Three Essays in Health Economics

by

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For Kim and my children, Theodore and Daphne.

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ABSTRACT

Three Essays in Health Economics

State governments play a major role in the United States health care market. Moreover, states administer much of the regulation, budgeting, and policy for their own markets, which creates idiosyncratic differences across states. This dissertation contributes to the literature by evaluating those differences to analyze the effectiveness of certain regulations and policies and to explore the relationship between state health care markets and other state obligations.

The first chapter uses state differences in the nurse practitioner (NP) market to evaluate the effects of state laws allowing NPs to prescribe controlled substances on prescription opioid use. I study these effects by merging nationwide data from the Medical Expenditure Panel Survey (MEPS) over 18 years (1996-2013) with data on state laws. I then exploit variation in these laws over time to create a quasi-natural experiment and to estimate the causal impact of NP deregulation on prescription opioid use. I find, relative to patients living in more restrictive states, that patients who live in states with more flexible NP laws *reduce* their prescription opioid use by 7 percent to 9 percent. I also find that health outcomes either slightly improve or remain unaffected by the enactment of these laws. Taken together, these results indicate that NP deregulation slows the trend in prescription opioid growth while potentially improving patient outcomes. Furthermore, suggestive evidence implies that these effects may be even larger for the least restrictive states, opening the door for future reforms.

The second chapter (co-authored with Andrew Litten) seeks to identify the causal relationship between increased state Medicaid obligations and higher education spending. After several decades of federal mandates and high rates of health cost inflation, Medicaid spending has taken an increasingly larger share of state budgets, forcing states to make offsetting cuts elsewhere. We argue that state governments are likely to cut higher education in response to these changes, as institutions of higher education have the capacity to find additional revenues elsewhere. We use federally administered Supplemental Security Income (SSI) enrollments to instrument for state Medicaid spending. We find that a one dollar increase

in Medicaid costs leads to a decrease in higher education subsidies of 20 cents to 37 cents. Our approach provides estimates which are both more credible and more precise than those which have previously been used in the literature.

The third chapter studies the effectiveness of Prescription Drug Monitoring Programs (PDMPs). These programs are widely considered to be a promising tool for preventing prescription opioid misuse. Using a nationally representative sample that spans the majority of PDMP implementation, I find little evidence that PDMP implementation is effective in preventing prescription opioid misuse. Nonetheless, I find that when states pair PDMPs with policies mandating health care provider use ("must access" laws), they can successfully reduce high-volume opioid prescriptions. States that add "must access" laws reduce high-volume prescriptions by about 20 percent. In addition, these states do not appear to affect overall prescribing behavior, suggesting that PDMPs with "must access" laws can target potential misuse without hindering medically appropriate access.

CHAPTER I

Nurse Practitioner Prescriptive Authority and Prescription Opioid Use

1.1 Introduction

In an effort to make affordable health care more accessible, state-level policymakers have turned to nurse practitioners (NP) for solutions. Not only are they the fastest growing primary care workforce, but they are also becoming more substitutable with physicians through NP deregulation. These reforms should improve access in the short run through substitution of the existing workforce. [Pylypchuk and Sarpong (2015)]. Then, in the long run, a more favorable regulatory environment should increase the overall supply of NPs [Graves et al. (2016)]. Furthermore, because NPs are frequently reimbursed at lower rates than physicians, these changes have the added benefit of potentially reducing the cost of health care per visit.

These regulatory reforms, however, are not universally supported. Physician groups have been especially vocal about the effect of these reforms on the quality of care. Citing disparities between physicians and NPs in training, a 2012 report by the American Academy of Family Physicians recommended for a physician-centered approach and against NP independence. Recent efforts to relax regulations by New York, Tennessee, and West Virginia have been met with hostility. In addition, physicians and nurse practitioners have drastically different perceptions about the quality of care that NPs provide. A survey published by the New England Journal of Medicine in 2013 showed that two thirds of primary care physicians thought that physicians could provide a higher quality examination and consultation than NPs even if the services provided are identical [Donelan et al. (2013)]. When asked the same question, over 90% of primary care NPs disagreed.

As with many debates, it is possible that both sides are right. Physicians may be relatively better at treating certain patients, while NPs may be relatively better at treating other

patients. By reforming regulations, policymakers allow physicians and NPs to sort their patients in a way that exploits each other's comparative advantages. The extent to which this sorting occurs will dictate the effects on cost and outcomes.

I explore this possibility by using variation in these state reforms over time to analyze how NP prescriptive authority for controlled substances (henceforth NP deregulation) affects prescription opioid consumption. I argue that NPs are relatively better with non-prescription treatments than physicians, while physicians are relatively better with prescription treatments. As such, NP deregulation allows NPs and physicians to sort the pain patients least appropriate for prescription opioids toward NPs and the pain patients most appropriate for prescription opioids toward physicians. Moreover, because of the nature of pain treatments and prescription opioids, those patients who are least appropriate are also more likely to be chronic, and therefore heavy, users of prescription opioids. Thus, NP deregulation can cause potentially sizable effects on opioid prescriptions, especially among positive long-term users.

My results indicate that NP deregulation has slowed the recent trend in opioid prescriptions with no associated decline in self-reported health. More specifically, I find that prescription opioid consumption falls by 7% to 9%. Decomposition of this result shows that it is driven almost exclusively by reductions among positive users rather than a fall in the likelihood of consuming prescription opioids. This result translates into one fewer opioid prescription for about a quarter of positive users.

Taken in the context of the current prescription opioid epidemic, the results suggest that policymakers may have unintentionally dampened prescription opioid consumption in their attempt to improve access to affordable health care. By extension, it is probable that this reduction indirectly prevented downstream abuse of prescription opioids by limiting availability. More importantly, many authorized states allow NPs to prescribe controlled substances but still require some involvement with physicians. My results suggest that the least regulated markets are the most effective at reducing opioid prescriptions, which highlights the potential for additional gains from future deregulation.

1.2 Background

1.2.1 Nurse Practitioner Market: Background and Recent Deregulation

NPs represent 74% of advance practice registered nurses (APRNs), which also include certified nurse midwives, certified registered nurse anesthetists, and clinical nurse specialists. To become an NP, registered nurses (RNs) must complete a master's or doctoral program and an advanced clinical training practicum. NPs are nationally certified and licensed by either the state's Board of Nursing or the state's Board of Medicine. Most NPs work in

clinics or private physician practices, but many also work in hospital units. Depending on state regulations, NP responsibilities can range from being able to practice and prescribe independently to being completely subordinate to a supervising physician.

The American Association of Nurse Practitioners (AANP) surveyed a random sample of 29,710 NPs for the 2012 National AANP Sample Survey. Of those sampled, 4,231 (14%) NPs responded. Of those who responded, the average NP is 51 years old with 25 years of experience as an RN and 11 years of experience as an NP. Most NPs are female (91%), and about 15% of NPs are non-white. Approximately 81% of NPs work in primary care, and 43% of NPs work in communities with fewer than 100,000 people. Although by 2012 48 states allowed NPs to prescribe controlled substances, almost 80% of NPs report prescribing fewer than five prescription opioids per week, and 39% of NPs claim to never prescribe opioids.

The NP profession grew dramatically over the past three decades to become a dominant player in primary care services. In 1988, there were approximately 37,000 NPs nationwide. This number more than doubled by 2000 to just over 90,000 NPs, and by 2013, there were 149,784 NPs. By comparison, there were 239,500 primary care physicians in 2013 nationwide, which implies that there are three NPs for every five primary care physicians in the market today [Stange (2014); AHRF (2013)].

One important reason for this increase is the emergence of pro-NP reforms during the 1980s and 1990s. These reforms were primarily initiatives to expand access to care by making it easier and more attractive for NPs to practice. Reformers believed this to be an effective strategy because NPs are both less expensive and more willing to live in less accessible areas than physicians. Early adopters of these reforms tended to be rural states, but more densely populated states like Massachusetts, New York, and Wisconsin were also active reformers, which suggests that cost-based concerns may have been important as well.

Overall, these reforms appear to be effective. Kleiner et al. (2014) find that NP deregulation reduces cost per visit without influencing health care quality. Alexander and Schnell (2016) show that prescriptive deregulation for non-controlled substances both reduce mortality and improve mental health, and Graves et al. (2016) show that less restrictive states have greater accessibility to care.

Graves et al. (2016) also find that NP densities are higher in rural areas than in urban areas, which suggests that reforms could have a stronger effect in rural areas. That being said, the 2012 AANP Survey reports that over 80% of NP respondents have a collaborative relationship with a physician and most NPs are employees. As such, differing effects from NP reforms due to the relatively stronger presence of NPs in rural areas may be muted by the near universal interdependency between NPs and physicians.

The reforms that have been passed can be broadly categorized into three groups: (1)

practice authority, (2) public and private reimbursement protection, and (3) prescriptive authority. Practice authority reforms pertain to the entity that regulates NPs and whether NPs were clearly defined as a separate profession in the state's nurse practice act. Public reimbursement protections define the rate at which Medicaid pays NPs relative to the physician rate. Private reimbursement protections are non-discrimination clauses and direct reimbursement requirements, which guarantee NP reimbursement from insurers. Prescriptive authority reforms dictate whether NPs can prescribe drugs.

States also differ on whether NPs must work with physicians, which could affect how NPs respond to reforms. If there is such a requirement, the relationship can vary from direct supervision to a more flexible arrangement dictated by a collaborative agreement. Because of ambiguity in how this relationship is defined in state statutes, I group all of these required relationships together as "physician involvement." In addition, I differentiate between physician involvement for *practice* authority and physician involvement for *prescriptive* authority, which can differ within a state for a given year.

With few exceptions, states progress from restrictive regulations to more favorable regulations, and legislation for practice authority tends to precede the other two forms. For my analysis, I focus on one of the more recent reforms, prescriptive authority for controlled substances. Details about other reforms and how these data were collected can be found in Appendix A.

Most states grant NPs prescriptive authority for controlled substances a few years after legislating non-controlled prescriptive authority, and the majority of states initiate this authority with a requirement for physician involvement. During the sample period of 1996 to 2013, 21 states authorized NPs to prescribe controlled substances, and of those 21 states, only four states eventually dropped the requirement for physician involvement [see Figure 1.1]. In addition, 27 states had authorized NPs prior to 1996, and of those states, five states dropped the requirement for physician involvement between 1996 and 2013. Alabama authorized NPs in 2014, and Michigan and Florida have yet to authorize NPs as of 2016.

The distinction between physician involved authority and independent authority offers two sources of variation across states over time. First, I can identify the effect of granting authority with physician involvement when there was previously no authority. Second, I can identify the effect of granting independent authority when there was previously a physician involvement requirement. I prefer the first source of variation because more states change regimes during the sample period and because these states are more representative of the country as a whole. By contrast, the nine states that change to independent authority are Colorado, Hawaii, Idaho, Maine, Minnesota, North Dakota, Rhode Island, Vermont, and Wyoming. Despite these limitations, results for independent authority may be useful for

policymakers considering further deregulation. As such, I include the results for independent authority in Section 1.7.4. In addition, as an alternative approach to measuring the effects of independence, I estimate the heterogeneous treatment effects of physician involved prescriptive authority across the different practice authority regimes. These results can also be found in Section 1.7.4.

1.2.2 Prescription Opioids

Prescription opioids are a class of drugs designed to relieve moderate to severe pain. All prescription opioids are chemical variants of morphine and include, among others, pure morphine, hydrocodone (e.g., Vicodin, Lortab, and Norco), oxycodone (e.g., OxyContin and Percocet), and codeine (e.g., Tylenol #3). They are often recommended for cancer and postoperative pain, and over the past few decades, they have grown as a long-term treatment option for chronic back and joint pain.

Because of their addictive properties, prescription opioids are regulated by the Drug Enforcement Agency (DEA) as controlled substances. In practice, this means that only providers with a DEA license number are allowed to prescribe opioids, and a provider's eligibility for a license depends on each state's laws. This license system is designed to serve at least two purposes. First, it identifies and tracks a provider's prescription history, which is useful for administrative and policing reasons. Second, it transfers responsibility for managing the supply of prescription opioids to licensed providers, who are most capable of assessing patients' needs.

Unfortunately, determining whether a patient needs prescription opioids is notoriously difficult. Not only does pain differ across patients, but it also can vary over time for the same patient. Furthermore, patients' memories of pain intensity often differ from their real-time reporting of the same pain episodes [Redelmeier and Kahneman (1996); Stone et al. (2000); Redelmeier, Katz and Kahneman (2003); Broderick et al. (2008)]. Thus, patients who are honestly reporting their symptoms may receive different treatments simply because of timing and subjective memory recall. The repercussions of getting the dosage incorrect can be injurious. Prescribing too little could lead to avoidable pain and inconvenient follow-up visits, while prescribing too much exposes patients to higher addiction risks. If we also account for intentional misreporting and addictive behavior, providers must simultaneously and subjectively assess the patients' pain while disentangling their true intentions.

These issues are of particular concern among chronic pain patients, whose pain is both less severe and more difficult to diagnose. Despite these limitations, the long-term nature of their pain implies that chronic pain patients demand long-term access to prescription opioids. Taken together, these two facts suggest that the patients who are least appropriate

for prescription opioids are also the ones with the greatest access to them.

Despite these concerns, physicians' attitudes toward prescription opioids and chronic pain softened in the 1990s [Turk, Brody and Okifuji (1994)]. This change was partly due to several publications downplaying the long-term risks of addiction from prescription opioid use [Porter and Jick (1980); Health and of the American College of Physicians (1983); McGivney and Crooks (1984); Schug et al. (1992)].[1] After amassing nine new patents from 1990 to 2006, prescription opioid manufacturers also aggressively marketed their drugs as safe and effective at treating chronic pain.[2] This reversal of position was made official in 1997 when the American Pain Society and American Academy of Pain Management jointly endorsed the use of prescription opioids for treating chronic pain. The Joint Commission on Accreditation of Healthcare Organizations (JCAHO) followed suit in 2000 when it issued its pain management standards.

As providers became more comfortable with prescribing opioids for chronic pain, the number of opioid prescriptions skyrocketed. From 1996 to 2013, opioid prescriptions increased by 140% [see Figure 1.2]. NP authority to prescribe opioids also increased over this period. To the casual observer these two trends may appear to be related, but a more careful assessment of results suggest that the prescription opioid trend would be even higher had NP deregulation not occurred.

As is clear from the Center for Disease Control and Prevention revised March 2016 guidelines, the risks of prescription opioids were understated before. An increase from 1 to 3 oxycodone 10mg pills per day can increase the probability of hospitalization due to overdose by a factor of 2 to 5 times. Even more worrying, the median prescribed dosage for patients with a fatal overdose is only 4 oxycodone 10mg pills per day.

Given these risks, problems associated with abuse inevitably followed the rise in opioid prescriptions. The estimated number of people who ever improperly used OxyContin increased dramatically from approximately 237,000 in 1999 to 3.5 million in 2005 to 7.1 million in 2014 [NSDUH (1999); NSDUH (2005); NSDUH (2014)]. Emergency department visits involving prescription opioid misuse increased 117% between 2005 and 2011 [DAWN (2013)], and the proportion of admissions to treatment centers for prescription opioids increased from 3% of admissions in 2003 to 9% in 2013 [TEDS (2015)]. Most concerning, the rate of death from prescription opioid overdose increased from 1.5 to 5.9 deaths per 100,000 persons from

[1] Porter and Jick (1980) was actually a short letter to the editor citing an observational study that found only 4 out of 11,882 patients who had received a prescription opioid suffered from addiction while admitted to the hospital. Although the letter did not include any analysis and was not peer reviewed, it is cited over 900 times and is used as evidence that the risk of addiction is low.

[2] The nine patents are MS Contin (1990), Duragesic (1990), OxyContin (1995), Kadian (1996), Actiq (1998), Percocet (1999), Ultracet (2001), Avinza (2002), and Opana (2006).

2000 to 2014 [Compton, Jones and Baldwin (2016)]. There is also some concern that trends in prescription opioid abuse may be associated with a similar increase in heroin use [Cicero et al. (2014); Case and Deaton (2015)].

1.3 Conceptual Framework

There are many reasons why the practice behavior of NPs would differ from that of physicians. First, NPs are nurses, and as nurses, they are trained to focus on personalized patient care and communication, which is evidenced by the strong relationship between nurses and patient satisfaction measures [Kutney-Lee et al. (2009)]. Second, as a relatively new profession with increasingly more authority, NPs worried about public perceptions will diagnosis and treat more cautiously. Lastly, while 93.1% of NPs are salary or wage-based employees, only 20.5% of physicians are paid exclusively through a salary [AANP (2012); AMA (2014)]. This discrepancy in pay structure gives physicians a stronger incentive to see more patients in a given period than NPs.

Thus, we should expect to see NPs, relative to physicians, providing longer visits with a heavier emphasis on patient communication and possibly more conservative treatment, and it appears that meta-analyses support these predictions [Horrocks, Anderson and Salisbury (2002); Naylor and Kurtzman (2010)]. They find that NPs spend more time with patients and have higher patient satisfaction ratings. They also appear to order more tests, which could be seen as NPs spending more time diagnosing before treatment.

If we extend these results to the diagnosis and treatment of pain patients, NPs should prescribe opioids more conservatively than physicians. As more practiced communicators with longer patient visits, they are likely better equipped at assessing patient need for prescription opioids. Moreover, some patients may be deterred from seeking prescription opioids if they must undergo an intermediary diagnostic test.

Even so, a newly authorized group of providers less willing to prescribe opioids does not necessarily reduce opioid prescriptions. If NPs exclusively treat patients who previously lacked access to care, some of whom have pain, then prescription opioid consumption should increase among these new patients, albeit less than if physicians treated them. I define this change as the "Access Effect." If instead, NPs treat patients who were previously being seen by physicians, then we should see a decrease in consumption among the patients who had access to care. I define this change as the "Substitution Effect."

Proposition I.1. *If the increase from the Access Effect dominates the decrease from the Substitution Effect, then the estimated effect of NP deregulation on prescription opioids is*

positive. If the Substitution Effect dominates the Access Effect, then the estimated effect is negative.

Following the approach used by Chandra and Staiger (2007), I adapt a Roy Model to predict and illustrate the Substitution Effect. Their model studied geographic variation in the prevalence of two different treatments (non-intensive intensive) designed to treat a single condition (acute myocardial infarction or AMI). The choice of treatment is determined based on providers' expertise and the patient's appropriateness for the intensive treatment. Because of spillovers in training and expertise, some regions of the country became more effective at treating patients when using the non-intensive treatment, while other regions became more effective when using the intensive treatment.

This framework parallels nicely with the prescription opioid market. When treating pain patients, providers must decide whether to prescribe opioids conservatively (non-intensive) or non-conservatively (intensive). They do so by determining the patient's appropriateness for prescription opioids and comparing the patient's expected net utility over the two treatments. The treatment with the highest utility is the treatment chosen. In Figure 1.3 below, the choice made by each provider is the upper envelope of the two treatments, which maximizes patient utility.

In the context of NP deregulation, pain patients living in a state with NP prescription authority can either visit a physician or an NP, and patients living in a state without NP authorization can only see a physician. For substitution to occur in this model, it must be the case that NPs are relatively better than physicians at treating patients without prescription opioids such that their utility is higher. Such a case is shown in Figure 1.4, which I show under assuming perfectly certain appropriateness for simplicity. Patients living in unauthorized states must exclusively follow the physician treatment path, but patients living in authorized states can sort between NPs and physicians depending on their level of appropriateness. The least appropriate patients are treated conservatively by NPs, and the most appropriate patients continue to be prescribed opioids non-conservatively by physicians. Without perfect certainty, NPs will occasionally treat a highly appropriate patient, but on average, they will treat less appropriate patients more frequently than physicians.

Substitution occurs for the patients who were previously treated with prescription opioids but were least appropriate among those who consumed them. The magnitude of the substitution effect depends on the relative difference between utilities when treated by an NP versus a physician. The larger the difference, the stronger the reduction in prescription opioids will be.

With perfectly certain appropriateness, an additional implication of this model is that patients who substitute from physicians to NPs will receive more utility. If I instead relax

perfect certainty, this implication should still be true on average. In addition, patients associated with the Access Effect should also improve because their demand for care is being met. I will attempt to measure this effect by measuring changes in self-reported health before and after deregulation across authorized and unauthorized states.

Proposition I.2. *Regardless whether the Access Effect or Substitution Effect dominates, self-reported health weakly improves following NP deregulation.*

I can also extend this model to consider geographic dispersion of providers. Consider two hypothetical NPs that are identically productive, except that one lives in a region with fewer NPs per capita ($NP_{\text{Low Density}}$), and the other lives in a region with a high number of NPs per capita ($NP_{\text{High Density}}$). Because there are more potential patients for each $NP_{\text{Low Density}}$, patients face higher congestion costs when seeking treatment from $NP_{\text{Low Density}}$ instead of $NP_{\text{High Density}}$. Graphically, higher costs will shift the NP envelope down because net utility will be lower for each level of appropriateness (see Figure 1.5).

Thus, if a patient is exogenously moved from a high density region to a low density region, the interval of patients switching from physicians to NPs and reducing prescription opioids will shrink from [A,C] in the figure to [A,B]. This result highlights a third implication.

Proposition I.3. *The magnitude of the Substitution Effect increases with the availability of NPs.*

1.4 Data

I use information from multiple sources to evaluate the impact of NP deregulation on consumption of opioids. In particular, I merge data on prescriptive authority for controlled substances (see Figure 1.1) to the confidential Medical Expenditure Panel Survey (MEPS) by state and year. I then supplement this master dataset with detailed data on local market characteristics, which I use to control for time-varying market heterogeneity.

1.4.1 Prescription Utilization and Individual Characteristics

Using state and year identifiers, I merge the prescription authority dataset to the Medical Expenditure Panel Survey (MEPS) Full Year Consolidated Data and Prescribed Medicines files over the period 1996-2013. These data are designed to be nationally representative of the U.S. civilian, noninstitutionalized population.

The sample contains 18 two-year panels with one observation per individual per year. Approximately 92% of individuals in my sample are surveyed for both years. In some instances, treatment can span over the two years of reported data, which causes some of the data to be reported in the first year and some in the second year. To avoid this type of truncation error, I collapse the data to the individual-level such that each observation represents two years of utilization. Prescriptions and other utilization are calculated as the sum of utilization over the two years. I use the first year of response for time varying characteristics such as income.

The MEPS documentation defines a prescription as "a single acquisition of a prescribed medicine reported by household respondents." The original purchase of a prescription is an acquisition, and any refill associated with that original purchase is defined as a separate acquisition. Henceforth, I use the term prescription to describe an acquisition.

Data on prescriptions were first obtained through the Household Component questionnaire. As part of this questionnaire, respondents are asked about their prescription history and are asked for authorization to contact one's four primary pharmacies. Then, survey administrators follow back with these pharmacies to cross-validate the self-reported prescription history. Over all years, about three-fourths of respondents authorize contact, and about three-fourths of contacted pharmacies participate in the follow-back component.

The Prescribed Medicines files contain detailed information about respondents' drug history for each year down to the prescription-level. Details include the National Drug Code (NDC), which specifies the chemical compound, manufacturer, quantity, and strength of medicine prescribed, and the Cerner Multum Lexicon therapeutic class. Consistent with Stagnatti (2015) and Frenk, Porter and Paulozzi (2015), I identify prescription opioids using the therapeutic classes 60 (narcotic analgesics) and 191 (narcotic analgesic combinations).

For the sample, I estimate that approximately 20.4% of respondents have acquired an opioid prescription during their two-year survey period. This estimate is slightly higher than an estimate of 17.5% reported by Buchmueller and Carey (2017) for one year of consumption in Medicare Part D claims data. While their Medicare sample is older and sicker than the broader population represented in the MEPS, the MEPS rate reflects a longer duration. As such, I view the rates to be roughly consistent with each other. Among positive users, Figure 1.6 shows that most respondents consume only 1 or 2 prescriptions over two years. The average number of opioid prescriptions over a two-year period is 4.33 prescriptions, and the average number of pills over the same period is 230 pills, which implies an average prescription of 53 pills per bottle or about 2 pills daily for a 30 day dose.

In addition to prescription opioids, I study other drug classifications as potential substitutes and as falsification tests. Further details on these drugs can be found in Section

1.6.3.1. Consumption patterns of all drugs over my sample period are available in Table 1.1. Prescription opioids, which are a central nervous system (CNS) agent, represent about 3.7% of all prescriptions acquired, and this fraction appears to be stable over time. The dominant classes of drugs prescribed are cardiovascular agents (23.7%), CNS agents (15.6%), psychotherapeutic agents (7.3%), and antibiotics (4.4%). I use non-opioid CNS agents to explore substitution, and I use cardiovascular agents, antibiotics, and all other drugs classes as falsification tests. Prescription classes in the other category include drugs for diabetes, hormone therapy, gastrointestinal problems, and asthma.

I use the confidential version of MEPS with state of residence identifiers to merge the state NP deregulation data. I then focus my analysis on respondents who are surveyed between five years before and nine years after NP deregulation for each state. I use all years for Florida and Michigan, which never pass NP deregulation through 2016. Because the sample ranges from 1996 to 2013 and NP deregulation occurs from 1984 to 2014, some states will have more pre-event years than others, while other states will have more post-event years than others. I choose five years of pre-event data and nine years of post-event data to ensure that I have enough years to study pre-existing trends for most states and to allow for any medium-term adjustment that may happen following the law's passage. Results are qualitatively similar for other cutoffs.

The MEPS also contain information on health utilization and individual characteristics, including demographics, education, marital status, income, employment status, and type of health insurance. I use the variables from Table 3.1 as controls in my empirical specification. My analytic sample includes almost 108,000 observations. The sample includes adults aged 19 to 90, and reported means use respondent-level survey weights. Roughly a quarter of the sample lacks consistent health insurance coverage. The average family income is a little over 400% of the federal poverty line, which is skewed by high earners. The median family income relative to the federal poverty line is 325.7%, which is about $79,000 for a family of four in 2016.

1.4.2 Market Characteristics

To address differences in local characteristics over time, I collect controls from the Area Health Resource File; the Bureau of Economic Analysis; the Surveillance, Epidemiology, and End Results Program; and self-compiled lists from online resources. For both state- and county-level characteristics, I include physician density, demographics, employment, and personal income. In addition, at the state level, I control for the party of the governor and NP regulations other than prescription authority for controlled substances. Lastly, I control for Prescription Drug Monitoring Programs (PDMPs), which are online portals that track

purchases of controlled substances within a state. Any provider with a DEA license can access a PDMP but is seldom *required* to access it when prescribing opioids. A list of PDMP implementation dates is available in Buchmueller and Carey (2017).

I use these controls to evaluate whether NP deregulation occurs in response to sudden changes in access to care, demographics, or the state labor market. If these characteristics influence opioid prescriptions and are omitted from estimation, then the coefficient on NP deregulation is biased. For example, if NP deregulation occurs in response to a sudden decline in physician supply, and a greater physician supply implies more opioid prescriptions, then failure to control for physician supply negatively biases the coefficient on NP deregulation.

To account for this concern, I plot several trends in state covariates around the year of implementation (see Figure 1.8). For each state and covariate, I standardize outcomes by calculating the percent change relative to the year before implementation (y=-1). Then, I report the average percent change across all states for each year. While some covariates, like personal income per capita, increase dramatically over the sample period, the year-over-year trends are smooth, especially through the implementation year. As a precaution, I include both state- and county-level controls in my preferred specification.

1.5 Empirical Strategy

I estimate the effects of NP deregulation on prescriptions using two different estimation strategies. First, I use a difference-in-differences framework with ordinary least squares (OLS) estimation. Second, I use a two-part model to estimate effects from a better fitting specification. An added benefit of the two-part model is that it decomposes this effect into an extensive effect—a change in the probability of consuming—and an intensive effect—a change in the amount of consumption conditional on positive consumption. Then, as an extension to the baseline analysis, I adapt the two-part model with year-specific dummies to analyze trends before and after NP deregulation. Lastly and also as an extension to the baseline analysis, I re-estimate the two-part model while including an interaction term for state-level NP density. This specification allows me to test Proposition I.3.

All results use the MEPS person-level sampling weights to account for potentially endogenous sampling concerns [Solon, Haider and Wooldridge (2015)]. To allow for the possibility of correlated errors among people residing in similar legal regimes, I cluster standard errors at the state-level.

1.5.1 OLS Difference-in-Differences

I use individual-level data grouped by the state of residence and year first surveyed, or multilevel data. Given this setup, I adopt the general difference-in-differences specification [Hansen (2007)]. For an individual i whose first survey year is year t and who is living in county c of state s at that time, the equation for the OLS estimates is:

$$y_{it} = \pi_{DD} NPDereg_{s(i)t} + I_{it}\beta_{\mathbf{I}} + S_{s(i)t}\beta_{\mathbf{S}} + C_{c(i)t}\beta_{\mathbf{C}} + \delta_{s(i)} + \delta_t + \varepsilon_{it}, \tag{1.1}$$

The primary outcome variables, y_{it}, are the number of opioid prescriptions and the number of opioid prescription pills for an individual in a given two-year period. In addition, I use Equation 1.1 to measure the effects of NP deregulation on access to care and health outcomes. Lastly, I rerun Equation 1.1 to explore potential mechanisms for my results. The outcomes I investigate are non-opioid prescriptions that NPs could use as substitutes, office-based visits, and outpatient visits.

$NPDereg_{s(i)t}$ is a dummy for whether state s allows NPs to prescribe controlled substances at year t, and π_{DD} is the coefficient of interest. Note that observations occurring the year before NP deregulation are classified as untreated even though they are technically treated for half of the two-year period. By classifying partially treated observations into the untreated group, I make the untreated group look more like the treated group and push estimates toward zero. Given that I am finding negative estimates, I choose this approach over dropping these observations or over classifying them as treated because it most strongly works against my findings and therefore is more conservative.

As a reminder, my conceptual framework generates two propositions regarding π_{DD}. First, the effect of NP deregulation on opioid prescriptions will depend on whether the Access Effect or the Substitution Effect dominates (Proposition I.1). If the Access Effect dominates the Substitution Effect, $\pi_{DD} > 0$ when prescription opioids are the outcome. If the Substitution Effect dominates the Access Effect, $\pi_{DD} < 0$. The second proposition is that patient health should weakly improve regardless of which effect dominates (Proposition I.2). Empirically, results consistent with this proposition should generate a positive π_{DD} when y_{it} is a good health outcome and a negative when y_{it} is a bad health outcome.

Individual controls, I_{it}, include gender, marital status, employment status, ethnicity, race, education level, health insurance status, Medicare and Medicaid enrollment, age and age squared, income relative to the federal poverty rate, and a dummy for whether the respondent had inpatient surgery. State controls, $S_{s(i)t}$, include a dummy for a Democratic governor, the general practitioner to state population ratio, the state employment rate, the share of state residents living in an urban area, real personal income for the state, and the share of

the state population by age, gender, and race. I also include state-level legislative dummies for Prescription Drug Monitoring Programs (PDMPs) and NP regulations other than the controlled substances regulation. County controls, $C_{c(i)t}$, include the general practitioner to county population ratio, the county employment rate, real personal income for the county, and the share of the county population by age, gender, and race.

I also estimate the effect of NP deregulation using two more flexible variations of Equation 1.1. Because of reasons mentioned in Section 1.5.2, I prefer to present results from these specification using the two-part model also described in detail below.

First, I use the more flexible event-study framework [Jacobson, LaLonde and Sullivan (1993)]. In fact, Equation 1.1 is a special case of the event-study framework where all years have been collapsed into pre- and post-event periods. This more general specification serves two purposes. First, it highlights whether the law has a differential effect over time, and second, it illustrates whether the empirical specification satisfies the parallel pre-trends assumption. Despite these benefits, the added flexibility reduces precision and is more subject to idiosyncratic, year-over-year variation introduced as part of the MEPS sampling procedure. As such, I prefer to use Equation 1.1, which maximizes power and smooths out year-over-year sampling variation, and I use the below specification in Equation 1.2 as a complement to those estimates.

$$y_{it} = \sum_{\tau=-5}^{-3} \pi_\tau D_{s(i)} \mathbb{1}(t - T^*_{s(i)} = \tau) + \sum_{\tau=-1}^{8} \pi_\tau D_{s(i)} \mathbb{1}(t - T^*_{s(i)} = \tau) + I_{it}\beta_{\mathbf{I}} + S_{s(i)t}\beta_{\mathbf{S}} + C_{c(i)t}\beta_{\mathbf{C}} + \delta_{s(i)} + \delta_t + \varepsilon_{it} \tag{1.2}$$

I use a binary indicator of treatment, $D_{s(i)}$, equal to one if the individual lives in a state that ever allowed NPs to prescribe controlled substances. The estimates characterizing the effects of these laws are the coefficients on the interaction of $D_{s(i)}$ with event-year dummies, $\mathbb{1}(t - T^*_{s(i)} = y)$, which are equal to one when the year of observation is $t = -5, \ldots, 0, \ldots, 8$ years from $T^*_{s(i)}$, the date when NP deregulation is effective in state s. I omit the year, t = -2, from my estimating equation because these observations represent the last two-year period before deregulation. As a result, π_{-2} is standardized to zero, and all π_τ's are estimated deviations from that year.

Lastly, to test the third proposition from my conceptual framework (Proposition I.3), I interact $NPDereg_{s(i)t}$ with the density of NPs in each state (see Section 1.7.1) and re-estimate the two-part model for prescriptions. Results consistent with this proposition will yield a coefficient on the interaction term that is negative, which implies that the magnitude of the Substitution Effect will increase with NP density. Thus, the reported average marginal

effect of $NPDereg_{s(i)t}$ will become more negative as density increases. For instance, if the Substitution Effect dominates the Access Effect, the effect of NP deregulation will be largest among the densest states.

1.5.2 Two-Part Model

The histograms shown in Figures 1.6 and 1.7 highlight several facts about non-linearities found in the distribution of prescription opioid use. First, 80% of respondents do not use prescription opioids in a given two-year period. Second, roughly 19% of positive users consume six or more prescriptions, which implies a distribution that is skewed by the right tail. Lastly, Figure 1.7 shows that prescription opioid pills appear to be log-normally distributed among users.

To better reflect these non-linearities, I use a two-part model [Cragg (1971); Newhouse and Phelps (1976); Belotti et al. (2015)]. The biggest advantage of the two-part model over OLS is that it explicitly decomposes the extensive and intensive margins by first estimating the likelihood of consuming any opioid prescription in a given two-year period and then by estimating the amount of prescription opioids consumed conditional on having positive use. The two-part model also performs better than alternative non-linear models as well. These models include Poisson, zero-inflated Poisson, negative binomial, and generalized linear models. I prefer the two-part model to these alternatives because it explicitly models the large mass of non-consumers and because it yields a lower Akaike Information Criterion (AIC), which suggests that it is better at fitting the data.

I estimate the extensive marginal effect using a logit model, where the outcome is a binary variable equal to zero if consumption is zero and equal to one if consumption is positive. The covariates in Equation 1.1 remain the same here and are labeled as X_{it} with a corresponding vector of coefficients labeled as α. Thus, the conditional probability of positive use is:

$$Pr(y_{it} > 0|X_{it}) = \frac{1}{1 + e^{-X_{it}\alpha}} \tag{1.3}$$

I estimate the intensive marginal effect using a generalized linear model (GLM) with a log link and gamma family, which is similar to a log-level OLS model but is better equipped to address skewed, heteroskedastic data. Similar to the extensive estimation, the covariates in Equation 1.1 remain the same here and are labeled as X_{it} with a corresponding vector of coefficients labeled as β. Thus, the conditional mean use for positive users is:

$$E[y_{it}|y_{it} > 0, X_{it}] = e^{X_{it}\beta} \tag{1.4}$$

Per Manning and Mullahy (2001), I use a modified Park test to determine the conditional variance function for this conditional mean, $var[y_{it}|y_{it} > 0, X_{it}] = \gamma \times E[y_{it}|y_{it} > 0, X_{it}]^{\delta}$. The Park test results suggest that the conditional variance is proportional to the square of the conditional mean ($\delta = 2$), which implies a gamma family. Depending on the choice of controls, δ ranges from 1.71 to 2.12 and is precisely estimated.

Another feature of the two-part model is that the extensive and intensive effects can be combined to estimate an overall effect. This fact becomes clear when we consider the combined conditional expectation.

$$E[y_{it}|X_{it}] = \frac{1}{1 + e^{-X_{it}\alpha}} \times e^{X_{it}\beta}$$

The overall marginal effect for NP deregulation is shown below, and the results from this calculation are most comparable to the marginal effects from the OLS difference-in-differences specification.

$$ME_{it}^{Overall} = \left\{ \left[\frac{1}{1 + e^{-X_{it}\alpha}} \middle| NPDereg = 1 \right] - \left[\frac{1}{1 + e^{-X_{it}\alpha}} \middle| NPDereg = 0 \right] \right\} \times e^{X_{it}\beta} + \left\{ \left[e^{X_{it}\beta} \middle| NPDereg = 1 \right] - \left[e^{X_{it}\beta} \middle| NPDereg = 0 \right] \right\} \times \frac{1}{1 + e^{-X_{it}\alpha}}$$

1.6 Results

The results presented in this section show that NP deregulation leads to less prescription opioid use. The analysis begins by examining the effects on both opioid prescriptions and opioid prescription pills. I then evaluate whether these laws had any adverse effects on access to care and health outcomes. Lastly, to explore possible mechanisms for these effects, I analyze whether NP deregulation affects substitute prescriptions or use of health care services.

1.6.1 Prescription Opioids

I report the results for opioid prescriptions in Table 1.3. Panel A presents the OLS difference-in-differences results, and Panel B presents the two-part model results. Overall, the weighted average number of opioid prescriptions across all respondents in a given two-year period is 0.88 prescriptions. When focusing on the 20.4% of respondents who consume prescription opioids over a two-year period, the weighted average increases to 4.33 opioid prescriptions over two years.

The OLS results imply a 0.13 to 0.09 reduction in the number of opioid prescriptions

following NP deregulation, which represents a 15.0% to 9.8% reduction in overall consumption. My preferred specification includes all controls and estimates about a 0.09 reduction in opioid prescriptions, which represents about a 9.8% reduction from the overall mean.

In Panel B, I decompose the overall result into an extensive and an intensive effect. My estimates of the extensive marginal effect are small and precisely estimated. I interpret this result to mean that NP deregulation has little to no effect on the probability of consuming prescription opioids. On the other hand, the estimates of the intensive marginal effect imply that NP deregulation reduces positive consumption by about 6.8% from the mean. Taken together, these results imply that most of the overall effect is driven by positive consumers reducing the number of prescriptions rather than fewer prescription opioid users.

While the results from Table 1.3 suggest that NPs respond to these laws by prescribing fewer prescriptions, they may be doing so while simultaneously prescribing more pills per prescription. Fortunately, the MEPS include the number of pills for each prescription. The estimates in Table 1.4 repeat the estimation strategy for prescription pills. As before, Panel A presents OLS difference-in-differences results, and Panel B presents results from the two-part model. The results from Table 1.4 are broadly consistent with Table 1.3. Like prescriptions, prescription opioid pills decrease after NP deregulation and these effects are concentrated among positive users. Both of these results are consistent with Proposition I.1, where the Substitution Effect dominates the Access Effect and NPs behave more conservatively than physicians.

One assumption necessary for my difference-in-differences specification is that of parallel pre-event trends between authorized and non-authorized states. To evaluate this assumption and to explore differences in the treatment effect over time, I repeat my analysis using the event study specification described in Equation 1.2.

Figures 1.9 and 1.10 illustrate the year-specific average marginal effects of NP deregulation on the opioid prescriptions. To create these estimates, I ran the individual parts of the two-part model on Equation 1.2 with controls for individual, state, and county characteristics. Figure 1.9 shows the year-specific extensive effects, which is estimated using a logit model and an outcome variable for whether the respondent has positive opioid prescriptions. Figure 1.10 shows the year-specific intensive effects, which is estimated using a generalized linear model with a log link and gamma family and is conditional on having positive opioid prescriptions. The outcome variable for this specification is the count of opioid prescriptions over the two-year period for each respondent.

Year 0 defines the first year of NP deregulation, and comparison of mean effects before and after this date roughly corresponds to the results found in Table 1.3. Note also that the base year is Year -2, which is the last set of observations with two years of untreated data.

All estimates are comparisons to this base year, and the estimate for Year -2 is standardized to zero.

The results do not indicate any differential pre-NP deregulation effect across states, which is consistent with the parallel trends assumption. In addition, there is no detectable change in the extensive margin over time (Figure 1.9). On the intensive margin (Figure 1.10), there does appear to be a large negative effect on opioid prescriptions immediately after NP deregulation, but the estimated effects fluctuate across years and weaken over time.

Overall, the year-specific effects are too imprecise to glean much additional information. In addition, the year-on-year fluctuations could be the result of sampling variation rather than an actual change in trends. For both of these reasons, I prefer the simpler difference-in-differences specification outlined in Equation 1.1 and the corresponding two-part model. For subsequent analyses, I focus exclusively on these specifications.

1.6.2 Health Outcomes and Access to Care

While Tables 1.3 and 1.4 imply that NP deregulation reduces the availability of prescription opioids, this result does not have to be universally positive. One concern might be that patients who need prescription opioids can no longer get them after NP deregulation. If this is true, then we should see a decline in well-being. There is no detectable evidence of this phenomenon occurring. Using a linear probability model and Equation 1.1, I estimate that NP deregulation increases the probability of having no physical limitation by half a percentage point, and the probability that self-reported health is rated as good or excellent increases by 1.63 percentage points. I also estimate that the probability of hospitalization decreases by 0.27 percentage points. Although these estimates are small and imprecise, the point estimates do not suggest worsening health.

I also regress the dummy for having a usual source of primary care using Equation 1.1, and while the point estimate suggests an increase in the probability by almost one percentage point, it is statistically insignificant and small relative to the mean. One implication of this result is that the Substitution Effect mentioned in Proposition I.1 of the Conceptual Framework is likely to dominate the Access Effect, which also implies that opioid prescriptions should decrease after NP deregulation.

1.6.3 Potential Mechanisms

This section is dedicated to exploring two possible mechanisms for how NPs reduce prescription opioid use while maintaining health outcomes. First, I explore substitution from prescription opioids to other prescriptions that can potentially treat pain. The results

indicate no measurable substitution along this dimension. Second, I evaluate whether NPs substitute prescription opioids for additional office-based or outpatient visits, and while I find no substitution toward more office-based visits, there appears to be a modest increase in outpatient visits following NP deregulation. From these results, I conclude that there is no substitution toward other prescription drugs, but there may be some substitution toward outpatient follow-ups, which includes physical therapy among other things. I then summarize other alternative mechanisms for reducing prescription opioid use.

1.6.3.1 Substitute Prescriptions

To test whether NPs recommend substitute drugs when treating pain patients, I re-run Equation 1.1 using four other drug classifications as outcomes. First, I evaluate whether there is any substitution regardless of drug classification by studying all non-opioid prescriptions. Then, I study the narrow class of non-opioid central nervous system (CNS) agents. Lastly, within non-opioid CNS agents, I study effects for non-opioid analgesics and non-analgesic CNS agents.

Given that opioids are an analgesic CNS agent, an obvious substitute group is non-opioid analgesics. These include non-steriodal anti-inflammatory drugs (NSAIDs), salicylates, antimigraine agents, and Cox-2 inhibitors. Common trade names for different NSAIDs include Advil, Aleve, Midol, and Motrin, and a common salicylate is Aspirin. Antimigraine agents and Cox-2 inhibitors are less widely known, but as analgesics, they can also be used to treat pain.

Non-opioid, non-analgesic CNS agents can also be considered substitutes because they suppress CNS receptors. These include anticonvulsants; hypnotics, anxiolytics, and sedatives (e.g., Xanax, Valium, and Ambien); and muscle relaxants. One potential drawback associated with using these classifications is that some drugs are controlled substances, which means that they will be partly influenced by NP deregulation and partly influenced by potential substitution. Despite this limitation, I include these drugs as an outcome because they can be perceived as a less risky alternative to treating pain than prescription opioids [Longo and Johnson (2000)]. Also, the Drug Enforcement Agency lists these drugs as Schedule III and above, whereas most prescription opioids are listed as more dangerous Schedule II drugs.

To evaluate these substitution effects, I rely on the fully specified two-part model for prescriptions. The results found in Table 1.6 indicate that substitute prescriptions do not appear to be a mechanism for how NPs reduce opioid prescriptions. No substitute prescriptions increase after NP deregulation, and the decreases across all margins are small and statistically insignificant.

1.6.3.2 Health Care Utilization

Unlike other prescription drugs, there appears to be some evidence of substitution with outpatient visits (see Table 1.7). Respondents with a positive number of outpatient visits are estimated to use half a visit more after NP deregulation. However, I estimate an opposite effect for office-based visits. Respondents with a positive number of office-based visits appear to reduce their visits by half a visit after NP deregulation.

The interpretation of these two seemingly opposite findings depends on the definition of office-based versus outpatient visits. Office-based visits are classified as care provided in a non-institutional setting, while outpatient visits are classified as health and medical services from a unit of a hospital or facility connected with a hospital. Outpatient visits include, among other things, physical therapy and drug abuse clinics, while office-based visits do not specifically list these services. In practice, there is likely some overlap in how patients utilize these two groups.

Given the ambiguous distinction, I take a cautious interpretation of these results. There may be some substitution toward alternative forms of treatment, but there could also be a decline in visits among positive users. One possible explanation that could reconcile these findings is that patients may use fewer office-based visits simply because they are not refilling as many opioid prescriptions, but I cannot test this explanation with the available MEPS data.

1.6.3.3 Other Alternatives

To summarize, NPs do not appear to substitute toward other prescriptions, and they may substitute toward outpatient visits. Given this weak evidence of substitution, it remains possible that they reduce opioid prescriptions through some other means. While data limitations prevent further exploration, other channels include over-the-counter and alternative therapies, no substitution, and improved health care resulting from specialization.

One such alternative therapy is medical marijuana, which has been shown to be correlated with lower opioid overdose mortality [Bachhuber et al. (2014)]. Although this is a promising topic, only four states (California, Colorado, Nevada, and New Jersey) in my sample have both NP deregulation and medical marijuana laws, and these states only represent 29 state-years in total, making the policy interaction too infrequent to investigate. In addition, data restrictions make it impossible to identify these states in my analysis, and I cannot evaluate whether these laws bias the estimated effect of NP deregulation. Given the infrequency, this seems unlikely.

Of the remaining channels, improved health care from specialization is most consistent

with the results. NP deregulation makes it possible for NPs to assume some responsibilities exclusively held by physicians, and physicians and NPs who work together can use this opportunity to reallocate patients relative to each other's strengths and weaknesses. In this setting, sorting can happen such that the patients are at least weakly better off than before. Per my conceptual framework, patients who switch to an NP are among the least appropriate patients for prescription opioids, and some of those patients were receiving opioid prescriptions prior to NP deregulation. Thus, a switch to an NP who conservatively prescribes opioids simultaneously reduces opioid prescriptions and improves patient outcomes. In addition, because these patients' health improves and they have no need for additional prescription opioid refills, they will also have no need for additional clinic visits. A similar story can be told for the patients who remain with the physician. Although these patients are still more likely to get prescription opioids, they might also be getting better care than before because physicians can spend more time with these remaining patients. If this is true, then these patients also get healthier and require fewer visits.

1.7 Robustness

1.7.1 Nurse Practitioner Density

As an extension of the main analysis shown in Section 1.6.1, I interact the NP deregulation variable with each state's NP density and re-estimate the two-part model for opioid prescriptions. I then report the overall average marginal effect of NP deregulation across different percentiles of NP density. Given the earlier estimates, results consistent with Hypothesis I.3 should yield a negative coefficient on the interaction term between NP deregulation and NP density, which implies that the reduction in opioid prescriptions intensifies for denser states.

Unfortunately for this analysis, data on NP density is unavailable for 10 states (Alaska, Alabama, the District of Columbia, Indiana, Maryland, Missouri, Mississippi, Montana, New Jersey, and Ohio). As such, the sample size decreases to 91,275, and results from Section 1.6.1 are not directly comparable. To see if this change in sample affects my main results, I re-estimate the fully specified two-part model using the sample with NP density data and with a new covariate for state-level NP density. Table 1.8 compares those results for opioid prescriptions to the results from the full sample found in Table 1.3. The estimates are qualitatively similar across the two samples, although the NP density sample has slightly larger point estimates.

Given that the results for the NP density sample are qualitatively similar to those from the full sample, I then proceed to interact the NP deregulation variable with NP density. Figure 1.11 shows the overall average marginal effects from the two-part model for prescriptions

over different percentiles of NP density. The 10th percentile of NP density is 12 NPs per 100,000 people, and the 90th percentile is 54 NPs per 100,000 people. The median is 28 NPs per 100,000 people, which is slightly lower than the average of 30 NPs per 100,000 people.

At the median, NP deregulation is estimated to reduce prescriptions by -0.12 prescriptions, which is statistically discernible from zero and similar to the average marginal effect at the mean. Looking across the distribution, estimated effects more than double in magnitude from -0.07 to -0.18 opioid prescriptions when NP density changes from the 10th percentile to the 90th percentile; however, these estimates are not statistically distinguishable from each other. Thus, while evidence is suggestive that Proposition I.3 holds, I cannot reject the null that effects are homogeneous across densities.

1.7.2 Discussion of Survey Data Limitations

Comparing the MEPS utilization patterns to administrative datasets, it appears that MEPS underestimates nationwide consumption. Figure 1.2 shows both the trend in estimated MEPS prescriptions and that from IMS Health's Vector One National (VONA) data, which are administrative transaction data collected from about half of pharmacies nationwide. These data are then aggregated using proprietary methods to estimate national transactions.

Compared to MEPS estimates, VONA estimates are consistently higher. There are four different variations of non-response for why this discrepancy may exist. First, while self-reported MEPS data is cross-validated with administrative pharmacy records, roughly a quarter of respondents opt to not release their records. If heavy users of prescription opioids intentionally misreport their use and refuse authorization because of this misreporting, then the difference between actual and reported opioid prescriptions will be missed in the MEPS. The size of this discrepancy depends on the size of misreporting and the strength of the correlation between misreporting and failing to authorize.

Second, roughly a quarter of authorized pharmacies do not participate on the cross-validation either because of direct refusal, non-response, or an inability to locate the respondent in their records. These non-cooperating pharmacies will affect the accuracy of self-reported use, but it is not immediately clear whether they will lead to overestimation or underestimation, which depend on the underlying reasons for why misreporting happens. Additionally, it is unclear how many prescriptions these non-cooperating pharmacies represent. If they are primarily the fourth reported pharmacy for each respondent, then the effects could be rather small.

Third, respondents can incorrectly report their pharmacies. This can happen in one of two ways. The respondent can give the name of pharmacies that she does not use, or if she

is engaged in pharmacy shopping, the censored limit of four pharmacies will automatically reduce the number of pharmacies the respondent is obligated to report. While both of these behaviors are possible, they require more complex calculus while still generating the same result as misreporting without authorization.

Lastly, and perhaps most importantly, MEPS sampling will cause underestimation. Because the sample is designed to represent civilian noninstitutionalized residents, many heavy users of prescription opioids may be omitted by construction. In addition, the MEPS is an involved survey with extensive questioning. While the survey administrators try to minimize this burden, it may still be too much for heavy users with long-term pain or mental health issues to participate. The combination of the two will likely lead the MEPS to lower estimates relative to administrative reports.

The existence of non-response suggests that the estimates reported in this paper are specific to the population amenable to being surveyed. An analysis of the broader population will require state-level administrative data, which is not subject to the non-response problems inherent in surveys. That being said, it is still possible that the survey estimates are unbiased.

For non-response to induce bias such that estimated effects are negative, several items must be true simultaneously. First, the non-response described above must vary either across states or over time. Otherwise, the state and year fixed effects will difference out this discrepancy. Second, the remaining time- and state-varying non-response must be correlated with prescription opioid use. Given the comments above, it appears likely that non-response is positively correlated with use. Assuming this is true, the remaining time- and state-varying non-response must be negatively correlated with NP deregulation to force a negative estimate.

One could test this last item by regressing NP deregulation on state-level non-response rates for each year in a specification with state and year fixed effects. Unfortunately, these data are unavailable for privacy reasons. If I instead postulate that increases in survey non-response are a proxy for a population's poor health, a negative correlation would imply that policymakers are less likely to pass NP deregulation and improve access to care when faced with a sicker population, which seems contradictory.

1.7.3 Falsification Tests

1.7.3.1 Placebo Year of Implementation

For estimates in Table 1.3 to have a causal interpretation, NP deregulation should be unrelated to trends in opioid prescriptions conditional on state and year fixed effects. In Table 1.9, the "Start" Year of 0 represents the actual year of NP deregulation and corresponds to

Model 4 in Table 1.3. For the other rows, I synthetically modify the year of NP deregulation to be either 1 to 4 years before or 1 to 4 years after the actual year of NP deregulation. I then re-estimate the average marginal effects using OLS and each part of the two-part model on the placebo years.

Of the 16 different specifications shown below, only two are statistically significant at the 10% level. In addition, the estimates alternate in sign and reveal no meaningful pattern. These results suggest that the observed reduction in opioid prescriptions is the result of NP deregulation rather than some idiosyncratic fluctuation.

1.7.3.2 Non-Controlled Prescriptions

In most states, prescriptive authority for non-controlled substances preceded authority for controlled substances. As such, additional NP deregulation is unlikely to alter use of antibiotics, cardiovascular drugs, and other non-controlled drugs. The results from Table 1.10 largely support this hypothesis, although there is some weak evidence that overall antibiotic use declined following NP deregulation. This overall reduction appears to be driven by a combination of extensive and intensive effects, neither of which are statistically significant. Changes to cardiovascular drugs and other non-controlled substances appear to be small and insignificant.

1.7.4 Independent Practice and Prescriptive Authority

As mentioned in Section 1.2.1, NP reforms have altered the way NPs practice as well how they prescribe medicine. This section highlights two different tests for how NP independence interacts with reforms. First, I evaluate the effects of NP deregulation, as described in Equation 1.1, across different *practice* authority regimes (shown in Table 1.11). Then, in a separate specification, I modify the NP deregulation definition such that $NPDereg_{s(i)t} = 1$ only if NPs are given fully independent *prescriptive* authority for controlled substances (shown in Table 1.12).

I categorize practice authority into three groups ranging from least restrictive to most restrictive: (1) states where NP regulations are administered exclusively by the state's Board of Nursing and NPs are allowed to diagnose and treat patients without physician involvement, (2) states where NP regulations are administered exclusively by the state's Board of Nursing and physician involvement is required, and (3) states where NP regulations are jointly administered by the state's Board of Nursing and Board of Medicine. Roughly one-quarter of the sample live in a state with full independence, over half of respondents in the sample live in a state with physician involvement, and the remainder live in a jointly administered

state.

Using these classifications, I modify Equation 1.1 to interact dummies for each practice authority regime with NP deregulation. This adjustment allows me to estimate NP deregulation effects on opioid prescriptions that are specific to each practice authority regime. Table 1.11 presents this specification with results estimated using the two-part model. Across all regimes, most point estimates are negative, and effects appear to be driven by reductions among positive users. Furthermore, the strongest reductions in opioid prescriptions are concentrated in states with the least restrictive regulations.

This distinction is most obvious for the intensive margin where positive users reduce their opioid prescriptions by an estimated 0.78 prescriptions over two years. This estimate is nearly three times the point estimates of the other two groups, and with a confidence interval of [-1.24,-0.32], I can reject homogeneous effects across the three regimes.

As an alternative measure of NP independence, I define NP deregulation using only reforms that grant NPs independent prescriptive authority for controlled substances. Unlike the specification for Table 1.11, this specification reduces the number of authorized states to nine states (versus 27 above) and does not interact NP deregulation with practice authority. In addition, these nine states are sparsely populated and predominately rural. Because of these limitations, I cannot claim that the results are representative of the nation as a whole.

That being said, the point estimates are also negative and consistent with the findings from Table 1.11. For the fully specified model, the probability of consuming a prescription opioid declines by almost an entire percentage point, albeit imprecisely, and positive users reduce consumption by almost one prescription. These results represent point estimates that are two to four times those found for the NP deregulation measure that includes physician involved authority (see Table 1.3).

Collectively, I interpret the results from Tables 1.11 and 1.12 as suggestive evidence that NPs diagnose and treat more conservatively than physicians and that this distinction increases as NPs become more independent. The exact mechanism for these results is unclear. A likely explanation is that more independence exposes NPs to more risk, and they internalize this higher risk by prescribing fewer opioids. Unfortunately, this hypothesis is untestable without provider identifiers, something the MEPS data lack.

1.8 Discussion and Conclusion

This paper indicates that when states grant NPs the authority to prescribe controlled substances, prescription opioid use declines without any corresponding decline in self-reported health. These welfare improving results are consistent with other studies on deregulation for

mid-level clinicians. Stange (2014) finds that NP deregulation may be just as important in improving access to care as policies designed to expand provider supply directly, and Kleiner et al. (2014) find that NP deregulation reduces cost per visit without influencing health care quality. Likewise in the dental market, Buchmueller, Miller and Vujicic (2014) see improvements in access to care when dental hygienists are given more autonomy. Alexander and Schnell (2016) show that prescriptive deregulation for non-controlled substances both reduce mortality and improve mental health, and Markowitz et al. (2016) find that deregulation for nurse midwives leads to fewer induced labor and Cesarean section births without harmful effects on maternal behavior or infant outcomes.

Even if I focus exclusively on the cost of prescribed opioids, the effects could still have substantial welfare implications. The results from Table 1.3 suggest that opioid prescriptions decline by 7% to 9%., and the estimated number of opioid prescriptions from the MEPS is 57.7 million in 1996 and 113.9 million in 2010 (see Table 1.1). Using these values, roughly 4.3 million to 11.3 million were not prescribed annually as a result of NP deregulation. If I use the average 2010 unit cost of an opioid prescription, $57 (2010 $s), then the approximate savings from foregone prescriptions ranges from $245.1 million to $644.1 million each year.

Furthermore, fewer opioid prescriptions may also lead to less misuse and more labor force participation, which will generate additional gains. Estimates of the societal cost of prescription opioid misuse is approximately $55 billion [Hansen et al. (2011*a*); Birnbaum et al. (2011)]. These estimates include costs from lost labor productivity, criminal justice, and medical treatment. More recently, Case and Deaton (2015) hypothesize that a rise in mortality among white males may be associated with the concurrent increase in prescription opioid use, and Krueger (2016) finds that over half of prime age men not in the labor force take prescription opioids daily. While the direction of causality in these studies is unclear, the strong correlation between outcomes and prescription opioid use suggest another potential opportunity for welfare gains.

Presumably, there are also costs associated with NP deregulation as state administrators adjust to new policies, but these adjustment costs are likely small by comparison because they are non-recurring.

Given the potential for sizable welfare gains, policies authorizing NPs to prescribe controlled substances should continue. Moreover, there remains the opportunity for further deregulation. As of 2016, over half of all states still require NPs to coordinate with physicians when treating patients. Meanwhile, Tables 1.11 and 1.12 offer suggestive evidence that these requirements may limit the gains from deregulation. Efforts to tease out the potential mechanisms leading to these results are promising avenues for future research.

1.9 Figures

Figure 1.1: Controlled Substance Prescription Authorization Timeline

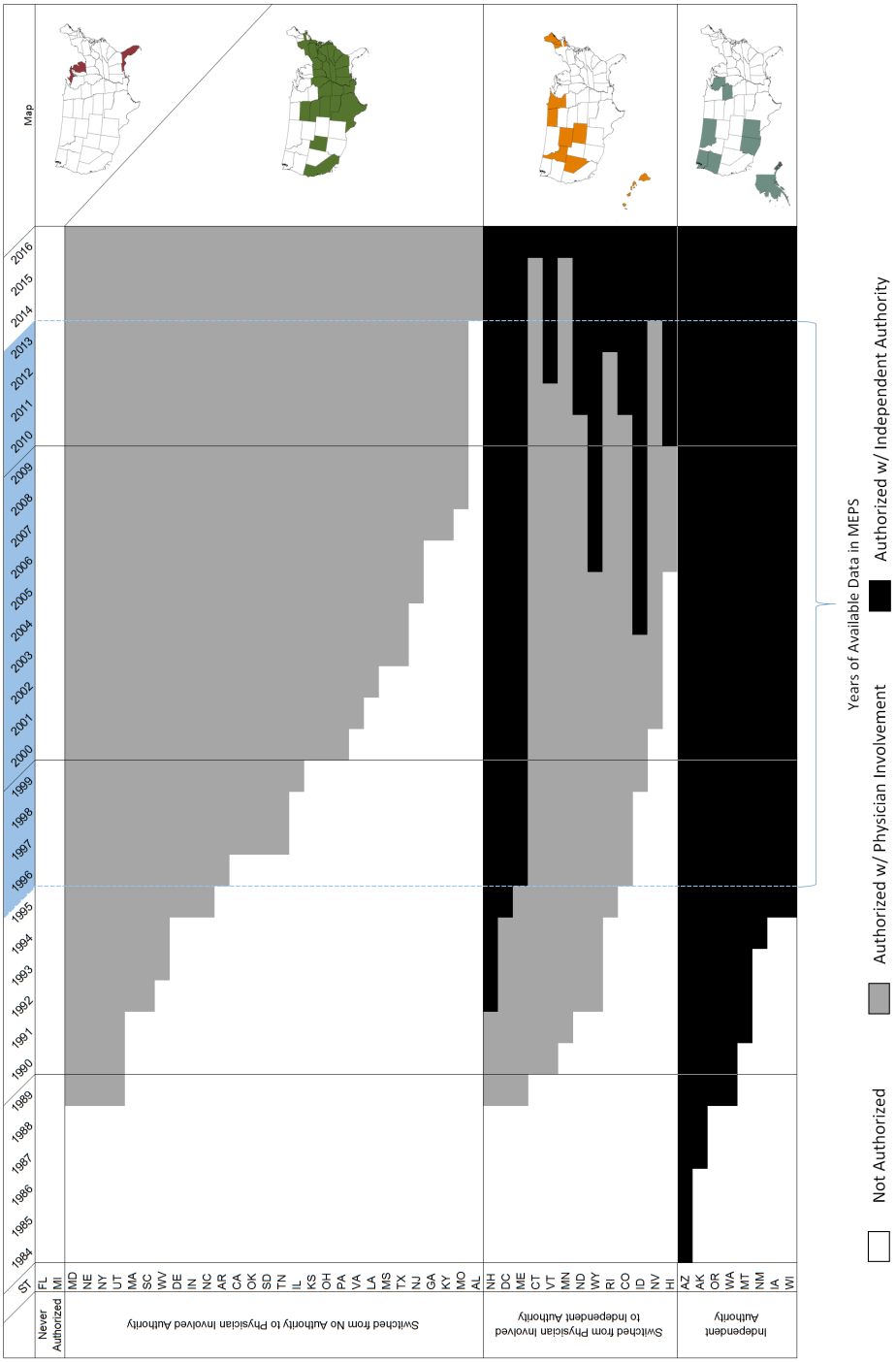
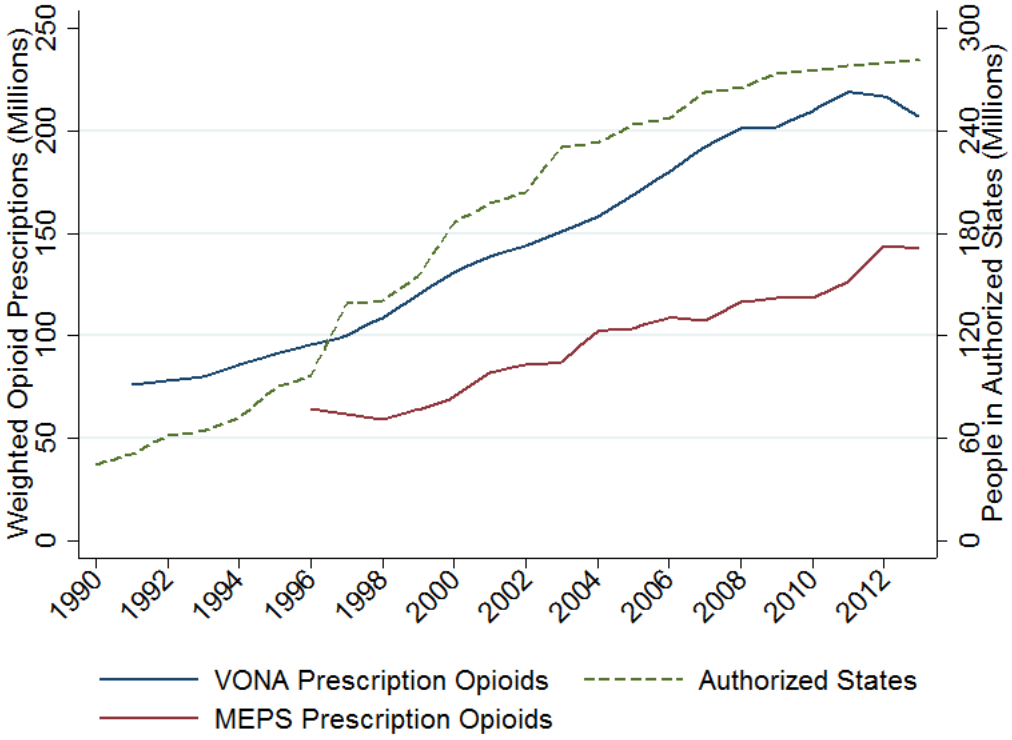
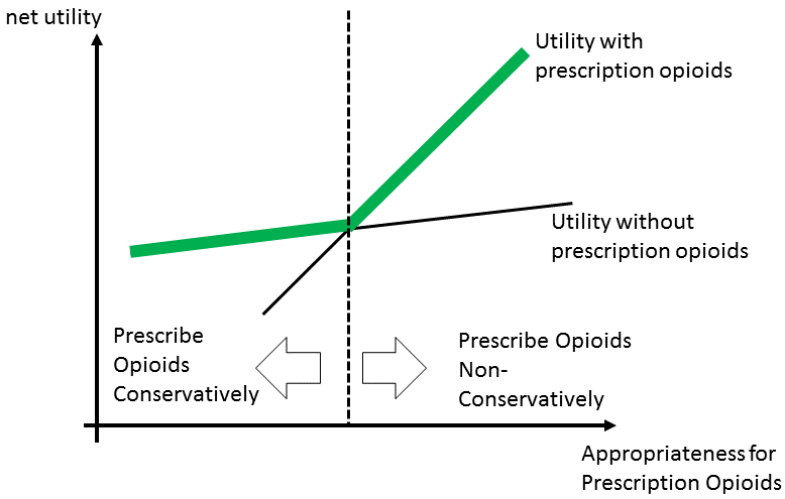


Figure 1.2: Trend in Prescription Opioids and NP Controlled Substances Regulations



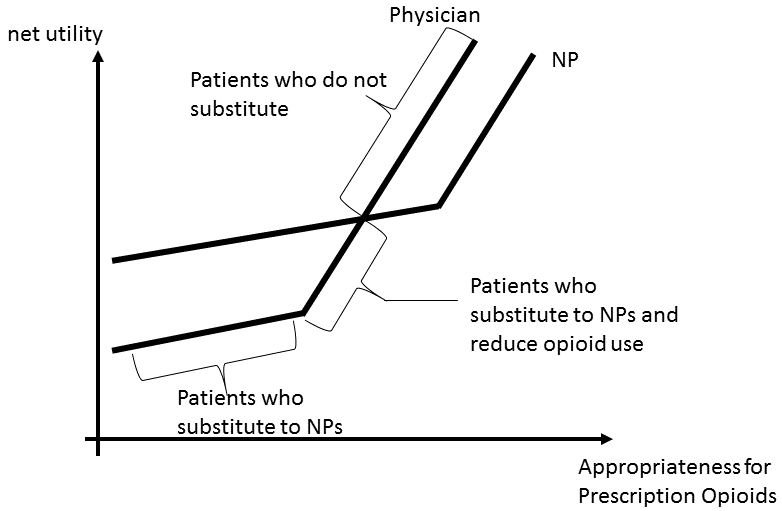
Source: Estimates IMS Health's Vector One National (VONA) database reported via drugabuse.gov. Adult Respondents and survey weights from MEPS. Authorization data from state legislation records and *The Nurse Practitioner*. Population estimates from Area Health Resource File.

Figure 1.3: Graphical Illustration of the Roy Model



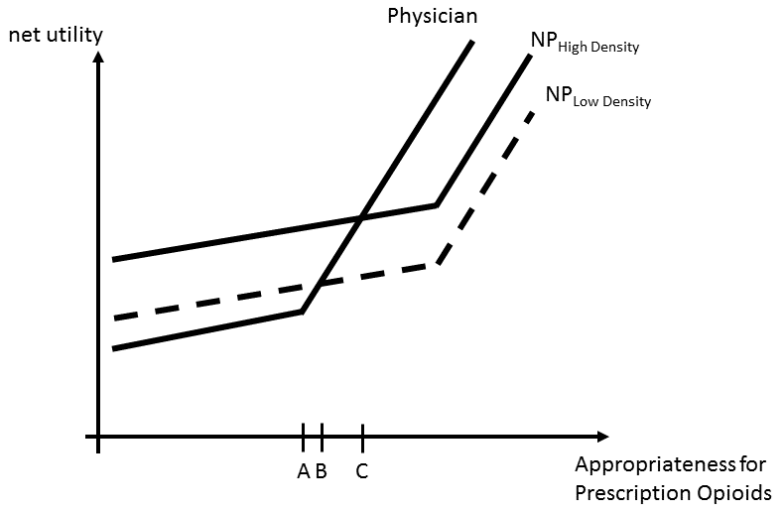
The above figure illustrates the prescription behavior of a provider, where appropriateness and the net utility gained from treatment is certain. Those most appropriate for prescription opioids receive them, and those least appropriate do not (illustrated with the bold green envelope).

Figure 1.4: Effects of NP Deregulation



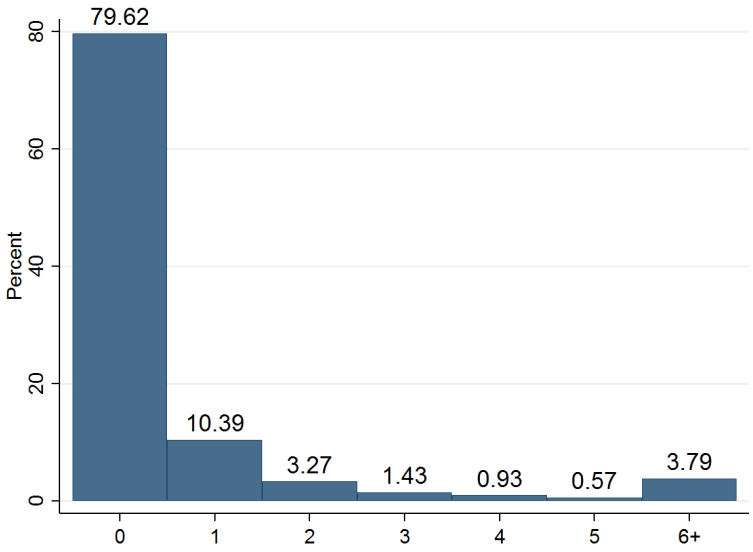
The above figure illustrates how substitution of treatment can occur following NP deregulation, where net utility gains from treatment and appropriateness are certain. Prior to NP deregulation, pain patients are only treated by physicians. After NP deregulation, the least appropriate patients are treated without prescription opioids by NPs, and the most appropriate patients continue to be prescribed opioids by physicians.

Figure 1.5: Effects of NP Deregulation by NP Density



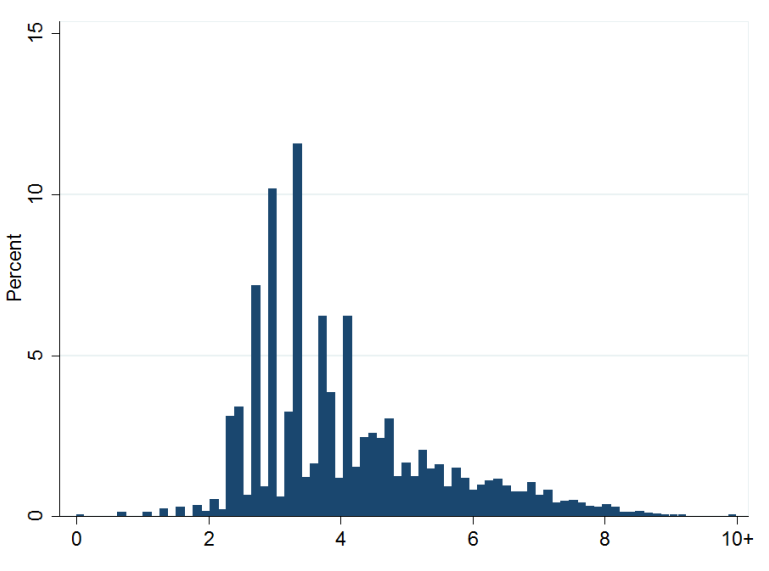
The above figure illustrates the effects of NP deregulation while varying accessibility to NPs. Patients living in a deregulated state with a high density of NPs will substitute to NPs if their appropriateness is between A and C. Patients living in a deregulated state with a low density of NPs will substitute to NPs if their appropriateness is between A and B. Details on the empirical test of this prediction are shown in Figure 1.11.

Figure 1.6: Histogram of Number of Prescriptions



The above figure prescription opioid consumption by each respondent and by the number of opioid prescriptions. Almost 80% of respondents do not consume opioid prescriptions, and about one-fifth of positive users have 6 or more prescriptions over the two-year survey. Results are censored at six prescriptions.

Figure 1.7: Histogram of Log Number of Pills



The above figure shows prescription opioid consumption by each respondent and by the log number of opioid pills. It only includes respondents with positive amounts of opioid use. Results are censored at log(pills) equal to 10.

Figure 1.8: Trends in Covariates Before and After NP Deregulation

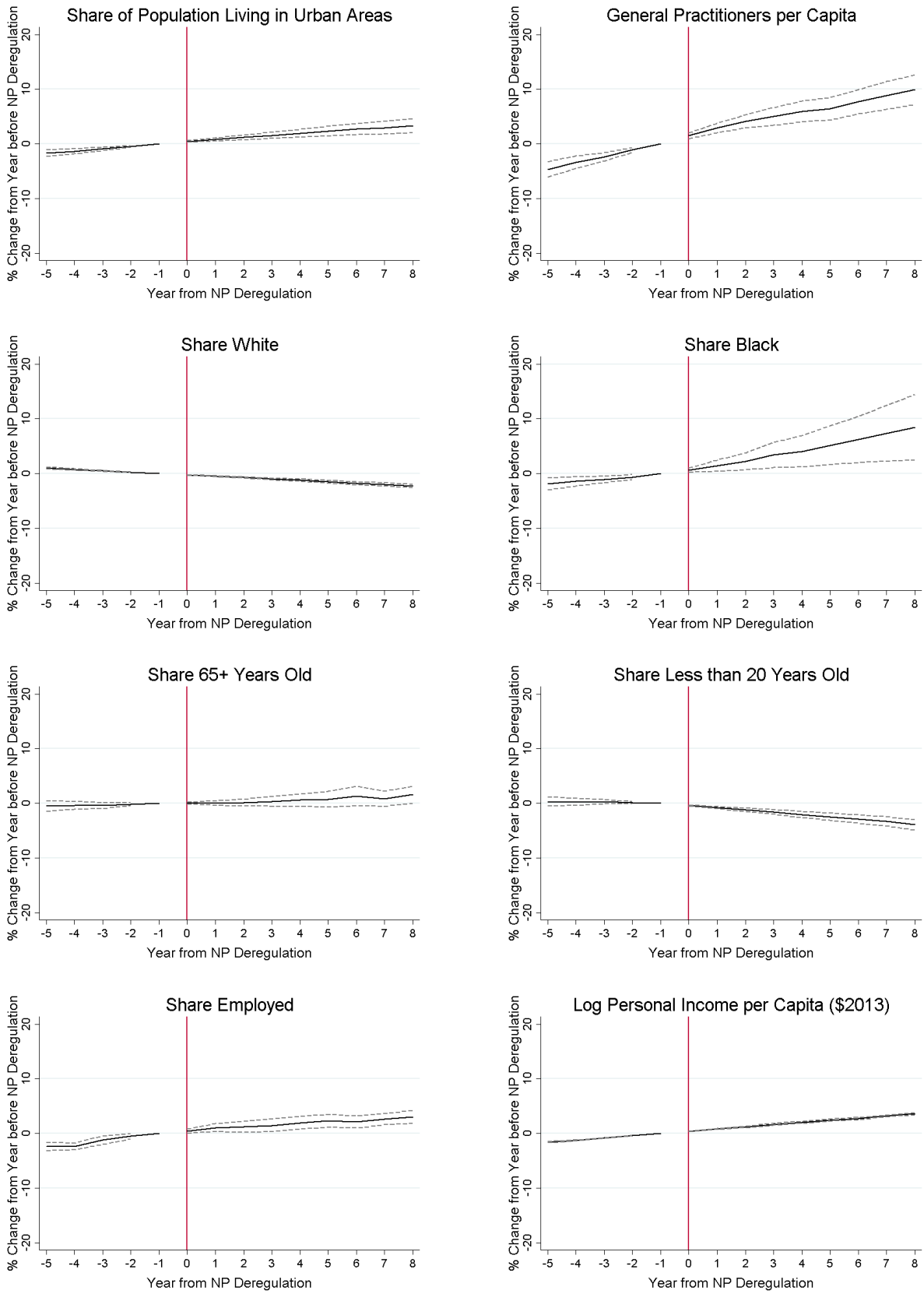
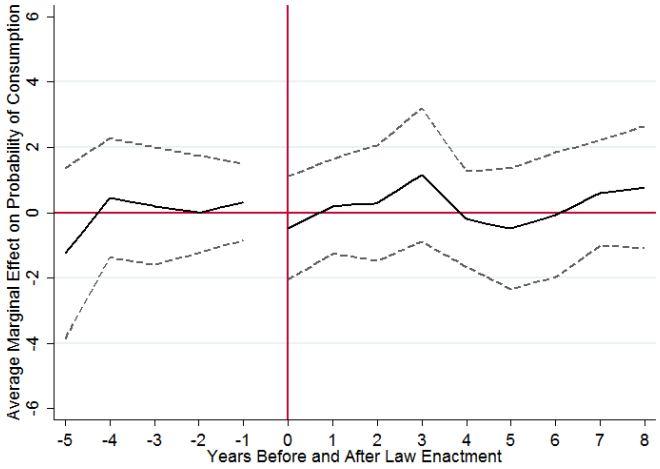
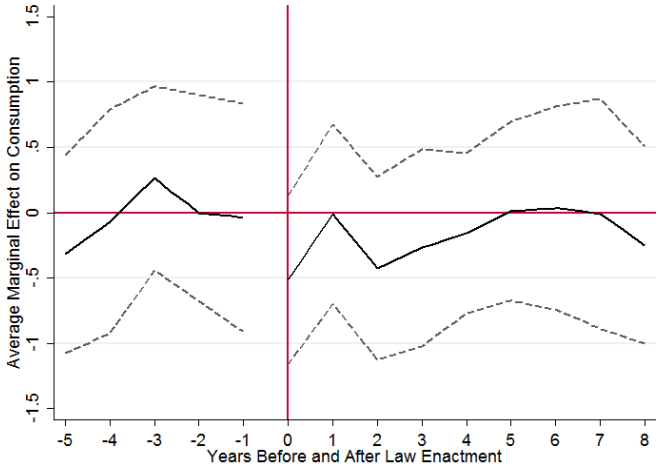


Figure 1.9: Estimated Year-Specific Extensive Effects on the Probability of Consuming a Prescription Opioid (ppt)



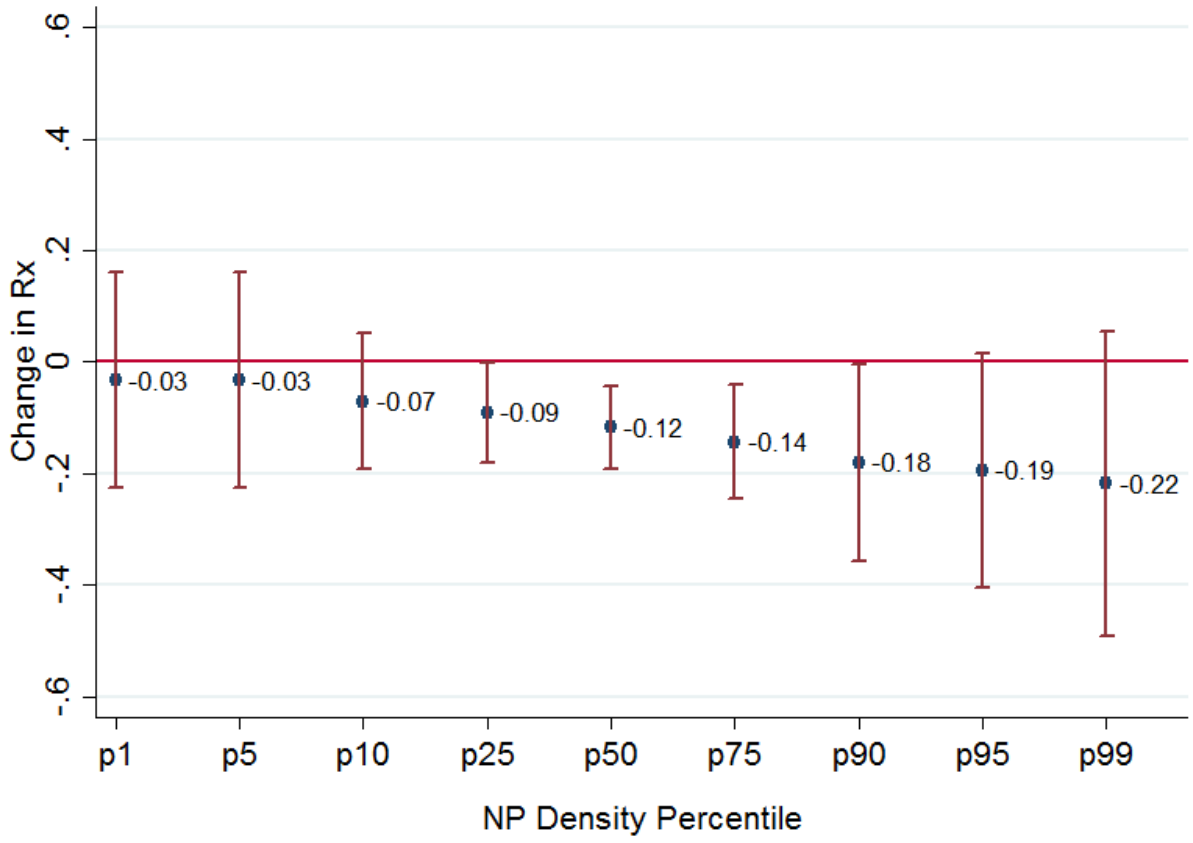
Comparisons of the means before enactment and after enactment roughly correspond to the two-part model estimates presented in Table 1.3. There does not appear to be any effect before enactment, which is consistent with the parallel pre-trend assumption. There also does not appear to be any change in the probability of consuming a prescription opioid after enactment.

Figure 1.10: Estimated Year-Specific Intensive Effects on Opioid Prescriptions (Rx)



Comparisons of the means before enactment and after enactment roughly correspond to the two-part model estimates presented in Table 1.3. There does not appear to be any effect before enactment, which is consistent with the parallel pre-trend assumption. The point estimates suggest that opioid prescriptions decrease after NP deregulation, but the estimate is imprecise. It also may subside over time.

Figure 1.11: Overall Effect of NP Deregulation over State-Level NP Density Percentiles



The above figure shows the estimated average marginal effect if I hold NP Density constant at certain state-level NP density percentiles. Estimates are generated from the two-part model and show the overall effect described in Section 1.5.2. Estimated effects more than double in magnitude from -0.07 to -0.18 opioid prescriptions when NP density changes from the 10th percentile at 12 NPs per 100,000 people to the 90th percentile at 54 NPs per 100,000 people; however, these estimates are not statistically distinguishable from each other. Thus, while evidence is suggestive that Hypothesis 3 holds, I cannot reject the null that effects are homogeneous across densities.

1.10 Tables

Table 1.1: Adult Drug Consumption by Therapeutic Classification

Category	Weighted Prescriptions (Millions) (% of Total)			Therapeutic Class Code
		Representative Years		
	All Years	1996	2010	
All Prescriptions for Adults	45,675.4 (100.0%)	1,601.4 (100.0%)	3,075.4 (100.0%)	—
Cardiovascular Agents	10,837.9 (23.7%)	367.1 (22.9%)	729.5 (23.7%)	40
CNS Agents	7,124.1 (15.6%)	232.6 (14.5%)	517.4 (16.8%)	57
Analgesics	3,805.2 (8.3%)	147.1 (9.2%)	235.1 (7.6%)	58
Opioids	1,680.8 (3.7%)	57.7 (3.6%)	113.9 (3.7%)	60, 191
NSAIDs	1,064.2 (2.3%)	62.6 (3.9%)	63.9 (2.1%)	61
Other Analgesics	1,067.0 (2.3%)	26.8 (1.7%)	58.4 (1.9%)	59, 62, 63, 193, 278
Anticonvulsants	1,415.3 (3.1%)	40.4 (2.5%)	108.4 (3.5%)	64
Anxiety & Sleep Aids	879.4 (1.9%)	26.5 (1.7%)	68.9 (2.2%)	67
Stimulants	184.1 (0.4%)	1.7 (0.1%)	19.7 (0.6%)	71
Muscle Relaxants	478.0 (1.0%)	15.5 (1.0%)	37.2 (1.2%)	73
Other CNS Agents	687.9 (1.5%)	29.6 (1.8%)	48.1 (1.6%)	65, 66, 86, 312, 313, 378
Psychotherapeutic Agents	3,348.4 (7.3%)	114.7 (7.2%)	213.0 (6.9%)	242
Antidepressants	2,592.2 (5.7%)	67.6 (4.2%)	181.6 (5.9%)	249
Antipsychotics	426.0 (0.9%)	18.9 (1.2%)	31.5 (1.0%)	251
Antibiotics	2,023.7 (4.4%)	120.0 (7.5%)	114.2 (3.7%)	1
Other Prescriptions	22,341.2 (48.9%)	766.9 (47.9%)	1,501.2 (48.8%)	—

Prescription estimates represent the weighted total of survey responses using person-level survey weights. Estimates only include adult consumption.

Table 1.2: Individual Sample Characteristics (Adults Only)

Characteristic	Weighted Mean	Standard Deviation
Female	52.1%	(50.0)
Married	56.7%	(49.5)
Working	69.1%	(46.2)
Hispanic	12.8%	(33.4)
Race		
White	81.9%	(38.5)
Black	12.2%	(32.7)
Other Race	5.5%	(22.7)
Family's Highest Degree		
Less Than High School	12.3%	(32.9)
High School	54.4%	(49.8)
Bachelor or Higher	33.3%	(47.1)
Enrolled in Medicare	19.3%	(39.5)
Enrolled in Medicaid	8.4%	(27.7)
Insurance Status		
Uninsured	14.7%	(35.4)
Insured (0,1) Qtrs	1.8%	(13.4)
Insured [1,2) Qtrs	2.7%	(16.3)
Insured [2,3) Qtrs	3.4%	(18.1)
Insured [3,4) Qtrs	2.7%	(16.3)
Insured All Qtrs	74.7%	(43.5)
Had Inpatient Surgery	8.2%	(27.4)
Age (19 to 90)	46.1	(17.4)
Family Income as a percent of FPL	402.1%	(324.5)
N	107,897	

Estimates represent the weighted average of survey responses using person-level survey weights.

Table 1.3: Average Marginal Effect of NP Deregulation on Opioid Prescriptions

		Average Marginal Effects			
	Outcome Mean	Model 1	Model 2	Model 3	Model 4
Panel A: Overall (OLS)					
NP Deregulation (Rx$\geq$ 0)	0.88	−0.13***	−0.11**	−0.09	−0.09*
(s.e.)		(0.05)	(0.04)	(0.05)	(0.05)
% Change		−15.0%	−12.3%	−9.7%	−9.8%
Panel B: Two-Part Model (Logit, GLM w/ Log Link and Gamma Family)					
Overall (Logit, GLM)					
NP Deregulation (Rx$\geq$ 0)	0.88	−0.10**	−0.09**	−0.07**	−0.08**
(s.e.)		(0.04)	(0.03)	(0.04)	(0.03)
% Change		−11.1%	−9.8%	−8.4%	−8.8%
Extensive (Logit)					
NP Deregulation (ppt)	20.4%	−0.19	−0.11	−0.25	−0.23
(s.e.)		(0.48)	(0.41)	(0.52)	(0.49)
% Change		−0.9%	−0.5%	−1.2%	−1.1%
Intensive (GLM; Positive Users)					
NP Deregulation (Rx$>$ 0)	4.33	−0.44**	−0.35***	−0.28**	−0.29**
(s.e.)		(0.18)	(0.13)	(0.13)	(0.12)
% Change		−10.2%	−8.1%	−6.4%	−6.8%
Positive N		20,943	20,943	20,943	20,943
N		107,897	107,897	107,897	107,897
Individual Controls			X	X	X
State Controls				X	X
County Controls					X
State and Year FE		X	X	X	X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights. With the exception of the extensive results, average marginal effects are reported as changes in the number of prescriptions. Extensive results are reported as percentage point changes.

Table 1.4: Average Marginal Effect of NP Deregulation on Opioid Prescriptions Pills

		Average Marginal Effects			
	Outcome Mean	Model 1	Model 2	Model 3	Model 4
Panel A: Overall (OLS)					
NP Deregulation (Pills≥ 0)	46.8	−7.1**	−5.6**	−3.4	−3.3
(s.e.)		(3.0)	(2.7)	(3.5)	(3.3)
% Change		−15.1%	−11.9%	−7.3%	−7.0%
Panel B: Two-Part Model (Logit, GLM w/ Log Link and Gamma Family)					
Overall (Logit, GLM)					
NP Deregulation (Pills≥ 0)	46.8	−5.5*	−7.0***	−6.5**	−7.0***
(s.e.)		(3.0)	(2.4)	(2.8)	(2.7)
% Change		−11.7%	−14.9%	−13.9%	−14.2%
Extensive (Logit)					
NP Deregulation (ppt)	20.4%	−0.19	−0.11	−0.25	−0.23
(s.e.)		(0.48)	(0.41)	(0.52)	(0.49)
% Change		−0.9%	−0.5%	−1.2%	−1.1%
Intensive (GLM; Positive Users)					
NP Deregulation (Pills> 0)	230.0	−24.1*	−27.4***	−24.3**	−25.0**
(s.e.)		(13.6)	(9.3)	(10.8)	(10.3)
% Change		−10.5%	−11.9%	−10.6%	−10.9%
Positive N		20,943	20,943	20,943	20,943
N		107,897	107,897	107,897	107,897
Individual Controls			X	X	X
State Controls				X	X
County Controls					X
State and Year FE		X	X	X	X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights. With the exception of the extensive results, average marginal effects are reported as changes in the number of prescription pills. Extensive results are reported as percentage point changes.

Table 1.5: Average Marginal Effect of NP Deregulation on Health Outcomes and Access to Care

	Average Marginal Effects			
Linear Probability Model	No Physical Limitation	Health Status Good or Excellent	Hospital-ization	Have Usual Source of Care
Outcome Mean	71.8%	49.2%	15.9%	75.6%
NP Deregulation (ppt)	0.56	1.63*	−0.27	0.98
(s.e.)	(0.72)	(0.89)	(0.53)	(0.80)
% Change	2.0%	3.3%	−1.7%	1.3%
Positive N	76,208	53,085	16,947	80,625
N	106,157	107,897	107,897	106,595
Individual, State, & County Controls	X	X	X	X
State and Year FEs	X	X	X	X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights.

Table 1.6: Average Marginal Effect of NP Deregulation on Substitute Prescriptions

	Average Marginal Effects			
	All Other Prescriptions	Other CNS Agents	Other Analgesics	Non-Analgesic CNS Agents
Overall (Logit, GLM w/ Log Link and Gamma Family)				
Outcome Mean	22.16	2.95	1.16	1.79
NP Deregulation (Rx$\geq$ 0)	−0.63	−0.10	−0.02	−0.07
(s.e.)	(0.44)	(0.08)	(0.05)	(0.08)
% Change	−2.9%	−3.4%	−1.7%	−4.0%
Extensive (Logit)				
Outcome Mean	76.9%	35.4%	24.1%	20.6%
NP Deregulation (ppt)	−0.21	−0.64	−0.10	−0.50
(s.e.)	(0.46)	(0.55)	(0.64)	(0.60)
% Change	−0.3%	−1.8%	−0.4%	−2.4%
Intensive (GLM; Positive Users)				
Outcome Mean	28.81	8.33	4.83	8.68
NP Deregulation (Rx$>$ 0)	−0.71	−0.13	−0.05	−0.13
(s.e.)	(0.52)	(0.18)	(0.15)	(0.29)
% Change	−2.5%	−1.6%	−1.1%	−1.5%
Positive N	79,254	36,531	25,427	20,857
N	107,897	107,897	107,897	107,897
Individual, State, & County Controls	X	X	X	X
State & Year FEs	X	X	X	X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights. With the exception of the extensive results, average marginal effects are reported as changes in the number of prescriptions. Extensive results are reported as percentage point changes.

Table 1.7: Average Marginal Effect of NP Deregulation on Clinic Visits

	Average Marginal Effects		
	Total Visits	Office-Based Visits	Outpatient Visits
Overall (Logit, GLM w/ Log Link and Gamma Family)			
Outcome Mean	12.29	11.09	1.20
NP Deregulation	−0.31	−0.44*	0.15**
(s.e.)	(0.26)	(0.24)	(0.07)
% Change	−2.6%	−3.9%	12.7%
Extensive (Logit)			
Outcome Mean	83.7%	83.1%	28.9%
NP Deregulation (ppt)	−0.29	−0.39	0.30
(s.e.)	(0.65)	(0.67)	(0.76)
% Change	−0.3%	−0.5%	1.0%
Intensive (GLM; Positive Visits)			
Outcome Mean	14.68	13.35	4.15
NP Deregulation	−0.34	−0.48*	0.45**
(s.e.)	(0.30)	(0.28)	(0.20)
% Change	−2.3%	−3.6%	10.8%
Positive N	86,445	85,675	27,937
N	107,897	107,897	107,897
Individual, State, & Individual Controls	X	X	X
State & Year FEs	X	X	X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights. With the exception of the extensive results, average marginal effects are reported as changes in the number of visits. Extensive results are reported as percentage point changes.

Table 1.8: Average Marginal Effect of NP Deregulation on Opioid Prescriptions by Sample

	Full Sample		NP Density Sample	
	Outcome Mean	Average Marginal Effect	Outcome Mean	Average Marginal Effect
Overall (Logit, GLM w/ Log Link and Gamma Family)				
NP Deregulation (Rx$\geq$ 0)	0.88	−0.08**	0.92	−0.11***
(s.e.)		(0.03)		(0.04)
% Change		−8.8%		−11.7%
Extensive (Logit)				
NP Deregulation (ppt)	20.4%	−0.23	20.6%	−0.01
(s.e.)		(0.49)		(0.46)
% Change		−1.1%		−0.0%
Intensive (GLM; Positive Users)				
NP Deregulation (Rx$>$ 0)	4.33	−0.29**	4.47	−0.46***
(s.e.)		(0.12)		(0.15)
% Change		−6.8%		−10.3%
Positive N		20,943		17,519
N		107,897		91,275
Individual,State, & County Controls		X		X
State and Year FE		X		X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights. With the exception of the extensive results, average marginal effects are reported as changes in the number of prescriptions. Extensive results are reported as percentage point changes.

Table 1.9: Average Marginal Effect of NP Deregulation with Placebo Years

	Average Marginal Effects							
	Overall		Overall		Extensive		Intensive	
	OLS		Two-Part Model		Logit		GLM (Log,Gamma)	
"Start" Year	Avg.ME.	(Std.Err.)	Avg.ME.	(Std.Err.)	Avg.ME.	(Std.Err.)	Avg.ME.	(Std.Err.)
-4	0.11	(0.08)	0.11	(0.07)	1.47	(1.04)	0.24	(0.27)
-3	0.06	(0.07)	0.03	(0.07)	0.25	(0.74)	0.11	(0.26)
-2	−0.06	(0.04)	−0.04	(0.04)	0.06	(0.65)	−0.19	(0.13)
-1	−0.09*	(0.05)	−0.06	(0.04)	0.05	(0.48)	−0.26	(0.17)
0	−0.09*	(0.05)	−0.08*	(0.03)	−0.23	(0.49)	−0.29**	(0.12)
1	0.00	(0.05)	0.01	(0.04)	0.26	(0.64)	−0.01	(0.13)
2	−0.03	(0.04)	−0.03	(0.04)	0.18	(0.65)	−0.14	(0.16)
3	0.03	(0.05)	0.02	(0.04)	0.03	(0.49)	0.09	(0.16)
4	0.01	(0.04)	0.02	(0.04)	−0.84	(0.58)	0.21*	(0.13)
N	107,897		107,897		107,897		107,897	

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. All results include individual-, state-, and county-level controls as well as state and year fixed effects. All estimates are measured in prescriptions except for the Extensive Logit estimates, which are measured in percentage points.

Table 1.10: Average Marginal Effect of NP Deregulation on Non-Controlled Prescriptions

	Average Marginal Effects		
	Anti-Biotics	Cardio-Vascular Drugs	Other Non-Controlled
Mean number of Prescriptions (All)	1.07	5.62	11.14
Share of Positive Users	36.2%	28.9%	63.3%
Mean number of Prescriptions (Rx> 0)	2.96	19.46	17.60
Two-Part Model (Logit, GLM w/ Log Link and Gamma Family)			
Overall (Logit, GLM)			
NP Deregulation (Rx≥ 0)	−0.05*	−0.12	−0.29
(s.e.)	(0.03)	(0.15)	(0.25)
% Change	−4.8%	−2.1%	−2.6%
Extensive (Logit)			
NP Deregulation (ppt)	−0.52	−0.11	−0.27
(s.e.)	(0.69)	(0.46)	(0.70)
% Change	−1.4%	−0.4%	−0.4%
Intensive (GLM; Positive Users)			
NP Deregulation (Rx> 0)	−0.10	−0.25	−0.36
(s.e.)	(0.06)	(0.33)	(0.34)
% Change	−3.5%	−1.3%	−2.0%
Positive N	36,286	30,035	64,161
N	107,897	107,897	107,897
Individual, State, & County Controls	X	X	X
State & Year FEs	X	X	X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights. With the exception of the extensive results, average marginal effects are reported as changes in the number of prescriptions. Extensive results are reported as percentage point changes.

Table 1.11: Average Marginal Effect of NP Deregulation on Opioid Prescriptions by Practice Authority

	Average Marginal Effects by Type of Practice Authority		
	Board of Nursing & Fully Independent	Board of Nursing & Physician Involved	Joint Boards of Medicine and Nursing
Overall (Logit, GLM w/ Log Link and Gamma Family)			
Outcome Mean	0.91	0.96	0.64
NP Deregulation (Rx≥0)	−0.25***	−0.05	−0.03
(s.e.)	(0.14)	(0.04)	(0.08)
% Change	−27.9%	−5.3%	−5.0%
Extensive (Logit)			
Outcome Mean	21.3%	20.9%	17.5%
NP Deregulation (ppt)	−1.60*	0.12	0.73
(s.e.)	(0.88)	(0.51)	(1.20)
% Change	−7.5%	0.6%	4.1%
Intensive (GLM; Positive Users)			
Outcome Mean	4.27	4.58	3.64
NP Deregulation (Rx> 0)	−0.78***	−0.25	−0.27
(s.e.)	(0.18)	(0.16)	(0.27)
% Change	−18.2%	−5.5%	−7.3%
Positive N	5,556	12,319	3,068
N	26,605	65,813	15,479
Individual, State, & County Controls	X	X	X
State and Year FEs	X	X	X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights.

Table 1.12: Average Marginal Effect of Independent Prescriptive Authority on Opioid Prescriptions

		Average Marginal Effects			
	Outcome Mean	Model 1	Model 2	Model 3	Model 4
Panel A: Overall (OLS)					
NP Deregulation (Rx≥ 0)	0.88	−0.41*	−0.27	−0.43***	−0.42***
(s.e.)		(0.24)	(0.22)	(0.13)	(0.12)
% Change		−46.1%	−31.1%	−48.3%	−47.3%
Panel B: Two-Part Model (Logit, GLM w/ Log Link and Gamma Family)					
Overall (Logit, GLM)					
NP Deregulation (Rx≥ 0)	0.88	−0.38**	−0.08	−0.27**	−0.27**
(s.e.)		(0.18)	(0.20)	(0.12)	(0.11)
% Change		−42.5%	−8.6%	−30.5%	−30.1%
Extensive (Logit)					
NP Deregulation (ppt)	20.4%	−1.01	0.64	−1.11	−0.95
(s.e.)		(2.44)	(2.27)	(1.18)	(0.99)
% Change		−4.9%	3.1%	−5.5%	−4.6%
Intensive (GLM; Positive Users)					
NP Deregulation (Rx> 0)	4.33	−1.63**	−0.43	−0.96**	−0.97**
(s.e.)		(0.74)	(0.75)	(0.45)	(0.45)
% Change		−37.7%	−22.2%	−22.5%	−22.5%
Positive N		20,943	20,943	20,943	20,943
N		107,897	107,897	107,897	107,897
Individual Controls			X	X	X
State Controls				X	X
County Controls					X
State & Year FEs		X	X	X	X

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before NP laws are implemented and are weighted using person-level survey weights.

CHAPTER II

Substituting Higher Education for Medicaid: A Study on the Growth of Entitlements

with Andrew Litten

2.1 Introduction

In this paper, we evaluate the causal relationship between state Medicaid obligations and state subsidies towards higher education. Medicaid and state higher education programs both constitute a large share of state spending. Moreover, these shares have been trending in opposite directions for years (see Figure 2.1 and Table 2.1). On average, Medicaid spending has grown to almost 20% of state budgets in 2010, relative to the 7% of state budgets at the beginning of our sample period in 1977. Over the same time period, higher education subsidies have fallen by almost 5 percentage points to about 10% of state budgets.

There is an intuitive explanation for why this relationship is likely to be causal. Growth in the Medicaid program is driven by technology and federal policy, much of which operates outside the state's policy purview. Funding for higher education, on the other hand, is inherently a state decision. As a result, increases in Medicaid obligations, driven by external factors, will require states to find a way to offset the new spending requirements elsewhere. States are likely to respond to this pressure through reduced higher education subsidies. Unlike other major categories of state spending, such as K-12 education or law enforcement, higher education is funded through both public and private funds. As a result, states can cut funding to higher education expecting that institutions will seek higher tuition, private donations, or federal research grants to mitigate any adverse effects.

We find evidence consistent with this framework. In our preferred specification, we find that a $1 shock to Medicaid spending reduces higher education subsidies by 20 to 37 cents. The effect on other state outlays appears to be less strong. The effects of Medicaid are

robust to different model specifications, and as we argue below, we interpret these estimates as causal. In addition, we show that the response in higher education spending is larger than other categories of spending through which the state might respond, such as K-12 education, transportation (roads, airports, trains), and justice (prisons and police).

One challenge in estimating the main effect in this paper is that there are many possible mechanisms through which Medicaid spending and higher education subsidies may be endogenous at the state level. Crucially, both categories of spending depend on overall state generosity. If more generous states are interested in spending more on both health care and education, the effect that Medicaid costs have on higher education spending will be understated in magnitude. Further complicating the matter is that states may become more or less generous over time.

To overcome these limitations in our analysis, we exploit *federal* variation in Medicaid policy. In particular, we use changes in federally administered Supplemental Security Income (SSI) policy to instrument for Medicaid spending. As an instrumental variable, changes to SSI policy have several attractive features. First, federal statute dictates that SSI enrollment automatically triggers Medicaid enrollment in most states. Second, SSI enrollment is administered by local Social Security offices and are unaffiliated with state authorities. Third, SSI enrollees are disproportionately more expensive than other Medicaid enrollees, which implies a strong link between SSI enrollment and Medicaid spending. Lastly, there have been numerous changes to federal SSI policy over the past four decades, which give us the variation we need to identify changes in Medicaid spending.

The estimates above are similar to but smaller in magnitude than previous ones found in Kane and Orszag (2003). In their work, the authors find that a dollar of Medicaid spending crowds out 30 to 50 cents of higher education spending. Kane and Orszag rely on two instruments to predict state Medicaid spending—the share of the population over age 65 and the share of the population below the federal poverty line (FPL). While these two variables reliably predict total Medicaid spending, it is not obvious that they meet the exclusion restriction assumption necessary for an instrumental variable. That is, there are many other ways age and poverty might affect state budgets other than through Medicaid.

In sum, our paper offers several important contributions to the existing literature. By digitizing expenditure and enrollment data from federal reports, we are able to identify subgroup-level changes in Medicaid spending by state. We then use SSI enrollment as an instrumental variable to correct for endogeneity problems not previously addressed by demography-based instruments and fixed effects. Next, we improve on the SSI enrollment instrument by interacting it with a technological adjustment factor. This methodological improvement allows us to estimate more precise results and to account for state-specific

technological changes in Medicaid policy. Finally, we analyze heterogeneity in state responsiveness to new Medicaid obligations.

2.2 Background

In this section, we discuss the institutional background of Medicaid, higher education, and state budgets with ultimate goals of motivating the causal relationship and the instrumental variable used to identify it. In particular, we describe trends in Medicaid and noteworthy federal policy changes that influence these trends. We then discuss the role for state flexibility in Medicaid generosity and differences in the costs of the Medicaid program at the state level. Finally, we discuss state fiscal issues, particularly those relating to the interplay between Medicaid and higher education spending.

2.2.1 Trends in Medicaid Enrollment and Expenditures

Medicaid began in 1965 with a simple directive: to replace two federally administered medical care programs for the poor, one for welfare recipients (low-income families, blind, and disabled) and one for low-income aged. Eligibility was restricted to these four groups, and the covered benefits were limited in scope. By 1975, every state but Arizona had adopted Medicaid. Also in 1975, the program enrolled 22 million beneficiaries across the nation, and national expenditures were $49.0 billion in 2010 dollars, or less than 1% of GDP.

Since that time, both enrollment and expenditures have grown considerably. By 2010, Medicaid enrollment tripled to 66 million beneficiaries, and expenditures rose even more to $339.0 billion in 2010 dollars, or 2.3% of GDP. At this pace, Medicaid easily qualifies as the fastest growing of the three largest mandatory entitlement programs (a list which also includes Social Security and Medicare). According to the National Health Expenditures published by the Center for Medicare & Medicaid Services, Medicaid expenditures have grown from 68.8% of Medicare expenditures in 1970 to be 76.5% in 2010, and after the Affordable Care Act was implemented, Medicaid expenditures grew to 84.4% of Medicare. A similar comparison to Social Security expenditures reveals that Medicaid grew from 17.5% in 1970 to 56.2% in 2010.

The Medicaid and CHIP Payment and Access Commission (MACPAC) describes this trend, along with its root causes, in their June 2016 report. It determined that 70.7% of the growth in real Medicaid benefit spending is due to new enrollment, while the remaining 29.3% is due to growth in spending per enrollee. Moreover, MACPAC decomposed the trend in spending by the four major eligibility classifications: aged, disabled, children, and adults. From this decomposition, we learn that approximately half of the growth in spending is

attributed to trends among disabled enrollees. The remaining growth is attributed roughly evenly across the other three eligibility groups.

There are at least two characteristics about Medicaid that led to this type of growth in spending. First, Medicaid is uniquely structured as a joint federal and state entitlement program. Second, because growth in spending is driven by new enrollment, much of the change in Medicaid spending is the result of new eligibility requirements dictated at the federal level. We discuss the role these characteristics played in greater detail below.

2.2.1.1 Medicaid's Federal-State Partnership

Unlike Social Security and Medicare, Medicaid is a joint federal-state program. While the Federal Government establishes mandated minimum guidelines for eligibility, benefits, and cost-sharing (insurance premiums and co-pays), each state administers its own Medicaid program. In addition, states can seek exemptions to certain guidelines through the federal waiver program. In practice, these waivers tend to expand eligibility and benefits. Moreover, as coverage for particular groups gains critical mass, several coverage expansions are eventually adopted by the federal government, leading to increases in enrollment and expenditures for all states [Currie and Gruber (1996), Cutler and Gruber (1996), Klemm (2000)].

Funds for Medicaid come from general state and federal revenue, and federal funds are delivered as matching grants to the states without a federal spending cap. The size of the grant is determined using a formula called the Federal Medicaid Assistance Percentage (FMAP), which is described in Section 2.3.

The nature of funding through matching grants has the potential to distort state spending priorities [Baicker (2005)]. As an example, consider a state with an FMAP of 60%. For each dollar that the state spends, it will receive 60 cents. As such, state policymakers can promise $1.60 worth of health care at the cost of $1.00, or they can promise $1.00 worth of something else—like higher education subsidies—at the cost of $1.00. As a result, state policymakers will face a stronger incentive to spend dollars on Medicaid over higher education.

2.2.1.2 Federal Eligibility and Benefits Changes to Medicaid and Supplemental Security Income (SSI)

Most federal expansions in Medicaid eligibility grew coverage for children and adults. By 1988, all pregnant women and young children with incomes below the federal poverty line (FPL) were eligible for Medicaid. By 1989, children under age 6 at or below 133% of FPL were eligible, and by 1990, children ages 6 to 18 at or below 100% of FPL were eligible. Lastly, the Balanced Budget Act of 1997 (BBA97) created the Children's Health Insurance

Program (CHIP), which mandated coverage assistance for children under 200% of FPL.

Because of the number and breadth of these expansions, children and adults represent a growing proportion of Medicaid enrollees—from 64.2% in 1975 to 70.5% in 2010; however, spending per enrollee for these two groups is consistently low relative to the aged and disabled. As such, new enrollment among these groups has contributed only 30.4% of long-run spending growth. By contrast, new enrollment among the disabled comprises 37.6% of long-run spending growth even though its share of enrollment grew by half that of children and adults [MACPAC (2016)].

Moreover, the growth in new enrollment among the disabled is potentially outside of state control. Supplemental Security Income (SSI) is by far the most common pathway to Medicaid enrollment for the disabled, and it is an exclusively federal program administered by the Social Security Administration (SSA). It is designed as a cash assistance program for the aged and disabled, and as an added benefit, SSI enrollees receive health coverage through Medicaid. For most states (41 and the District of Columbia), SSI enrollment automatically triggers either Medicaid enrollment or Medicaid eligibility. The other nine states use more restrictive SSI eligibility criteria, but this distinction does not affect our results (see Footnote 4). Thus, Medicaid enrollment for SSI recipients is largely a federal process, and because the disability determination process for SSI eligibility is both fully funded and mandated by the SSA, SSI eligibility is also a federal process.

To see this, consider the steps associated with determining SSI eligibility [Wixon and Strand (2013); Duggan, Kearney and Rennane (2016)]. For initial determination, local SSA field offices must first determine applicants' income. Applicants who earn below the "substantial gainful activity" amount ($1,170 per month in 2017) proceed to the federally funded state Disability Determination Service for medical eligibility assessment. There, applicants' impairments are evaluated for severity and whether they limit applicants from working. While these steps seem local and potentially influenced by state policy, the existence of an appeals process allows these local administrators to be more conservative. Only about a quarter of applicants are accepted during the initial determination [Wixon and Strand (2013)], while the overall award rate tends to be closer to 50%. In practice, these cases tend to be inframarginal. They are the most straightforward cases of disability and therefore should be similarly accepted no matter the state of residence.

The applicants who are rejected after the initial determination include the marginal cases, which are both less straightforward cases and likely to appeal, and the inframarginal cases, which are obvious rejections and may never appeal. Those cases that appeal first do so with the state Disability Determination Service, and then, if unsuccessful, they can appeal to administrative law judges and higher federal courts.

French and Song (2014) use Social Security Disability Insurance (SSDI) application data to argue that the majority of accepted appeals are successful at the administrative law judge level and above and that assignment to these judges is idiosyncratic. While SSI and SSDI are different programs, they use the same administrators to assess medical eligibility and handle appeals. Thus, if we extend these results to the SSI population, we can infer that the marginal cases that appeal are more likely to succeed after a hearing with an administrative law judge. In addition, administrative law judges and the federal courts have jurisdictions that can—and often do—extend beyond state borders. For instance, both the Milwaukee and Toledo hearing offices serve counties in Michigan.

Thus, much of marginal SSI-related Medicaid spending should be considered outside of state control, idiosyncratic, and more exposed to national changes in eligibility rules over time. Although numerous changes have occurred to SSI since its inception in 1972, the two most prominent changes to eligibility are the redetermination of disability-related eligibility criteria beginning in 1985 and the contractionary reforms beginning in 1995 [Autor and Duggan (2003)].

Amid a public outcry about draconian disability determination rules during the early years of SSI, the Federal Government enacted the Social Security Disability Reform Act of 1984 [Rupp and Scott (1998), Stapleton et al. (1998), Autor and Duggan (2003)]. Among other things, this act required that the SSA establish new standards for qualifying disabilities, especially for mental illness. In 1985, the SSA published new rules, which were much more inclusive than previous guidelines, particularly for mental illness. In addition, AIDS was listed as an impairment for the first time in February 1985. As a result of these revised guidelines, enrollment accelerated, especially for non-aged, mentally ill adults whose numbers grew almost 50.0% from 605,900 in 1986 to 886,400 in 1990 and continued to increase to 1.5 million in 1995 (see Figure B1).

In response to concerns about this dramatic growth, the Federal Government implemented several reforms in the mid-1990s to slow SSI enrollment. These reforms were implemented from 1995 to 1998 and enforced disability reviews of existing enrollees, removed drug addiction and alcoholism as listed impairments, and tightened SSI eligibility guidelines for children.

These measures were largely successful at halting SSI enrollment growth through the remainder of the 1990s, but they did not change the underlying structure for how disabilities were assessed, especially for marginal cases like mental illness. As a result, these reforms were inevitably short-lived. Without continued pressure to downsize, SSI enrollment continued to increase from 1999 onwards. From 1999 to 2010, SSI enrollment increased from 5.8 million to 7.1 million (see Figure 2.3). Moreover, this increase occurred despite the continued decline

in SSI enrollment among the aged. As of 2010, they represent only 1.2 million of the 7.1 million enrollees, whereas enrollees with mental disorders total 3.4 million, or nearly half of all enrollees.

2.2.2 Medicaid at the State Level

There are two key ways in which states can acquire flexibility in their Medicaid programs. They can opt not to participate, or they can expand above federally dictated core requirements. Other than Arizona, which began in 1982 and has been dropped from our sample, all states participate in the Medicaid program. Therefore, we focus this discussion on the options available to states for expanding above the federally dictated core requirements. Of these options, there are two main methods for achieving expansion: medically needy programs and Medicaid waivers.

As long as they meet the federal core requirements for Medicaid, states can choose the generosity of their Medicaid program. To begin with, state participation is voluntary, and it was not until 1982 that Arizona joined the Medicaid program. More recently, 19 states have refused to expand Medicaid as part of the Affordable Care Act. Even if the states decide to participate, they can still provide differing levels of care through the medically needy program and Medicaid waivers.

As part of the original 1965 legislation, states hold the option to create medically needy programs. These programs allow individuals with significant medical expenses to qualify for Medicaid. In addition to being able to choose whether the state has a program, states can also choose which individuals can qualify as medically needy. For instance, they can cover aged enrollees under this program but not the disabled. As of 2017, 32 states and the District of Columbia have medically needy programs in place.

The 1965 legislation also created Section 1115 Demonstration waivers. These waivers are broad in scope and can be used to expand coverage, add benefits, change delivery systems, or reduce costs. They must be approved by the Department of Health and Human Services and must still guarantee the federal core requirements. They are typically approved for five years with a three-year extension. They must also be budget neutral for the federal government. As of 2017, there are 42 Section 1115 Demonstration waivers over 29 states and the District of Columbia.

The two other major waiver programs are Section 1915(b) and Section 1915(c), which were created as part of the Omnibus Budget and Reconciliation Act of 1981 to reduce costs per enrollee. The Section 1915(b) waiver program allows states to mandate managed care enrollment for Medicaid recipients. The Section 1915(c) waiver program allows states to offer long-term care services through home- and community-based services (HCBS) rather

than through more traditional institutional care. As of 2017, there are 64 Section 1915(b) waivers across 34 states, and there are 302 Section 1915(c) waivers across 47 states and the District of Columbia.

Because of the flexibility that the medically needy program and waivers provide, Medicaid spending per enrollee varies considerably from state to state. In addition, as waivers become available and more popular, the variation in spending per enrollee has increased. In 1977, the standard deviation of spending per enrollee was just $68. By 2010, it had increased to $5,644.

2.2.3 State Finances and Higher Education

State governments typically spend their resources on a small handful of major expenditure categories. A summary of state and local fiscal priorities is included in Table 2.1 below. State fiscal priorities are primarily in education (30%), welfare, and other expenditures such as highways and prisons. Within education, 15% is spent on higher education, versus 85% spent on K-12. Medicaid represents 77% of welfare spending.

According to the National Conference of State Legislatures, every state but Vermont has either a constitutional provision or a statutory requirement requiring a balanced budget [NCSL (2010)]. Given this fact, an increase in mandated benefits mechanically requires an offsetting increase in revenues or decrease in another type of expense. Because state revenues likely affect a broader base and are sensitive to economic conditions, we expect policymakers to prefer reductions in spending over increases in revenue.

We focus on higher education subsidies as the primary target for reductions in spending. Institutionally, only a portion of higher education is funded from state budgets. Higher education institutions collect revenue from the Federal Government in the form of research grants, from students in the form of tuition, and from alumni in the form of donations. In addition, institutions of higher education have some flexible control over their spending, by, for example, taking fewer students in a given year.

Previous empirical works highlight this institutional responsiveness to financial shocks. Kane and Orszag (2003) and Bell (2008) both describe the empirical relationship between higher education spending and short term fluctuations in the business cycle. Bound et al. (2016) show how higher education programs respond to short term cuts in state subsidies by raising revenue from out of state students, especially international students.

Our paper adopts a slightly different approach. We assume state legislatures understand the flexibility that higher education institutions possess, and as such, they choose to cut higher education subsidies when faced with long-term growth in Medicaid spending. This assumption seems relatively benign when we compare higher education to other line-items in

the state budget (see Table 2.1). Prison expenses, for example, are determined by sentencing guidelines and a judiciary which has independence from the legislature. Transportation spending is often directly earmarked from specific revenue sources such as state gas taxes and, as such, cannot be changed easily. Lastly, K-12 education spending is calculated by a pre-determined formula, and K-12 schools serve a broader base of students than higher education. Moreover, because K-12 schools rely more heavily on state budgets as their primary source of funds, cuts will feel more severe and affect more families than cuts to higher education.

States routinely have to deal with volatility based on the business cycle, and predicting the exact revenue and spending needs over a long period of time is impractical. In addition, state balanced budget requirements mandate that legislators be responsive to this volatility. As such, all states–including the biennial states–rely on supplemental budget amendments to smooth out differences. In his discussion of tax revenue volatility, Seegert (2012) argues that revenue volatility implies expenditure volatility, and shows that compensating responses to shocks are often contemporaneous. In Section 2.6.4, we explore the lag structure of the relationship between increased Medicaid obligations and other expenses in greater detail. We also highlight what contemporaneous means in the context of our data in Section 2.3.5.

2.3 Data

In this section we describe the data used in the final analysis. The majority of our analysis studies the period 1977 to 2010, which represents the intersection of time for available Medicaid, SSI, and state budget data. All data are collected at the state level for each fiscal year. First, we describe the Medicaid data, which are used as the main treatment variables. Then, we describe the SSI data which are used to construct the main instrumental variable. Next, we describe the FMAP formula, which is used to calculate each state's share of Medicaid spending. Lastly, we describe the state higher education, demographic, and financial data.

2.3.1 Medicaid Enrollment and Expenditures

Medicaid enrollment and expenditure data were collected at the state level for each fiscal year from 1975 to 2010. These data came from two sources: the Medicaid Statistical Information System (MSIS) and its predecessor report known as the Health Care Financing Administration-2082 (HCFA-2082). MSIS data were collected for FY1999 to FY2010 and are available online from the Center for Medicare and Medicaid Services. HCFA-2082 data were collected from FY1975 to FY1998. Unlike the MSIS data, these data required independent

collection and digitization.

Among all of the Medicaid data collected, our analysis focuses on state-level enrollment and expenditures in total and by reason of eligibility. These reasons consistently classify into four groups: the aged, low-income children, low-income adults (typically the parents of the eligible children), and the blind and disabled.

Expenditure data are reported in nominal dollars. Because our preferred specification uses Medicaid expenditures as a share of the state personal income, we seldom need to adjust for inflation. But, on the few occasions when we compare expenditures across periods, we use the Bureau of Labor Statistics's Consumer Price Index-Urban (CPI-U) to normalize expenditures to 2010 dollars for all periods.

There are a few caveats when using these data. First, following the Balanced Budget Act of 1997 (BBA97) and the creation of CHIP, Medicaid enrollment and expenditure values include enrollment and expenditures for CHIP enrollees even if the state operates a stand-alone CHIP that is separate from Medicaid. Because CHIP expenditures are reimbursed at a higher match rate, we will overstate the state share of Medicaid costs from FY1998 onward. As an empirical note, this error will be minor given that CHIP expenditures relative to the overall size of Medicaid are less than three percent. Also, Arizona did not enter Medicaid until 1982, and it did not begin reporting MSIS data until 1991. As a result, we exclude Arizona from our analysis. Also, because the District of Columbia has less autonomy than regular states, we omit it from our analysis as well.

2.3.2 Supplemental Security Income (SSI)

SSI data were collected from the SSA's annual statistical supplement for the years 1975 to 2010. These reports provide state-level enrollment as of December for each year. In addition, enrollment is reported in total as well as by reason of eligibility: aged, blind, and disabled. Because the policy for aged eligibility has never changed, changes in aged enrollment over time only reflect changes in state demographics and economic conditions, which we control for in our estimation. Thus, to target the variation that is most closely tied to federal policy variation, we use blind and disabled SSI enrollment as an instrumental variable for Medicaid expenditures.

2.3.3 Federal Medicaid Assistance Percentage (FMAP)

Because Medicaid data report the total expenditures made by both federal and state governments, we require data on the FMAP to isolate the state-funded portion of Medicaid expenditures. The FMAP is calculated using the below formula for state i, and it is designed

to direct more funds to poorer states.

$$FMAP_i = \max\left\{50, \min\left\{\left[100 - \left(\frac{(\text{State per capita income}_i)^2}{(\text{National per capita income})^2} * 45\right)\right], 83\right\}\right\} \quad (2.1)$$

Note that the FMAP is bounded such that federal funds do not exceed 83 cents for each state dollar spent and do not fall below 50 cents for each state dollar spent. Per capita income is calculated as the three-year average used prior to announcement. Because of lags in data collection and reporting and the need to announce the FMAP rates in advance of each fiscal year, the three years chosen are the third, fourth, and fifth years before the fiscal year of interest. For example, per capita income for the FY2012 FMAP formula is calculated as the average over CY2007, CY2008, and CY2009. Calculated FMAP rates are reported each year in the Federal Register, and they have been collected and reported by the Office of the Assistant Secretary for Planning and Evaluation (ASPE), which is a part of the Department of Health and Human Services (HHS).

These rates, as reported, are the baseline rates only and have not been modified for legislative adjustments, including four major adjustments. First, as part of the Omnibus Budget and Reconciliation Act of 1981 (OBRA81), the Federal Government cut the FMAP rate across all states by 3.0ppt, 4.0ppt, and 4.5ppt for fiscal years 1982, 1983, and 1984, respectively. The one exception is that the statutory floor of 50.0% matching was retained, meaning that a state with a 50.0% FMAP and a state with a 52.0% FMAP would both receive 50.0% matching in these three years. Second, BBA97 created CHIP, and for any spending on CHIP-eligible children, the Federal Government paid an enhanced FMAP ranging between 65.0% and 88.1%. Third, as part of the Bush stimulus known as the Jobs and Growth Tax Relief Reconciliation Act of 2003 (TRRA), the FMAP temporarily increased from April 2003 to June 2004. Most states' FMAPs increased by 2.95ppt over this period, and 21 states received slightly more than that. Lastly, the American Recovery and Reinvestment Act of 2009 (ARRA) temporarily increased the FMAP rate from October 2008 to June 2011 by 6.2ppt plus an additional unemployment-related increase. In practice, the average state's FMAP increased by 10ppt, and the range was between 6.2ppt and 17.87ppt.

To more accurately represent the proportion of state funding, we modify the baseline FMAP rates to reflect all these adjustments except for the CHIP adjustments. To properly adjust for CHIP enhanced rates, we need additional information on CHIP-related expenses, which are not reported separately in the Medicaid data. A detailed distribution of adjusted FMAP rates by state are shown in Figure D1. Each point represents the FMAP rate for a particular state in a given year.

2.3.4 Education, Demographics, and Finance

Education data are take from the Center for Education Policy Study at Illinois State University ("Grapevine"). Grapevine data were chosen over other data sets such as the Delta Cost database ("Delta") due to the length of availability. Whereas Delta only exists for the period following 1986, Grapevine data extend back to 1977. Data included are the direct subsidies of the higher education system coming from the state budget. That is, this excludes items such as tuition and research grants.[1]

In addition, we take state demographic information from the annual March Current Population Survey. This includes average state age, share of the population over 65, share of the population under 24, and state-level race and ethnicity. Information of state per-capita income and state-level program spending are taken from the Bureau of Economic Analysis.

2.3.5 Timing of the Data

Because the data are collected from multiple different sources, there is some discrepancy regarding when data are reported and what periods they correspond to. Medicaid and education data are collected annually for the federal fiscal year, which begins on October 1, while SSI rolls are an enrollment snapshot reported on December of each calendar year. We match each year of SSI data with the following year of Medicaid and education data. For example, SSI enrollment as of December 1999 is matched with FY2000 Medicaid and education data, which spans from October 1, 1999 to September 30, 2000. Because SSI changes are measured during the first quarter of the annual Medicaid and education, there are at least nine months of expenditures that occur after SSI changes but appear contemporaneously in our data.

2.4 Methods

In this section we describe the main analysis used in this paper. First, we discuss the OLS model and the challenges in causally interpreting the results from this specification. Next, we discuss a possible solution to the identification problem in the OLS model. We then describe the preferred two-staged least squares specification. In Section 2.6 below, we discuss robustness tests and extensions. Overall, we find the results to be robust to a variety of model assumptions.

[1] We take these measures to capture the consumption value of higher education, either at the personal level (tuition) or transactionally at the state-level (research funding). Limiting the analysis to subsidies more directly captures the public investment of spending.

2.4.1 The Key Causal Relationship

We are interested in the long-run causal relationship between total Medicaid spending at the state level and total state subsidies for higher education. In the following equation for state s and year t:

$$Y_{st} = \gamma Medicaid_{st} + \beta X_{st} + \theta_s + \delta_{st} + \phi_t + \varepsilon_{st} \tag{2.2}$$

The outcome variable, Y_{st}, and the explanatory variable of interest, $Medicaid_{st}$, represent the share of state income spent on higher education and on Medicaid, respectively.[2] Because Medicaid is a federal-state partnership, we measure only the share of total spending for which the state is responsible.[3] Higher education subsidies are measured as the total state grants towards institutions of higher education. Costs borne by the users of higher education, such as tuition, are not included in this measure.

X_{st} represents a vector of state-year characteristics likely to be correlated with education spending. These characteristics include each state's share of the population by race and age, mean age, and business cycle environment, which is measured by the lagged state unemployment rate. We chose these covariates over other candidates, such as total income or education shares that could plausibly be interpreted as an outcome. The measures θ_s and δ_{st} capture the levels and trends in unobserved state characteristics, respectively. Depending on the specification, we also include policy period fixed effects (ϕ_t), which are designed to control for any short-run variation caused by sudden changes in federal policy. Finally, ε_{st} represents the remaining unexplained state-year variation in higher education spending.

Under specific conditions, ($E[\varepsilon_{st}|Medicaid_{st}] = 0$), the parameter γ identifies the causal relationship between the additional share of state income spent on Medicaid spending, but Medicaid spending is likely correlated with state preferences for higher education spending and thus violates the exclusion condition. First, states with more generous attitudes towards social spending may be likely to spend more on both Medicaid and higher education, which would bias γ upward. Moreover, because states have some flexibility in Medicaid spending through waiver programs, a decision to adjust higher education subsidies could affect Medicaid spending and not the other way around. Thus, a causal interpretation of γ will require variation in Medicaid which is not attributable to state voter preferences or state policy consideration.

[2] Because incomes are growing over time, in both a nominal and per capita sense both Medicaid and higher education subsidies are also growing over time, albeit at different rates. Using income shares properly captures the displacement effect on the state's consumption bundle.

[3] Specifically, the total reimbursements for Medicaid patients in the state times one minus the state's federal matching rate for that year.

One straightforward solution to this threat is to include state fixed effects and state-specific trends, which should control for average state preferences over our sample period and linear growth in state spending over time. Including these measures, however, still does not address all possible concerns regarding endogeneity. Crucially, because we are looking at long-run effects over several decades, state preferences are not guaranteed to be constant for the entire observation period, nor to change in a monotonic, linear manner. For example, if a state's preference for social safety nets grows exponentially relative to Medicaid growth—possibly as a critical mass of voters enroll—we would still expect Equation 2.2 to downwardly bias the absolute magnitude of γ. Alternatively, state preferences for generosity could contract as more enrollment in Medicaid increases salience and antipathy for redistributive programs. This type of variation would increase the magnitude of γ.

Previous efforts to address this problem [Kane and Orszag (2003)] have used demographic-driven variation among the poor and the aged. This approach is at best limited for at least two reasons. First, the aged and poor can have a variety of unobservable policy preferences which affect higher education through channels other than Medicaid. To suggest a few examples, the aged may be less inclined to subsidize higher education, desire lower taxes, or require other social safety net programs that crowd out higher education spending. Without controlling for them, these preferences will be incorrectly attributed to changes in Medicaid spending caused by changing demographics and will bias γ downward. Moreover, changes in these preferences over time may exacerbate the bias, and these changes are likely to occur as the composition of who is poor and aged changes over our 34-year sample period. Second, the poor and the aged can migrate to states because of state higher education policy, which introduces simultaneity concerns.

2.4.2 The Identifying Variation

As an alternative, we use variation in Medicaid spending driven by long-run changes in per capita blind and disabled Supplemental Security Income enrollment (henceforth SSI enrollment). Compared to demographics, SSI enrollment has a number of advantageous features. First, Section 2.2 indicates that SSI enrollment is a primary driver of Medicaid spending over the past four decades. Second, as part of the Social Security Administration, an exclusively federal program, changes in SSI policy are inherently federal. Moreover, because it is federally administered and funded, changes in state policy and state voter preference will not directly affect changes in SSI policy. The combination of these factors make SSI both a strong and plausibly exogenous instrument.

While these qualities make SSI enrollment more attractive than previously studied alternatives, there remains some concern that states could control SSI enrollment by adjusting

other programs that are substitutable with SSI. Bound, Kossoudji and Ricart-Moes (1998) and Schmidt and Sevak (2004) find that tightening state welfare policy leads to an increase in SSI enrollment. Black, Daniel and Sanders (2002) and Charles, Li and Stephens (Forthcoming) show that improving local economic conditions reduce SSI enrollment. These findings indicate that states may be able to manipulate SSI enrollment through state-level welfare and economic policies.

While we concede this is a valid concern, it is only a threat to our specification if these policies are implemented in a way that also affects education spending, conditional on covariates and fixed effects, and thus introduces simultaneity bias. The type of variation required to bias estimates seems unlikely.

A more plausible alternative for the source of variation in our paper is that long-run federal changes in policy are implemented differently at the state-level because of differences in demographic and economic factors and in the disability determination process. Because we are controlling for demographic and economic factors both through covariates and state-specific trends, it seems likely that much of the variation we are using is due to differences in the disability determination process. Maestas, Mullen and Strand (2013) use variation in the stringency of local disability examiners to identify the effects of Social Security Disability Insurance (SSDI) receipt on labor force participation. French and Song (2014) perform a similar analysis but instead used variation in the stringency of administrative law judges. Both studies argue that the variation is idiosyncratic in how applicants are assigned and that it is substantial enough to yield strong first stage results.

Our approach is similar to these two studies in that we use variation in the disability determination process; however, it differs in several ways. First, our study evaluates SSI instead of SSDI. As mentioned in Section 2.2, SSDI differs from SSI in terms of the types of benefits received, but the disability determination process is identical. Second, while they explicitly use this variation to identify changes in disability receipt, we rely on changes in federal SSI policy interacted with this variation to identify changes in Medicaid spending. Lastly, our approach ignores the distinction between variation derived from disability examiners versus that derived from administrative law judges. Rather, we simply acknowledge variation exists from both sources and exploit it to estimate changes in Medicaid spending caused by long-run federal changes in SSI policy.

Empirically, we can evaluate how this residual variation changes over time. Figure 2.4 illustrates the variation in SSI enrollment over each year in our sample period after adjusting for controls, state fixed effects, and linear state trends. Even after these adjustments, there remains considerable variation across states and over time. The average range of residual state SSI enrollment is 470 enrollees per 100,000 people, which we view as sizable given that

the national rate of SSI enrollment is about 2,500 enrollees per 100,000 people in 2017. For a given year, the range of residual state SSI enrollment can be as small as 242 enrollees per 100,000 people in 2004 and as large as 923 enrollees per 100,000 people in 1995.

Figure 2.4 also reveals that the years with the highest range in residual SSI enrollment are concentrated in the 1990s and are driven by a handful of outlier states, namely Kentucky, Louisiana, Mississippi, and West Virginia. We evaluate how these deviations affect results in Section 2.6.

2.4.3 The Causal 2SLS Model

The preferred causal model for this analysis is the two-stage least squares (2SLS) instrumental variable model shown below for state s and year t:

$$Medicaid_{st} = \omega SSI_{st} + \alpha X_{st} + \rho_s + \pi_{st} + \phi_t + \nu_{st} \tag{2.3}$$

$$Y_{st} = \gamma \widehat{Medicaid}_{st} + \beta X_{st} + \theta_s + \delta_{st} + \Phi_t + \varepsilon_{st} \tag{2.4}$$

In this model, the key causal parameter of interest is still γ, which now measures the change to state higher education subsidies caused by changes in SSI-related Medicaid spending. The terms ρ_s and θ_s are state-level fixed effects, and π_{st} and δ_{st} are linear state trends. ϕ_t and Φ_t are policy period fixed effects, which are motivated in Section 2.6.3. X_{st} represents the vector of state-year characteristics described for Equation 2.2 above. As with Equation 2.2 both Medicaid and higher education spending are normalized as a share of total personal income for the state, whereas SSI enrollments are normalized as a share of total population.[4]

Given the direct link between SSI and Medicaid enrollment and the relative expense of disabled enrollees, we see a strong positive relationship between the instrumental variable (SSI enrollment) and the endogenous regressor (total Medicaid spending). Figure 2.5 visualizes this relationship using the specification outlined in Equation 2.3. The effect of SSI enrollments is economically significant, with a 1ppt increase in percent of the population enrolled yielding a 0.2ppt increase in total personal income in the state spent on Medicaid. The relationship does not appear to be driven by outliers. More conventionally, Table 2.2

[4] Note that as discussed in Section 2.2, states do not universally accept federal SSI guidelines as the basis of SSI enrollment eligibility. In order to address the potential confounding effect of these policy differences, we also run an additional specification which interacts the SSI enrollment instrument with a binary indicator for the nine states which do not conform to these guidelines. This allows us to have two instruments, and in theory would capture any systemic difference between Medicaid costs from conforming and non-confirming states. In practice, including this additional instrument makes no difference, suggesting that non-conforming states are similar enough to conforming states, on average, as to not make this additional step necessary.

shows that the F-statistic ranges from 11 to 117. Our preferred specification with state fixed effects and linear state trends has an F-statistic of 43.

2.5 Results

Table 2.3 reports the OLS and 2SLS estimates of the effects of state Medicaid obligations for three sets of regression specifications. The first set (Columns 1 and 2) has no fixed effects. Column 1 includes only lagged state unemployment, to control for business cycle effects on state budgets. Column 2 adds state demographic controls, including race and age. The second set (Columns 3 and 4) repeat Columns 1 and 2, but with the addition of state fixed effects. Including state fixed effects controls for differences in state higher education spending which are fixed between states. This might include state political preferences or generosity towards welfare systems. The third set (Columns 5 and 6) repeats Columns 3 and 4 and adds state specific linear time trends. These trends control for changes in a given state over time, such as a differential state trend in demographics or wealth.

The results across model choices generally behave as anticipated. Adding state fixed effects increases the (negative) coefficient in absolute magnitude for both OLS and 2SLS. In addition, we find that the 2SLS estimates are consistently larger than the OLS estimates. Both of these results are consistent with our expectation that state generosity and preferences for public spending are a significant confounder. Importantly, in every model with fixed effects (Columns 3-6), the 2SLS estimates are larger in absolute magnitude than the OLS estimates, suggesting that these preferences are not simply constant over time.

The estimates range from a \$0.09 to \$0.37 reduction in higher education spending for each dollar spent on Medicaid. These estimates are somewhat lower than those used by Kane and Orszag, which range from \$0.37 to \$0.53. As part of this analysis, we also replicated the Kane and Orszag instrument using our specification and panel, and found results comparable to those reported in their paper. Overall, we argue that our lower point estimates reflect that we are better isolating the variation in Medicaid spending and not picking up other preferences driven by age or economic conditions, which may have contaminated the instrument in the Kane and Orszag paper.

Our preferred specification is the 2SLS model, including state fixed effects and state specific linear trends. In this model, every additional dollar of additional state Medicaid obligations lowers state grants for higher education by \$0.33. Overall, we find that the inclusion of state-specific linear trends reduce the effects of demographic controls. This result indicates that these controls are likely changing linearly and slowly enough to be controlled for with trends. Including state-specific linear trends also allows us to control for

linear unobservable trends that could potentially confound estimates.

In Table 2.4, we repeat the analysis but allow other state expenditures to take the place of higher education subsidies. Specifically, we look at "justice" (police, courts, prisons), "transportation" (primarily roads, but also trains and airports), and "K-12 education." Looking at these other major spending categories allows us to see why higher education is a uniquely flexible component of state budgets, and thus the preferred outcome of interest. Note, for Table 2.4, we include "Policy Period Fixed Effects," which are described in the Section 2.6.3.

Within each of the tested spending categories, we fail to replicate the same powerful economic and statistical relationship with Medicaid that persists for higher education. Justice spending is a precisely estimated zero—we can say with confidence that this does not respond to higher Medicaid costs. K-12 education spending is near zero but imprecisely estimated. Transportation is the only category that produces a large point estimate. The preferred specification suggests that a dollar of Medicaid spending reduces transportation spending by 18 cents. As with K-12 spending, this estimate is unfortunately lacking the precision to draw inference.

Each of these types of spending behaves differently than higher education because of the difference in institutions and political economy. Justice spending is driven by factors such as crime rates and convictions, over which the state has limited short term power. Unlike higher education, K-12 education is universal and, thereby, more politically sensitive. The size of and large standard error for the transportation estimate is likely a result of heterogeneity in state institutions. Some states have a separate fund for transportation spending, which is determined by gas tax revenues. Other states rely more on the general fund for highways, and as a result are more likely to respond to increased Medicaid costs.

2.6 Robustness and Extentions

In the sections below, we will discuss the robustness of these estimates, with regards to both instrument selection and model uncertainty. Finally, we discuss the heterogeneity in results by state subgroup.

2.6.1 Technological Endogeneity

One potential threat to the above 2SLS specification is that SSI enrollment may still not be exogenous. While it seems unlikely, that states can have any meaningful control of SSI enrollment, they may be able to control its relationship with state Medicaid spending via its management of spending per enrollee. For instance, innovative use of Home and Community

Based waivers (Section 1915(c)) may successfully reduce cost per disabled enrollees for a particular state. If for some reason this technology does not transfer to other states, SSI enrollment would generate differential effects on Medicaid spending, and more importantly, these differences are potentially within the purview of state policymakers.

To explore this threat, we propose adapting the Bartik instrumental variable (Bartik (1991)) to model SSI-related Medicaid spending for each state-year observation. The purpose of this approach is to use national variation in technology while excluding state-level variation.

We achieve this objective through the below specification:

$$Z_{st} = SSI_{st} * \frac{DisabilitySpend_{-s,t}}{DisabilityEnroll_{-s,t}} \tag{2.5}$$

Here, $DisabilitySpend_{-s,t}$ and $DisabilityEnroll_{-s,t}$ refer to total disability patient expenses and enrollments in states outside of state s. We take the ratio of these two values to construct an out-of-state measure of per disabled enrollee costs, and we then interact that measure with SSI disability enrollments for state s in year t. The resulting instrumental variable (called a "technology-adjusted IV" or more simply "adjusted IV") represents the predicted Medicaid spending for state s if we exclusively use out-of-state spending technology. Z_{st} replaces SSI_{st} in Equation 2.3 when using the adjusted IV.

In addition to addressing the endogeneity concerns mentioned related to within state patient costs, there are additional benefits to using the adjusted IV. As it explicitly models changes in Medicaid costs, using this instrument will also improve the power of Equation 2.3. It also reduces the threat of a year-specific shock in per-patient medical costs driven by new procedures or technology.

The results from the adjusted IV are included in Table 2.5. In general, these results are both lower in absolute magnitude and more precise than the estimates described in Table 2.3, using the SSI enrollment instrument. The preferred model using the adjusted IV (column 6) shows a reduction in higher education spending of -$0.198 for every additional dollar in Medicaid spending (as compared to -$0.326 in table 2.3). In this case, the estimated standard error is 0.039, considerably lower than the standard error of 0.154 in Table 2.3. While we cannot conclude that the point estimate using the adjusted IV is statistically excluded from the model using the SSI instrument, we prefer the adjusted IV estimate, both for its more credible exogeneity and for its more precise results.

Notably, the key relationships between the models persist in the technology-adjusted specification. In particular, the point estimates are larger with fixed effects included, larger in 2SLS than OLS, and relatively insensitive to the inclusion of state-specific linear time

trends.

2.6.2 First Differences

Next, we consider a differenced regression model (delta model), given by:

$$\Delta Medicaid_{st} = \omega \Delta SSI_{st} + \alpha \Delta X_{st} + \rho_s + \pi_{st} + \nu_{st} \tag{2.6}$$

$$\Delta Y_{st} = \gamma \Delta \widehat{Medicaid}_{st} + \beta \Delta X_{st} + \theta_s + \delta_{st} + \varepsilon_{st} \tag{2.7}$$

This is the same as equations 2.3 and 2.4, differenced by year to produce implied state fixed effects. There are two main benefits to running this model in addition to those already shown. First, this model is responsive to short-run changes in Medicaid costs. That is, while the fixed effects model compares current year Medicaid and Higher Education costs to the state average over the total sample period, the differenced model is more focused on discrete, year-over-year changes. In addition, the differenced model allows for a more flexible state-specific trend controls. The differenced model implicitly includes state fixed effects, and adding state fixed effects to this framework (as in ρ_s and θ_s in Equations 2.6 and 2.7) is the equivalent of adding trends. Adding trends to this framework (as in π_{st} and δ_{st} in Equations 2.6 and 2.7) is thus more flexibly (non-linearly) controlling for state-specific trends.

The results of this model are included in Table 2.6. The preferred point estimate in Column 6 shows that a dollar in additional Medicaid costs reduces higher education spending by 34 cents. This is comparable to the basic SSI specification in Table 2.3, and somewhat larger than the adjusted IV estimates. The delta model estimates are fairly insensitive to fixed effects, trends, and year effects (see Section 2.6.3). In addition, these estimates suggest that the linear time trend is a reasonable approximation for more flexible options.

2.6.3 Time-Specific Variation

In this section, we discuss two possible concerns related time-specific changes. First, we address the concern that the increase in Medicaid costs and the decrease in higher education subsidies are both secular, unrelated trends. Then, we briefly discuss the spike in residual state SSI enrollment in the mid-1990s shown in Figure 2.4.

Table 2.7 shows the SSI-enrollment instrument results from Table 2.3, with three additional specifications (Columns 2, 4, and 6) added to include year fixed effects. Including year fixed effects dramatically increases the standard errors of these estimates. The point

estimate of the preferred specification is near zero, but so noisy as to be essentially meaningless. In some sense, this result is to be expected. Both the increase in Medicaid costs and the decrease in higher education subsides are relatively slow-moving, long run trends. The fully saturated set of year fixed effects is too closely correlated with variation in annual increases in SSI enrollment to generate meaningful results. Nevertheless, without year fixed effects, we fail to test whether the threat of spuriously correlated long run trends is a valid concern.

We attempt to address this threat parametrically. As discussed in Section 2.2, two major demarcations in SSI policy are the expansionary disability redetermination in 1985 and the contractionary reforms beginning in 1995. As the main episodes of significant time series changes in disability-related Medicaid spending, we add two fixed effects identifying years that follow these policy changes (1986-2010 and 1996-2010). We also add in six year-specific fixed effects to control for the first three years following each policy change (1986-1988 and 1996-1998). We call these eight fixed effects "Policy Period Fixed Effects." These additional controls, in combination with our flexible state-specific trends, should allow us to address this time series threat without over-saturating the model.

The results from this specification are included in Table 2.8. This table is comparable to Table 2.7; however, it uses less flexible controls in lieu of year fixed effects. The point estimate here is somewhat larger than those without time period fixed effects (-$0.373 versus -$0.326, respectively), but neither estimate statistically excludes the other.

As an additional check, we consider the spike in residual state SSI enrollment during the 1990s shown in Figure 2.4. Table 2.9 shows two different sets of specifications designed to investigate this anomaly. First, in Panel A, we estimate technology-adjusted IV model using only data from the 1990s. While we lose precision because of the smaller sample size, the estimates are largely consistent with Table 2.5. All estimates are negative, and the 95% confidence intervals frequently include the point estimates from Table 2.5. As a secondary check in Panel B, we exclude the four outlier states—Kentucky, Louisiana, Mississippi, and West Virginia—and re-estimate the technology-adjusted IV model. While point estimates are slightly larger than the original technology-adjusted IV estimates, they are not statistically different from one another.

2.6.4 Lagged Effects

As noted in Section 2.3.5, SSI data is reported in December, which would be during the first quarter of each federal fiscal year. As a result, we match the prior calendar year SSI data to Medicaid spending data when running stage one of the 2SLS Model (that is, SSI enrollment in December of 1995 will match with Medicaid spending for federal fiscal year

1996). Thus, what is shown as contemporaneous effects can on Medicaid and education spending can actually occur up to nine months after SSI is measured.

Here, we ask whether incorporating lags is appropriate for analyzing the state budgetary response to changes in Medicaid spending. Intuitively, incorporating lags may make sense, as states may need time to adjust to budgetary limitations and to gain full knowledge of annual Medicaid costs. On the other hand, most states have balanced budget requirements and flexible tools such as supplemental budget amendments, which dictate real-time responses to budget shocks. Given the opposing stories, the ultimate answer is likely an empirical one.

In an attempt to answer the question about lags, we re-estimate the technology-adjusted IV specification, except that we use lagged Medicaid spending and SSI enrollment rather than contemporaneous measures. We perform this re-estimation three times, each time using a different set of lagged variables. Table 2.10 compares the fully specified technology-adjusted IV model from Column 6 of Table 2.5 (labeled "Contemporaneous") to the same model using different lagged variables for Medicaid spending and SSI enrollment. The column labeled "Lag 1" estimates the OLS and 2SLS effects of changes in Medicaid spending and SSI enrollment from the preceding period on higher education subsidies in the current period. "Lag 2" uses variables from two periods prior, and "Lag 3" uses variables from three periods prior. Results from the lagged specifications appear to be slightly larger than those found in Table 2.5, which probably accounts for the cumulative effect over several periods. That being said, these differences are not statistically different from the Contemporaneous specification. Given the statistical similarity and the expositional simplicity of the contemporaneous specification, we prefer it over a lagged specification.

2.6.5 Heterogeneity

As a final extension, we consider how the willingness of the state to reduce Medicaid spending may be heterogenous across states with different underlying characteristics. In order to do this, we group states according to two different criteria. The first criteria is the share of total residents in 1980 with some college experience. The measure is meant to capture the political will in the state to finance higher education, the local economy's reliance on human capital, and the existing baseline of investment. The next criteria is the share of the state's population over 65 in 1980. This is meant to measure the political will for spending on human capital. Poterba (1996) argues that higher shares of an elderly population reduces investment in K-12 education. It is intuitive that the same dynamic may exist for higher education.

For each measure, we group states into terciles and run Equations 2.3 and 2.4 by each subgroup. We use the adjusted IV to maximize the power of these regressions, given the

reduced sample size within each tercile. The results of this subgroup analysis are available in Table 2.11.

The sensitivity of higher education spending to additional Medicaid obligations is increasing in the share of the population with a college degree. For the lowest group of states, an additional dollar of Medicaid results in a 7 cent decline in college subsidies. For the group with the most well educated baseline, an additional dollar results in a 42 cent reduction. In addition, given that these point estimates lie outside each other's 95% confidence intervals, we can reject that the effects are homogeneous across terciles. This result could be interpreted as a form of mean reversion. The high baseline states were likely already spending a large amount of their total earnings on higher education. Thus, when pressed, they could adopt the strategies of other states and lower their overall spending. Alternatively, states which were already poorly educated could have seen preserving their spending on higher education as a higher priority.

In the bottom rows of Table 2.11, we see the sensitivity of higher education spending to additional Medicaid obligations is decreasing in the share of the population over age 65. These results also reject homogeneity across terciles. While this result might seem counter-intuitive, it is again possible to interpret it through the lens of mean-reversion. If states with a larger shares of the elderly are already spending less on higher education, there is less for the state to cut.

2.7 Conclusion

Overall, we find that an additional dollar of Medicaid obligations reduces spending on higher education by 20 cents, in our preferred model. This number is both precisely estimated, and large enough to say that the financial obligations imposed on state governments by the Medicaid program are economically meaningful. At the same time, the estimate is smaller than those identified previously in the literature. This general finding is robust to a variety of modeling assumptions.

We argue that this paper makes a number of important contributions to the literature on this topic. First, we use state-level data relating to SSI disability enrollments covering the entire Medicaid period. This is both a data contribution, which involves digitizing older records, and a methodological improvement, as SSI is more credibly exogenous to other determinants of state higher education spending than instruments used in previous studies. This paper also integrates technology-adjusted instruments into this approach, improving the power of our estimates and addressing an additional potential threat related to endogenous per-enrollee cost.

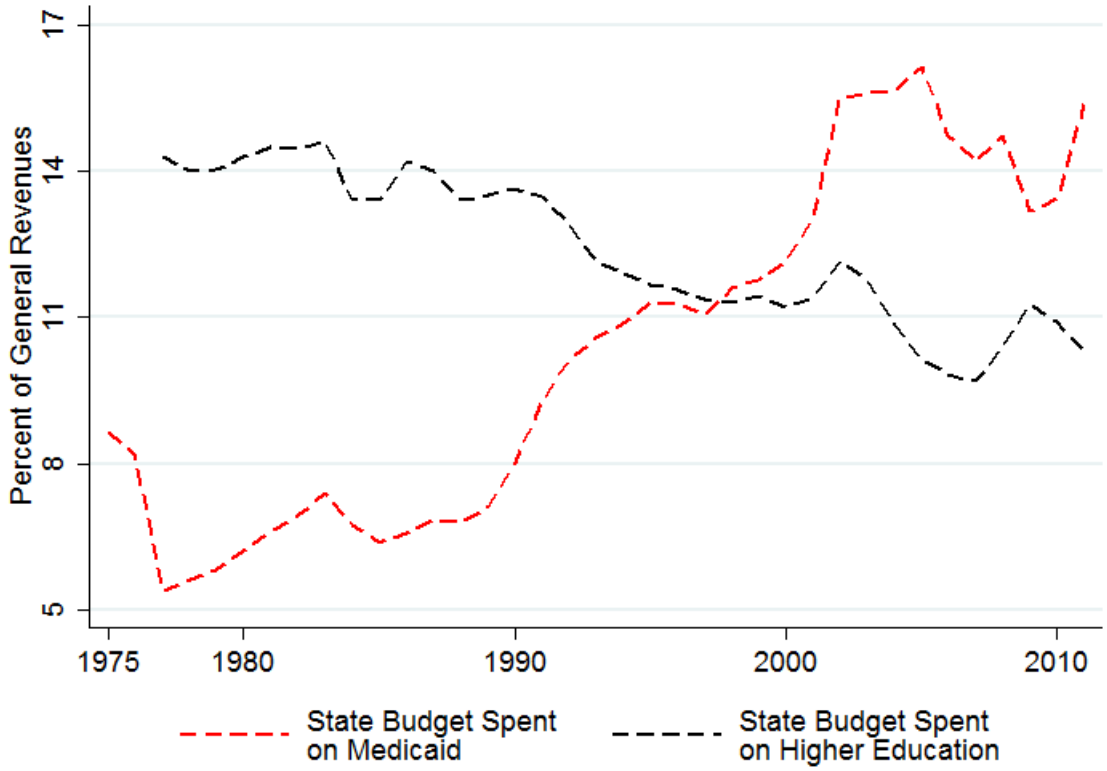
Finally, we find that the results are highly sensitive to the state's baseline level of spending for higher education. Thus, we see that in addition to lowering the overall spending on higher education, the growth of state Medicaid obligations also has the effect of compressing the distribution to total Medicaid spending across states.

Our research highlights the importance of understanding the trade-offs and substitution between public goods. While the Medicaid program undoubtedly helps people in both the short and long-run [Finkelstein et al. (2012), Baicker et al. (2013), Wherry et al. (2015), Cohodes et al. (2016)], it also indirectly imposes costs on human capital development in the form of less public money for higher education. In addition, the intrinsic inefficiencies in the eligibility determination process for SSI and Medicaid suggest that funds may be more productively spent elsewhere [Kleven and Kopczuk (2011)].

We consider this paper to be the first step in understanding the relationship between higher education and Medicaid. Additional research may help us understand how the relationships in state policy and budgets affect individuals' higher education outcomes, especially for enrollment, attainment, and out-of-pocket expenditures.

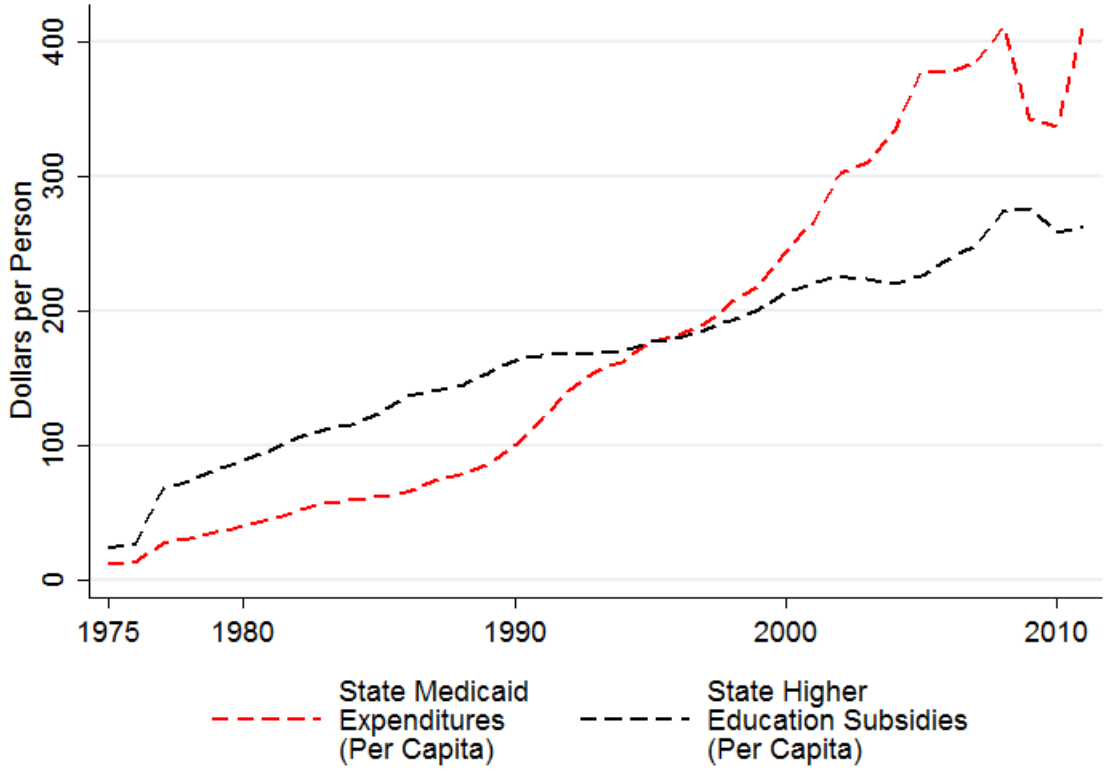
2.8 Figures

Figure 2.1: Trend in State Spending on Medicaid and Higher Education, Share Tax Revenue



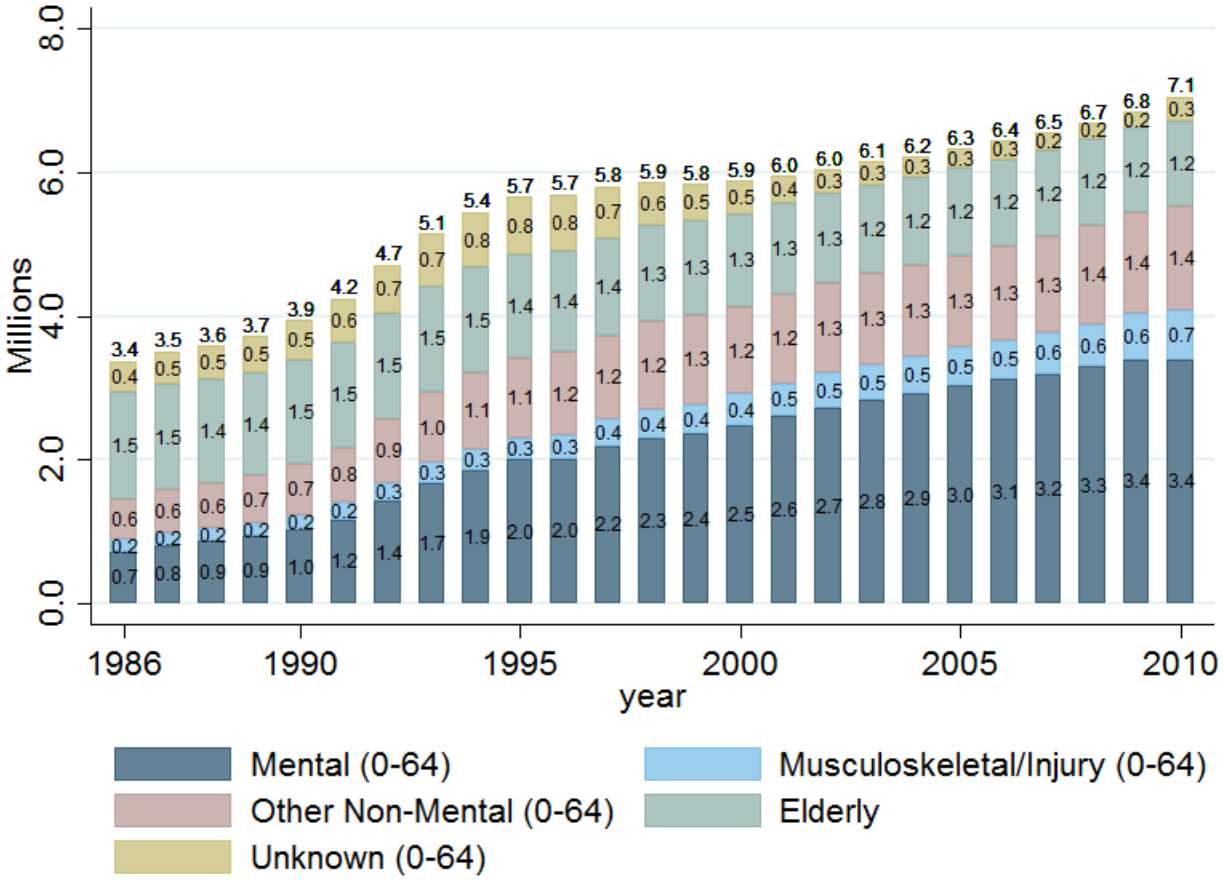
Source: Census of Governments and Bureau of Economic Analysis (1975-2011). Figure shows the comparative trend in the share of state spending directed toward higher education vs. Medicaid. Over time, an increasingly large share of general state revenues are directed towards Medicaid, while the share dedicated to funding institutions of higher education drops over time.

Figure 2.2: Trend in State Spending on Health Care and Higher Education, Per Capita



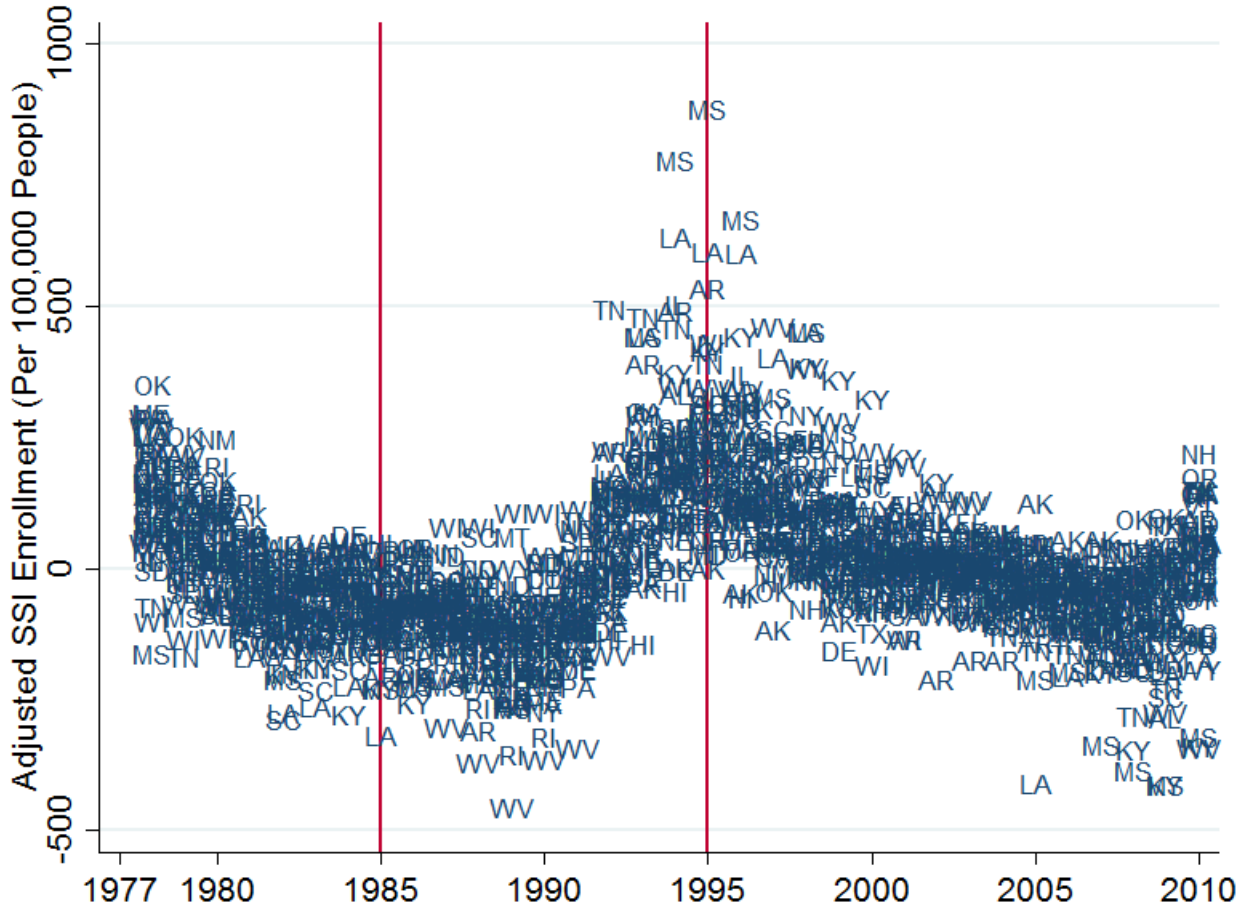
Source: Census of Governments and Current Population Survey (1975-2011). Figure shows the comparative trend in the share of state spending directed toward higher education vs. Medicaid. Over time, an increasingly large share of general state revenues are directed towards Medicaid. Higher Education increases on a per capita basis. This differs from Figure 2.1 because of the growth in the size of the economy along with overall population growth. The difference highlighted by these figures motivates using an income-based, rather than population-based normalization in this analysis.

Figure 2.3: Trend in Total SSI Recipients by Diagnosis



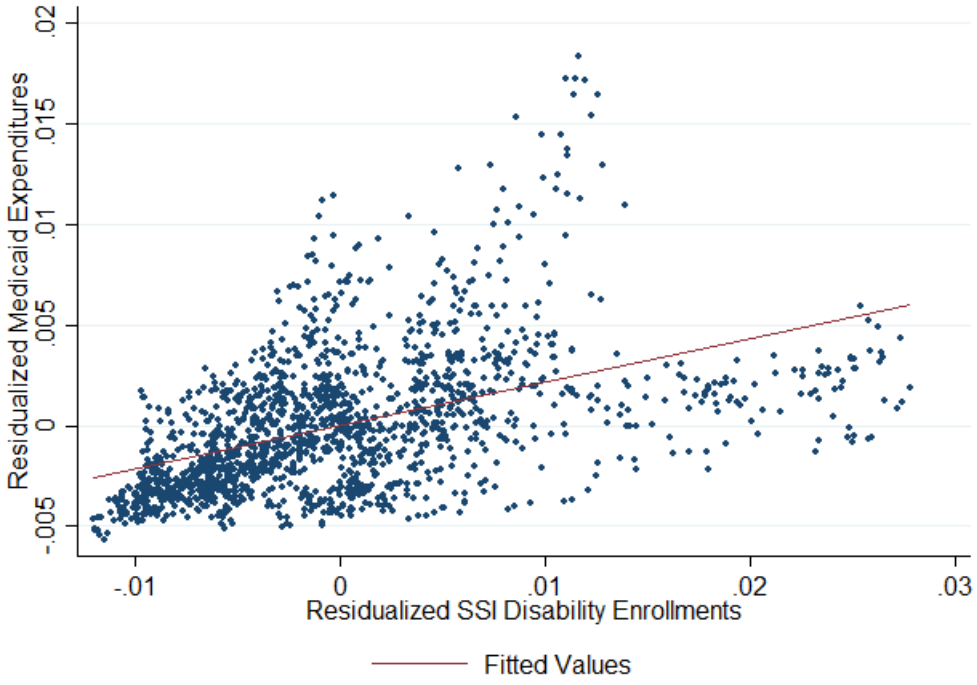
Source: SSA Annual Statistical Supplement (1986-2010). This figure demonstrates that the disability, particularly mental health disability, is the dominant source of growth in SSI recipients. Importantly, despite the aging of the baby boomer generation, the elderly actually make up a decreasing share of SSI recipients over this time frame, due to reductions in elderly poverty. Elderly recipients are exclusively ages 65 and older. They do not need a diagnosis to enroll in SSI as long as they meet the income and asset eligibility requirements. Recipients with an associated disabling diagnosis are all 64 years old and younger. The lowest group in the figure is Mental (0-64), and the second lowest group in the figure is Musculoskeletal/Injury (0-64).

Figure 2.4: Distribution of SSI Enrollment Over Time



Source: Authors' analysis. The above figure shows the time series in blind and disabled SSI enrollment per 100,000 people in each state, for each state. Values have been adjusted for state fixed effects, linear state trends, demographic characteristics, and the business cycle. In any given year, residual SSI enrollment ranges from 242 to 923 per 100,000 people. Despite SSI eligibility being mandated by the Federal Government, the eligibility rules can be open to interpretation by the disability examiner and administrative law judges, which drives idiosyncratic variation at the state-year level. Changes in federal policy over time can magnify or contract this variation.

Figure 2.5: Relationship Between SSI Enrollment and Medicaid Spending



Source: Authors' analysis. Figure shows state Bartik IV (SSI enrollment times out of state average disabled patient cost) and total state-level Medicaid payments plotted together after being adjusted (residualized) for state fixed effects, linear state trends, demographic characteristics, and the business cycle. The effect of SSI enrollments is economically significant, with a single new dollar in the Bartik measure increasing state Medicaid obligations by 22 cents. This relationship does not appear to be driven by outliers.

2.9 Tables

Table 2.1: State and local expense by functional category

State and Local Functional Spending	1977	2010
General public service	17%	16%
Public order and safety	10%	13%
Economic affairs	11%	8%
Health (net)	12%	21%
Education	39%	33%
Income security	10%	7%

Source: BEA National Economic Accounts data, table 3.16. Local expenses are concentrated in categories for Public Order and Safety (Police / Fire), and Education (Particularly K-12). Table shows growth in total spending on Health, and the largest decrease in Education.

Table 2.2: First stage regressions

	Model					
Stat	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	0.213***	0.154***	0.433***	0.270***	0.209***	0.223***
SE	(0.048)	(0.045)	(0.041)	(0.035)	(0.036)	(0.034)
F	17.972	11.075	117.044	62.468	33.666	42.716
r2	0.177	0.462	0.774	0.846	0.933	0.938
N	1666	1666	1666	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x	x	x	x
Demographic Controls		x		x		x
State FE			x	x	x	x
State Trends					x	x

Table shows the power of first stage regression results of Equation 2.3. The coefficients represent the effect of the instrument, SSI enrollment per capita, on the state Medicaid spending, measured as a share of total personal income. As an example, the specification shown in Column (6) indicates that a 1ppt increase in SSI disability enrollment per capita increases the share of total income spent on Medicaid by 0.2ppt. The F statistics are the reported first stage F statistics from the 2SLS regression. They indicate a strong relationship between the instrument and Medicaid spending. Standard errors are clustered by state.
*** p<.01, ** p<.05, * p<.10

Table 2.3: Effect of state Medicaid obligations on Higher Education Subsidies

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Higher Ed Spending (OLS)	-0.297***	-0.081	-0.305***	-0.159***	-0.100***	-0.089***
	(0.052)	(0.066)	(0.035)	(0.042)	(0.031)	(0.029)
Higher Ed Spending (2SLS)	-0.071	0.676**	-0.351***	-0.215***	-0.370**	-0.326**
	(0.189)	(0.315)	(0.037)	(0.052)	(0.158)	(0.154)
N	1666	1666	1666	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x	x	x	x
Demographic Controls		x		x		x
State FE			x	x	x	x
State Trends					x	x

Table shows the effects of state Medicaid spending on higher education subsidies. Higher education subsidies and Medicaid obligations are both expressed in terms of the share of total state income spent on these items. Medicaid obligations are instrumented by the share of the population eligible for SSI disability benefits. Column 6 in the 2SLS specification shows that an additional dollar in Medicaid obligations lowers state higher education subsidies by about 32.6 cents.
*** p<.01, ** p<.05, * p<.10

Table 2.4: Effect of state Medicaid obligations on budget items outside of Medicaid

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Justice Spending (2SLS)	0.089	0.028	0.055**	0.026	0.003	0.000
	(0.067)	(0.074)	(0.023)	(0.028)	(0.027)	(0.024)
Transportation (2SLS)	-0.201	-0.479	-0.216	-0.110	-0.139	-0.184
	(0.322)	(0.362)	(0.133)	(0.160)	(0.135)	(0.140)
K-12 Ed (2SLS)	-0.408	-0.135	0.034	0.099	0.002	0.031
	(0.333)	(0.415)	(0.037)	(0.067)	(0.130)	(0.128)
N	1666	1666	1666	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x	x	x	x
Demographic Controls	x	x	x	x	x	x
Policy Period FE		x		x		x
State FE			x	x	x	x
State Trends					x	x

Table shows the 2SLS effect of an additional dollar of Medicaid spending on other types of state spending, using SSI enrollment as the instrumental variable. "Justice" includes prisons and law enforcement. "Transportation" includes roads and other types of infrastructure such as airports or trains. Point estimates for K-12 and Justice spending are small and statistically insignificant. Point estimates for Transportation are economically significant but imprecise, which we interpret as institutional heterogeneity in the state legislature's control of the transportation fund.
*** p<.01, ** p<.05, * p<.10

Table 2.5: Technology-Adjusted Instruments

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Higher Ed Spending (OLS)	-0.081	-0.116*	-0.159***	-0.102*	-0.089***	-0.079***
	(0.066)	(0.058)	(0.042)	(0.052)	(0.029)	(0.029)
Higher Ed Spending (2SLS)	-0.099	-0.183	-0.227***	-0.153***	-0.185***	-0.198***
	(0.127)	(0.133)	(0.042)	(0.052)	(0.043)	(0.039)
N	1666	1666	1666	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x	x	x	x
Demographic Controls	x	x	x	x	x	x
Policy Period FE		x		x		x
State FE			x	x	x	x
State Trends					x	x

Here, we redefine the instrument of the main analysis to compensate for exogenous trends in the costs of providing health care. Each state's increase in SSI enrollment is multiplied by the nationwide increase in average disability patient cost outside of that state. The result is a more powerful instrument which more accurately predicts total Medicaid costs. The main results here are slightly lower than those in the main specification, but much more precise.
*** p<.01, ** p<.05, * p<.10

Table 2.6: Delta model

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Higher Ed Spending (OLS)	-0.218***	-0.216***	-0.220***	-0.222***	-0.225***	-0.228***
	(0.052)	(0.052)	(0.051)	(0.052)	(0.052)	(0.052)
Higher Ed Spending (2SLS)	-0.370***	-0.365***	-0.370***	-0.377***	-0.367***	-0.343***
	(0.114)	(0.115)	(0.113)	(0.114)	(0.112)	(0.108)
N	1666	1666	1666	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x	x	x	x
Demographic Controls	x	x	x	x	x	x
Policy Period FE		x		x		x
State FE			x	x	x	x
State Trends					x	x

Here we repeat the main analysis, but use a model specifying changes in enrollment and spending, rather than levels. This model emphasizes the short term changes, rather than the longer term changes implied by the fixed effects model. This model also has the additional benefit that including state effects and state trends in this model allows for a more flexible, nonlinear state trend assumption. We find that the point estimates are roughly identical to those in our main specification in Table 2.3.
*** p<.01, ** p<.05, * p<.10

Table 2.7: Sensitivity to year effects

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Higher Ed Spending (OLS)	-0.081	-0.110*	-0.159***	-0.017	-0.089***	0.018
	(0.066)	(0.060)	(0.042)	(0.070)	(0.029)	(0.037)
Higher Ed Spending (2SLS)	0.676**	1.342	-0.215***	0.437	-0.326**	0.011
	(0.315)	(0.923)	(0.052)	(0.325)	(0.154)	(0.354)
N	1666	1666	1666	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x	x	x	x
Demographic Controls	x	x	x	x	x	x
Year FE		x		x		x
State FE			x	x	x	x
State Trends					x	x

Table shows the sensitivity of the main analysis to the inclusion of year fixed effects. The preferred specification (6) with year fixed effects shows a point estimate of 0.011. Due to the low point estimate and large standard error associated with this model, it is difficult to draw much meaningful inference from this specification. The model here can neither reject the point estimates in Table 2.3 nor a point estimate of zero. *** p<.01, ** p<.05, * p<.10

Table 2.8: Sensitivity to level policy changes

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Higher Ed Spending (OLS)	-0.081	-0.116*	-0.159***	-0.102*	-0.089***	-0.079***
	(0.066)	(0.058)	(0.042)	(0.052)	(0.029)	(0.029)
Higher Ed Spending (2SLS)	0.676**	0.954	-0.215***	-0.040	-0.326**	-0.373**
	(0.315)	(0.599)	(0.052)	(0.109)	(0.154)	(0.177)
N	1666	1666	1666	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x	x	x	x
Demographic Controls	x	x	x	x	x	x
Policy Period FE		x		x		x
State FE			x	x	x	x
State Trends					x	x

Table 2.8 imposes more structure on the year fixed effects in Table 2.7, and instead includes policy period fixed effects for major federal policy changes. These changes include a 1985 adjustment to disability determination and contractionary reforms beginning in 1995. Including these measures does not significantly change the results from Table 2.3. *** p<.01, ** p<.05, * p<.10

Table 2.9: Sensitivity to Time-Specific Outlier Variation during the 1990s

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Only 1990s						
Higher Ed Spending (OLS)	-0.220***	-0.194***	-0.239***	-0.190***	-0.034	-0.019
	(0.069)	(0.063)	(0.049)	(0.040)	(0.033)	(0.039)
Higher Ed Spending (2SLS)	-0.448**	-0.419**	-0.408***	-0.362***	-0.167*	-0.074
	(0.194)	(0.194)	(0.076)	(0.073)	(0.096)	(0.104)
N	490	490	490	490	490	490
Years	1990-1999	1990-1999	1990-1999	1990-1999	1990-1999	1990-1999
Panel B: Exclude Outliers						
Higher Ed Spending (OLS)	-0.069	-0.092	-0.160***	-0.099*	-0.091***	-0.079**
	(0.064)	(0.058)	(0.045)	(0.055)	(0.032)	(0.032)
Higher Ed Spending (2SLS)	-0.190	-0.266**	-0.255***	-0.190***	-0.215***	-0.229***
	(0.116)	(0.128)	(0.047)	(0.051)	(0.046)	(0.042)
N	1666	1666	1666	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x	x	x	x
Demographic Controls	x	x	x	x	x	x
Policy Period FE		x		x		x
State FE			x	x	x	x
State Trends					x	x

Table 2.9 shows two different sets of specifications designed to investigate the spike in residual state SSI enrollment during the 1990s shown in Figure 2.4. Both sets of specifications use the technology-adjusted IV described in Section 2.6.1. Panel A reports results for only the 1990s. While there is some loss in precision due to a smaller sample size, most estimates have a 95% confidence interval that includes the results in Table 2.5. Panel B reports a re-estimation of Table 2.5 after dropping outlier states with particularly pronounced increases in residual SSI enrollment, namely Kentucky, Louisiana, Mississippi, and West Virginia. After removing these states, point estimates are slightly larger than the original technology-adjusted IV estimates but not statistically distinguishable from each other.

*** p<.01, ** p<.05, * p<.10

Table 2.10: Technology-Adjusted IV Specification using Lagged Instruments

	Model			
	Contemporaneous	Lag 1	Lag 2	Lag 3
Higher Ed Spending (OLS)	-0.079***	-0.095***	-0.121***	-0.074*
	(0.029)	(0.028)	(0.038)	(0.040)
Higher Ed Spending (2SLS)	-0.198***	-0.209***	-0.312***	-0.223**
	(0.039)	(0.050)	(0.093)	(0.099)
N	1666	1617	1568	1519
Years	1977-2010	1978-2010	1979-2010	1980-2010
Lagged Unemployment	x	x	x	x
Demographic Controls	x	x	x	x
Policy Period FE	x	x	x	x
State FE	x	x	x	x
State Trends	x	x	x	x

Table 2.10 compares the fully specified technology-adjusted IV model from Column 6 of Table 2.5 (labeled "Contemporaneous") to the same model using different lagged variables for Medicaid spending and SSI enrollment. The column labeled "Lag 1" estimates the OLS and 2SLS effects of changes in Medicaid spending and SSI enrollment from the preceding period on higher education subsidies in the current period. "Lag 2" uses variables from two periods prior, and "Lag 3" uses variables from three periods prior. Results from the lagged specifications appear to be slightly larger than those found in Table 2.5, which probably accounts for the cumulative effect over several periods. That being said, these differences are not statistically different from the Contemporaneous specification. Given the statistical similarity and the expositional simplicity of the contemporaneous specification, we prefer it over a lagged specification.
*** p<.01, ** p<.05, * p<.10

Table 2.11: Heterogeneity by State Subgroup

	Tercile		
	(1)	(2)	(3)
Subgroup by college share	-0.073	-0.211***	-0.420***
	(0.054)	(0.042)	(0.113)
Subgroup by elderly share	-0.310***	-0.189**	-0.147***
	(0.083)	(0.081)	(0.040)
N	1666	1666	1666
Years	1977-2010	1977-2010	1977-2010
Lagged Unemployment	x	x	x
Demographic Controls	x	x	x
Policy Period FE	x	x	x
State FE	x	x	x
State Trends	x	x	x

In this table we present regression results for states divided into terciles based on state characteristics. The first grouping is based on the share of state population having attended some college in 1980. The second is based on the share of state population over age 65. We believe higher education spending in these subgroups may exhibit different sensitivities based on state political economy. We find that states which have a higher share of the population having attended college show larger declines in spending. We also find states which had a lower share of the population over age 65 saw larger declines in higher education spending. Both of these results may be attributable to mean reversion, or higher sensitivity from places which were already overspending. In each case we present the preferred regression results, which include all control variables and use the technology-adjusted IV.
*** p<.01, ** p<.05, * p<.10

CHAPTER III

Prescription Drug Monitoring Programs and Prescription Opioid Overuse

3.1 Introduction

Recent reports of prescription opioid abuse suggest an alarming trend. The National Survey of Drug Use and Health reports that the estimated number of people who ever used OxyContin improperly increased from 3.5 million in 2005 to 7.1 million in 2014 [NSDUH (2005), NSDUH (2014)]. Emergency department visits involving prescription opioid misuse increased 117% between 2005 and 2011 (DAWN (2013)), and more than 25,000 deaths in 2015 involved opioids other than heroin [Rudd et al. (2016)].

To combat this trend, most states have adopted prescription drug monitoring programs (PDMPs). These programs collect individually identifiable electronic prescription data for controlled substances. Data are provided by the dispensing pharmacy, and any provider licensed to prescribe controlled substances can access PDMP data via an online portal. After logging in, a provider can query a particular patient's prescription history. A typical report includes the prescription date, dosage, and drug classification as well as detail for the patient's previous prescribers and pharmacies (see Figure 3.1 for an example).

Theoretically, providers equipped with this detail should have the tools necessary to identify patients who are at risk of misusing prescription opioids. Unfortunately, however, providers must access the PDMP for it to be useful, and this does not seem to be happening. Only a tenth of physicians had created a PDMP login within one year of Florida's rollout [Poston (2012)], and within two years of Rhode Island's rollout, only 20% of providers had enrolled [Arditi (2014)].

Registration is not the only problem though. A common complaint among providers is that the PDMP system is ungainly and disrupts workflow [Perrone and Nelson (2012)]. In addition, data were collected too infrequently. In 2010, PDMP data was collected monthly in

all states [PDMP TTAC (2016)]. This lag made it harder to monitor doctor and pharmacy shopping in real time and added to the perception that PDMP queries were a waste of time.

Policymakers have responded to these issues by mandating that prescribers register and use PDMPs in 18 states (see Figure 3.2). Even when participation is voluntary, many states have upgraded their systems so that the burden of use is less dramatic. For example, PDMP queries that previously took up to 10 minutes became instantaneous after Michigan upgraded its system in 2017 [Greene (2017)]. States have also reduced the data collection interval. As of 2016, two-thirds of states collect PDMP data daily [PDMP TTAC (2016)].

Given the uneven PDMP participation among health care providers, it is not surprising that the literature on the efficacy of PDMPs has been mixed. Early cross-sectional analyses find no relationship between PDMPs and opioid prescriptions [Paulozzi, Kilbourne and Desai (2011) and Jena et al. (2014)]. Likewise, Meara et al. (2016) use a panel of Medicare disabled data and find that PDMPs have no detectable effect on high-volume opioid prescriptions.

Bao et al. (2016) explore the effects of granting providers online access to PDMPs, which is a subtler distinction than other research. For instance, while California was the first state to create a PDMP in 1939, it did not grant online access to providers until September 2009. Using the National Ambulatory Medical Care Survey (NAMCS) from 2001 to 2010, the authors restrict their sample to outpatient visits related to pain and differentiate between Schedule II drugs, which have the highest potential for misuse, and other prescription opioids. They find a 30% reduction in prescriptions for Schedule II drugs, and a 10% reduction in opioid prescriptions generally. The latter result, however, is statistically insignificant.

Buchmueller and Carey (2017) adopt a similarly nuanced approach. While they use a less stringent definition of PDMP implementation, they also evaluate laws that require physicians to access PDMPs (referred to as "must access" laws). Using a 5% sample of Part D Medicare data from 2007 to 2013, they find little effect from PDMP implementation, but they find some evidence that "must access" laws reduce high-volume opioid prescriptions and provider shopping behavior.

This paper combines parts of Bao et al. (2016) and Buchmueller and Carey (2017) to reevaluate the effectiveness of PDMPs using the Medical Expenditure Panel Survey (MEPS). The MEPS is both a longer and more representative dataset than either of these two studies. These data extend from 1996 to 2013, which allows me to include more states, and as a nationally representative survey, the MEPS allows me to study population effects rather than exclusively focusing on pain patients or Medicare enrollees. In addition, the MEPS includes detailed information about each respondent, which allows me to explore heterogeneous effects across individuals.

I find little evidence that PDMP implementation is effective in preventing prescription

opioid overuse, but when paired with "must access" laws, PDMPs can be effective at reducing overuse. I estimate that "must access" laws reduce high-volume prescriptions by about 20%, although the small number of states with these in place warrants some caution. I also find that "must access" laws have a limited effect on opioid prescriptions overall, which suggests that PDMPs may be successfully targeting potential misuse without restraining access for other patients.

3.2 Data

To evaluate the effectiveness of PDMPs, I primarily rely on two datasets, one on PDMPs and the Medical Expenditure Panel Survey (MEPS). I also use data from the Bureau of Economic Analysis and the Area Health Resource file for state- and county-level controls.

For the PDMP dataset, I construct two variables: (1) PDMP implmentation and (2) "must access" laws. I define PDMP implementation as the first full year that a state grants health providers online access to its database. The majority of PDMP implementation dates can be found in Bao et al. (2016). The remaining dates were located through online queries and through research provided by the Prescription Drug Abuse Policy System (pdaps.org) and the Prescription Drug Monitoring Program Training and Technical Assistance Center (pdmpassist.org). These websites also provide detail on whether a prescriber is required to access a PDMP prior to prescribing opioids. Like Buchmueller and Carey (2017), I call these laws "must access" laws.

As of 2016, every state but Missouri has enacted legislation for a PDMP (see Figure 3.2), and Missouri legislation has been stymied by a single state senator since 2011. There are currently 18 states with "must access" laws, and 10 states passed laws by 2013, which is the last year in my sample period. In practice, this requirement varies. For instance, Louisiana, Ohio, and Oklahoma only require access for chronic pain patients. Nevada only requires access for new patient visits, and Tennessee simply requires periodic access. Figure 3.2 shows that the timing and enactment of laws is geographically dispersed, but "must access" laws do appear to be concentrated in the Northeast and Midwest.

I merge the PDMP data by state and year onto corresponding identifiers from the restricted MEPS data for years 1996 to 2013. The MEPS is a nationally representative survey of non-institutionalized patients. The survey is conducted over two years for each respondent, and approximately 92% of respondents participate for both years. MEPS data include a rich set of demographic and household characteristics, medical utilization and cost details, and self-reported health. Importantly for this analysis, the MEPS Prescribed Medicines files record individual prescription transactions that are cross-validated by respondents' phar-

macies. Moreover, these data include a field for Cerner Multum Lexicon therapeutic class, which allows me to identify opioid prescriptions. Consistent with Stagnatti (2015) and Frenk, Porter and Paulozzi (2015), I identify prescription opioids using the therapeutic classes 60 (narcotic analgesics) and 191 (narcotic analgesic combinations).

In some instances, treatment can span over the two years of reported data, which causes some of the data to be reported in the first year and some in the second year. To avoid this type of truncation error, I collapse the data to the individual-level such that each observation represents two years of utilization. Prescriptions and other utilization are calculated as the sum of utilization over the two years. I use the first year of response for time varying characteristics such as income.

My analytic sample includes almost 196,000 observations. I use the variables from Table 3.1 as individual controls in my empirical specification. The sample includes adults aged 19 to 90, and reported means use respondent-level survey weights. Roughly a quarter of the sample lacks consistent health insurance coverage, while one-fifth of the sample is enrolled in Medicare. The average age of the sample is 46.4 years old, and about 8.0% of the sample had an inpatient surgery during the two year period.

I estimate that approximately 19.6% of respondents have acquired an opioid prescription during their two-year survey period. Over the same timeframe, 1.5% of respondents acquire 12 or more opioid prescriptions, and 2.2% acquire 360 or more opioid prescription pills. The slightly higher mean for 360 pills suggests that fewer than 12 prescriptions can frequently yield 360 pills over two years.

3.3 Empirical Strategy

To measure the effects of PDMPs on prescription opioid utilization, I use a difference-in-differences (DD) specification. My primary estimation equation for individual i living in state s and county c during year t is:

$$y_{it} = \pi_{PDMP} PDMP_{s(i)t} + \pi_{MA} MA_{s(i)t} + \mathbf{I_{it}}\beta_{\mathbf{I}} + \mathbf{S_{s(i)t}}\beta_{\mathbf{S}} + \mathbf{C_{c(i)t}}\beta_{\mathbf{C}} + \delta_{s(i)} + \delta_t + \varepsilon_{it} \quad (3.1)$$

For my primary specification, I consider four measures of the outcome (y_{it}): (1) a dummy for whether the respondent acquired an opioid prescription over the two-year survey, (2) a dummy for whether the respondent acquired 12 or more opioid prescriptions over the two-year period, (3) a dummy for whether the respondent acquired 360 or more opioid prescription pills over the two-year period, and (4) a dummy for whether the respondent had been hospitalized. Outcomes (2) and (3) represent high-volume prescriptions, which

serve as proxies for misuse. Loosely speaking, they represent a year's worth of prescription opioids over a two-year period. A more detailed analysis of different cutoffs is summarized in Section 3.5. I include hospitalizations as an outcome to see if reductions in misuse prevent opioid-related hospitalizations.

$PDMP_{s(i)t}$ is an indicator for whether state s granted online PDMP access in year t, and $MA_{s(i)t}$ is an indicator for whether state s has a "must access" law in year t. I_{it} is a vector of time-varying and time-invariant characteristics for individual i. These covariates include controls for gender; race; ethnicity; age and age squared; insurance status; employment status; marital status; education; household income relative to the federal poverty level; and whether the respondent had an inpatient surgery during the first year of the survey. $S_{s(i)t}$ is a vector of time-varying characteristics for the state that individual i lives in during year t, and $C_{c(i)t}$ is a vector of time-varying characteristics for the county that individual i lives in during year t. To address concerns about within state correlations, I cluster standard errors at the state-level. Because the MEPS oversamples minorities and low-income households, I use the person-level weights provided by MEPS to correct for endogenous sampling [Solon, Haider and Wooldridge (2015)].

Because only 19.6% of respondents ever acquired prescription opioids and substantially fewer were heavy users, I use a logit model to estimate Equation 3.1. This specification avoids bias and consistency problems introduced in a more common linear probability model [Horrace and Oaxaca (2006)].

As an extension of Equation 3.1, I also analyze the effects of PDMP implementation and "must access" laws using an event study framework. This specification decomposes the pre-post dummies of $PDMP_{s(i)t}$ and $MA_{s(i)t}$ into dummies for years before and years after the policies are enacted. This framework serves two purposes. First, the policies should have no effect before they are implemented. A test for no pre-policy effect is equivalent to the parallel trends assumption for difference-in-differences estimation. Second, measuring the post-policy effects illustrates the timing of effects and whether the effects are persistent.

It is possible that the effects of PDMPs will vary across different groups of respondents. I explore this possibility across Medicare coverage and across age groups. The below specification is an extension of Equation 3.1 where $H(j)_{it}$ is an indicator variable for each of these

subgroups. $\bar{j} = 2$ for Medicare, and $\bar{j} = 3$ for age group.

$$
\begin{aligned}
y_{it} = \sum_{1}^{\bar{j}} \left[\gamma^{j}_{PDMP} PDMP_{s(i)t} * H(j)_{it} + \gamma^{j}_{MA} MA_{s(i)t} * H(j)_{it} + H(j)_{it}\right] + \\
\pi_{PDMP} PDMP_{s(i)t} + \pi_{MA} MA_{s(i)t} + \\
\mathbf{I_{it}}\beta_{\mathbf{I}} + \mathbf{S_{s(i)t}}\beta_{\mathbf{S}} + \mathbf{C_{c(i)t}}\beta_{\mathbf{C}} + \delta_{s(i)} + \delta_t + \varepsilon_{it}
\end{aligned}
\tag{3.2}
$$

Like the main specification, I estimate Equation 3.2 using a logit specification. For each subgroup j, $(\gamma^{j}_{PDMP} + \pi_{PDMP})\bar{p}$ represents the average marginal effect from PDMP implementation, where $\bar{p}$ is the average product of $p_{it}(1-p_{it})$ and p_{it} represents the underlying probability of $y_{it} = 1$. Similarly, $(\gamma^{j}_{MA} + \pi_{MA})\bar{p}$ represents the marginal effect from MA implementation.

I choose Medicare coverage as a characteristic of interest because several previous studies used Medicare claims data to evaluate PDMPs. The Medicare population is both older and more infirm than the overall population, and because they may have a medical need for high volumes of prescription opioids, their use of prescription opioids may be less affected by PDMPs.

For age groups, I categorize respondents into three groups: ages 19 to 34, ages 35 to 64, and ages 65 and up. This approach serves two purposes. First, I isolate middle age respondents, who have been shown to have increasing rates of poisoning, suicide, and liver mortality possibly as a result of addictive behavior [Case and Deaton (2015)]. Second, I also separate elderly Medicare enrollees from non-elderly. By doing this, I can study effects just on elderly Medicare beneficiaries, who are heavy prescription opioid users but may be less subject to misuse and therefore less subject to PDMP changes.

3.4 Results

Overall, the results show slight evidence in favor of PDMPs generally and stronger evidence in favor of PDMPs paired with more stringent "must access" laws. In Table 3.2, I label the effect of granting online access to PDMPs as "PDMP Implementation." Although no result is significantly different from zero, the point estimates for the prescription outcomes (Columns 1-3) are all negative and range from -3.1% to -5.1% changes from the pre-PDMP mean. Inpatient hospitalizations, on the other hand, reveal a slightly positive result. The sign of the result could be consistent with Alpert, Powell and Pacula (2017), which finds substitution from prescription opioids toward heroin after prescription opioids are more heavily regulated, but the imprecision makes it difficult to argue convincingly. Given that the

upper confidence level only yields a 5.5% in hospitalizations, I argue instead that PDMP implementation has a limited effect at most.

In contrast to the small, negative, and imprecise estimates for PDMP implementation, "must access" laws appear to have large negative effects on high volume use. The decrease in high-volume use ranges from -16.3% to -26.7%, and although only the latter is significant at the 5% confidence level, both are quite large in magnitude. By comparison, the effect on opioid prescriptions generally is slightly positive. Taken together, these results suggest that "must access" laws successfully reduce high-volume prescriptions without negatively affecting access to prescription opioids for medically appropriate reasons. Like PDMP implementation, "must access" laws do not appear to demonstrably change hospitalizations.

The event study figures convey a similar story to Table 3.2, especially for PDMP implementation. Figure 3.3 shows the event study graphs for PDMP implementation, and Figure 3.4 shows the event study graphs for "must access" laws. Results are presented as the average marginal effect for each year dummy. Regardless of outcome choice, the figures show no detectable pre-PDMP trend, which is consistent with the assumptions of a difference-in-differences specification. After PDMP implementation, there is a modest reduction in the likelihood of receiving any opioid prescription, but this effect is statistically insignificant and wears off over time. In addition, there is no evidence of this change affecting the likelihood of high-volume prescriptions.

The "must access" figures show a reduction in opioid prescriptions after legislation, but the trends lack stability. This volatility is likely the result of having so few states with post-legislation data. There also appears to be a substantial drop in year +3 for all figures. Because only Nevada, Ohio, and Oklahoma have three years worth of post-legislation data, these states must be driving the "must access" result. Also, only Nevada has more than three years of post-legislation data, and the estimates for year +4 and years ¿+4 hover closer to zero.

3.4.1 Heterogeneity

Figures 3.6 and 3.7 show heterogeneous effects across the two classes of subgroups mentioned above in Section 3.3. Because there was little variation across subgroups for PDMP implementation, these figures only show the differential effects of "must access" laws. In each figure, the left bar represents the pre-legislation mean, and the right bar represents the estimated post-legislation mean, which is the sum of the pre-legislation mean and the estimated treatment effect. The whiskers represent the 95% confidence interval of the estimated post-legislation mean. The left figure shows the effect on high-volume prescriptions using 12 or more prescriptions as the cutoff, and the right figure shows the effect on high-volume

prescriptions using 360 pills or more as the cutoff.

The figures comparing non-Medicare respondents to Medicare enrollees (Figure 3.6) first show that Medicare enrollees are far more likely to take high volumes of prescription opioids. This result fits the notion that Medicare enrollees are more infirm than non-Medicare respondents. In addition, it appears that the effects of "must access" laws are similar across the two groups, indicating that earlier research using only Medicare claims data can potentially inform non-Medicare behavior.

The figures by age group (Figure 3.7) suggest, however, that effects are concentrated among middle aged respondents. In fact, "must access" laws have a positive, but imprecise, effect for both the elderly and young adults. Thus, while Figure 3.6 shows that results are similar between non-Medicare respondents and Medicare enrollees, Figure 3.7 indicates that the same is not true between elderly Medicare enrollees and disabled Medicare enrollees. In fact, the decrease in high-volume opioid prescriptions seems to be dominated by the disabled Medicare enrollees. This result is consistent with a finding by Buchmueller and Carey (2017), who find that more than two-thirds of the reduction in addictive behavior is attributable to disabled Medicare enrollees.

In addition, these results are consistent with Case and Deaton (2015). They argue that middle aged Americans are suffering from "diseases of despair" and are more prone to addictive behaviors. As such, "must access" laws designed to reduce misuse should and do have the strongest effects among the middle aged cohort.

3.5 Extensions

3.5.1 High-Volume Prescription Cutoffs

For my primary specification, I define high-volume prescriptions as having more than 12 prescriptions or having more than 360 prescription pills in a two-year period. I chose these cutoffs because they loosely refer to a year's supply of prescription opioids.

In an attempt to explore this cutoff choice more rigorously, I re-estimate Equation 3.1 nine times where the prescription count cutoff increases by three prescriptions from zero to 24 prescriptions over two years. I then repeat this process but instead vary the prescription pill count cutoff by 120 pills for each interval from zero to 720 pills over two years.

Panel A of Figure 3.5 shows the estimated effects for different prescription cutoffs, and Panel B shows the estimated effects for different prescription pill cutoffs. These figures indicate that PDMP implementation has no effect regardless of the choice of cutoff, while "must access" laws appear to be more effective as the cutoff increases. Each point represents the point estimate from a separate regression, and the whiskers represent the estimated

confidence interval. Because the outcome variable has a different mean for each regression, I normalize the estimates in the figures by the outcome mean.

The figure for "must access" laws and prescription opioid pills is particularly compelling. The estimated effect for zero and 90 prescription opioid pills is nearly identical, and the effects steadily become more negative until 360 pills where they level off. This pattern is consistent with an effective PDMP intervention, which is designed to prevent misuse without affecting medically appropriate use.

3.5.2 Two-Part Model

Because 80.4% of respondents have not used prescription opioids over the two-year period, it may be more appropriate to estimate the marginal effect of PDMPs while explicitly accounting for the large mass of zeros. To do this, I use a two-part model [Belotti et al. (2015)]. This framework decomposes estimation into two phases, an estimation of the change in the likelihood of consuming prescription opioids ($Pr(y > 0|\mathbf{x})$) and an estimation of the change in the amount of consumption conditional on consuming positive amounts of prescription opioids ($E[y|y > 0, \mathbf{x}]$). I estimate the first part using a logit model, and I estimate the second part using a generalized linear model with a log link and a gamma family [see Manning and Mullahy (2001) for an explanation of model choice].

An added feature of the two-part model is that the product of the two parts above is the estimated overall effect ($E[y|\mathbf{x}]$). Thus, unlike Column 1 of Table 3.2, the two-part model allows me to estimate PDMP effectiveness while still accounting for different types of positive users. As a stylized example, consider the case where PDMPs only cause respondents with two opioid prescriptions to reduce their use to one prescription. No other changes occur. I would interpret this type of change as a negative effect on access without affecting misuse. The two-part model will capture this interpretation, whereas Column 1 will not.

Given that PDMPs are designed to prevent misuse but not to interfere with non-abusers, we should expect to see the overall effect of PDMPs to be negative. Also, given that only a small fraction of patients misuse, the overall effect should be small.

Table 3.3 show the estimated effects of PDMPs for opioid prescriptions and prescription pills. Panel A shows the overall effect, Panel B shows the first part of the two-part model, and Panel C shows the second part. Note that results for the Panel B are identical for both outcomes, and these are identical to Column 1 in Table 3.2.

Results mostly reflect the above predictions. Estimated overall effects are small, ranging in magnitude of percent change from 0.3% to 3.5%. Three out of four estimates are positive, but the point estimate for PDMP implementation does yield a slight increase in prescription pills. This result is noisy, however, and I cannot rule out a three pill decrease with standard

95% confidence.

3.5.3 Non-Opioid Prescriptions

As a falsification test, I evaluate whether the PDMPs had an effect on non-opioid prescriptions. To do this, I choose three classes of drugs: cardiovascular agents, antibiotics, and all prescriptions other than opioids (Other). Like my primary specification, I re-estimate Equation 3.1 using a dummy representing positive use for each class of drugs.

As expected, the results yield no discernible pattern. Estimated effects are both positive and negative, and the estimated percent change never exceeds 1.9%.

3.6 Conclusion

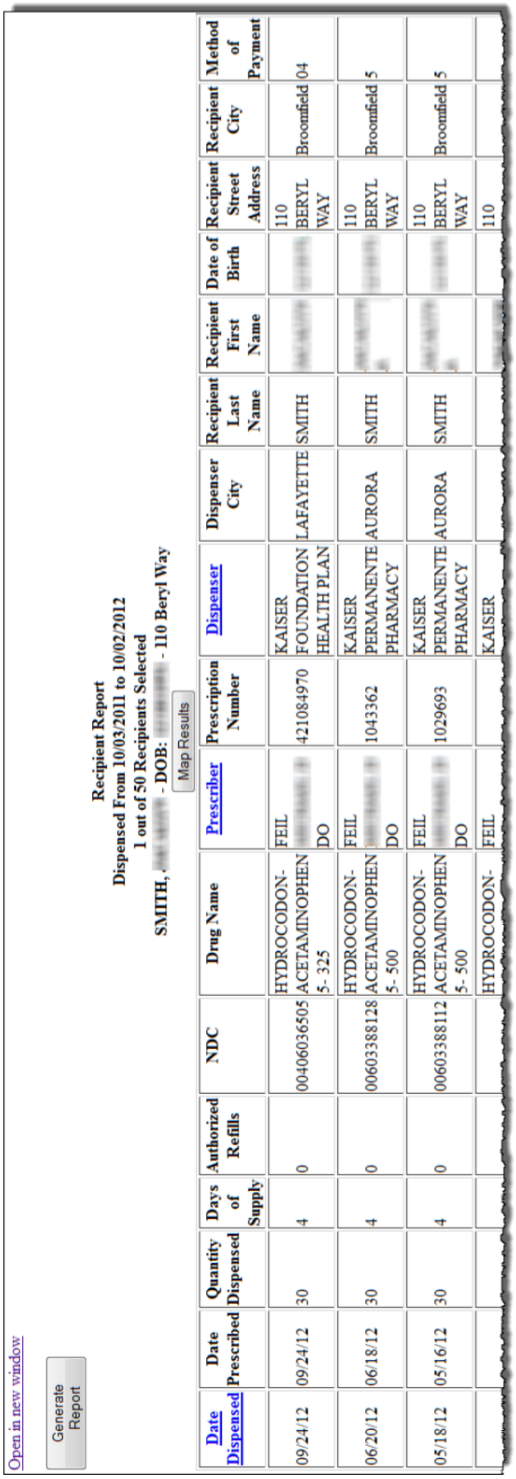
This study indicates that PDMPs have made modest improvements in preventing prescription opioid misuse, especially if they are paired with "must access" laws. These improvements come at the cost of creating a data collection infrastructure that collects real-time prescription data, is readily accessible, and is secure enough to protect patient privacy. These costs should not be ignored. The State of Michigan appropriated $2.47 million to upgrade its system in 2016 and an additional $2.02 million for ongoing maintenance and support [Lawler (2017)]. Nonetheless, by some accounts the current opioid epidemic accounts for as much as $50 billion in economic costs annually [Birnbaum et al. (2011) and Hansen et al. (2011*b*)].

Given these figures, even small improvements in preventing misuse can have potentially outsized effects. An added benefit of PDMPs is that they are designed to target misuse while not affecting patients with medically appropriate reasons to take prescription opioids. By leaving the prescription decisions up to the provider's discretion, well functioning and actively used PDMPs give providers the information they need without burdening them with excessive compliance costs or preventing them from providing treatment. In comparison, blunter policies like pain clinic regulations and prescription limits can increase costs or prevent treatment, even though they have also been shown to have weak effects [Meara et al. (2016)].

To date, the story of PDMPs seems to be incomplete. While all but Missouri has implemented a PDMP, only 18 states have "must access" laws, and many states should consider an upgrade to their system. Likewise, the research on PDMP effectiveness is incomplete. Several early attempts, including this study, yield heartening but mixed results. Despite the need for better answers, surprisingly only nine states require regular evaluation, and six states ban research on PDMP data. Continued efforts to make the data available should be the necessary next steps going forward.

3.7 Figures

Figure 3.1: Screenshot from Training Guide for Colorado Practitioners and Pharmacists [Colorado State Board of Pharmacy (2014)]



Open in new window

Generate Report

Recipient Report
Dispensed From 10/03/2011 to 10/02/2012
1 out of 50 Recipients Selected
SMITH, [redacted] - DOB: [redacted] - 110 Beryl Way

Map Results

Date Dispensed	Date Prescribed	Quantity Dispensed	Days of Supply	Authorized Refills	NDC	Drug Name	Prescriber	Prescription Number	Dispenser	Dispenser City	Recipient Last Name	Recipient First Name	Date of Birth	Recipient Street Address	Recipient City	Method of Payment
09/24/12	09/24/12	30	4	0	00406036505	HYDROCODON-ACETAMINOPHEN 5- 325	FEIL [redacted] DO	421084970	KAISER FOUNDATION HEALTH PLAN	LAFAYETTE	SMITH	[redacted]	[redacted]	110 BERYL WAY	Broomfield	04
06/20/12	06/18/12	30	4	0	00603388128	HYDROCODON-ACETAMINOPHEN 5- 500	FEIL [redacted] DO	1043362	KAISER PERMANENTE PHARMACY	AURORA	SMITH	[redacted]	[redacted]	110 BERYL WAY	Broomfield	5
05/18/12	05/16/12	30	4	0	00603388112	HYDROCODON-ACETAMINOPHEN 5- 500	FEIL [redacted] DO	1029693	KAISER PERMANENTE PHARMACY	AURORA	SMITH	[redacted]	[redacted]	110 BERYL WAY	Broomfield	5
						HYDROCODON-	FEIL		KAISER					110		

Figure 3.2: Year of PDMP Legislation

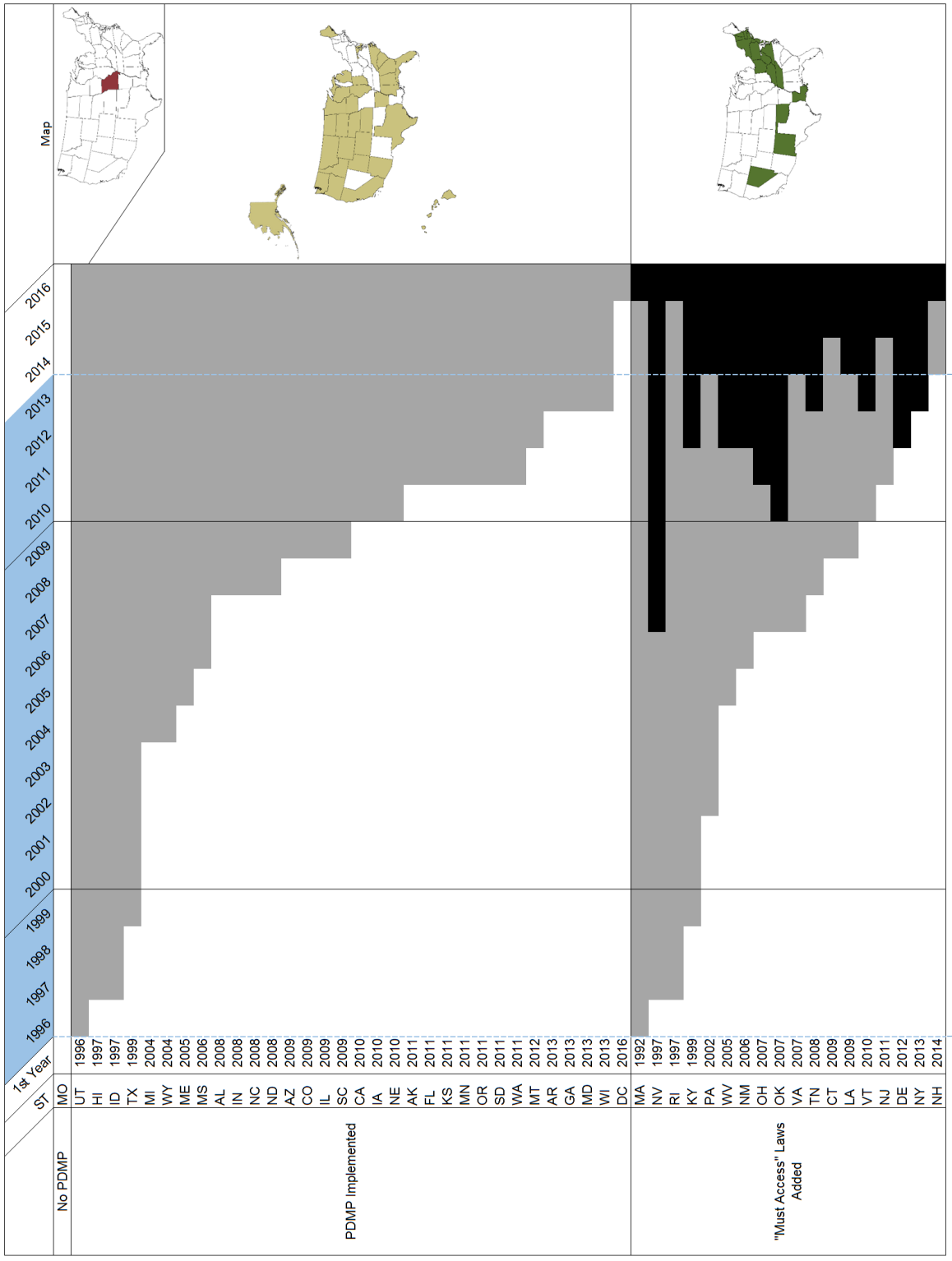


Figure 3.3: Event Study Figures for PDMP Implementation

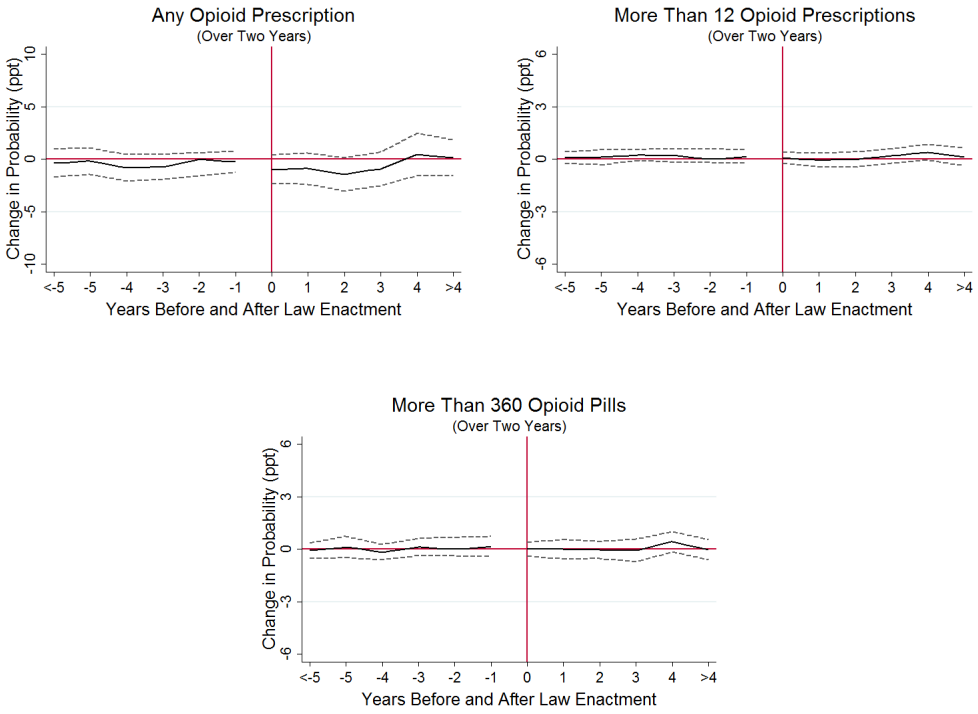


Figure 3.4: Event Study Figures for Adding "Must Access" Laws

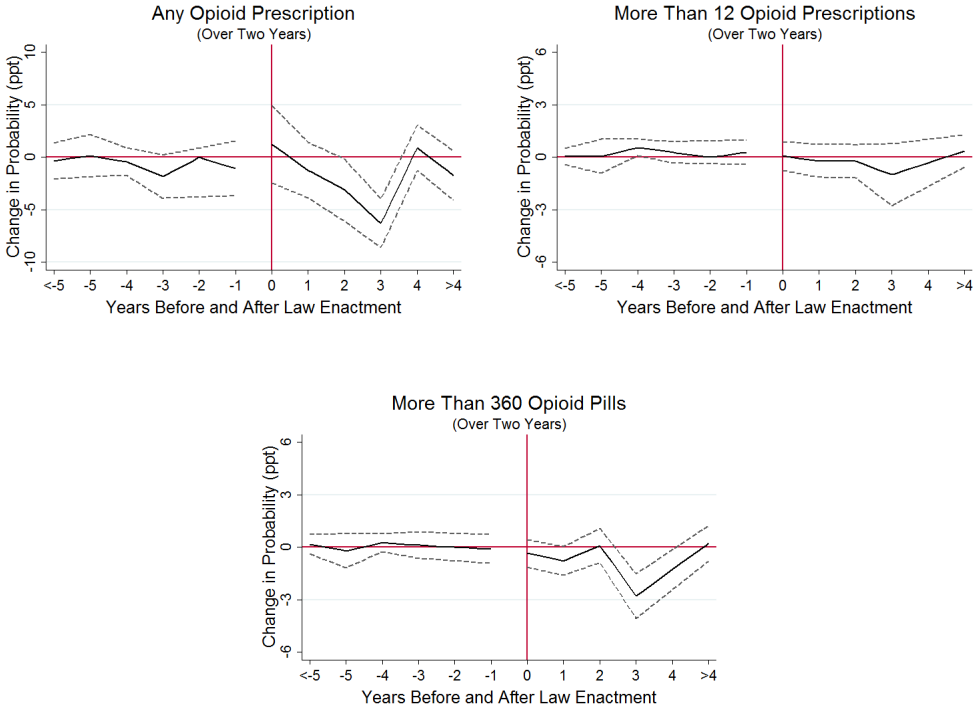
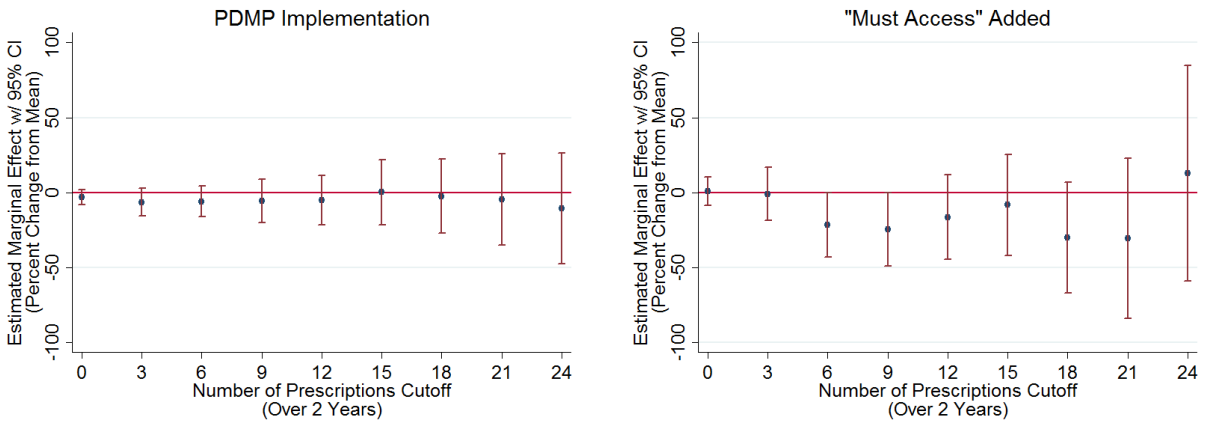
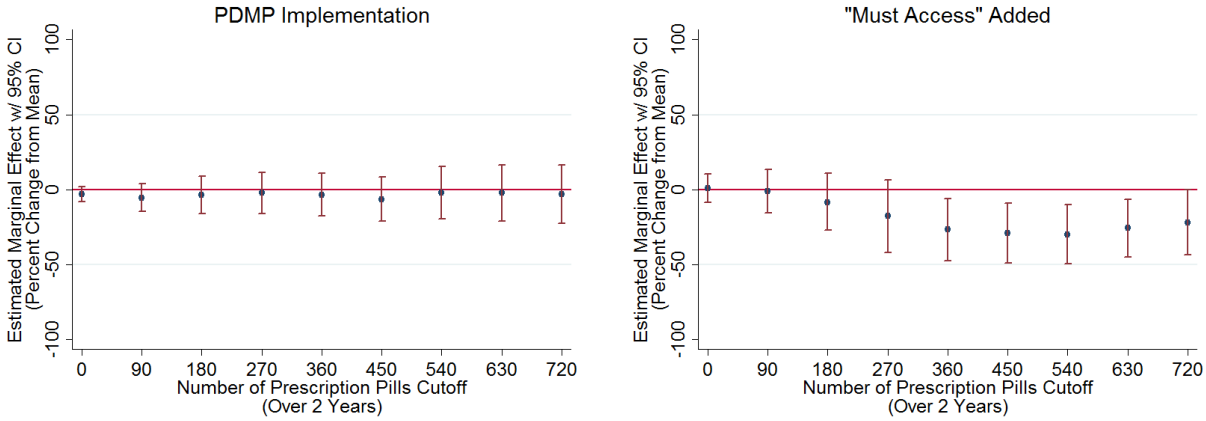


Figure 3.5: Estimated Marginal Effect when Varying Cutoff

A: Estimated Marginal Effects Using Prescription Counts as a Cutoff



B: Estimated Marginal Effects Using Prescription Pill Counts as a Cutoff



All estimates are presented as a percentage change from the pre-PDMP mean. Whiskers represent 95% confidence intervals.

Figure 3.6: Estimated Marginal Effects of "Must Access" Laws on High-Volume Prescriptions
By Medicare Enrollment

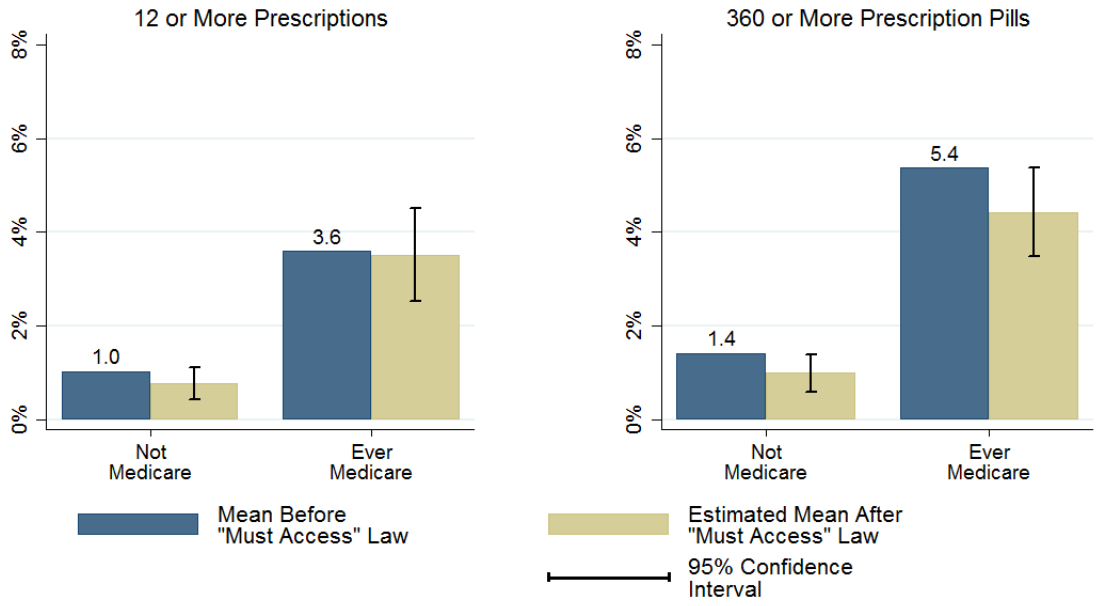
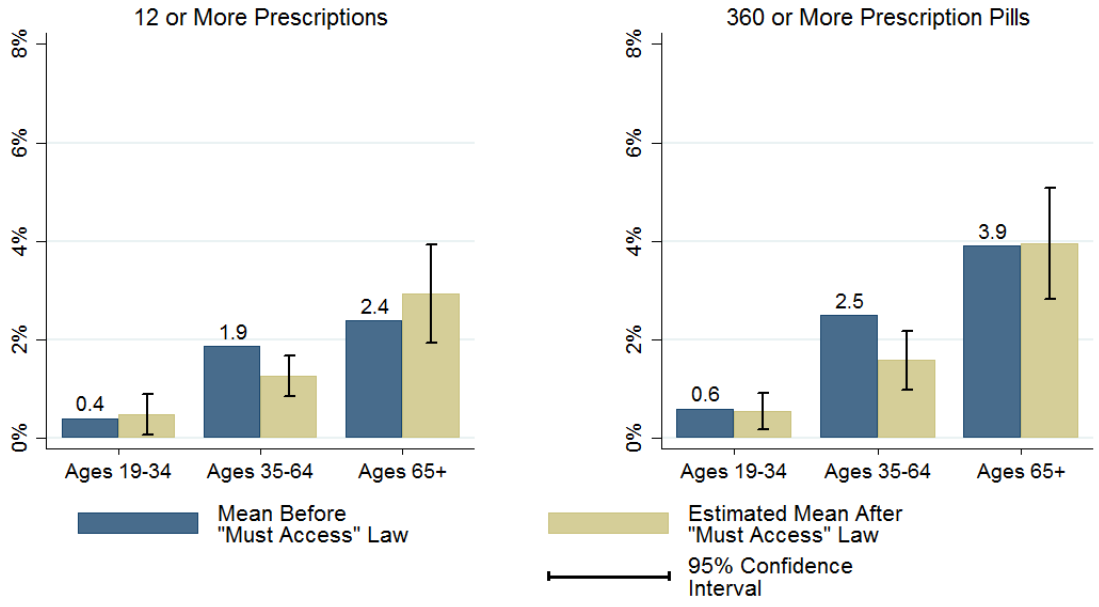


Figure 3.7: Estimated Marginal Effects of "Must Access" Laws on High-Volume Prescriptions
Across Age Groups



3.8 Tables

Table 3.1: Individual Sample Characteristics (Adults Only)

Characteristic	Weighted Mean	Standard Deviation
Female	52.0%	(50.0)
Married	56.0%	(49.6)
Working	69.1%	(46.2)
Hispanic	12.4%	(32.9)
Race		
White	82.2%	(38.2)
Black	11.5%	(31.9)
Other Race	5.9%	(23.5)
Family's Highest Degree		
Less Than High School	11.4%	(31.7)
High School	53.2%	(49.9)
Bachelor or Higher	35.4%	(47.8)
Enrolled in Medicare	19.6%	(39.7)
Enrolled in Medicaid	9.2%	(28.9)
Insurance Status		
Uninsured	13.9%	(34.6)
Insured (0,1) Qtrs	1.8%	(13.2)
Insured [1,2) Qtrs	2.7%	(16.1)
Insured [2,3) Qtrs	3.3%	(17.9)
Insured [3,4) Qtrs	2.8%	(16.5)
Insured All Qtrs	75.5%	(43.0)
Had Inpatient Surgery	8.0%	(27.1)
Age (19 to 90)	46.4	(17.5)
Family Income as a percent of FPL	411.4%	(335.0)
N	195,776	

Estimates represent the weighted average of survey responses using person-level survey weights.

Table 3.2: Primary Specification: Estimated Marginal Effects

Logit Model	Any Opioid Rx	More Than 12 Opioid Rx	More Than 360 Opioid Pills	Any Hospital-ization
PDMP Implementation	−0.60	−0.08	−0.07	0.39
(s.e.)	(0.50)	(0.13)	(0.16)	(0.25)
% Change	−3.1%	−5.1%	−3.4%	2.6%
"Must Access" Added	0.16	−0.25	−0.58**	0.16
(s.e.)	(0.94)	(0.22)	(0.23)	(0.48)
% Change	0.8%	−16.3%	−26.7%	1.0%
Outcome Mean	19.6%	1.5%	2.2%	15.1%
N (Outcome = 1)	37,942	3,482	5,250	29,343
Sample N	195,776			

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before PDMPs are implemented and are weighted using person-level survey weights.

Table 3.3: Two-Part Model: Estimated Marginal Effects

Two-Part Model	Opioid Rx	Opioid Rx Pills
Panel A: Overall		
Outcome Mean	0.97	62.42
PDMP Implementation	−0.003	2.16
(s.e.)	(0.04)	(2.83)
"Must Access" Added	−0.02	−1.77
(s.e.)	(0.07)	(5.22)
Panel B: Logit		
Outcome Mean	19.6%	19.6%
PDMP Implementation	−0.60	−0.60
(s.e.)	(0.50)	(0.50)
"Must Access" Added	0.16	0.16
(s.e.)	(0.94)	(0.94)
Panel C: GLM (Log Link, Gamma Family)		
Outcome Mean	3.96	200.64
PDMP Implementation	0.09	13.72
(s.e.)	(0.12)	(10.50)
"Must Access" Added	−0.12	−8.44
(s.e.)	(0.24)	(19.32)
Sample N	195,776	

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before PDMPs are implemented and are weighted using person-level survey weights.

Table 3.4: Non-Opioid Prescriptions: Estimated Marginal Effects

Logit Model	Cardiovascular Agents	Antibiotics	Other
PDMP Implementation	0.18	−0.08	−0.34
(s.e.)	(0.47)	(0.68)	(0.61)
"Must Access" Added	0.52	0.40	0.76
(s.e.)	(0.70)	(1.11)	(1.25)
Outcome Mean	27.6%	35.9%	62.7%
N (Outcome = 1)	55,702	64,198	115,321
Sample N		195,776	

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Standard errors are clustered at the state-level. Reported outcome means only include observations before PDMPs are implemented and are weighted using person-level survey weights.

APPENDIX A

Other NP Regulations and Laws

As stated in Section 1.2.1, there are three types of NP reforms: (1) practice authority, (2) public and private reimbursement protection, and (3) prescriptive authority. Details of these regulations are outlined in annual surveys published by *The Nurse Practitioner* from 1989 to 2016 [Pearson (1989-2004); Phillips (2005-2016)]. These surveys are completed either by representatives of state nursing organizations or from a member of the state Board of Nursing. For the purposes of this study, I use these surveys to classify each reform into categorical variables for each state in each year. I also cross-check these surveys with state legislation to ensure consistency across time. A reform is attributed to a given year if it takes effect before July 1 of that year. This section reviews the reforms for other than granting NPs prescriptive authority for controlled substances.

I define practice authority by the state entity regulating NPs and by whether the NP is required to work with a physician when diagnosing and treating patients. Prior to the 1990s, many states lacked statutes clarifying this authority. By 2000, however, only two states lacked a clearly specified definition of practice authority, Pennsylvania and Mississippi, and these states passed laws in 2003 and 2010, respectively (see Figure A1). Currently the state's Board of Nursing exclusively regulates NP practice rules and licensure for all but four states. These four states require cooperation with the state's Board of Medicine, which is more restrictive in practice.

NPs can be reimbursed directly or indirectly by insurers for their services. If NPs are reimbursed indirectly, then reimbursement flows through the employer before being shared with the NP. The exact mechanism for how reimbursement is shared indirectly depends on the employment contract of the NP and employer. Medicare began reimbursing NPs directly in 1998 following the Balanced Budget Act of 1997. It currently pays 85% of the physician rate for the same services. Preceding that, the Omnibus Budget Reconciliation Act of 1989 required that state Medicaid programs reimburse pediatric and general practice NPs

directly for their services; however, because many states had not even established a licensing program for NPs by 1989, full implementation of this law did not occur until 1998 (see Figure A2). Medicaid reimbursement rates vary by state and currently range from 75% to 100% of the physician rate. The private insurance market is less consistently regulated. Most states have non-discrimination clauses that require reimbursement (direct or indirect) of NP services, and only seven states require that insurers directly reimburse NPs (see Figure A3). Reimbursement rates in the private insurance market depend on the negotiated contract between insurers and providers.

Prescriptive authority laws typically begin by granting authority for non-controlled substances. Like practice authority, prescriptive authority often requires physician involvement. As of 2016, all states had granted prescriptive authority for non-controlled substances, and 40% of states had granted independent authority (see Figure A4).

Figure A1: Advanced Practice Authorization and Governance Timeline

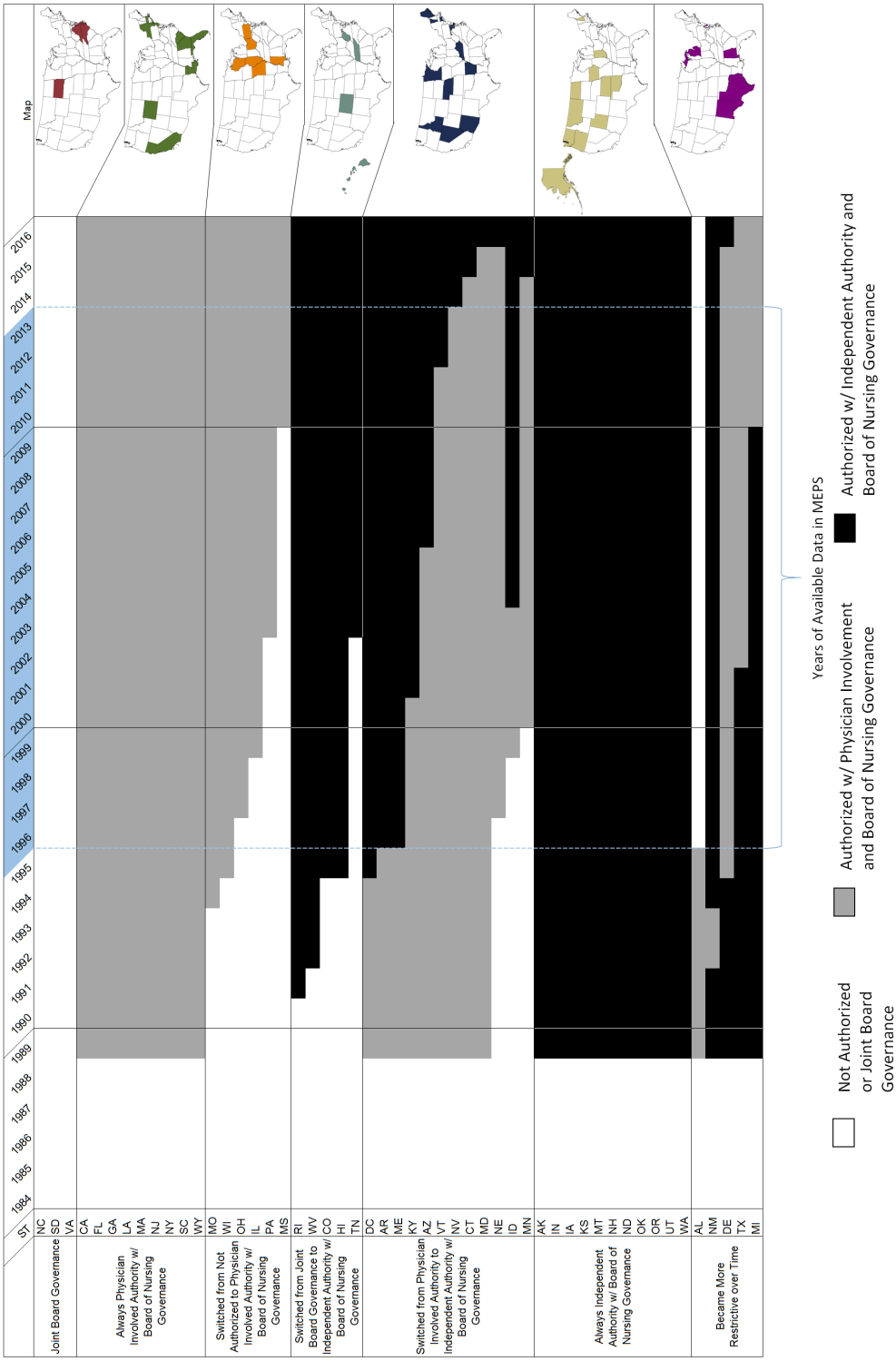


Figure A2: Medicaid Reimbursement Timeline

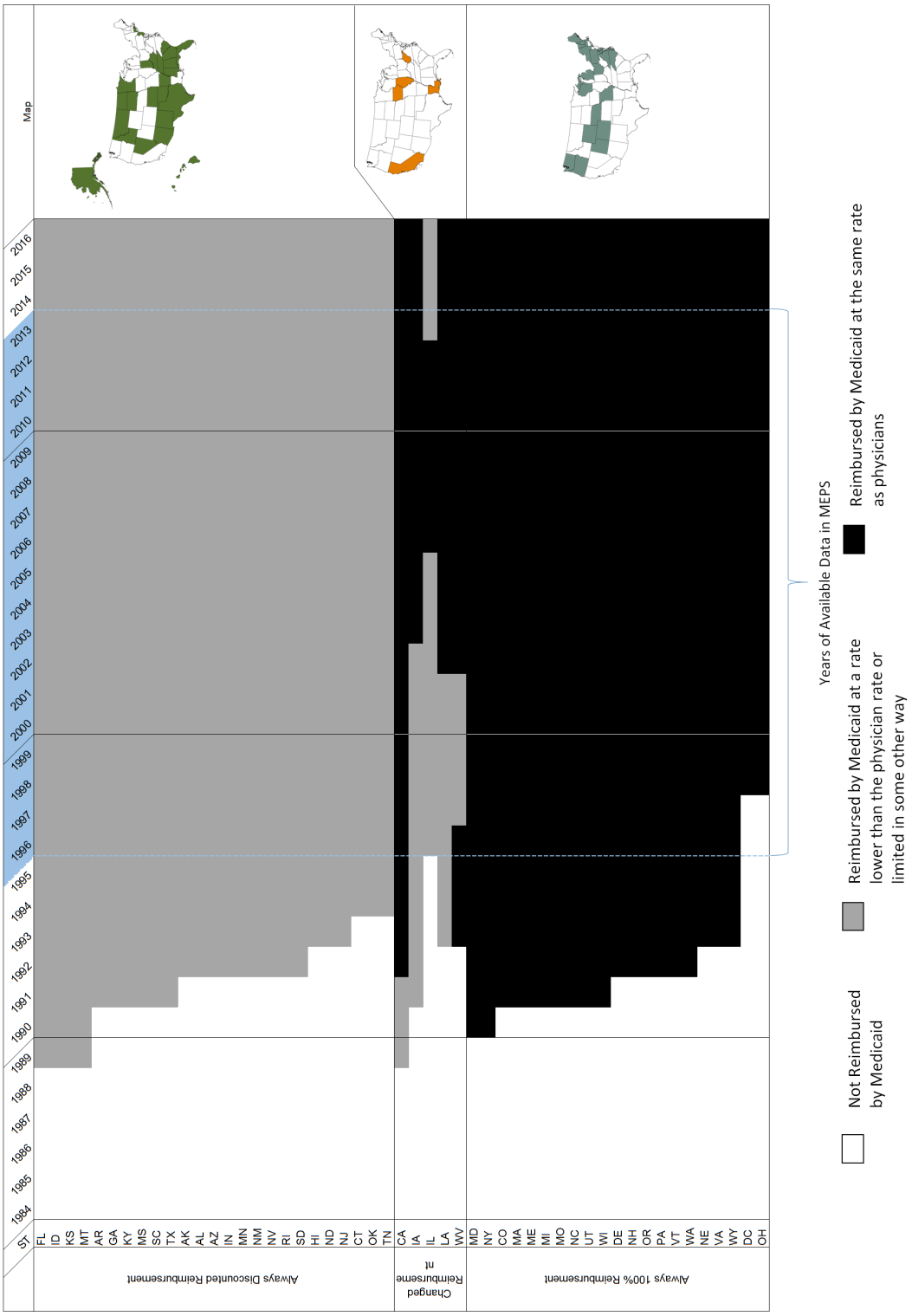


Figure A3: Third Party Reimbursement Timeline

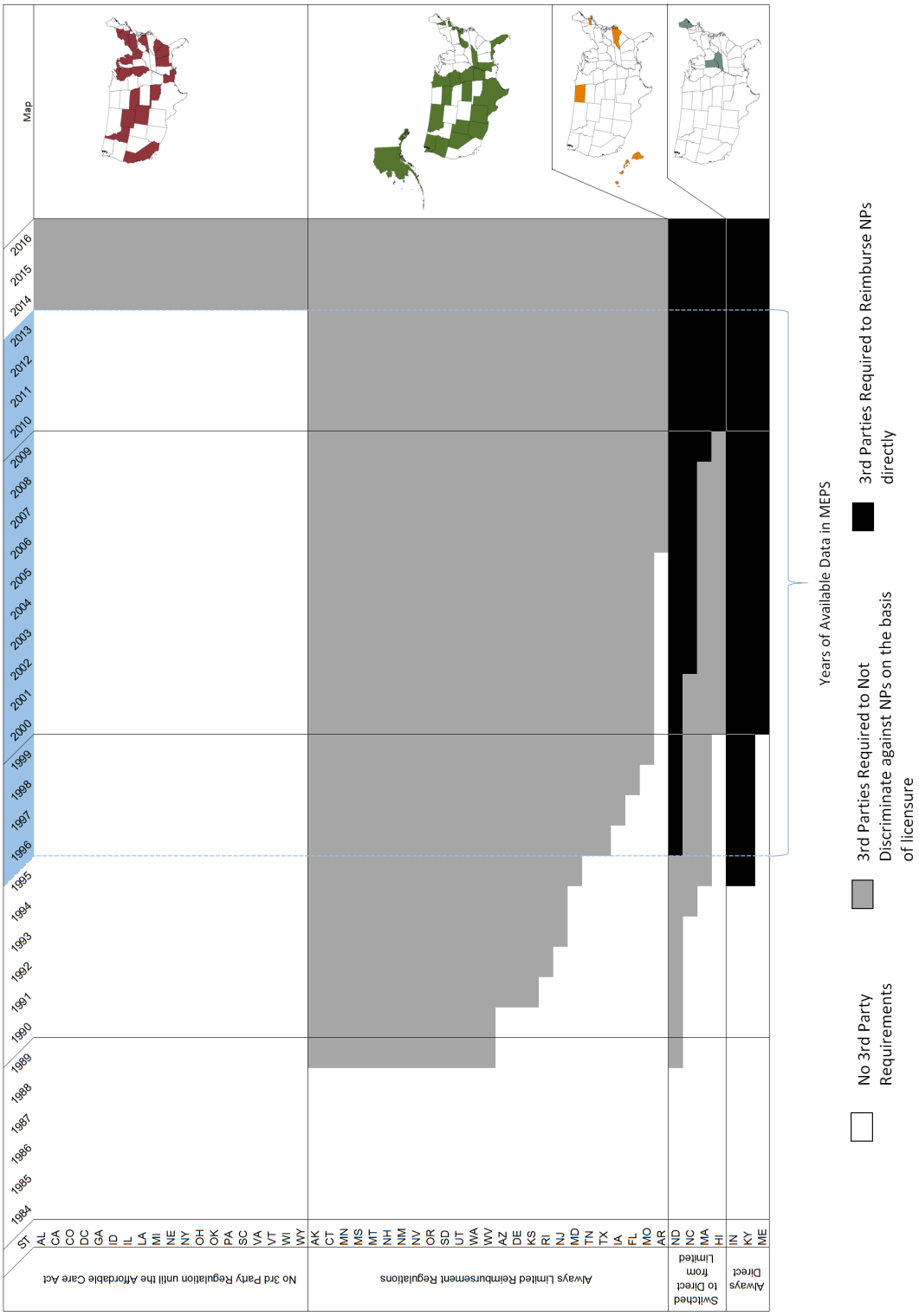
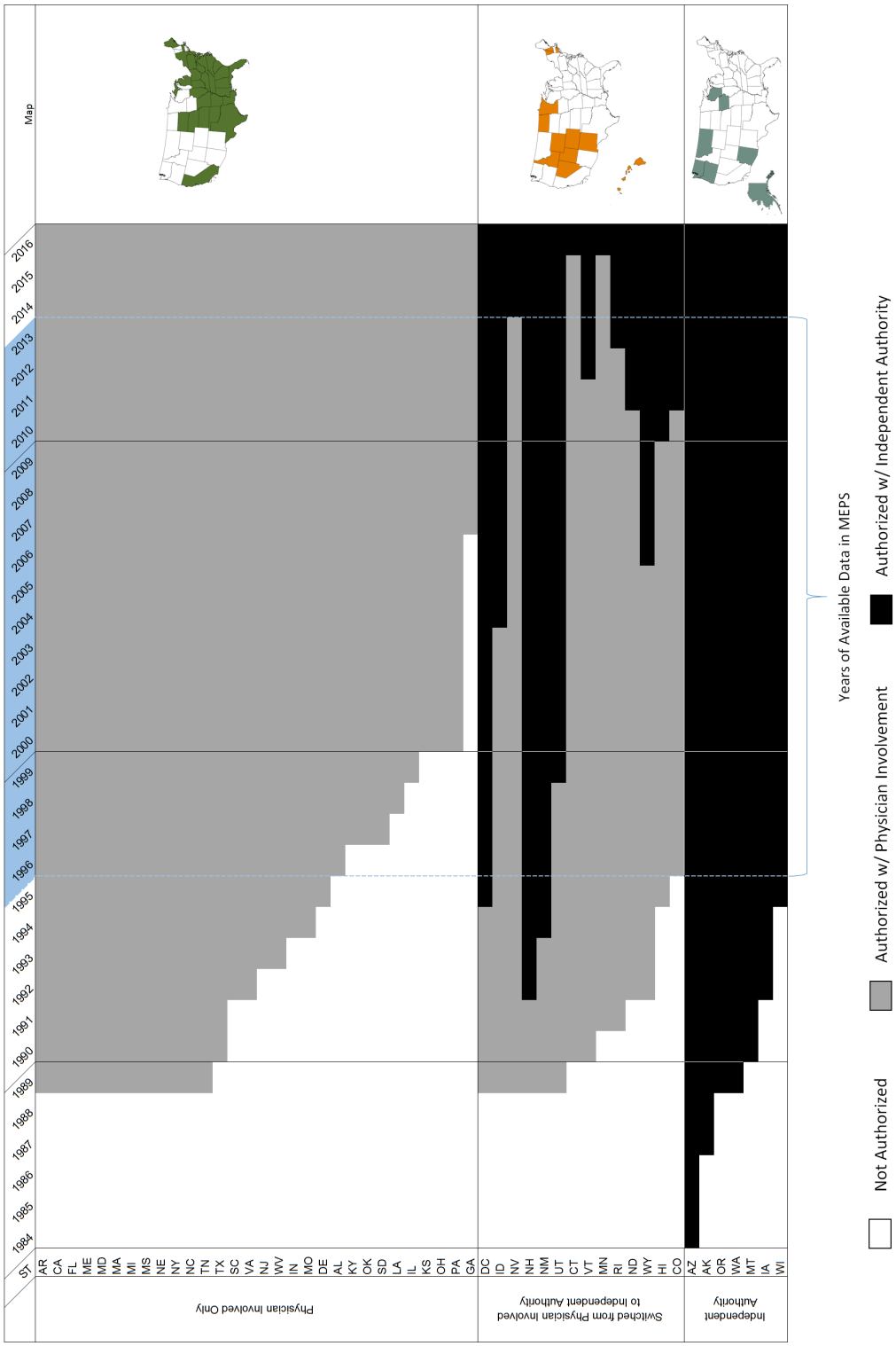


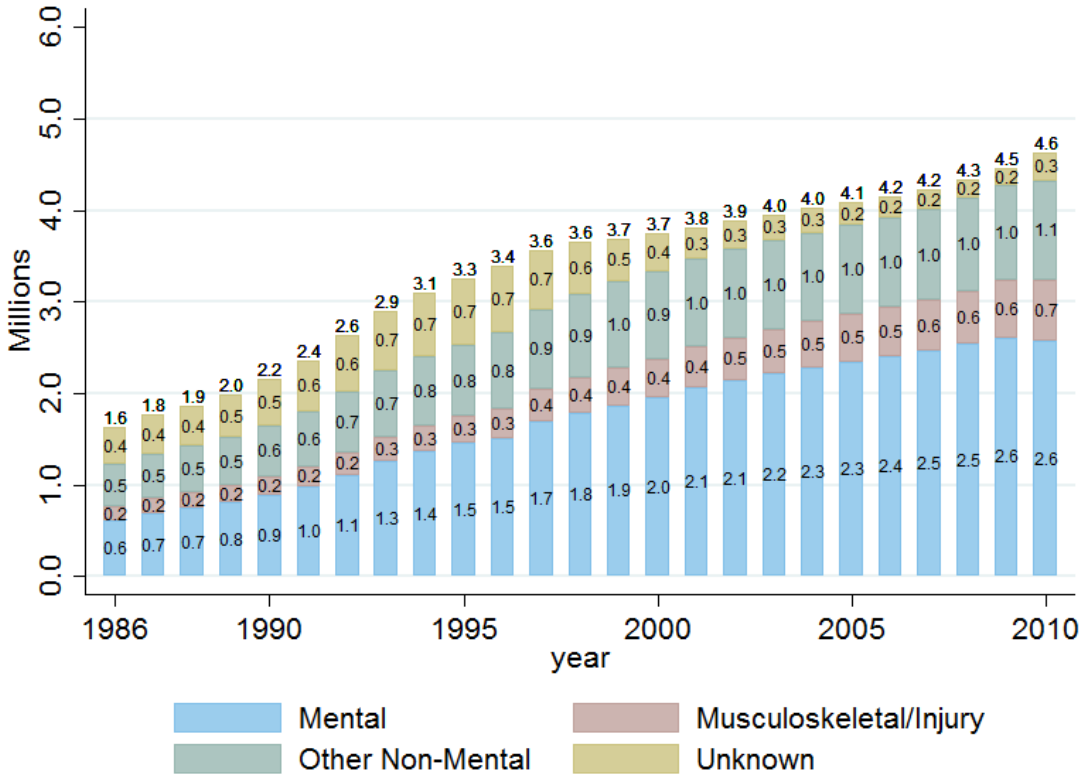
Figure A4: Non-Controlled Prescription Authority Timeline



APPENDIX B

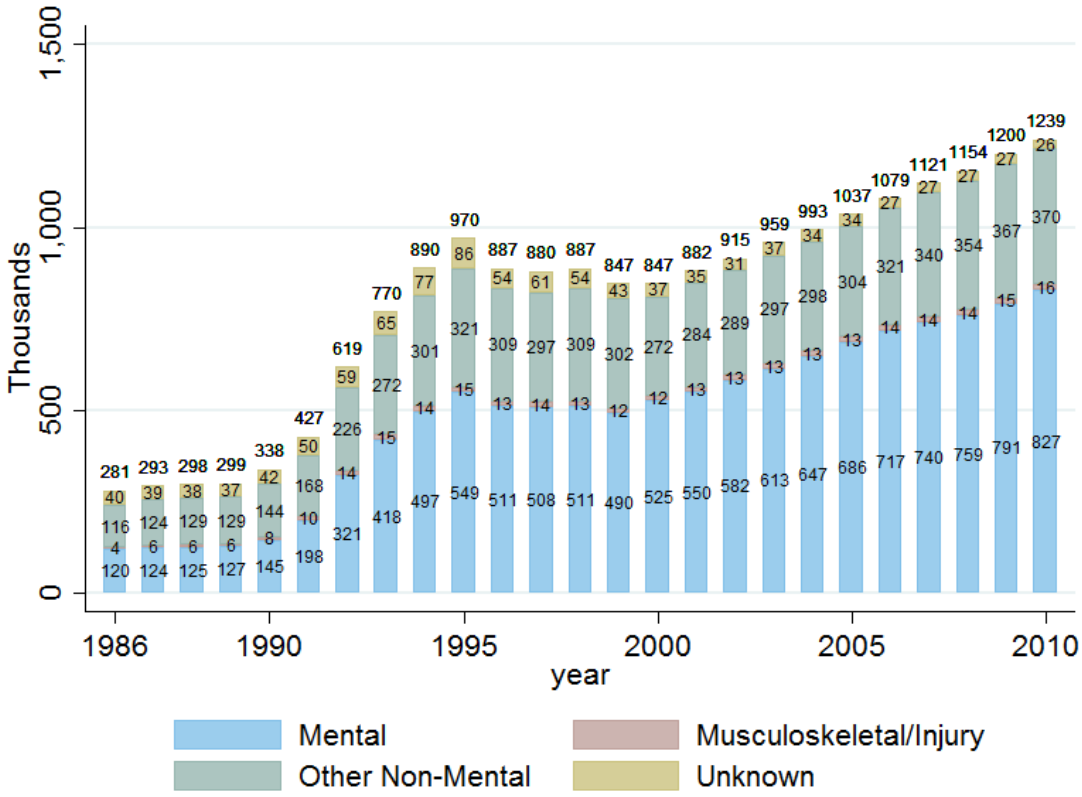
SSI Enrollment by Diagnosis and by Age Group

Figure B1: Trend in Non-Elderly Adult SSI Recipients by Diagnosis



Source: SSA Annual Statistical Supplement (1986-2010). Non-elderly adults represent ages 18 to 64. The lowest group in the figure is Mental (0-64), and the second lowest group in the figure is Musculoskeletal/Injury (0-64). Similar to Figure 2.3, this figure includes raw counts of enrollments, rather than population shares.

Figure B2: Trend in Children SSI Recipients by Diagnosis



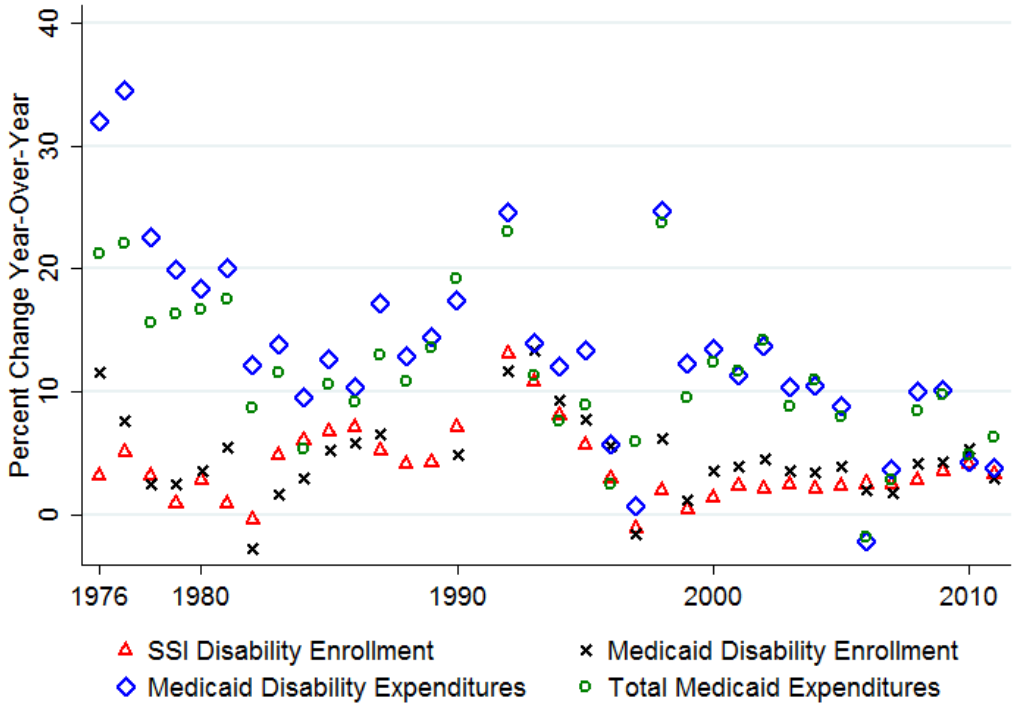
Source: SSA Annual Statistical Supplement (1986-2010). Children represent ages 0 to 17. The lowest group in the figure is Mental (0-64), and the second lowest group in the figure is Musculoskeletal/Injury (0-64). Similar to Figure 2.3, this figure includes raw national counts of enrollments, rather than population shares. Here, we see that mental health disabilities are the driving force for child SSI eligibility, especially following the Sullivan v. Zebley case and subsequent legislation in 1990.

APPENDIX C

Decomposition of Year-over-Year Change

Using year-over-year changes instead, Figure C1 allows us to see each step of the linkage between SSI enrollment and total Medicaid spending. Year-over-year changes in SSI enrollment are strongly correlated with Medicaid disability enrollment. In turn, year-over-year changes in Medicaid spending for disabled enrollees is also strongly correlated with total Medicaid spending.

Figure C1: Delta plots

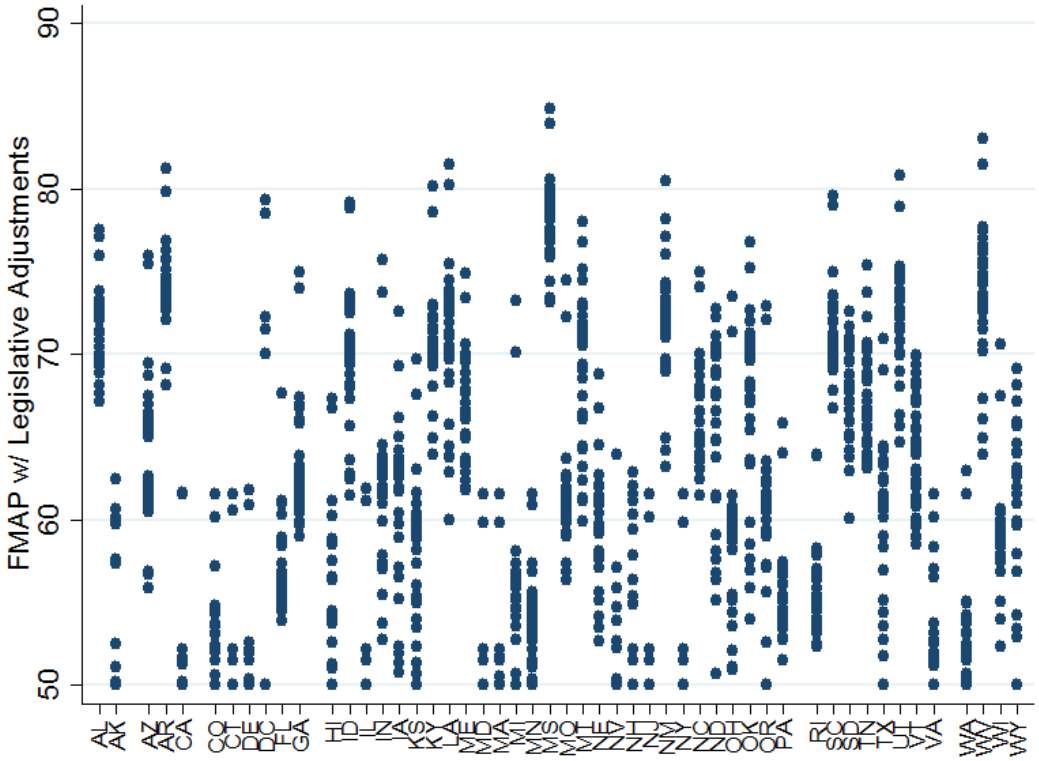


This figure shows the percent change in SSI disability enrollment, Medicaid disability enrollment, Medicaid disability payments, and Total Medicaid payments for each year, from 1976 to 2011. Overall this shows that Medicaid disability closely tracks SSI disability, and Total Medicaid costs closely track disability Mediciad costs. The relationship between Medicaid disability enrollment and Medicaid disability costs is visible but less powerful because of strong secular growth in average patient cost.

APPENDIX D

Distribution of state-level FMAPs

Figure D1: FMAP distribution



This figure shows the overall distribution of the Federal Medical Assistance Percentage within each state over 25 years. In general most states stay within a range of about 10 points. This relationship is particularly strong for states close to the lower bound of the federal matching rate. By design, these states have higher per capita income than states with higher matching rates.

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