}, \tag{2.1}$$

where $elig_{st}$ denotes the average[14] eligibility rate for s and t; IV_{st} is one of the two IVs, X_{st} contains control variables; δ_s and δ_t are fixed effects for state and year, respectively; α_0, α_1, β are coefficients; and ε_{st} is the residual term.

2.4.2 Second Stage

For the second stage, I regress outcomes on the predicted values for eligibility for each state s and year t, using predictions generated from the first-stage regression. This regression follows the equation

$$Y_{st} = \gamma_0 + \rho_s + \rho_t + \gamma_1 \widehat{elig_{st}} + \xi X_{st} + \nu_{rt}, \tag{2.2}$$

where Y_{st} is an outcome related to SUD treatment (or enrollment)[15]; $\widehat{elig_{st}}$ is the predicted

of college, as well as indicators for whether a state has an existing PDMP, a "must-access" PDMP, medical marijuana laws, active and legal marijuana dispensaries, and pill-mill laws.

[14]Weighted using CPS weights

[15]One might suspect that effects on each of the outcomes may take more time to materialize, but I leave estimation of dynamic effects to future research.

eligibility rate; γ_1 is the coefficient of interest that denotes the estimated effects of eligibility rates on outcome Y; X_{st} contains the same control variables as in the first stage; ρ_s and ρ_t are fixed effects for state and year, respectively; γ_0 and ξ are coefficients, and ν_{st} is a residual term.

2.5 Results

2.5.1 First Stage

First stage results[16] are reported in Table 2.2, Panel A, Columns (2) and (3). Intuitively, the coefficients should be close to 1 if the IVs properly represent shocks to eligibility, and they are indeed statistically equivalent to 1. Both IVs are also strong: the BH IV has an F statistic of 5,606, and the CG IV has an F statistic of 155.

Panel B shows effects on enrollment rates[17] of at least 0.17, regardless of which model is used (Column 1: OLS; Column 2: 2SLS with BH IV; Column 3: 2SLS with CG IV). The estimate in Column 2 (using the BH IV) suggests that for every 100 individuals who are newly eligible in a state, about 17.1 are predicted to enroll[18].

[16]Note that these results differ from those in Table 1.2 because of the following differences in methodology: 1) in this paper, I use population weights unless stated otherwise, while in the first chapter of this dissertation, my main results draw from regressions that weight each observation equally, and 2) as discussed in Section 2.3.1, this paper only uses "employed" Medicaid income thresholds, while the first chapter attempts to use both "employed" and "unemployed" thresholds.

[17]Recall that both eligibility rates and enrollment rates are estimated for states' populations aged 19 to 64 years old.

[18]As such, for the 2SLS effects that follow, each effect can be divided by 0.171 to approximate the effect of a commensurate increase in the state's enrollment rate (as opposed to the eligibility rate).

Table 2.2: Effects on Eligibility and Enrollment

VARIABLES	(1) Elig.	(2) BH IV	(3) CG IV
Panel A: First Stage (Effects of IVs on Eligibility)			
IV	–	0.990***	0.942***
		(0.013)	(0.076)
R^2	–	0.990	0.979
Outcome mean	–	0.051	0.051
Observations	–	765	765
Panel B: Effects on Enrollment Rate			
Eligibility	0.197***	0.171***	0.173***
	(0.039)	(0.040)	(0.041)
R^2	0.863	0.247	0.247
Outcome mean	0.073	0.073	0.073
Observations	765	765	765
Model	OLS	2SLS	2SLS
Controls	X	X	X
Fixed effects	X	X	X
State coverage	51 states	51 states	51 states
Time coverage	1996-2010	1996-2010	1996-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: The IVs used in Columns (2) and (3) are generated following Borusyak and Hull's (2023) and Currie and Gruber's (1996) methodology, respectively. Panel A shows the effects of each IV on state-by-year eligibility rates, using an OLS regression. Panel B shows the effects of either the eligibility rate (Column 1) or the eligibility rate, as proxied by an IV (Columns 2-3), on the state-by-year enrollment rate. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Standard errors are clustered at the state level, and observations are weighted by population. Outcome means are in per-capita units. Sources: Modified MWD, CPS.

2.5.2 Overall Results

Table 2.3, Panel A, Column (2) shows that for the sample time range, a 1 p.p. increase in a state's Medicaid eligibility rate is estimated to lead to an increase of approximately 740 (sample mean: 703 per 100,000 persons) SUD treatment admission episodes per 100,000 persons. This aligns strongly with results on OUD treatment episodes and prescription OUD treatment episodes, as discussed in Section 1.5.

The estimated effect on number of SUD facilities per 100,000 persons are displayed in Panel B, Column (2). In contrast to the strong, positive effects on SUD treatment episodes, the number of SUD facilities is estimated to have fallen by about 2.45 facilities per 100,000 persons, for each 1 p.p. increase in a state's Medicaid eligibility rate. That said, the standard error for this estimate is fairly large, and the estimate is not statistically significant at the 5% level.

Estimated effects on overall daily client loads at SUD facilities are economically small and statistically insignificant, as shown in Panel C, Column (2)[19].

For each of these outcomes, results are similar when using either the CG IV (Column 3) or an OLS regression (Column 1), although the estimate for number of SUD facilities is statistically significant when using the CG IV. Estimates change meaningfully when controls and/or fixed effects are omitted (Columns 3 and 4 of Tables 2.A2, 2.A3, and 2.A4), suggesting that their inclusion is important, especially for numbers of SUD facilities and client loads at SUD facilities. This sensitivity to omitting control variables suggests that the variables derived from N-SSATS data may be particularly sensitive to omitted variable bias, while effects on SUD treatment episodes are consistently positive, large, and statistically significant

[19]Effects on annual client counts are more negative (with the 2SLS model using the BH IV, I estimate an effect of -480 per 100,000 persons, relative to a mean of 1,411), but still not statistically significant at the 5% level. Only the OLS estimate is statistically significant at the 10% level, with a magnitude of -670, relative to a mean of 1,411. I prefer to use client loads on the reference dates because N-SSATS provides more detailed information on its reference-date client population than the population served annually. Also, survey respondents report client loads in 2,671 more observations (about 1.7% more) than annual counts.

at at least the 10% level.

2.5.3 Results by Type of SUD Facility

In Section 2.5.2, I find that while SUD treatments increase in response to increasing access to Medicaid, there are no increases in numbers of SUD facilities or their observed daily client loads. How is this possible?

One possibility is that one type of SUD facility might be growing in number and drive results for episodes, but I find that the suggestive decline in SUD facility number appears to be broad-based across a variety of types of facilities[20].

Once I look at effects on clients, however, again split by service type (in this case, whether they received detoxification services) and facility type, I do find heterogeneous effects. Results from 2SLS regressions using the BH IV are shown in Table 2.4. The estimated effect on inpatient client counts (Column 1) is positive, large, and highly significant. Notably, the effects on clients receiving detoxification services at inpatient facilities (Column 5) is also positive and statistically significant, and it accounts for more than half of the size of the overall effect[21] on the client load in inpatient SUD facilities. Effects on other categories, including the number of clients receiving detoxification services, are negative but statistically insignificant.

2.5.4 Robustness

For my estimation strategy to be valid, the IVs must be independent of any unobserved variable that affects one or more of the outcomes. This assumption could fail, for example, if other federal or state policy changes were correlated both with the IVs and with SUD

[20]Results are reported in Table 2.A5 in the Appendix.

[21]The remaining portion is almost entirely explained by an increase in clients receiving inpatient rehabilitation treatment, the effect on which is 5.90 (std. err.: 3.44).

Table 2.3: Overall Results for SUD Treatment Admissions, Facilities, and Client Loads

	(1)	(2)	(3)
VARIABLES	OLS	2SLS: BH	2SLS: CG
Panel A: SUD Episodes per 100,000 Persons			
Eligibility	834***	740***	859***
	(217)	(228)	(276)
Observations	745	745	745
R^2	0.930	0.107	0.107
Outcome mean	703	703	703
Time coverage	1996-2010	1996-2010	1996-2010
Panel B: SUD Facilities per 100,000 Persons			
Eligibility	-2.52	-2.45*	-3.74**
	(1.71)	(1.48)	(1.63)
Observations	561	561	561
R^2	0.942	0.146	0.144
Outcome mean	6.05	6.05	6.05
Time coverage	1998, 2000, 2002-2010		
Panel C: SUD Facility Client Load per 100,000 Persons			
Eligibility	-73.4	-12.5	21.6
	(141.8)	(150.1)	(170.7)
Observations	561	561	561
R^2	0.945	0.183	0.182
Outcome mean	402.1	402.1	402.1
Time coverage	1998, 2000, 2002-2010		

Clustered standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: Column (1) lists OLS estimates of effects of the state-by-year eligibility rate on the respective outcomes, while Columns (2) and (3) use 2SLS models with simulated IVs (Borusyak and Hull, 2023, and Currie and Gruber, 1996, respectively). Outcomes are state-by-year SUD treatment admission episodes per 100,000 persons (Panel A), SUD treatment facilities per 100,000 persons (Panel B), and client loads per 100,000 persons at SUD treatment facilities (Panel C). Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. 1996, 1997, 1999, and 2001 are missing in N-SSATS data. Sources: TEDS-A, N-SSATS, Modified MWD, CPS.

Table 2.4: 2SLS Effects Using BH IV (Types of SUD Treatment Clients)

VARIABLES	(1) Inpatient	(2) Residential	(3) Outpatient	(4) Detox	(5) Inp. x Detox	(6) Res. x Detox	(7) Outpat. x Detox
Eligibility	15.4***	-11.5	-16.4	-9.99	9.49***	-8.20	-11.3
	(5.9)	(21.0)	(137.4)	(14.91)	(3.14)	(5.62)	(15.7)
Constant	200*	769	12,473***	-34.7	50.5	326***	-411
	(113)	(471)	(3,403)	(495.5)	(86.1)	(107)	(467)
Observations	561	561	561	561	561	561	561
R^2	0.083	0.161	0.176	0.135	0.045	0.092	0.157
Outcome mean	5.31	33.9	363	10.6	2.21	2.92	5.43
Controls	X	X	X	X	X	X	X
Fixed effects	X	X	X	X	X	X	X
Time coverage	1998, 2000, 2002-2010						

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. Outcomes are effects on client loads per 100,000 persons for major types of SUD facilities, observed on the facilities' respective reference dates: clients at inpatient, residential, and outpatient facilities, clients that receive detoxification services, and clients that receive detoxification services at inpatient, residential, and outpatient facilities, respectively. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The years 1996, 1997, 1999, and 2001 are missing for the outcome variables. Sources: N-SSATS, Modified MWD, CPS.

treatment outcomes.

It is unlikely that the above results are driven by changes to federal programs other than Medicaid. For this to be the case, my variation in eligibility thresholds would have to correlate strongly enough with the between-state differences in and/or within-state changes to a separate program's characteristics over the course of the sample. As a test of whether this might be the case for one program affected by the 1996 welfare reforms, I repeat the main analysis, this time including a control for the annual share (calculated from CPS data) of households in each state with someone enrolled in SNAP (also known as food stamps). Including this control changes results very little (see Table 2.A6 in the Appendix), suggesting that SNAP is not driving results.

Additionally, it does not appear that contemporaneous changes to alternative pathways to Medicaid are driving results. As discussed in Section 1.5.6 in the first chapter of this dissertation, I am unable to reject the hypothesis that effects of the IVs on enrollment are similar for individuals who report unemployment due to disability or retirement[22], compared to the effects for the full sample. This suggests that my main results are not being driven by confounding effects from disabled individuals.

As one might be concerned that politics may influence both Medicaid policies and opioid use, I default to including controls for Democratic vote shares in general presidential elections. If I also include Democratic vote shares in general elections for senator and governor, estimated effects (columns 4-6 in Table 2.A7) are statistically similar to effects (columns 4-6 in the same table) estimated from a similar (i.e. excluding Washington, DC) regression in which these two variables are omitted (columns 1-3).

Another concern is measurement error. To test the sensitivity of my analysis to measurement error, I create a smaller sample that omits a subset of states that may be particularly susceptible to having either poorly measured (in some sense) eligibility thresholds or in-

[22]Although effect for this population are imprecisely estimated

terstate opioid spillovers. To create this sample, I follow Hamersma (2013) in dropping Minnesota and Oregon from my analysis due to their large state health programs, and I also drop Vermont, because (as discussed in Section 3.4) the eligibility thresholds I use for that state may be more generous than actually applied to most Medicaid applicants. One more reason to drop Oregon is that it capped enrollment every year beginning in 2004, so the income thresholds I use were not as meaningful for determining enrollment during the affected years. Additionally, there may be spillovers in opioid access from Florida, so I also drop Florida and its adjacent neighbors (South Carolina, Alabama, and Georgia) to reduce the role that interstate spillovers might play in affecting results. Table 2.5 shows that when applying the main 2SLS estimation strategy to this sample, effects on all outcomes are similar to effects across the full sample. This suggests that measurement error and cross-state opioid spillovers do not drive results.

The data on treatment admissions has reporting differences between states and is not balanced across years, with ten different states missing data over a combined 20 years. Table 2.6 shows that when I omit these ten states from the sample, estimates are again similar, albeit less precisely estimated, across all three of the main outcome variables. This suggests that differential TEDS-A reporting practices among the unbalanced states are not driving results for treatment episodes.

Another question is whether effects on SUD treatment episodes would be meaningfully different if I restrict the treatment episode sample for the years covered in the N-SSATS data I use: 1998, 2000, and 2002 through 2010. Table 2.7 shows estimated effects if I restrict my analysis to these years. The estimated effect on SUD treatment episodes (Column 1) is somewhat smaller in magnitude on this sample than estimated using the full sample, but it is still economically large and statistically significant.

Taken together, the results presented in Section 2.5 appear to be robust and do not appear to depend on factors such as separate policy changes or imperfect data reporting.

Table 2.5: 2SLS Estimates (Within Subset of States)

	(1)	(2)	(3)
VARIABLES	Episodes	Facilities	Client Load
Eligibility	709***	-2.05	-11.7
	(259)	(1.51)	(154.1)
Constant	-2,444	68.1	13,986***
	(9,202)	(80.5)	(4,539)
Observations	641	484	484
R^2	0.118	0.183	0.188
Outcome mean	695	6.26	411
Controls	X	X	X
Fixed effects	X	X	X
State coverage	44 states	44 states	44 states
Time coverage	1996-2010	1998, 2000, 2002-2010	

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates, within a restricted sample of states: I remove states (OR, MN, VT) with thresholds that may be especially poor reflections of true access to subsidized healthcare through Medicaid or a similar program, and FL (a pill-mill hub leading up to 2010) and its adjacent neighbors, AL, SC, and GA. Outcomes are, respectively, state-by-year SUD treatment admission episodes per 100,000 persons, SUD treatment facilities per 100,000 persons, and client loads per 100,000 persons at SUD treatment facilities on the given reference dates. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. Sources: TEDS-A, N-SSATS, Modified MWD, CPS.

Table 2.6: 2SLS Estimates (Within Balanced TEDS-A Sample)

	(1)	(2)	(3)
VARIABLES	Episodes	Facilities	Client Load
Eligibility	719**	-2.69	51.3
	(339)	(2.26)	(222.6)
Constant	1,291	-18.1	11,348***
	(9,392)	(95.7)	(3,290)
Observations	615	451	451
R^2	0.125	0.159	0.188
Outcome mean	731	5.89	397
Controls	X	X	X
Fixed effects	X	X	X
State coverage	41 states	41 states	41 states
Time coverage	1996-2010	1998, 2000, 2002-2010	

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates, within a restricted sample of states. I remove states that have at least one year missing in TEDS-A data: AK, AL, AR, AZ, DC, IN, KY, MS, WV, and WY. Outcomes are, respectively, state-by-year SUD treatment admission episodes per 100,000 persons, SUD treatment facilities per 100,000 persons, and client loads per 100,000 persons at SUD treatment facilities on the given reference dates. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Sources: TEDS-A, N-SSATS, Modified MWD, CPS.

Table 2.7: 2SLS Estimates (Dropping Years Missing in N-SSATS)

	(1)	(2)	(3)
VARIABLES	Episodes	Facilities	Client Load
Eligibility	513***	-2.67*	-152
	(192)	(1.57)	(152)
Constant	3,428	-12.0	9,202***
	(10,779)	(62.8)	(2,718)
Observations	548	561	561
R^2	0.113	0.417	0.187
Outcome mean	712	6.05	402
Controls	X	X	X
Fixed effects	X	X	X
State coverage	51 states	51 states	51 states
Time coverage	1998, 2000, 2002-2010		

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates, within a restricted sample of years: I remove 1996, 1997, 1999, and 2001, which are missing in N-SSATS. Outcomes are, respectively, state-by-year SUD treatment admission episodes per 100,000 persons, SUD treatment facilities per 100,000 persons, and client loads per 100,000 persons at SUD treatment facilities on the given reference dates. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. Sources: TEDS-A, N-SSATS, Modified MWD, CPS.

2.5.5 Discussion

I have shown that between 1996 and 2010, increasing access to Medicaid in a state led to a highly statistically significant increase in SUD treatment episodes. At the same time, the number of facilities offering SUD services has a suggestive negative effect across types of SUD facilities, and while client loads do not increase overall, they do increase dramatically at inpatient facilities. Within inpatient facilities, there is a particularly large increase in clients receiving detoxification services, which tend to be shorter in length than rehabilitative treatment.

Taken together, these results suggest the following sequence of events may be occurring. First, an increase in access to Medicaid allows hospitals with inpatient SUD-treatment facilities to admit more SUD patients, perhaps because the marginal patient presents less financial risk to the hospital's inpatient services than previously. Second, these patients are disproportionately placed in intensive, but relatively quick, detoxification treatments. Third, given that overall client numbers at a given point in time are not increasing, but total SUD treatment episodes are increasing, this discrepancy suggests that client stays must become shorter on average after Medicaid expansions[23] – which is consistent with the finding that client loads increase for inpatient detoxification services, which end more quickly than other SUD treatment options.

Is increased use of fast-paced detoxification services at inpatient facilities a good thing or a bad thing? It depends. If none of the individuals who received detoxification services went on to receive rehabilitation or maintenance treatment, we might suspect that clients are simply churning faster through the system without meaningful long-term health benefits. However, hospitals also increased their client loads for rehabilitation, and while we cannot track individual clients, this suggests that for many clients, hospitals may have continued

[23] This could be tested explicitly in future research. One option would be to use SAMHSA's "Treatment Episode Data Set – Discharges" (TEDS-D) data, although the data therein does not begin until 2006.

treatment for the longer term after leaving detoxification[24].

With the TEDS-A data, I can make some progress in exploring whether the same clients were receiving more treatments in states with Medicaid expansions. While I cannot track individual clients over time, TEDS-A does include a variable for number of prior visits for each client who receives SUD treatment. If I repeat my preferred 2SLS model with the BH IV, this time using the state-by-year mean number of clients' prior visits as the dependent variable, I estimate an effect of 0.0587 (std. err.: 0.0348, sample mean: 0.0614), which is large and statistically significant at the 10% level. However, as some of this increase in number of prior visits may account for clients transitioning from detoxification to rehabilitation treatments, it is not clear how much of this increase, if any, should be considered concerning.

Some of the estimated increase in treatment episodes appears to be driven by long-term treatment regimes that should have higher long-term success rates than detoxification treatments alone. That said the larger coefficient for client loads in detoxification services than in rehabilitation, combined with a positive and large (though imprecise) coefficient for average number of prior visits, do give some cause for concern. More research is needed on what these results mean and whether they apply to the current Medicaid program, which has changed substantially, but these results suggest that Medicaid brought differentially beneficial impacts for at least some individuals with SUD, which may explain at least part of the negative effects found on opioid overdose deaths in Section 1.5.

[24]Another possibility, which I am unable to test within the data I have, is that the main change occurred within hospitals, with hospitals simultaneously reducing their outpatient SUD treatment client loads and increasing their inpatient SUD treatment client loads. If this is the case, then the public health outcomes from this change would simply depend on whether hospitals provide more effective SUD treatment services within outpatient or inpatient facilities. I suspect that simple shifts of clients within hospital complexes are not driving my results, however, because if this were the case, then it is unclear why I find that SUD treatment episodes increased dramatically despite daily client loads not increasing overall.

2.5.6 Comparison to Past Literature

Consistent with my findings for SUD treatment episodes and facilities after Medicaid expansions between 1996 and 2010, Meinhofer and Witman (2018) find that after the Medicaid expansions between 2007 and 2016 (most of which took place following the 2010 passage of the Affordable Care Act, or ACA), OUD treatment episodes increased, while there was no statistically significant increase in the number OUD facilities reported in N-SSATS. At the same time, there was a positive effect on specialized clinics that focused on treating OUD with buprenorphine and were not covered by N-SSATS. While these clinics did exist during some of my sample period (especially closer to 2010), buprenorphine was not approved by the FDA until 2002, and these clinics were capped at 30 clients each until 2006, after which point the patient limit grew to 100 (Jones et al., 2018). As such, any unobserved role of specialized, buprenorphine-dispensing clinics omitted from N-SSATS is expected to be much smaller within this paper's sample time frame than in the later time frame used by Meinhofer and Witman (2018). Even so, caution is warranted, as there could be other SUD facilities that are not captured in N-SSATS and could have had economically meaningful effects from Medicaid that I do not observe between 1996 and 2010.

Meinhofer and Witman (2018) do not look at client loads reported by N-SSATS respondents, but they do find that within TEDS-A data between 2007 and 2016, OUD treatment episodes increase for rehabilitative services in outpatient settings and not for other types of services and settings. This is markedly different from my finding that between 1996 and 2010, increases in SUD client loads[25] were concentrated in inpatient settings, with a stronger estimate for detoxification treatments in inpatient facilities than for rehabilitation treatments therein. The difference between this set of results and those from Meinhofer and Witman (2018) aligns fairly well with changes in Medicaid coverage of SUD treatment before versus

[25] And presumably also SUD treatment episodes, although I am unable to verify this, because the variable in TEDS-A for service and setting type is missing in about 76% of episodes in my sample.

after the ACA. Before the ACA, coverage of SUD treatment varied from state to state and often focused on short-term, detoxification treatments (Andrews et al., 2015), which could explain why inpatient detoxification treatments saw the most growth in client loads, according to my findings. In contrast, the ACA forced Medicaid programs across the country to cover a much wider range of SUD treatment services, which is consistent with Meinhofer and Witman's 2018 findings that from 2007 to 2016, effects on outpatient rehabilitation treatments were strongest, with effects driven by Medicaid enrollees. This connection between results and Medicaid policy is only suggestive, however, and more research is warranted.

2.6 Conclusion

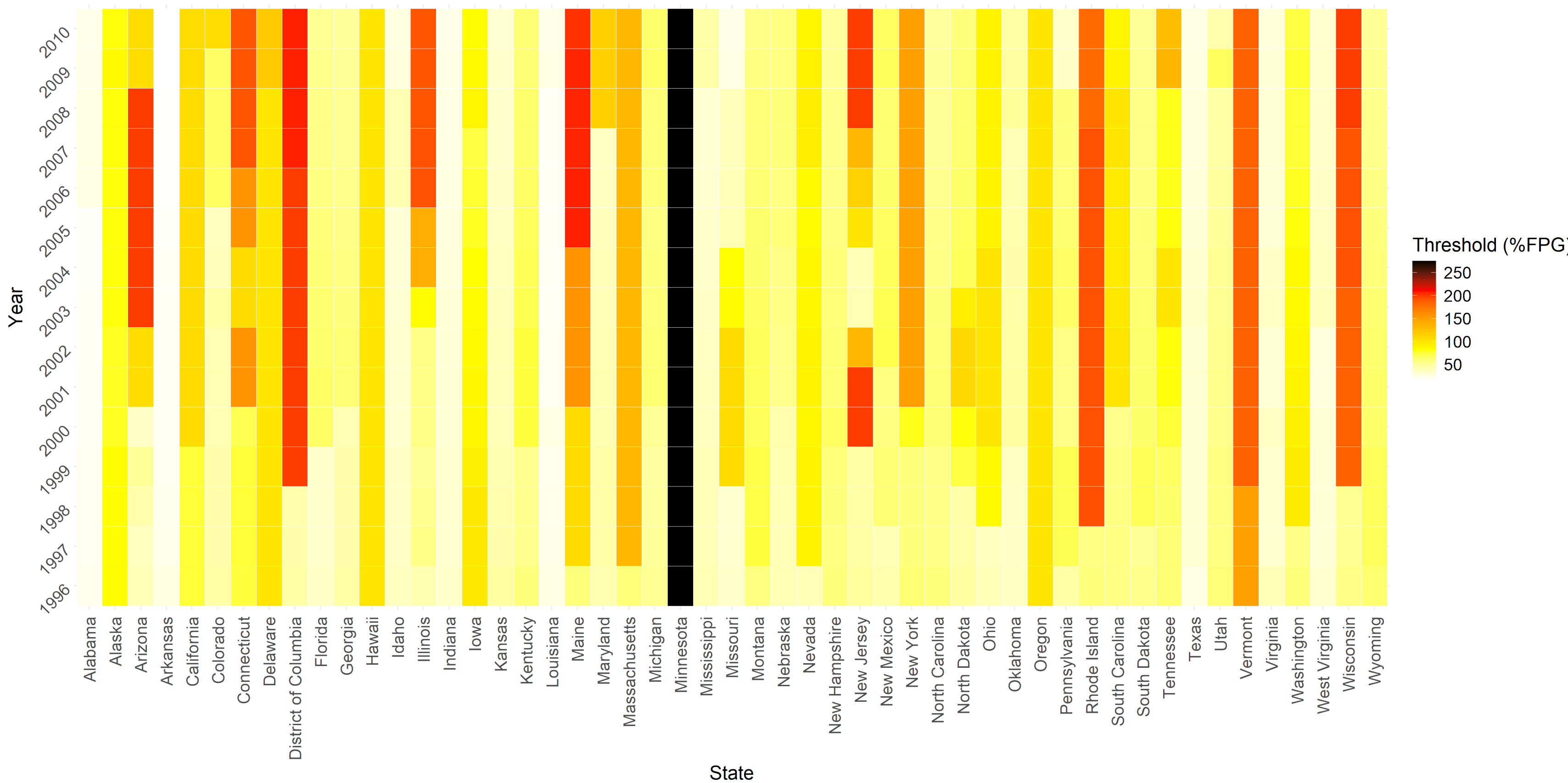
In this paper, I show that increasing access to Medicaid between 1996 and 2010 had the following same-year effects on SUD treatment utilization. First, SUD treatment episodes increased dramatically. Second, numbers of SUD treatment facilities and client loads (as given on a specific reference date) at those facilities did not increase. Third, client loads only increased at inpatient facilities, with a stronger increase at these facilities among detoxification treatments than rehabilitation treatments. Tying these results together, there is strong evidence that increases in Medicaid access at the state level led not to increased market-wide SUD treatment capacity, but to increased capacity for SUD treatment only within inpatient facilities, and more especially within detoxification programs. In other words, hospital inpatient facilities gained market share within the SUD treatment market, perhaps because the marginal client presented less financial risk to inpatient detoxification programs, while other programs may have had less access to Medicaid payments for SUD treatment at the time. Sharply increasing SUD treatment episode counts came as a result of the fact that these inpatient detoxification programs were short-term programs that typically ended much more quickly than rehabilitation or maintenance programs. For clients showing up with SUD treatment episodes, there was a large, though imprecisely estimated, increase in the number of the average client's prior treatment episodes, suggesting an increasing rate of clients churning multiple times through separate SUD treatments. While this would potentially be a good indicator if it suggested that more clients were transitioning from detoxification to rehabilitation, and the positive and fairly large coefficient on client loads in inpatient rehabilitation programs suggests this was happening, the much larger coefficient on client loads for inpatient detoxification treatments suggests that many patients who were induced into detoxification did not receive the longer-term care they might need to help control their addictions over the long run.

Meinhofer and Witman (2018) provide a useful contrast to this paper, suggesting that at least for OUD treatments, ACA policies led more recent Medicaid expansions to have effects concentrated more on long-term rehabilitation programs, rather than short-term detoxification programs. Further research is necessary to understand whether ACA policies can be ascribed to this difference in results, what public health outcomes can be attributed to varying changes in SUD treatment provision over the past three decades, and whether dynamic effects differ from the same-year effects I estimate in this paper.

2.A1 Appendix

2.A1.1 Figures

Figure 2.A1: Medicaid Income Eligibility Thresholds for Parents



Notes: FPG stands for "Federal Poverty Guideline." Thresholds based on data for family sizes of three. Source: Modified MWD.

2.A1.2 Tables

Table 2.A1: State Eligibility Thresholds (%FPG) for adults

	Parents		Childless	
State	1996	2010	1996	2010
AK	83	81	0	0
AL	23	24	0	0
AR	27	17	0	0
AZ	40	106	0	100
CA	76	106	0	0
CO	47	106	0	0
CT	75	191	0	56
DC	47	207	0	133
DE	100	120.5	100	100
FL	36	53	0	0
GA	47	50	0	0
HI	100	100	100	100
IA	98	83	0	0
ID	38	27	0	0
IL	43	191	0	0
IN	35	25	0	0
KS	48	32	0	0
KY	59	62	0	0
LA	26	25	0	0
MA	60	133	0	0

Continued on next page

Table 2.A1 (continued)

	Parents		Childless	
State	1996	2010	1996	2010
MD	43	116	0	0
ME	59	203	0	100
MI	53	64	0	0
MN	275	275	75	75
MO	35	25	0	0
MS	42	46	0	0
MT	58	56	0	0
NC	59	49	0	0
ND	48	59	0	0
NE	42	58	0	0
NH	59	49	0	0
NJ	49	200	0	0
NM	44	67	0	0
NV	40	88	0	0
NY	62	150	78	100
OH	40	90	0	0
OK	37	47	0	0
OR	100	100	100	100
PA	47	34	0	0
RI	59	181	0	0
SC	57	89	0	0
SD	55	52	0	0

Continued on next page

Table 2.A1 (continued)

	Parents		Childless	
State	1996	2010	1996	2010
TN	62	129	0	0
TX	26	26	0	0
UT	61	44	0	0
VA	41	29	0	0
VT	150	185	150	150
WA	59	74	0	0
WI	56	200	0	0
WV	33	33	0	0
WY	63	52	0	0

Source: Modified MWD.

Table 2.A2: 2SLS Effects on SUD Treatment Admissions per 100,000 Persons

	(1)	(2)	(3)	(4)	(5)
VARIABLES	OLS	2SLS	2SLS	2SLS	2SLS
Eligibility	834***	4,756***	674***	2,500*	740***
	(217)	(1,665)	(255)	(1,314)	(228)
Constant	3,833	397***	613***	-12,692	3,704
	(9,064)	(85)	(13)	(9,382)	(8,039)
Observations	745	745	745	745	745
R^2	0.930	0.208	0.016	0.523	0.107
Outcome mean	703	703	703	703	703
Controls	X			X	X
Fixed effects	X		X		X
Time coverage	1996-2010	1996-2010	1996-2010	1996-2010	1996-2010

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: Column (1) lists OLS effects of the state-by-year eligibility rate on the respective outcomes. Columns (2) through (5) list 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome is state-by-year SUD treatment episode admissions per 100,000 persons. Columns (1), (3), and (5) control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Columns (1), (3), and (5) control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. Sources: TEDS-A, Modified MWD, CPS.

Table 2.A3: 2SLS Effects on SUD Facilities per 100,000 Persons

	(1)	(2)	(3)	(4)	(5)
VARIABLES	OLS	2SLS	2SLS	2SLS	2SLS
Eligibility	-2.52	12.1**	-2.72	2.85	-2.45*
	(1.71)	(5.9)	(2.33)	(4.55)	(1.48)
Constant	29.9	4.36***	5.19***	-13.1	26.3
	(85.7)	(0.48)	(0.13)	(49.1)	(77.3)
Observations	561	561	561	561	561
R^2	0.942	0.035	0.014	0.504	0.146
Outcome mean	6.05	6.05	6.05	6.05	6.05
Controls	X			X	X
Fixed effects	X		X		X
Time coverage	1998, 2000, 2002-2010				

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: Column (1) lists OLS effects of the state-by-year eligibility rate on the respective outcomes. Columns (2) through (5) list 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome is state-by-year SUD treatment facilities per 100,000 persons. Columns (1), (3), and (5) control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Columns (1), (3), and (5) control for state and year fixed effects. Sources: N-SSATS, Modified MWD, CPS.

Table 2.A4: 2SLS Effects on SUD Facility Client Load per 100,000 Persons

	(1)	(2)	(3)	(4)	(5)
VARIABLES	OLS	2SLS	2SLS	2SLS	2SLS
Eligibility	-73.4	1,485**	0.473	404	-12.5
	(141.8)	(585)	(153.2)	(434)	(150.1)
Constant	13,093***	294***	377***	-8,270***	13,442***
	(3,785)	(39)	(8.570)	(2,998)	(3,400)
Observations	561	561	561	561	561
R^2	0.945	0.158	–	0.617	0.183
Outcome mean	402	402	402	402	402
Controls	X			X	X
Fixed effects	X		X		X
Time coverage	1998, 2000, 2002-2010				

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: Column (1) lists OLS effects of the state-by-year eligibility rate on the respective outcomes. Columns (2) through (5) list 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. The outcome is state-by-year client count per 100,000 persons at SUD treatment facilities on the given respective date. Columns (1), (3), and (5) control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Columns (1), (3), and (5) control for state and year fixed effects. Sources: N-SSATS, Modified MWD, CPS.

Table 2.A5: 2SLS Effects Using BH IV (Types of SUD Facilities)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Inpatient	Residential	Outpatient	Detox	Inp. x Detox	Res. x Detox	Out. x Detox
Eligibility	-0.057	-1.43*	-2.05	-1.44	-0.311	-0.535	-0.580
	(0.234)	(0.79)	(1.57)	(1.90)	(0.266)	(0.632)	(0.685)
Constant	20.5**	9.87	-60.9	72.1	34.8***	2.40	35.5**
	(9.0)	(23.74)	(55.2)	(90.3)	(8.4)	(14.35)	(14.8)
Observations	510	510	510	510	459	459	459
R^2	0.113	0.110	0.126	0.122	0.143	0.140	0.089
Outcome mean	0.427	1.53	5.08	6.04	0.321	0.460	0.899
Controls	X	X	X	X	X	X	X
Fixed effects	X	X	X	X	X	X	X
Time coverage	2000, 2002-2010				2002-2010		

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. Outcomes are effects on counts per 100,000 persons for major types of SUD facilities: inpatient, residential, and outpatient facilities, facilities that offer detoxification services, and inpatient, residential, and outpatient facilities that offer detoxification services, respectively. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Regressions also control for state and year fixed effects. The years 1996, 1997, 1999, 2000, 2001, and, for facilities with detox services, 1998 are missing for the outcome variables. Sources: N-SSATS, MCOD, Modified MWD, CPS.

Table 2.A6: 2SLS Effects (Add SNAP)

	(1)	(2)	(3)
VARIABLES	Episodes	Facilities	Client Load
Eligibility	744***	-2.43*	-8.69
	(230)	(1.47)	(141.3)
Constant	3,524	19.0	11,988***
	(8,102)	(74.7)	(3,286)
Observations	745	561	561
R^2	0.107	0.149	0.205
Outcome mean	703	6.05	402
Controls	X	X	X
SNAP Control	X	X	X
Fixed effects	X	X	X
Time coverage	1996-2010	1998, 2000, 2002-2010	

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. Outcomes are state-by-year SUD treatment admission episodes per 100,000 persons, SUD treatment facilities per 100,000 persons, and client loads per 100,000 persons at SUD treatment facilities on the given reference dates, respectively. Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; unemployment rate; and share enrolled in SNAP. Regressions also control for state and year fixed effects. Some states are missing for one or more years in TEDS-A data. 1996, 1997, 1999, and 2001 are missing in N-SSATS data. Sources: TEDS-A, N-SSATS, Modified MWD, CPS.

Table 2.A7: 2SLS Effects (Add Senatorial and Gubernatorial Vote Shares)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Episodes	Facilities	Client Load	Episodes	Facilities	Client Load
Eligibility	724***	-2.65*	-12.7	740***	-1.90	35.3
	(229)	(1.41)	(154.8)	(232)	(1.43)	(147.3)
Constant	4,504	-7.37	14,006***	4,584	-22.9	13,843***
	(8,331)	(86.20)	(3,409)	(8,405)	(76.9)	(3,300)
Observations	734	550	550	734	550	550
R^2	0.106	0.149	0.174	0.106	0.220	0.218
Outcome mean	702.9	5.985	392.1	702.9	5.985	392.1
Controls	X	X	X	X	X	X
All Poli. Controls				X	X	X
Fixed effects	X	X	X	X	X	X
State coverage	50 states	50 states	50 states	50 states	50 states	50 states
Time coverage	1996-2010	1998, 2000, 2002-2010		1996-2010	1998, 2000, 2002-2010	

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows 2SLS effects using Borusyak and Hull's (2023) IV to proxy for state eligibility rates. Outcomes are state-by-year SUD treatment admission episodes per 100,000 persons (Columns 1 and 4), SUD treatment facilities per 100,000 persons (Columns 2 and 5), and client loads per 100,000 persons at SUD treatment facilities on the given reference dates (Columns 3 and 6). Regressions control for the following variables: state population, state female, Hispanic, White, and Black shares; state share 0-11, 12-17, 18-24, 25-44, 45-64, 65+ years old; state share educated with no college, some (<4 yrs.) college, and at least 4 years of college; state Democratic vote shares in the most recent general presidential elections and, in Columns 4-6, gubernatorial, and senatorial elections; state PDMP (any and must-access) existence, pill mill laws, medical marijuana laws, and existence of active and legal medical marijuana dispensaries; and unemployment rate. Some states are missing for one or more years in TEDS-A data. Washington, DC is dropped from this sample, as it does not hold gubernatorial or senatorial elections. Sources: TEDS-A, N-SSATS, Modified MWD, CPS.

Chapter 3

A Novel Collection of Medicaid Thresholds from Between 1996 and 2010

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Abstract

In the United States, each state is allowed to set a different income threshold for individuals to receive Medicaid eligibility, and these thresholds occasionally change within states. This creates a series of natural experiments that researchers can leverage to study the effects of healthcare access for the poor in the country. However, there is no official record containing all thresholds used from the 1990s through today, and existing collections of thresholds have missing years for at least some states. Contradictions also exist between sources. I gather thresholds from various sources, attempting to resolve missing data and conflicts in a transparent manner, in order to produce the first complete collection of income thresholds for Medicaid eligibility for all working age, able-bodied adults, in all states and all years from 1996 through 2010 – a time period during which many states changed their Medicaid eligibility criteria, and some features of the Medicaid program were fundamentally different than they are now, due in large part to the 2010 Affordable Care Act.

3.1 Introduction

Medicaid, the US government's affordable health insurance program for the poor and needy, has experienced wide variation between states and over time, particularly in terms of eligibility criteria for individuals to enter the program. Under certain assumptions, this variation creates a natural experiment that researchers could potentially use to test effects of Medicaid access, which is a key policy question since Medicaid is a costly program[1]. To this day, an authoritative collection of how these criteria have evolved over time in each state is not provided publicly by the federal government, however.

In lieu of official, government-sponsored documentation, the Kaiser Family Foundation (KFF) has posted highly-cited tables of income thresholds for Medicaid eligibility in every state and every year from 2011 onwards, plus most years before 2011 all the way back to 2002 [2]. This easily-accessible data has been crucial for the now-large literature on the effects of the 2010 Affordable Care Act (ACA), which led many states to expand their Medicaid programs by increasing their income thresholds, over the following years. And yet, Medicaid expansions did not begin with the ACA, and information on income thresholds across the country becomes harder to find the earlier one looks.

For the years preceding the ACA, what has evolved is an occasional effort by researchers to use what contemporary documentation is available[3], plus whatever information they can get from government officials and other sources, to piece together a patchwork of income thresholds for whatever time period and population they intend to focus on. I build on these efforts by combining contemporary reports and existing collections of thresholds to create, to my knowledge, the first complete collection of income thresholds that states used

[1]Total Medicaid spending reached $919 billion in the 2024 federal fiscal year (Williams et al., 2026).

[2]Their tables for parents can be found at Kaiser Family Foundation (2025).

[3]This documentation is typically provided by reports from KFF and the Center on Budget and Policy Priorities.

to determine Medicaid eligibility for non-elderly, non-disabled adults by all states, for all years between 1996 and 2010.

In other parts of this dissertation, I demonstrate the potential of this collection of thresholds by exploring impacts of Medicaid access on opioid use and on substance use disorder treatment. In this chapter, I describe how I collected these thresholds and discuss key decisions that I made to prepare and clean it, and I also discuss how some of my thresholds differ from those in past collections and why.

The remainder of this paper is constructed as follows. Section 3.2 discusses the Medicaid program in the context of this paper. Section 3.3 introduces the data I use. Section 3.4 explains my methodology for determining which threshold to use for each state and year, and Section 3.5 shows how I use the thresholds to determine whether people in individual-level data appear to be eligible for Medicaid. In Section 3.6, I compare my collection of thresholds to the collections that have hitherto been the most comprehensive, and I conclude in Section 3.7.

3.2 Background

Each state sets its own eligibility requirements for individuals to be allowed to enroll in Medicaid. As Medicaid is intended to be targeted mainly to the poor, household income plays a role as a key determining factor towards eligibility (Currie and Gruber, 1994). For most non-elderly adults[4], eligibility depends on whether their household income falls below the applicable income threshold set by their state of residence. This is normally set as a percentage of the Federal Poverty Guideline (FPG)[5] for an individual's household size,

[4]There are eligibility categories that are particularly generous for certain populations, such as pregnant individuals (Gruber, 2000) and children (defined as younger than 19 years old; Dubay, Hill, and Kenney, 2002). There are also special sets of criteria that do not focus so strongly on income, which can apply to certain populations, such as disabled and/or elderly individuals (Gruber, 2000; Rudowitz et al., 2024).

[5]FPG values are obtained at https://aspe.hhs.gov/topics/poverty-economic-mobility/poverty-guidelines/prior-hhs-poverty-guidelines-federal-register-references.

and parents usually have higher thresholds (in terms of percent of the FPG) than childless adults[6]. Constructing these thresholds for non-elderly adults without disabilities, between the years of 1996 and 2010, is the focus of this paper.

Medicaid offers coverage to beneficiaries at low (and often no) cost for a wide range of medical services, including primary, urgent, and emergency care, and surgeries. As discussed by Gruber (2000), states have far less leeway over which services are covered by Medicaid, compared to their ability to choose eligibility criteria. As such, the range of healthcare options that an enrolled individual has access to in one state should be largely similar to the options they would have if enrolled in another state.

This paper focuses on the time frame between 1996 and 2010 for the following reasons. First, in 1996, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) was signed in 1996. Prior to this law, states' thresholds for Medicaid eligibility were the same as their thresholds for eligibility for Aid to Families with Dependent Children (AFDC), a cash aid program for the poor. As such, states' Medicaid thresholds only rose when the states were also willing to expand who was eligible for cash aid. By 1996, coverage was available for children, pregnant women, and certain other needy populations in most states, but coverage was limited for adults in most states. The passage of PRWORA ended AFDC, replacing it with Temporary Assistance for Needy Families (TANF), and de-linked eligibility criteria for Medicaid from those for TANF. States were now allowed to submit Section 1115 Waivers to change their income thresholds for Medicaid coverage without any other program being affected. Over the fifteen years that followed, many states expanded Medicaid at least once for parents, and in some cases even for childless adults.

I end my sample at 2010 because of the Affordable Care Act (ACA), which among many

[6] Note that in chapter 1 of this dissertation, I also split thresholds between unemployed versus employed individuals. As discussed in Section 3.4, I have since discovered that this distinction is not helpful when using data from the Current Population Survey (although it does not alter results meaningfully), and I no longer make this distinction in my complete eligibility thresholds in this chapter and in the second chapter of this dissertation. In future versions of the first chapter, I will apply this change as well.

other things included in this law (Rudowitz et al., 2024; Wachino, Artiga, and Rudowitz, 2014; Zuckerman, Skopec, and Epstein, 2017), dramatically expanded Medicaid's coverage of substance use disorder (SUD) treatment options . Thus, a Medicaid expansion before 2010 is not necessarily equivalent in coverage options to a Medicaid expansion after 2010, particularly within topics related to SUD outcomes, which are the focus of this dissertation.

3.3 Threshold Sources

The goal of this paper is to construct a complete, internally consistent dataset with reasonable estimates for the income thresholds used by states to determine eligibility for Medicaid each year, between 1996 and 2010, for parents and childless adults.

Since the federal government funds much of Medicaid, one might reasonably expect a comprehensive database with these thresholds to be maintained by the federal government on an official, public-facing website. Such a database does not appear to exist, unfortunately, so researchers have gone to painstaking lengths to document and collect the income thresholds over the past few decades.

3.3.1 Medicaid Waiver Database

The most comprehensive collection of Medicaid income thresholds for the time period of interest is the Medicaid Waiver Database (MWD) [7], which was constructed by Burns, Dague, and Kasper (2018a,b). This dataset includes "employed" and "unemployed" income eligibility thresholds (as a percent of the FPG) for childless adults and parents in household sizes of three, with documentation on important details behind most meaningful eligibility changes. There are also flags for years in which premiums or enrollment caps were used for a given

[7] Accessible at https://oaktrust.library.tamu.edu/items/e946d181-73a3-4d4f-91b8-9f7e463832b8, last accessed on May 19, 2025.

state policy. The authors report an extensive process for collecting and compiling the data they use, which included reviewing various types of documents and even physically traveling to the Centers for Medicare and Medicaid Services (CMS).

3.3.2 Other Collections of Thresholds

Medicaid thresholds from this time period have also been used by a handful of other papers. In particular, I draw from (Hamersma, 2013) and (Hamersma and Kim, 2013), who carefully document how they arrived at their threshold values for parents in household sizes of three, which are fully populated from 1996 through 2007. They also post these thresholds and summarize their methods for extrapolating from 3-person households to other household sizes[8].

I also draw from data collected by (McMorrow et al., 2016), which focuses only on thresholds before and after fairly sizable Medicaid expansions and contractions from this time period. This paper identifies a few key primary sources for the late 1990's that are not leveraged by MWD and are consistent with the numbers in MWD.

3.3.3 Contemporary Documentation of Thresholds

The backbone of the data collected by these researchers and I is formed by contemporaneous reports published by a variety of sources, especially the Kaiser Family Foundation (KFF) and the Center on Budget and Policy Priorities. Without these reports, this research would be impossible.

In particular, the following documents are especially helpful in deciding what thresholds to use for state-by-year observations: Guyer and Mann (1998b) provides values for 1996[9],

[8]They also offer to provide their data for other household sizes upon request, although I have not yet received a response to my request at the time of writing this paper.

[9]As a note on the values I use, I use Table 2, Column 1, compared to the 1996 FPG.

Guyer and Mann (1999b) and State Policy Documentation Project (2000) for 1999, Broaddus et al. (2002) for 2000, and Kaiser Family Foundation (2025) provides most of the values in MWD from 2002 onwards, along with context behind those values.

3.4 Threshold Construction

The MWD is arranged as follows. There are two spreadsheets, one with thresholds for parents in households of three people, and one for childless adults. Each of these files has two columns with eligibility thresholds, one for employed individuals and one for unemployed individuals. According to MWD, most states have no eligibility option for childless adults throughout most or all years in the sample period.

After reviewing source documents (e.g. Ross and Cox, 2002), the difference between "employed" versus "unemployed" thresholds appears to be based not directly on employment, but on how earned versus unearned income counted towards eligibility in each state[10]. While this differentiation is worth being aware of, the two thresholds are often relatively close together (when both are known), and distinguishing earned versus unearned income is impossible in the Current Population Survey (CPS) data that I use to calculate individuals' eligibility for Medicaid[11]. As such, I choose to ignore the "unemployed" thresholds when discussing how I construct this dataset. Instead, I use only the "employed" thresholds for all individuals[12].

A row in the MWD spreadsheets is defined as a Medicaid program within a state-year observation. A state may have multiple open programs in any given year, for the given

[10] Some states had earnings disregards for working adults, which were normally relatively small.

[11] As a side note, I do subtract welfare income from households' total income when I determine individuals' eligibility according to CPS responses.

[12] Main results within the first chapter in this dissertation (Section 1.5, where I am currently taking into account individuals' employment status when determining which threshold to use, are not meaningfully different if I instead use the employed threshold for everyone. Future iterations of that chapter will follow this methodology, which I view as more correct and more precise.

demographic (childless adults or parents), each with their own income threshold: 1) each state typically has one (sometimes more) Section 1931 Medicaid program, which maintains a minimum eligibility level based on 1996 AFDC eligibility requirements; 2) many states implement expansions through Section 1115 Waivers, which provide federal funding for proposed changes to the state's Medicaid program; and 3) some states have state health insurance programs whose reach goes beyond federally-funded Medicaid programs, although they often also deviate in other ways from a typical federally-funded program, e.g. by charging larger (or any) premiums, restricting access in atypical ways, or restricting coverage to a more limited array of services. MWD also records income thresholds for Medicaid programs implemented in response to the ACA, although these do not exist until after the ACA was passed in 2010.

For the purposes of this dissertation, I need to come up with one threshold per type of adult (parent or childless), per state and year. To achieve this, I must first define a "real" Medicaid program. I define this as a program that provides full Medicaid benefits to all people with income under the given threshold, within the applicable demographic, with low or no premiums[13]. I then use my judgment to choose which program best represents a "real" Medicaid program for each demographic, for each state and each year. This choice is easy when there is only one Section 1931 policy, and no other listed Medicaid program, in a given state and year: I just use that policy. When a state implements a Section 1115 Waiver, the Section 1931 policy typically continues to be recorded in MWD but is functionally overridden by the Section 1115 Waiver for applicants, so I use the Section 1115 Waiver once it launches – as long as the notes in MWD support the default assumption that this Section 1115 Waiver is a normal Medicaid program. When a state has a state health policy (often recorded as a separate Section 1931 program) or multiple Section 1115 Waivers in a given year, then I make a judgment call of which program to use based on the notes made in the MWD

[13] As an example of what I aim not to count as a Medicaid program, some programs in this database instead offer premium assistance for employment-based health insurance plans.

and comparisons between the different programs' eligibility values to those recorded in any documents that I can find from KFF and the Center on Budget and Policy Priorities (CBPP).

MWD's data on parents[14] transparently records gaps instead of numbers for years where the authors found no threshold for a given state. For this dissertation, I need to find a reasonable way to fill in each gap. As I fill each gap, the priority I choose is internal consistency: if one source gives a value that I think on its own might be more correct on its own, and a separate source with a value that presents less of a deviation between the values for the previous and the next year, then I choose the latter with the goal of limiting how much noise there is between my thresholds for each year within each state. This is important when thinking about how I apply the data from Hamersma (2013, see also Hamersma and Kim, 2013), since they report having been careful to choose values in line with my criteria for a "real" Medicaid program. The key problem with their thresholds is that they end in 2007, with no clear guidance on how to extend them to 2010. If I were to simply copy and paste MWD's values from 2008 through 2010 onto those thresholds from 1996 through 2007, many states would have large changes from 2007 to 2008 – not because of real policy changes, but because of a change in the source of the underlying data. A large, but spurious, threshold change would induce unnecessary noise into my analysis, so when in doubt, I seek to prioritize smoothness (whenever no actual policy change appears to have actually occurred) over scrutinizing which dataset has the "correct"[15] value. In practice, this means that I give the most weight to CBPP and KFF sources.

When there are gaps in the eligibility thresholds for parents given by MWD, which there often are (particularly for 1997 and 1998, and often in 2002, 2007, and 2010), I leverage data from the following sets of documents to choose which value to use to fill the gaps in: 1)

[14]The data on childless adults is fully populated, though recorded thresholds for this group are 0% in most states, for most years.

[15]Note that, as a sign of how difficult this task is, the data from Hamersma (2013) cannot simply be assumed to always identify the most accurate values for each state and year. For example, they try to choose only thresholds below which there is no premium, but if MWD's notes are to be believed, then they did not do this in Illinois in 2006 and 2007.

documents from CBPP and KFF[16], 2) McMorrow et al. (2016), which has values for 1997 that often work to fill in gaps in that year[17], and 3) Hamersma (2013).

To apply my preference for defaulting to values that maintain internal consistency, I proceed as follows when available KFF and CBPP documents and McMorrow et al. (2016) do not provide a value that can fill in a given gap. I first consider the values immediately preceding and immediately following the gap. If each of these values are within 2 p.p. of the corresponding values in Hamersma (2013), then I fill in the gap using the value from Hamersma (2013)[18]. Otherwise, I default away from using the thresholds in Hamersma (2013).

Many of the gaps in 2002, 2007, and 2010 are left open, even after pasting in data from other sources where applicable. The reason these gaps exist is because the authoritative dataset on thresholds since 2002, Kaiser Family Foundation (2025), has no records of thresholds in 2001, 2007, or 2010. The closest reports to these years are from January 2002[19], January 2008, December 2009[20], and January 2011. Taking this into account, I use MWD's 2001 value for 2002 if there is not a large jump or dip between the 2001 and 2003 values in MWD. Similarly, I use the 2008 value for 2007 if there is not a large difference between the 2006 and 2008 values. For 2010, the values from December 2009 and January 2011 are often exactly the same, in which case, I use them for 2010. When they are not the same, I use the December 2009 value for 2010.

Having followed the above process, I then fill in the remaining gaps, which are generally

[16]These documents are generally consistent in magnitude with the KFF and CBPP documents used by MWD, so these are my preferred source to fill in any gaps.

[17]i.e. using their values does not imply a jump or dip in thresholds that would seem inconsistent with what I can gather about the policy trajectory in the state

[18]Normally, I require both values to be exactly the same as the values in MWD before using Hamersma (2013) to impute the value for a gap, although I do make some exceptions to that more stringent rule when there appear to be no good alternatives.

[19]The values for 2001 in MWD appear to come from the January 2002 KFF values.

[20]There was typically at most one report per year, but 2009 had two: one in January and one in December

small, using the average between the preceding and following values from within the given program.

There is one more apparent issue that I attempt to resolve in MWD. Some of the values recorded in MWD for parents are quite different than the values I found in the CBPP, KFF, and aligned documents – sometimes implying large jumps or dips in the state's Medicaid trajectory, despite no apparent record of such a policy change. These discrepancies arise particularly often within the MWD's values for 1996 sourced from U.S. Department of Health and Human Services (1998)[21]. In order to maintain internal consistency in these cases, I replace these questionable MWD values with the same procedure as outlined above for filling in gaps.

There are three states whose values in the MWD are particularly questionable, even after following in gaps and replacing questionable values: Oregon, Minnesota, and Vermont. Hamersma (2013) drops Oregon and Minnesota from her analysis, due to the presence of their state health plans, which "complicate the role of Medicaid." Oregon's original state health plan (Oregon Health Plan) charged premiums at all income levels and also implemented enrollment freezes beginning in 2004. Minnesota's state health plan has a benefit cap for beneficiaries who make over 175% of the FPG, and there is also a premium, but the MWD authors were unable to find the scale or the starting income level at which the premium applied. Vermont's 1115 Waiver (Vermont Health Access Plan) had a threshold of at least 150% throughout the sample period, but according to MWD's notes, it was only accessible to individuals who had gone uninsured for at least 12 months, which strikes me as very restrictive. MWD also does not record any values for the Vermont's Section 1931 program throughout most of the time period, which I would normally use to fall back on if

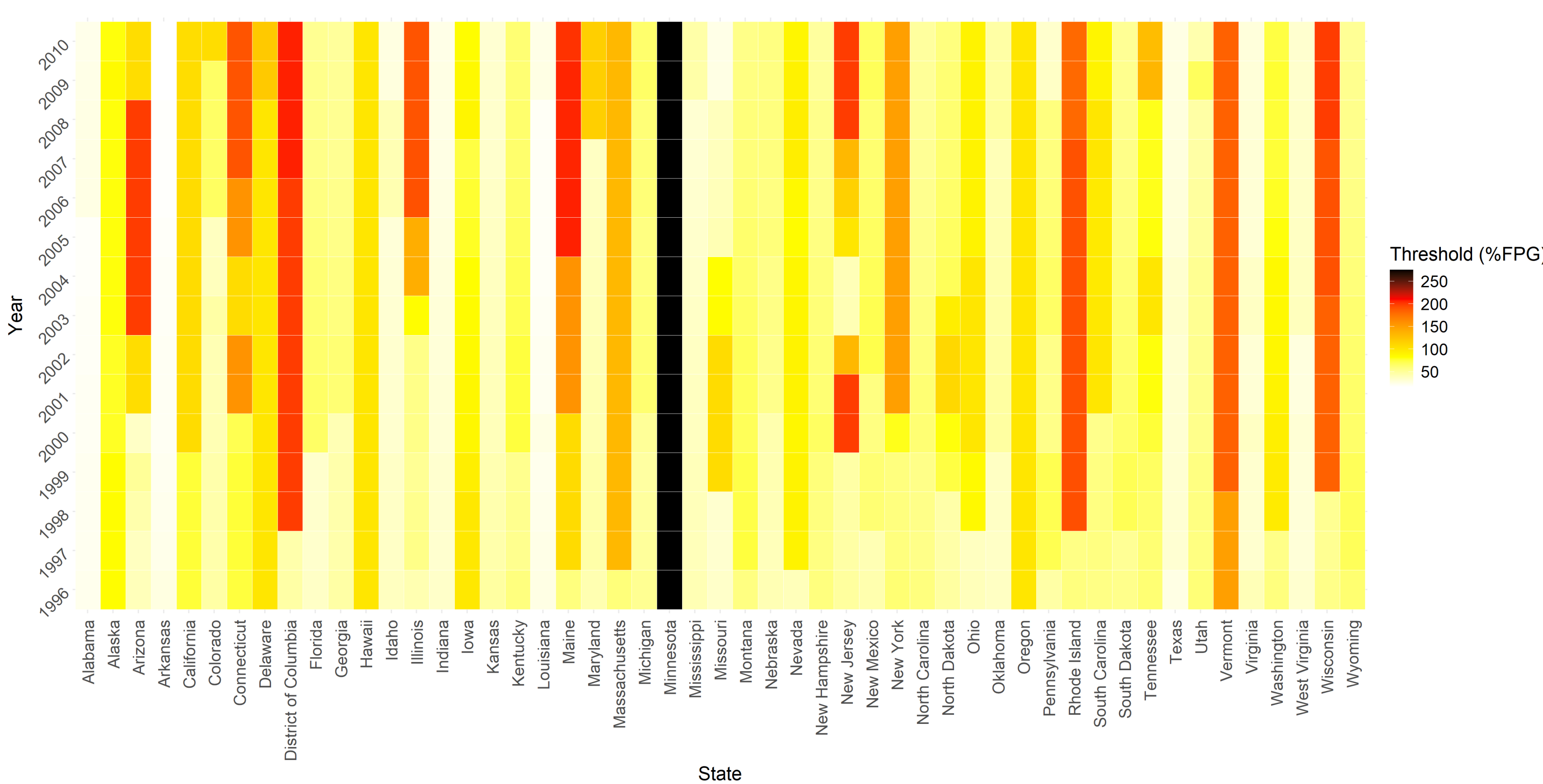
[21] I viewed this source early on in my work on this dissertation, likely sometime around early 2024. Upon returning to the provided link in July 2025, I have discovered that it is no longer publicly available. The reason appears to be given in a banner across the top of the website: "Due to current HHS restructuring, the information provided on aspe.hhs.gov is not being updated currently. Please refer to hhs.gov for more information."

I do not think a state's 1115 Waiver meets the criteria (such as broad accessibility for individuals below the threshold) for me to call it a Medicaid program. Notably, Kaiser Family Foundation (2025) uses the 1115 Waiver values for their thresholds. Hamersma (2013) uses thresholds for Vermont that range from 70 to 90%, which may be better values to use for this state. For now, I stick to the values suggested by MWD. For robustness, I find that throughout this dissertation, dropping these states from my analysis does not meaningfully affect my results.

Hamersma (2013) constructs her main thresholds for parents in households of three people, which is the typical benchmark demographic used in CBPP and KFF reports. She then extends the thresholds to employed parents in other household sizes (two to six people). To do so, her first step is to, "for any state with thresholds tied to the federal poverty line," use that same federal poverty line as the threshold for other household sizes. After which, she investigates remaining states using 1996 AFDC thresholds in the Urban Institute's Welfare Rules Database and "state-specific websites and officials." For now, I simply follow her first step as a simple extrapolation to adults of all family sizes within each parental and employment group, with the intention of emulating her exact process in a later iteration of this paper. Similarly, I apply the same logic to childless adults, who could have family sizes of one or two. I call the finished dataset "Modified MWD," reflecting the outsize role the MWD plays within my dataset.

The state-level thresholds in 1996 and 2010 are recorded in Table 3.A1, and the annual path each state took for parents is shown in Figure 3.1. Many, though not all, states increased their thresholds for some or all categories over time, and states varied widely in their annual thresholds and in the trajectories they followed between 1996 and 2010.

Figure 3.1: Medicaid Income Eligibility Thresholds for Parents



Notes: FPG stands for "Federal Poverty Guideline." Thresholds based on data for family sizes of three. Source: Modified MWD.

3.5 Applying Thresholds to Data

3.5.1 Current Population Survey

I collect data on individuals' household size, income, and parental status from the individual-level, nationally-representative samples from the March Supplement of the Current Population Survey (CPS), which I obtain from IPUMS CPS (Flood et al., 2024). This and other variables that I draw from the CPS are answered in March of a given year, in reference to the previous year, so I use the surveys from 1997 through 2011 to infer values from 1996 through 2010.

When constructing individuals' eligibility for Medicaid using their information in the CPS, I restrict the sample to each household head and their spouse or unmarried partner[22], and I then define the following variables for this restricted sample.

1. Household size: A sum of one (for the individual's self), plus one if married and the spouse[23] lives in the same household, plus the number of the household head's children, step children, and foster children who are under 19 years old and living in the home.

2. Family income: the sum of an individual's own income, plus their married spouse's (if the individual is married and the spouse lives in the same household) income, plus income earned by any of the household head's children, step children, and foster children who are under 19 years old and living in the home, minus any income that each of these individuals receives from welfare sources.

[22]Defining household income for other individuals in the household is difficult, if not impossible, because the relationship variable is given only with respect to the household head. Also note that the CPS data excludes individuals "living in institutions (for example, a correctional institution or a residential nursing or mental health care facility) and those on active duty in the Armed Forces" (U.S. Bureau of Labor Statistics, 2025).

[23]An unmarried partner is not counted as a family member in this definition.

3. Parental status: 1 if the household head has any children, step children, and foster children who are under 19 years old and living in the home; 0 otherwise.

I then define an individual's eligibility as 1 if their family income lies at or below their state's income threshold for their family size and parental status, and 0 otherwise. I also define eligibility as 0 if the applicable income threshold is recorded as 0% of the applicable FPG.

3.5.2 Measurement Error

While the CPS data are rich, there are still important details that I do not know about respondents. These details include whether an individual's household size or state of residence has changed meaningfully in the last year, and if so, when it happened. I could build a proxy for whether a birth might have happened last year, using the age of the youngest child, but this proxy still would not tell me in what month or even what year they were born. I also do not know if there was a death or a marriage (or new unmarried partnership) in the family, or if a child or unmarried partner left home last year. There is a variable for the individual's year of their most recent divorce, but this variable is only available once every two years instead of annually, so I ignore this variable.

There is also a variable for whether the individual moved from another house within the past year (i.e. since the beginning of last March), and whether this move crossed state or country boundaries. However, this does not tell me if they spent any time living in the current state last year, and if so, how much time. For example, they could have moved last month and not lived in the current state at all last year – or they could have moved last April and spent eight months living in the current state last year. As such, I ignore this variable.

In addition, for households that had a large variation in income last year, it is generally impossible to infer exactly what their monthly earnings were throughout the year. It is therefore possible that one or more adult family members were eligible or ineligible for Medicaid during a given month even if the yearly household income implies the opposite. The ideal scenario would be to know household earnings for each month. But, that is impossible within these data[24].

Given the lack of month-to-month information in the CPS data, one concern is that some individuals may report that they were enrolled in Medicaid last year, but were actually enrolled only in another state where they resided for much or all of that year, and I would have no way of knowing this. Another concern is that some individuals may appear ineligible for Medicaid given their current family size, but they may have had an unobserved change to their family structure last year, before which they were eligible for Medicaid.

For eligibility thresholds specifically, one source of measurement error is that my eligibility thresholds only reflect one pathway into eligibility. There are other pathways that I am not capturing, including through being a child, being elderly, or being disabled or pregnant, as well as through idiosyncratic state health programs that provide separate pathways into health insurance that often bear similarities to Medicaid. I exclude children and the elderly for this reason, and as long as other pathways do not experience changes that interact with changes in my IV, then the existence of other pathways does not pose an identification problem. In Section 1.5.6, I explore whether changes to the disability pathway drives results, and I do not find evidence that this is the case[25].

Since eligibility thresholds, family size, family income, parenthood, and employment sta-

[24] Gruber and Simon (2008) use the Survey of Income and Program Participation (SIPP), which has rich, monthly, longitudinal data but less precise granularity (some states aren't specifically identified). This is especially important for their purpose of exploring crowd-out effects, because there is a lot of overlap between private insurance and Medicaid in the CPS. Future research could attempt to replicate this dissertation using SIPP data.

[25] As an aside, note that the exclusion restriction would not be violated if some fraction of already-eligible, disabled individuals were induced into enrollment by eligibility expansions for non-disabled adults, e.g. by increased awareness about their own eligibility (these have been referred to as woodwork effects).

tus (not to mention that I ignore possible eligibility through alternative routes, such as disability and pregnancy) are all observed with some noise, there is inherent measurement error in the eligibility data. Any unobserved mismeasurement in family size, income, parenthood, and employment may plausibly be assumed to be uncorrelated with the eligibility IVs used in Sections 1.4 and 2.4, and may make results less precise but should not bias them. Mismeasurement in eligibility thresholds, however, could bias the IVs, so in Sections 1.5.6 and 2.5.4, I explore whether results might change when using time frames for which the thresholds I use might plausibly be more trustworthy, and I find no evidence that measurement error is meaningfully altering results.

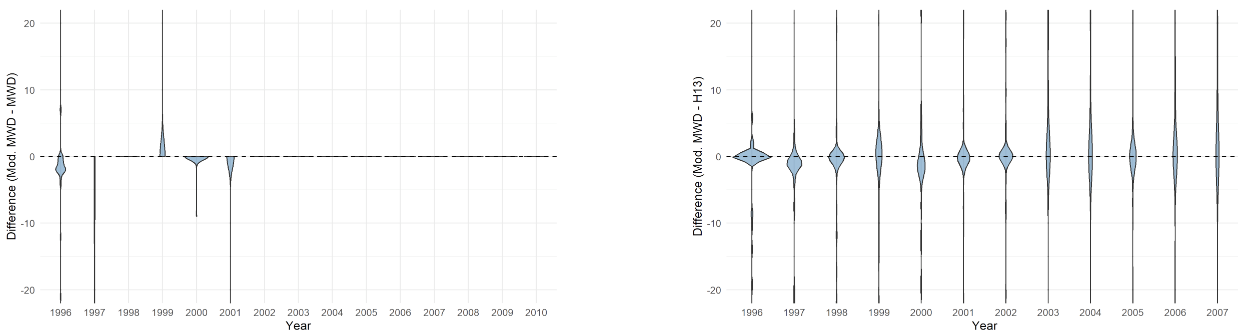
3.6 Comparing Modified MWD to Existing Collections of Thresholds

By construction, the Modified MWD database that I have constructed is, for the time period from 1996 to 2010, more complete than any other collection of thresholds from this time period that I am aware of, in the sense that this database is the first to have plausible Medicaid income thresholds for parents in all states and all years for this time frame. It also incorporates MWD's pioneering collection of thresholds on childless adults. I have also done so in a way such that most thresholds are traceable through publicly-available contemporary reports, and any exceptions can be justified using either the observed policy trajectory or context provided through later collections of thresholds.

One question that remains is how well Modified MWD thresholds compare to other collections of thresholds for states and years that are present in both. To explore this question, I compare the thresholds for employed parents in household sizes of three from MWD and Hamersma (2013) to the thresholds I collect in Modified MWD.

In the violin plots in Figure 3.2[26], I show that for each state-by-year threshold value, differences between Modified MWD and either MWD[27] and Hamersma (2013) are clustered around zero.

Figure 3.2: Violin Plots: Differences Between Modified MWD and Other Collections of Thresholds



(a) Modified MWD and MWD

(b) Modified MWD and Hamersma (2013)

Notes: Differences are constructed for each state and year as income thresholds from Modified MWD minus thresholds in the same state and year from the other collection of thresholds. Units are percentage points, relative to the FPG. Sources: Modified MWD, MWD, Hamersma (2013).

Table 3.1 displays summary statistics for the differences shown in Figure 3.2. While differences are generally close to zero, thresholds in Modified MWD are, on average, 1.09 p.p. (relative to the FPG) below thresholds in MWD and 5.63 p.p. above thresholds in Hamersma (2013). These differences are statistically different from zero at the 1% level.

Figure 3.3 shows what states and years these differences appear in, when comparing thresholds for employed parents in Modified MWD to MWD. While the two collections of thresholds overlap for most values, there are some exceptions. The majority of these exceptions occur for the values in 1996, with 1997 accounting for another four. The differences

[26]Differences are defined as thresholds in Modified MWD minus thresholds in the comparison collection, with each threshold's units given as percentages of the FPG. Absolute differences beyond 20 percentage points are removed for the purpose of more clearly visualizing the region around zero. There are some especially large outliers, exceeding an absolute value of 200 percentage points, when comparing Modified MWD to Hamersma (2013). Versions of these figures that include outliers are shown in Figure 3.A1 in the Appendix.

[27]Using the same program choices as chosen for Modified MWD, but with no modifications to the original values or gaps therein.

Table 3.1: Statistics for Differences Between Modified MWD and Other Collections of Thresholds

Statistic	Mean	Median	Std. Dev.	Min	Max	N	P-value
MWD	−0.818	0	6.11	−57	40	581	0.001
Hamersma (2013)	5.89	−0.019	38.3	−136	221	612	0.000

Notes: Differences are constructed for each state and year as income thresholds from Modified MWD minus thresholds in the same state and year from the other collection of thresholds. Units are percentage points, relative to the FPG. The "P-value" column reports p values for a test of the hypothesis that the average difference between Modified MWD and each collection is equal to zero. Sources: Modified MWD, MWD, Hamersma (2013).

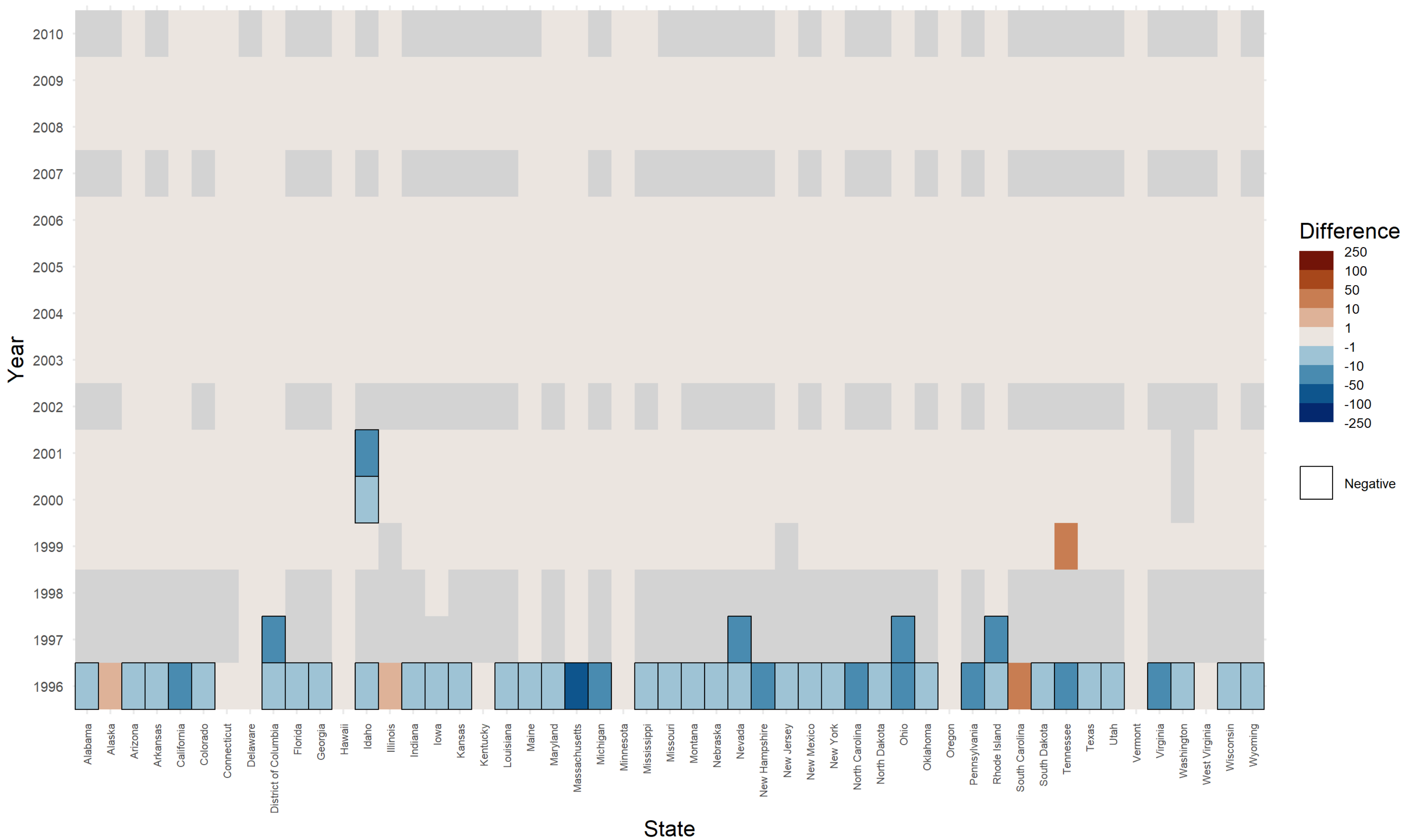
between MWD and Modified MWD in both of these years are mostly small (within 10 p.p.) and come from my use of values from Guyer and Mann (1998b), Hamersma (2013), and McMorrow et al. (2016). The values used in MWD in these cases were typically sourced from U.S. Department of Health and Human Services (1998) (for 1996) and Aizer and Grogger (2003) (for 1997), both of which were written soon after the beginning of the time frame of interest, but for unclear reasons, their thresholds often differ substantially from the policy trajectories suggested by CBPP and KFF sources. I am agnostic about which values are correct in a literal sense, but the values I choose appear more consistent with the rest of the data I collect.

The largest difference between Modified MWD and MWD[28] is in Massachusetts in 1996, where the value given in U.S. Department of Health and Human Services (1998) is 117%, while Aizer and Grogger (2003) and Hamersma (2013) report values of around 90%, and Guyer and Mann (1998b) (from CBPP) report a value of 60%. Faced with this disparate array of options, I follow my default choice of prioritizing the CBPP value, which is 60%.

Figure 3.4 reports differences between Modified MWD and Hamersma (2013) for parents with family sizes of three. These two collections of thresholds are more different from each

[28]As a note for this dissertation, the data used in this paper has a large expansion for District of Columbia's in 1998, whereas the first two chapters set DC's expansion date as 1999. This change will be applied to later versions of the first two chapters[29] and was motivated by Guyer and Mann (1999a), which specifically says DC's expansion happened in October 1998.

Figure 3.3: Heatmap: Differences Between Modified MWD and MWD



Notes: Missing values are shown in light gray. Differences are constructed for each state and year as income thresholds from Modified MWD minus thresholds in the same state and year from the other collection of thresholds. Units are percentage points, relative to the FPG. Sources: Modified MWD, MWD.

other than Modified MWD and MWD, but there is still substantial overlap. The largest deviations, each with absolute magnitudes of over 100 p.p., are in Minnesota and Vermont, which had their own state health programs and are treated in this dissertation as having values that may be particularly inaccurate in representing people's access to healthcare through Medicaid, as well as District of Columbia (DC) in 1998 and Rhode Island in 1996 and 1997. As mentioned in a previous footnote, I follow Guyer and Mann (1999a) in determining that DC increased its income threshold in 1998, rather than 1999 as in Hamersma (2013). In the case of Rhode Island, Hamersma (2013) has a value of 193% that begins in 1996 and continues for several years, whereas MWD, McMorrow et al. (2016), and Guyer and Mann (1998b) all indicate that Rhode Island did not adopt this value until 1998. As a result, I use the values from Guyer and Mann (1998b) and McMorrow et al. (2016).

3.7 Conclusion

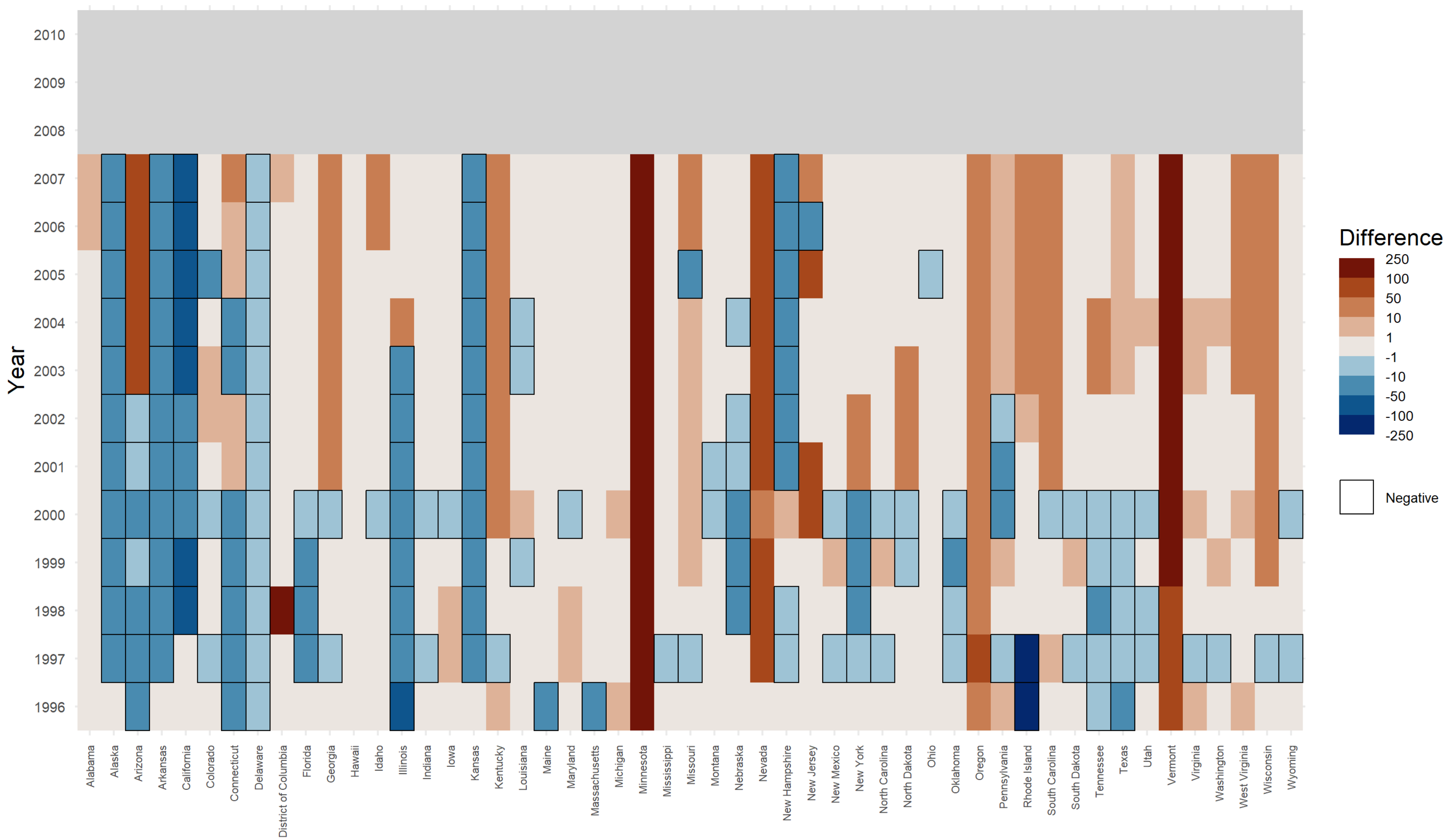
For the time period of interest, 1996 through 2010, there is no existing collection of income thresholds for Medicaid eligibility for all states in all years. For the years in which collections do have values for each state, there are frequently conflicts in which is the "correct" value, In this paper, I produce the first collection of Medicaid income thresholds that covers non-elderly, non-disabled adults in all states and years between 1996 and 2010, and I attempt to be transparent in which sources I defer to when there are conflicts between sources, and how I fill in any gaps in my values.

these data have already demonstrated their worth in the rest of this Dissertation (see, for example, Sections 1.5 and 2.5), and they are intended to be used to explore other questions in the future[30]. The Medicaid program had meaningful differences before, compared to after, ACA was passed[31], so these thresholds allow researchers to leverage pre-ACA Med-

[30] For access to the full collection of thresholds, please contact me at bennssmith@gmail.com.

[31] Coverage of substance-use disorder treatment, as discussed in the second chapter of this dissertation

Figure 3.4: Heatmap: Differences Between Modified MWD and Hamersma (2013)



Notes: Missing values are shown in light gray. Differences are constructed for each state and year as income thresholds from Modified MWD minus thresholds in the same state and year from the other collection of thresholds. Units are percentage points, relative to the FPG. Sources: Modified MWD, Hamersma (2013).

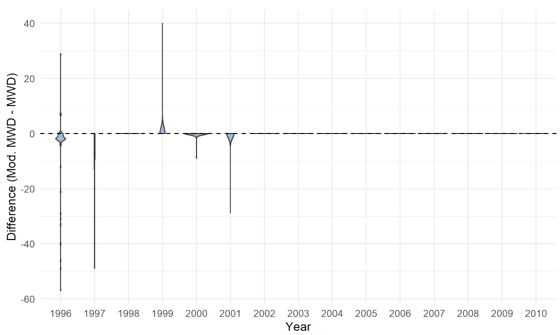
icaid expansions to explore how important features of the ACA are to current estimates of Medicaid's impacts.

(see Section 2.1), is an example.

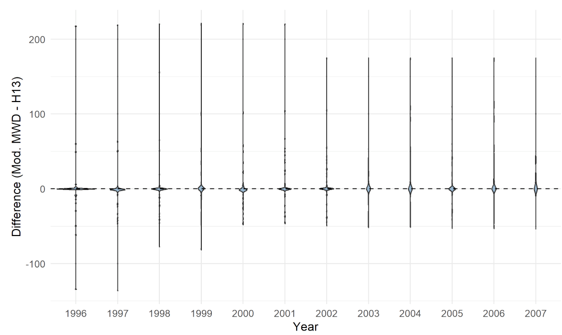
3.A1 Appendix

3.A1.1 Figures

Figure 3.A1: Violin Plots: Differences Between Modified MWD and Other Collections of Thresholds



(a) Modified MWD and MWD



(b) Modified MWD and Hamersma (2013)

Notes: Differences are constructed for each state and year as thresholds from Modified MWD minus thresholds in the same state and year from the other collection of thresholds. Units are percentage points, relative to the FPG.

3.A1.2 Tables

Table 3.A1: State Eligibility Thresholds (%FPG) for Adults

	Parents		Childless	
State	1996	2010	1996	2010
AK	83	81	0	0
AL	23	24	0	0
AR	27	17	0	0
AZ	40	106	0	100

Continued on next page

Table 3.A1 (continued)

	Parents		Childless	
State	1996	2010	1996	2010
CA	76	106	0	0
CO	47	106	0	0
CT	75	191	0	56
DC	47	207	0	133
DE	100	120.5	100	100
FL	36	53	0	0
GA	47	50	0	0
HI	100	100	100	100
IA	98	83	0	0
ID	38	27	0	0
IL	43	191	0	0
IN	35	25	0	0
KS	48	32	0	0
KY	59	62	0	0
LA	26	25	0	0
MA	60	133	0	0
MD	43	116	0	0
ME	59	203	0	100
MI	53	64	0	0
MN	275	275	75	75
MO	35	25	0	0
MS	42	46	0	0

Continued on next page

Table 3.A1 (continued)

	Parents		Childless	
State	1996	2010	1996	2010
MT	58	56	0	0
NC	59	49	0	0
ND	48	59	0	0
NE	42	58	0	0
NH	59	49	0	0
NJ	49	200	0	0
NM	44	67	0	0
NV	40	88	0	0
NY	62	150	78	100
OH	40	90	0	0
OK	37	47	0	0
OR	100	100	100	100
PA	47	34	0	0
RI	59	181	0	0
SC	57	89	0	0
SD	55	52	0	0
TN	62	129	0	0
TX	26	26	0	0
UT	61	44	0	0
VA	41	29	0	0
VT	150	185	150	150
WA	59	74	0	0

Continued on next page

Table 3.A1 (continued)

	Parents		Childless	
State	1996	2010	1996	2010
WI	56	200	0	0
WV	33	33	0	0
WY	63	52	0	0

Source: Modified MWD.

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