# Private vs. Public Provision of Social Insurance: Evidence from Medicaid*

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**Abstract**

Public health insurance benefits in the U.S. are increasingly provided by private firms. We assess the consequences of private provision by exploiting the staggered introduction of enrollment mandates across counties in Texas and New York, which required disabled Medicaid beneficiaries to shift to private health plans. In Texas, where the public program uses strict rationing to control costs, privatization led to higher Medicaid spending but also improvements in healthcare. In New York, where the public program is more generous, privatization did not affect Medicaid spending but resulted in a large decrease in inpatient admissions. We conclude that the consequences of private provision depend critically on the design of the public and private programs.

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# 1 Introduction

The question of whether private firms can provide public services more efficiently than government is fundamental to public policy and economics. Nowhere is the public versus private question more controversial—and perhaps more consequential—than with respect to public health insurance programs in the United States (Gruber, 2017). In Medicaid, the program that provides health insurance coverage to low-income Americans, including low-income people with disabilities, the proportion of beneficiaries receiving their benefits through a private health plan increased from 60% in 1999 to over 80% in 2012 (Congressional Budget Office, 2018). In Medicare, the program providing health insurance coverage to disabled workers and the elderly, about 19 million people (33% of beneficiaries) are in a private medical plan, while all of Medicare drug coverage comes through private Part D plans (Kaiser Family Foundation, 2017*b*). The use of private plans to provide public health insurance benefits is also widespread in several European countries, including the Netherlands, Switzerland, and Germany (McGuire and Van Kleef, 2018).

Prior work on the private provision of public health insurance benefits has produced mixed findings. In theory, competing private plans are incentivized to use the technologies available to them (some of which may not be available to a public program) to efficiently ration access to healthcare services. Profit-maximizing private plans desire to keep spending low because they are often the residual claimants on any savings generated, and the combination of competition for enrollees and regulatory action by government prevents them from rationing "too much." In some contexts, there is empirical evidence supporting this theory (Newhouse and McGuire, 2014; Dranove, Ody and Starc, 2017; Curto et al., 2019). However, when competition is weak and regulatory supervision is lax, the potential gains from private provision may not be realized (Curto et al., 2014; Duggan, Starc and Vabson, 2016; Cabral, Geruso and Mahoney, 2018). Indeed, there is evidence of private provision in some settings costing governments at least as much as public provision (Duggan and Hayford, 2013) while also resulting in reduced quality (Aizer, Currie and Moretti, 2007). Additionally, when coupled with adverse selection, competition may produce harmful instead of beneficial outcomes (Geruso and Layton, 2017; Kuziemko, Meckel and Rossin-Slater, 2018).

In this paper, we study the consequences of the shift to private provision of public health insurance benefits in the U.S. Medicaid program. There are several advantages to using Medicaid to study this question. First, Medicaid is the setting where the public vs. private question is most relevant:

Over 43 million Medicaid beneficiaries receive their health insurance benefits from a private health plan, with $162 billion paid to these plans each year (Centers for Medicare and Medicaid Services, 2016). Second, credible identification is possible due to mandates that shifted Medicaid beneficiaries from public to private Medicaid plans in some counties—but not others—within the same state. Third, and novel to this paper, the existence of variation across state Medicaid programs presents an opportunity to uncover the mechanisms behind the effects of shifting to private provision, facilitating the construction of a more general framework for assessing potential consequences of shifting from public to private provision in other social insurance settings.

To leverage these advantages, we make use of natural experiments in Texas and New York in the mid-2000s, when both states transitioned adults with disabilities—most of whom qualified for Medicaid due to their enrollment in the federal Supplemental Security Income (SSI) program—from the state-run public insurance plan to private Medicaid plans. The transition was mandatory, resulting in a rapid and dramatic increase in the portion of adults with disabilities enrolled in private plans, with private enrollment rising from around 10% to almost 80% instantaneously in Texas. Moreover, Texas and New York implemented this coverage change in only a subset of counties, providing a clean natural experiment that we exploit in a difference-in-differences design. We use this setting to estimate how a variety of relevant outcomes changed differentially in counties where private provision was implemented, relative to similar, contiguous counties that maintained the publicly managed, fee-for-service (FFS) Medicaid program.

Our focus on the disabled Medicaid population is motivated by its complexity and cost. In 2014, Medicaid spending for this population amounted to almost $187 billion or 40% of total Medicaid spending, even though individuals with disabilities make up only 13.5% of total Medicaid enrollment (Kaiser Family Foundation, 2014*a*,*b*). Because the individuals in the SSI population have severe health problems, the consequences of changes to their healthcare are also likely to be more easily observed in the data relative to other, healthier populations. Finally, this is currently the most policy-relevant population with respect to the question of public vs. private provision: While most states have already shifted healthier Medicaid populations to private Medicaid plans, the transition of individuals with disabilities to private plans is either recent, ongoing, or currently under consideration.

We find clear evidence that rationing of most healthcare services is *relaxed* in Texas under private provision. Specifically, we find that private provision increases outpatient medical spending and prescription drug spending in Texas. The increase in outpatient spending comes partly from increased outpatient utilization (8% increase in outpatient services), and partly from private plans paying higher prices (8% higher on average). We interpret these results as suggestive evidence that the supply curve for outpatient services in Medicaid is upward-sloping (consistent with evidence from Medicare in Clemens and Gottlieb, 2014), and that private plans relax rationing of access to care by paying providers higher rates for the same services. In New York, on the other hand, we find no evidence of any difference in rationing of prescription drugs or outpatient services between public and private provision.

With respect to prescription drug spending, we find that strict rationing in Texas's public program is responsible for increased spending under private provision. While both states "carved out" prescription drugs from private plans and continued to instead pay for drugs via their public programs, Texas strictly rationed public drug coverage through a three-drug prescription limit, a limit that was relaxed under private provision. Although not widely known, strict rationing of prescription drugs using *ad hoc* quantity controls is a common feature of public (but not private) Medicaid plans (Council of State Governments Midwest, 2013). Importantly, we find that these caps appear to be binding for a meaningful share of disabled beneficiaries. Indeed, they appear to prevent disabled Medicaid beneficiaries from taking a variety of drugs used to treat the chronic conditions prevalent in this population. For example, we find strong extensive margin responses to the relaxation of the drug cap (under private provision) for insulins, anti-psychotics, anti-depressants, and statins, as well as drugs used to treat asthma and pain. These responses suggest that the drug cap's relaxation may have led to fairly large improvements in quality of life for many of these Medicaid beneficiaries. The relaxation of this limit and the subsequent increase in drug utilization thus represent a second instance of relaxed rationing of access to healthcare services under private vs. public provision.[1] The blunt *ad hoc* rationing of drugs via quantity limits that we document in Texas's public program stands in stark contrast to evidence of more efficient, targeted rationing of prescription drugs by private plans in Medicare Part D (Einav, Finkelstein and Polyakova, 2018).

[1]We note that because drug coverage is "carved-out" of the private plans' contracts (i.e. always paid by the public program) the private plans have little incentive to ration access to drugs. However, in a later year, Texas "carved in" drugs to the private plan contracts. In Section 8, we show that there was no effect of the carve-in on drug utilization, indicating that the degree to which private plans ration drugs is invariant to whether drugs are carved-in or carved-out of the contract, consistent with recent evidence from Dranove, Ody and Starc (2017). This suggests that even though drugs are carved-out at the time of the transition, the shift in drug utilization under private provision provides evidence of public vs. private rationing of drugs.

While rationing was relaxed for drugs and outpatient care in Texas, we find clear evidence that the shift to private provision led to a *reduction* in inpatient spending for individuals affected by the enrollment mandate of at least 8%, consistent with other work on private provision in Medicaid and elsewhere (Van Parys, 2015; Vabson, 2017). Importantly, this reduction is largely concentrated in inpatient admissions related to mental illness, with some evidence in Texas of additional reductions in admissions related to diabetes and respiratory conditions (such as asthma and COPD). While we cannot rule out increased rationing (i.e. "stinting") by private plans, we argue that among Texas plans there was little direct incentive to stint since a unique feature of Texas's contracts with private plans was that inpatient care was "carved out" of these contracts with the state covering inpatient care for beneficiaries in both public and private plans. Because these types of admissions (for mental illness, diabetes, asthma, and COPD) are often considered "avoidable" given appropriate disease management, these reductions are likely a direct product of actions by plans to manage their enrollees' conditions in order to limit costly inpatient events. Further, in Texas we find strong evidence that the decreases in inpatient admissions are related to increased access to prescription drugs. Indeed, we find that the drugs with the largest increases in utilization under private provision tend to treat the conditions associated with the largest decreases in inpatient admissions. Additionally, we find that the groups of beneficiaries that see the largest increases in drug utilization also have the largest reductions in inpatient spending. Taken together, these results suggest that the reduction in inpatient spending reflects an improvement in the quality of healthcare received by Medicaid beneficiaries as well as the actual health of these individuals under private provision rather than stinting by private health plans. Additional analyses of the effects of private provision on other outcomes such as mortality, employment, and exit from the SSI program all result in coefficients whose signs point in the direction of improvements in health and functional capacity, but with confidence intervals that include zero as well as reasonably sized effects in the opposite direction.

In New York we also observed a reduction in inpatient admissions, though the New York reduction was much larger than the reduction in Texas, with individuals affected by the mandate in New York reducing inpatient admissions by 33-50%. While these effects are large, they are consistent with prior work on private provision in Medicare (Duggan, Gruber and Vabson, 2018). Here, stinting is a possible explanation. Indeed, the much larger inpatient reduction we observe in New York may be at least partially explained by the fact that private plans were responsible for inpatient spending in New York while the state continued to pay for inpatient stays for private plan enrollees in Texas. It is also possible, however, that under New York's more generous public program, there was more potentially inefficient inpatient use to cut. Unlike for Texas, we have little evidence to say whether the New York inpatient reductions reflected positive or negative outcomes for Medicaid beneficiaries. Again, we find no significant effects of private provision on mortality, employment, or non-mortality exit from the SSI program.

Finally, we show that the weakened rationing and improved quality under private provision in Texas came at a cost: Fiscal (i.e., program) spending increased by 12% under private provision for individuals affected by the enrollment mandate. This spending increase was driven mostly by capitation payments to private plans being higher than the counterfactual fee-for-service cost of plan-covered services under public provision, and not by the private plans driving up spending on uncovered services that continued to be paid on a fee-for-service basis, even for those under private provision (i.e., drugs). Importantly, however, these spending increases were accompanied by increases in healthcare utilization. Indeed, we find that in Texas the vast majority (80%) of this spending increase was passed-through to providers/beneficiaries, in the form of additional healthcare spending. In New York, on the other hand, we find no effect of private provision on fiscal spending, suggesting little difference in the cost to the state of providing Medicaid benefits via a public plan vs. via private plans.

To summarize, we find that private provision leads to higher spending for the state and weaker rationing of healthcare services in Texas while in New York private provision has little effect on spending for the state but results in large decreases in inpatient utilization. The Texas results go against the conventional wisdom among policymakers that private provision saves money (Lewin Group, 2004), though they are in line with previous findings in the economics literature (Duggan and Hayford, 2013). These results also go against the conventional wisdom among economists that private provision typically leads to worse outcomes in Medicaid (Aizer, Currie and Moretti, 2007; Kuziemko, Meckel and Rossin-Slater, 2018), with strong evidence that Medicaid enrollees in Texas were better off in private plans. The New York results also confirm that private provision leads to lower inpatient use and does not decrease state spending, though there is less evidence of increased access to care or other clearly positive outcomes.

Our findings make an important contribution to the literature on private vs. public provision of social health insurance benefits (see Sparer, 2012, for a comprehensive review of this literature). First, we are able to unpack the "black box" of managed care by revealing mechanisms behind the increases in drug and outpatient spending and the decrease in inpatient utilization we document in Texas. Specifically, we provide suggestive evidence that the relaxation of a public plan drug cap and increases in payments to providers led to increases in drug and outpatient utilization, and in turn to decreases in inpatient admissions. At the same time, we also provide suggestive evidence that carving-out inpatient services (as was the case in Texas but not in New York) may limit the extent to which private plans ration access to these services. Second, our results suggest that the consequences of private provision are more nuanced than previously recognized. At a minimum, our findings suggest that private provision does not cause adverse outcomes in all settings. Furthermore, our results provide suggestive evidence of when private provision might be more vs. less helpful: The shift to private provision led to decreased inpatient use in both states but a much more pronounced relaxation of rationing (along with an accompanying increase in Medicaid spending) in Texas than in New York. This suggests that the shift to private provision may be more consequential in states with stingier public plans (Texas) than in states with more generous public plans (New York). In other words, the design of both the private *and the public* programs matters for determining the effects of public vs. private provision. We interpret this finding as an outcome of a political economy problem where conservative state legislatures are willing to loosen constraints (budgetary and otherwise) on the state Medicaid program if the state moves to private provision, under the assumption that marginal (as well as inframarginal) dollars will be spent more efficiently by private plans. An implication of this is that while one might argue that the state could have achieved similar improvements by weakening rationing in the public plan (by relaxing the drug cap), political constraints may have made such modifications to the public plan infeasible, i.e. there may not be a realistic counterfactual world where Texas relaxed the drug cap without enrolling beneficiaries in private plans.

These findings have important implications not only within social health insurance programs but also for other government services where private provision is common, including education (Epple, Romano and Urquiola, 2017), incarceration (Mumford, Schanzenbach and Nunn, 2016) and defense procurement (Rogerson, 1994), or where it has been considered, including Social Security (Feldstein, 1998), disability insurance (Autor, Duggan and Gruber, 2014), and infrastructure (Winston and Yan, 2011). Indeed, in the last section of the paper, we tie our results to the economics literature on government contracting with private firms. We focus our attention on the two tools available to states when contracting with private Medicaid insurers: exclusion and payment. Most state Medicaid programs (including New York and Texas) divorce plan selection and plan payment by setting payments administratively rather than requesting that insurers "bid" for contracts. Payments are then set to evolve according to the evolution of costs across all insurers chosen to participate in the program. This results in a payment system that is a hybrid of "cost-plus" payment (Bajari and Tadelis, 2001) and "yardstick competition" (Shleifer, 1985). We discuss how such a system may lead to weak incentives for insurers to exert costly effort to reduce healthcare spending, but may also protect the state against the possibility of insurer exit or *ex-post* payments or contract renegotiation (Decarolis, 2014). We conclude that this procurement system may in some cases lead to higher levels of healthcare and fiscal spending (which we observe in Texas), but it may also be the optimal system in this complex contracting environment.

Finally, our findings make an important contribution to the literature on disability insurance. Most of the literature on federal disability insurance programs, including the Social Security Disability Insurance (SSDI) program and the Supplemental Security Income (SSI) program, has focused on the impact of these programs on employment, earnings, and other economic outcomes.[2] The lack of work on this population's medical outcomes is surprising, given that its Medicaid expenditures ($187 billion in 2014) dwarfed expenditures on cash transfers ($48.2 billion). Moreover, the quality of care received through Medicaid by SSI beneficiaries has clear spillover effects on economic outcomes such as employment and earnings, through impacts on health, functioning, and quality of life. In sum, this paper represents an important contribution to the literature on disability insurance because it examines an aspect of disability policy that has been overlooked and because it finds that private provision of Medicaid services ultimately benefits the adults with disabilities in some settings.

[2]In the SSDI context, recent work includes von Wachter, Song and Manchester (2011) , Maestas, Mullen and Strand (2013), French and Song (2014), Moore (2015), Autor et al. (2015), and Gelber, Moore and Strand (2017). In the SSI context, recent work includes Neumark and Powers (2000), Duggan and Kearney (2007), Deshpande (2016*a*), and Deshpande (2016*b*).

## 2 Background

### 2.1 Rationing in Public and Private Medicaid

Unlike most health insurance programs, Medicaid does not employ demand-side cost sharing as a tool for reducing healthcare utilization. There is no deductible, no coinsurance, and typically no copayments for services or drugs.[3] Despite this, Medicaid is widely perceived as a relatively low-cost form of health insurance coverage (Kaiser Family Foundation, 2016). How can this be?

Medicaid instead employs non-cost-sharing tools for rationing access to healthcare. With respect to medical services, Medicaid's primary rationing tool is the level of the fees it pays to providers of healthcare services. Most state Medicaid programs pay notoriously low fees to providers, with only two states (Alaska and Montana) paying more than Medicare and over 30 states paying less than 80% of Medicare fees (Kaiser Family Foundation, 2018). Low prices directly reduce Medicaid spending. They also indirectly reduce spending by lowering the supply of care—fewer providers are willing to treat Medicaid patients compared to those with other forms of coverage. A simple economic model would suggest that, conditional on consumers facing zero prices, lower provider prices would lead to supply "shortages" in places where consumers demand more care than what is available at the price paid by the Medicaid program. As a consequence, state Medicaid programs effectively outsource the rationing of healthcare services to providers, who must choose which (if any) of the many Medicaid enrollees demanding their services they will treat. Low fees may also cause providers to offer lower-quality care (Hackmann, 2019). Texas and New York, the states we study in this paper, both pay notoriously low fees, ranked 37th and 46th among states in terms of how their Medicaid fees compare to Medicare fees (Kaiser Family Foundation, 2018).

With respect to prescription drugs, Medicaid has even fewer rationing tools available. The prices paid by Medicaid programs for drugs are generally determined by formula (see Alpert, Duggan and Hellerstein, 2013, for a comprehensive review). Rebates from drug manufacturers are also largely determined by formula. States can negotiate supplemental rebates, but state Medicaid formularies must include all FDA-approved drugs and can only limit utilization of specific drugs through prior authorization requirements. This greatly reduces states' bargaining power with manufacturers and

[3] Nominal cost-sharing is permitted for some services and drugs, with cost-sharing limits varying by income category. For those below 100% of FPL, the maximum copay ranges between \$4 to \$8 for most medical and drug services (Kaiser Family Foundation, 2017*a*).

the rebates they can obtain. Thus, in order to limit utilization of prescription drugs, states have opted for a more draconian and *ad hoc* (but legal) tool: quantity limits.

The number of states imposing some form of prescription drug cap in their Medicaid programs increased from 12 states in 2001 to 20 states in 2010 (Lieberman et al., 2016). The caps vary from broad caps that apply to nearly all drugs and nearly all populations to highly targeted caps that do not apply to sicker populations, to generic drugs, or to drugs used to treat chronic conditions such as AIDS or diabetes (Council of State Governments Midwest, 2013). They also vary from strict caps of as low as 3 prescription fills up to relatively generous caps of as many as 8 fills, with the modal cap being 4 fills. The two states we study, New York and Texas, differ significantly in their rationing of drugs, with Texas imposing a near-universal cap of 3 drug fills per person per month and New York imposing no quantity limit on drugs.

An alternative cost-containment method for state Medicaid programs is to outsource rationing of healthcare services to private health plans. Under private provision (also known as Medicaid managed care, or MMC), states pay private health plans fixed per-person, per-month fees to provide all or some of the healthcare services covered by the Medicaid program. Private plans can then impose their own rationing tools, which are often much more expansive than those available to public Medicaid programs. On the medical side, private plans construct provider networks that may include some providers that accept patients enrolled in the public Medicaid program and some providers that do not. Private plans independently negotiate fees with these providers and sometimes impose additional prior authorization requirements for certain services or for access to certain providers that may have higher prices or treat patients more intensively. In addition, private plans often use care managers to ensure that patients get needed treatment in order to prevent potentially costly hospitalizations. With respect to drugs, private plans may have more scope to impose prior authorization requirements than the public program, particularly for very expensive drugs or drugs with cheaper substitutes. Moreover, unlike the public program, private plans could exclude some drugs from coverage entirely through the use of closed formularies (Manatt, 2016). For both medical services and prescription drugs, private plans are able to pass financial risk on to providers, rewarding providers who limit spending (via fewer referrals to specialists and lower utilization of tests, labs, etc.) and penalizing providers whose patients' spending levels are unreasonably high.

These tools, plus the incentives provided to private plans to use them, can potentially allow private plans to provide higher-quality care than the public program or to provide care of similar quality at a lower price. However, this is far from guaranteed. The outcomes under private provision depend critically on the design of the program. Particularly important are the roles of regulatory supervision, competition for enrollees, and the structure of payments to private plans. Indeed, in Section 8.4 we characterize the state's objective function and draw on the economics literature on government procurement of services from private firms to discuss how different procurement options may or may not result in the state meeting its objectives related to state spending and the quality of care provided to Medicaid beneficiaries.

### 2.2 Texas and New York Managed Care Programs

Texas and New York both transitioned adults with disabilities out of their publicly managed fee-for-service Medicaid programs and into private Medicaid managed care (MMC) plans during the mid- to late-2000s. Because this transition was much sharper in Texas than in New York, we emphasize the Texas results throughout the paper, using the New York results to help us interpret the results of the Texas analysis. We describe the institutional background in Texas in detail here, while providing a more basic description for New York, as most of our analysis focuses on the Texas transition to private provision and the New York program mimics the Texas program in many ways. In Section 2.3 we also describe the SSI population, which comprises the vast majority of our study sample.

#### 2.2.1 Texas

Texas's Medicaid program is divided into ten service areas, shown in the left panel of Figure 1 (where the 10th service area comprises much of the state and is shown in white). Starting in February 2007, four of those service areas (Bexar, Harris, Nueces, and Travis), all large urban areas of the state, required that all disabled Medicaid beneficiaries over the age of 21 and not dually enrolled in Medicare enroll in a private Medicaid managed care (MMC) plan as part of the STAR+Plus program. Nearly all of these individuals were SSI beneficiaries. We refer to this group of individuals as "adults with disabilities" for the remainder of the paper.

Prior to February 2007, the vast majority of adults with disabilities in Texas were enrolled in a traditional fee-for-service public Medicaid program, under which the state directly reimbursed physicians for healthcare services using the state's fee-for-service price schedule.[4] Starting in February 2007, enrollment in STAR+Plus became mandatory for all adults with disabilities in the four affected service areas, and this group was shifted into private managed care plans on February 1. Prior to February 1 all adults with disabilities received information about the transition and were given an opportunity to choose one of two or three plans available in their service area. Beneficiaries who did not make a choice were assigned to a plan by the state. Adults with disabilities outside these service areas remained in the public Medicaid program.

Under STAR+Plus, instead of directly reimbursing physicians for the services they provide, the state outsourced the provision of healthcare services to private managed care plans, paying those plans a fixed monthly premium or capitation payment for each individual they enrolled. Base payments were set at the county level by independent actuaries. The actuaries took data on prior spending for all adults with disabilities in a given county (in the public or private plans) and used that data to project future spending based on a time trend in healthcare spending plus adjustments for any new services offered. Base payments were set equal to the projected level of spending plus a fixed amount to cover administrative costs ($50 per person per month). In the early years, when data from the public plan was used to project future spending, projected spending was further reduced by around 15% to account for "anticipated managed care savings." Plan payments were then set equal to the base rate multiplied by a budget-neutral risk adjustment factor that accounts for differences in enrollee health status (as recorded in diagnoses on claims) across plans participating in a given service area. Plans then used these payments to pay providers for all healthcare services received by their enrollees, with the exception of any "carved-out" services as discussed in more detail below. Plans were the residual claimant on all healthcare spending for their enrollees, keeping any savings and absorbing any losses generated by healthcare spending exceeding their payments from the state.

Texas selected a limited number of insurers to participate in STAR+Plus through a periodic procurement process that awarded multi-year contracts renewable for a cumulative period of eight years. Throughout our study period, the set of participating carriers included Amerigroup, Molina, EverCare, and Superior HealthPlan, with a subset of two or three of these insurers participating in each service area. The private managed care plans then contracted with physicians and hospitals

[4] Harris County is the only exception to this, as this service area transitioned adults with disabilities to STAR+Plus at an earlier date. Because of this, we omit Harris County from our sample, though we include other counties in the Harris service area.

to provide care to their members, negotiating their own prices and building their own networks of providers. Under the STAR+Plus model, all enrollees were required to choose a primary care physician (PCP) (or were assigned one), and this PCP acted as a gatekeeper to all non-primary care medical services. All members were to have access to a 24-hour nurse line, and the managed care plans were required to contact all members and ascertain need for long-term services and supports (LTSS). Enrollees were also given access to a new benefit: annual wellness check-ups that were not previously covered by Medicaid.

Like many state Medicaid managed care programs at the time, Texas excluded ("carved out") prescription drug services from its contracts with private plans, continuing to pay for all prescriptions on a fee-for-service basis through the public program even for beneficiaries enrolled in a private plan. As a result, the state, rather than the private plan, served as the residual claimant on all drug spending. Additionally, Texas's public Medicaid program capped the number of drugs it would pay for in any given month at just three prescriptions per beneficiary. Such caps are common in public Medicaid programs (Lieberman et al., 2016). Importantly, Texas lifted this cap for beneficiaries enrolled in a private plan, *even though* the state continued to pay for all drugs through the public program. The assumption was that the private plans would use other tools to control healthcare spending, including spending on drugs.

A distinguishing feature of STAR+Plus was the additional carve-out of inpatient services for adults with disabilities.[5] While the carve-out of inpatient services for adults with disabilities may have affected the behavior of private plans, its effects may have been diminished by the fact that the carve out did not extend to the larger Medicaid population enrolled in private plans (unlike the drug carve-out), but was unique to adults with disabilities. Adults with disabilities make up a relatively small share of all private plan enrollees. As a result, managed care plans may not have adopted cost-containment strategies customized for this particular population, and may have instead maintained a single strategy across all populations. This raises the possibility that the overall behavior of managed care plans was only moderately influenced by the carve out of inpatient spending.[6]

[5] This was done to retain eligibility for federal matching of supplemental payments made to hospitals under Upper Payment Limit (UPL) regulations MACPAC (2012). Intended to augment Medicaid's low hospital payment rates, UPL payments are based on the number of FFS inpatient days by Medicaid beneficiaries in the state. Had inpatient services been included in private plan contracts, hospitals would have lost a substantial amount of UPL revenue.

[6] STAR+Plus is also unique in covering various forms of behavioral health and long-term care, in contrast to other states that exclude these services from their managed care contracts. Specifically, STAR+Plus plans cover a comprehensive set of mental health services, including cognitive behavioral therapy. STAR+Plus plans also cover home health services, along with institutional long-term care stays shorter than 4 months in duration. Due to data limitations that make it difficult for

### 2.2.2 New York

While the option of enrolling in a private plan was available to Medicaid beneficiaries in New York as early as the 1990's, it was only introduced to adults with disabilities in the mid-2000's. For this group, private plans became available in different counties at different times, with counties falling into three groups: early transition counties, mainstream transition counties, and late transition counties. The early transition counties consisted of New York City and surrounding counties, while the mainstream and late transition counties were spread throughout the state. In both early and mainstream transition counties, private plans became available to adults with disabilities in the early 2000's, but enrollment was not mandated. In late transition counties, private plans were mostly unavailable to adults with disabilities until after 2010 (the end of our sample period).

Following the introduction of private plan options for adults with disabilities in the early and mainstream counties, New York began a staggered roll-out of private plan enrollment mandates for certain adults with disabilities, with the first mandates implemented in early transition counties in 2007-08, and extended to mainstream transition counties during 2008-09. Throughout this period, certain adults with disabilities remained ineligible for enrollment in private plans and were instead required to be in the public Medicaid plan. These included long-term nursing home residents and dual-eligibles (those Medicaid recipients who were simultaneously enrolled in Medicare). Other adults with disabilities were also exempted from the private plan enrollment mandate, though we are unable to differentiate these exempted individuals from mandated individuals in our data. New York's private Medicaid plans covered inpatient, outpatient, and laboratory services. Meanwhile, certain services remained carved-out of managed care contracts, including prescription drugs, mental health, and long-term care, and consequently remained covered by fee-for-service Medicaid even for beneficiaries enrolled in private managed care plans. Payments to private plans in New York were set by actuaries using methods similar to those used in Texas.

## 2.3 Medicaid and the Supplemental Security Income Program

The majority of adults with disabilities enrolled in Medicaid are eligible for Medicaid due to their enrollment in the Supplemental Security Income (SSI) program. The SSI program is one of the largest

us to differentiate these services from other non-inpatient services, we do not explore the effects of private provision on these services.

welfare programs in the United States, providing monthly payments to more than 8.2 million disabled or elderly beneficiaries in December 2017. Of these, 4.8 million were adults with disabilities between the ages of 18 and 64, and the average monthly payment for this group was $564.34 (Social Security Administration, 2018). For the non-elderly, eligibility for SSI is based on medical criteria as well as income and asset tests. SSI has the same medical eligibility criteria for adults as the Social Security Disability Insurance (SSDI) program, but does not share SSDI's work history requirements. Approximately one-third of SSI beneficiaries are also enrolled in the SSDI program because they have sufficient prior work history for SSDI but low enough income to quality for SSI as well.

SSI beneficiaries are categorically eligible for Medicaid in most states, meaning that they can enroll in Medicaid without having to apply separately.[7] SSDI beneficiaries are categorically eligible for Medicare, making those SSI beneficiaries who also qualify for SSDI dually eligible for both Medicaid and Medicare. In both Texas and New York (as well as in most other states where private provision has been rolled out to adults with disabilities), dually eligible beneficiaries were excluded from the shift to private managed care plans. Thus, our analysis focuses on the two-thirds of SSI beneficiaries who were not also eligible for SSDI.

Cash benefit payments for disabled SSI beneficiaries quadrupled between 1990 ($12.2 billion) and 2017 ($48.2 billion) (Social Security Administration, 2018); however, these expenditures are dwarfed by Medicaid expenditures for this population— $187 billion in 2014 (Kaiser Family Foundation, 2014*b*). Adults with disabilities are the most expensive group in Medicaid, with per capita spending equal to $16,859 in 2014, almost five times higher than per capita spending for adults without disabilities ($3,278) (Kaiser Family Foundation, 2014*c*). One reason for this higher spending profile is that SSI beneficiaries disproportionately qualify for the program due to mental disorders: 57.4% of SSI beneficiaries qualified for SSI due to a mental disorder, with intellectual disabilities (19% of beneficiaries who qualify due to a mental disorder) being the largest sub-category, followed by mood disorders (16%), and schizophrenic and other psychotic disorders (8.9%). After mental disorders, the next largest categories are musculoskeletal disabilities (13%) and nervous system disabilities (7.7%) (Duggan, Kearney and Rennane, 2015). Thus, this population differs greatly from the average non-disabled Medicaid beneficiary and even from the typical SSDI beneficiary, in its high prevalence of mental illness, indicating a high level of need for mental healthcare. Also contributing to high costs

[7] 10 states have stricter criteria, while 7 states require a separate application but have no additional criteria. In Texas and New York, Medicaid eligibility is automatic for SSI beneficiaries

is the fact that individuals in this population suffer from multiple serious health problems. This suggests that (1) the tools of managed care may be particularly effective for this group and (2) strict rationing in public FFS Medicaid programs (such as Texas's three drug cap) is likely to be binding for this group and could potentially have detrimental (and observable) health effects.

# 3 Data and Sample

We use several administrative datasets from the Centers for Medicare and Medicaid Services (CMS) for the state of Texas for 2004-2010 and for the state of New York for 2006-2010. These datasets contain information on Medicaid enrollment status as well as healthcare utilization in the inpatient, emergency department, outpatient, and prescription drug settings. Uniquely, the data allow for construction of an individual-level panel of utilization, which covers everyone in public as well as private Medicaid plans, including those switching between the two. Furthermore, inclusion in these data is not conditional on utilization of healthcare; this stands in contrast with hospital discharge data (often used in the Medicaid literature) where individuals are only observed if they are utilizing care. These data are also unique in tracking outpatient and prescription drug utilization in addition to inpatient care, allowing us to build a more complete picture of patient care compared to past studies that have investigated inpatient care alone. This is particularly valuable, given that non-inpatient care accounts for over 65% of this population's healthcare spending.

Using these data, we can precisely identify the cohorts of interest in our analyses. Specifically, we restrict our analysis samples to Texas residents who were enrolled in Medicaid in a given month during 2004-2010 and New York residents who were enrolled in Medicaid in a given month during 2006-2010, who qualified for the program on the basis of disability, and who were not simultaneously enrolled in Medicare. Finally, we restrict our main analyses to individuals over 21, because private Medicaid plan enrollment always remained optional in Texas for those under 21.

## 3.1 Beneficiary Characteristics and Enrollment Information

We obtain information on beneficiary characteristics and enrollment status from the CMS Medicaid Analytic eXtract (MAX) Personal Summary (PS) files, which contain person-month-level enrollment status in Medicaid as well as Medicare. For individuals enrolled in Medicaid, these files identify whether their Medicaid coverage in a given month comes through public or private Medicaid plans. These files also identify the basis for each beneficiary's eligibility for Medicaid, such as through qualification for SSI, Temporary Assistance for Needy Families, or other eligibility pathways.

## 3.2 Inpatient, Outpatient, and Prescription Drug Utilization Data

We track inpatient, outpatient, and prescription drug utilization using claims-level information from the MAX Inpatient (IP), Other Therapy (OT), and Prescription Drug (Rx) files. These data track claims paid by the public Medicaid program as well as those paid by private Medicaid plans. The public data capture all healthcare utilization for those in public Medicaid, as well as utilization of carved-out services for those in private Medicaid plans.

Previous work comparing private and public provision of social health insurance has suffered from data quality issues arising from differential reporting of service use under the public and private programs. Our work does not face these issues for some categories of services but does potentially suffer from reporting issues for other categories. We therefore describe data quality for each broad category of healthcare services (inpatient care, non-inpatient medical services, and prescription drugs) in turn.

### 3.2.1 Prescription Drug Data

As discussed in Section 2, prescription drugs in both states are "carved-out" of private plan contracts. This means that they are always paid by the public program both for beneficiaries enrolled in the public program and for beneficiaries enrolled in a private plan for their medical benefits. There is thus no change in the source of the prescription drug claims data as beneficiaries shift from public to private plans, which means there is no concern about differential reporting affecting our estimates of the effects of private provision with respect to prescription drugs. The prescription drug data include the prescription cost, the dates on which the prescription was written as well as filled, the days' supply associated with the fill, and the drug identifier (NDC code), which we link to external data in order to group drugs by therapeutic class. Based on a drug's therapeutic class, we are able to identify the types of chronic conditions that it could be meant to treat.

### 3.2.2 Inpatient Data

Inpatient services are treated differently in Texas and New York. In Texas, they are carved-out of private plan contracts, implying that, like prescription drugs, there is again no concern about differential reporting confounding our estimates of the effects of private provision. However, in New York, inpatient services are included in private plan contracts, implying there is a change in the source of inpatient claims data pre- vs. post-introduction of private provision. Fortunately, we have acquired a separate inpatient discharge dataset covering the state of New York, which includes the universe of inpatient admissions and where discharges are reported by the hospital rather than the payer. In these data, there is no change in the source of the data before and after the introduction of private provision in New York. We linked these data to our Medicaid enrollment records via Social Security Number and birth date, providing a second source of inpatient data at the level of the individual that does not suffer from differential reporting. We use the records from the discharge data to validate results using records from the claims data. Thus, in both states, there is no concern about differential reporting affecting our estimates of the effects of private provision with respect to inpatient services.

The inpatient utilization data record the date of each hospital visit, as well as the type of hospitalization, length of hospital stay, set of procedures performed, and total visit costs (in Texas but not in New York). Using this information, it is possible to classify hospitalizations into various relevant categories, including elective, emergency, and surgical admissions.

### 3.2.3 Outpatient Data

Unlike for inpatient services and prescription drugs, for outpatient services differential reporting could potentially be a concern. While outpatient data for Medicaid beneficiaries enrolled in public plans comes from fee-for-service claims paid directly by the state, outpatient data for private Medicaid beneficiaries comes from claims paid by the private plans themselves and then reported to the state. A specific concern is under-reporting of visits by private plans (Lewin Group, 2012). This concern is less applicable to our setting because private plans had already provided coverage to other Medicaid populations for many years, allowing time for issues with data reporting to have been worked out. Concerns are also mitigated by our finding of generally *higher* outpatient utilization under private Medicaid plans, since under-counting of private plan visits would most plausibly produce the opposite effect. This suggests that if there is a reporting issue, our estimates of outpatient utilization increases are a lower bound for the true effect of private provision. However, the possibility for differential reporting does make it difficult to differentiate between short-term effects of the shift to private plans and changes in reporting.

That said, while the outpatient claims data appear to be of sufficiently high-quality to allow for analyses of changes in aggregate outpatient utilization (spending and number of days with an outpatient claim), inconsistencies appear as outpatient utilization is broken down into finer categories of services. Specifically, it appears that private plans and public plans code specific outpatient services differently, making disaggregation of the effects of private provision on outpatient utilization infeasible. Dissaggregation of the effects of private provision on inpatient and prescription drug utilization, however, is completely feasible due to the consistent source of the data across the public vs. private divide.

The Texas outpatient data includes information on actual cost amounts for both the public *and the private* programs. Specifically, the data contains the negotiated amounts actually paid to providers by the public or private plans at the claim-line level. These actual provider payment amounts are available for all public Medicaid claims in both states, as well as for about 80% of all private Medicaid plan claims in Texas. For the 20% of Texas private plan claims missing cost information, we are able to impute this information, based on median observed private Medicaid rates for a given procedure. After imputation, we observe payments for 99.6% of private Medicaid claims. Unfortunately, the New York data only include payment amounts for public plan claims; payment amounts for private plan claims are missing.[8]

### 3.3 Government Expenditure Metrics

We construct beneficiary-level measures of government (state + federal) Medicaid expenditures using information contained in the CMS MAX files. We define government spending as the sum of any spending on healthcare services paid directly by the government and any premium payments paid by the government to private Medicaid plans. Spending on healthcare services paid directly by the government consists of spending on all services for beneficiaries enrolled in the public Medicaid plan and carved-out services for beneficiaries enrolled in private Medicaid plans. This spending is observed directly in the fee-for-service claims appearing in the inpatient, outpatient, and prescrip-

[8]We again note that records of outpatient services performed are present in the New York data. It is only the payment amount that is missing.

tion drug files. Monthly premium payments paid by the government to private Medicaid plans are also directly observed in the MAX files for beneficiaries enrolled in private plans. We measure total government spending as the sum of these two forms of spending.[9]

# 4 Empirical Framework

## 4.1 Control and Treatment Counties

To study the effects of private provision of Medicaid for adults with disabilities, we leverage the introduction of the STAR+Plus program to four of the ten Medicaid service areas in Texas (Bexar, Harris, Nueces, and Travis) starting in February 2007. As discussed in detail in Section 2.2.1, at the time of the introduction of STAR+Plus to these service areas, all eligible disabled Medicaid beneficiaries who were not also eligible for Medicare were disenrolled from Texas's public Medicaid program and enrolled in a private Medicaid plan. Disabled beneficiaries residing in other service areas remained in the public program throughout the study period. We thus use a difference-in-differences strategy to estimate the effects of private provision.

Treatment (red) and control (blue) counties are shown in the middle panel of Figure 1. The set of treatment counties is defined as any county in the affected service areas that is contiguous to at least one county in an unaffected service area. The set of control counties is similarly defined as any county in the unaffected service areas that is contiguous to at least one county in an affected service area. Table 1 shows summary statistics for the treatment counties, the contiguous control counties, as well as the full set of non-treatment counties in the state. The summary statistics reveal that for many variables all three groups of counties look similar. For most variables, however, contiguous control counties are more similar to treatment counties than the full set of non-treatment counties. These differences are likely due to the fact that STAR+Plus was implemented in urban areas of the state, while the vast majority of Texas is sparsely populated and rural. By implementing the contiguity requirement, we restrict to relatively populated control counties, making the treatment and control groups much more comparable.[10] In Appendix C we provide additional results where we zoom in

[9]Private plan premium payments include $50 per person per month in administrative costs. Because administrative costs are not observed for public Medicaid enrollees, in examining the effects of the shift to private provision on total Medicaid spending we remove $600 per person-year from private Medicaid premium payments. This allows us to study the effects of private provision on Medicaid spending *related to healthcare*. These estimates necessarily abstract from any additional spending or savings on administrative costs due to private provision.

[10]An alternative strategy would be to use urban counties where STAR+Plus was not rolled out as control counties. These

on zip codes on the service area borders, requiring that treatment and control zip codes be within 25 miles of each other.

In addition to the contiguity restriction, we divide control counties into four groups, matching the four service areas where STAR+Plus was implemented. These groups are illustrated in the right panel of Figure 1. We use these groups to construct a set of indicators we refer to as "service area grouping"-by-quarter fixed effects. For each service area, the indicator is equal to one if the individual resides in either a treatment county or a control county assigned to that service area grouping, as illustrated in the right panel of Figure 1. We include these fixed effects in all regressions to control for any local shocks in healthcare utilization. The inclusion of these fixed effects effectively ensures that a particular treatment county is compared only to control counties that are contiguous to counties in the treatment county's service area.

The left panel of Figure 2 shows the portion of disabled Medicaid beneficiaries in our sample who enrolled in a private Medicaid plan in treatment and control counties in Texas for each month between January 2004 and December 2010. STAR+Plus was introduced in the treatment counties in February 2007. It is clear that the switch from the public program to private plans was swift and sharp. Effectively overnight, the portion of disabled Medicaid beneficiaries enrolled in a private Medicaid plan in treatment counties went from around 10% to almost 80%.[11] This sharp variation in enrollment in private plans is the variation we exploit to identify the effects of private provision. While take-up of private plans is sharp, it is not complete. There are several possible reasons for this. First, some groups within the disabled population were exempted from the requirement that they enroll in a private managed care plan. Unfortunately, our data does not allow us to identify these exempted groups. Second, upon enrolling in Medicaid, individuals receive retroactive coverage for any healthcare expenditures they may have incurred in the previous three months. This retroactive coverage is provided by the public Medicaid program, appearing in our data as enrollment in that program.

To provide additional evidence on the consequences of private provision, we exploit a similar county-level transition to private Medicaid plans in New York, described in Section 2.2.2. We define mainstream transition counties with at least one contiguous late transition county as treat-

counties would potentially include the El Paso and Houston areas. Unfortunately, the state rolled out other programs in these cities around this time that make this infeasible.

[11] In our analyses we drop all beneficiaries who are enrolled in a private plan at any point before February 2007.

ment counties and late transition counties with at least one contiguous mainstream transition county as control counties. We drop early transition counties entirely, as these are almost all in the New York City metropolitan area, for which there is no comparable control. This results in the treatment/control/other county classification shown in the middle panel of Appendix Figure B1. We also group counties by rating area according to the New York Health Insurance Marketplace rating areas, constructing rating area-by-quarter fixed effects that we include in all regressions to control for any local shocks that might be related to healthcare utilization and to ensure that treatment counties are only compared to local control counties. These rating areas are shown in the left panel of Appendix Figure B1 and they are shown applied to our treatment/control county categorization in the right panel of Appendix Figure B1.

The right panel of Figure 2 shows the portion of adults with disabilities enrolled in a private Medicaid plan in treatment and control counties in New York. It is clear that the transition to private plans is much more gradual than in Texas. Throughout our study period, enrollment in private plans is increasing gradually, but between July 2008 and January 2010, the rate at which enrollment is increasing increases modestly due to the roll-out of enrollment mandates in the treatment counties. This transition period is indicated by the red vertical lines. Because take-up in New York is much more gradual and far less complete, we emphasize the Texas results throughout, but use the New York results to help us draw inferences about mechanisms.[12]

### 4.2 Regression Framework

Because take-up is incomplete, we present reduced form estimates as well as instrumental variable (IV) estimates. The IV estimates are local average treatment effects (LATEs) specific to the population of disabled beneficiaries who complied with the private plan enrollment mandate (70% of the population). Our reduced form specification is a difference-in-differences specification in event-study form:

$$Y_{it} = \beta_0 + \sum_{t=Q1\_2004}^{Q4\_2010} \beta_t Treat_{it} + \alpha_{st} + \gamma_i + \epsilon_{it} \tag{1}$$

where $Y_{it}$ is the outcome of interest, $Treat_{it}$ is an indicator equal to one if person $i$ is living in a treatment county in quarter $t$ and zero otherwise, $\alpha_{st}$ represents the full set of service area-by-quarter

[12]To address the slow upward trend in private plan enrollment during the "pre-period", we drop all beneficiaries who are enrolled in a private plan prior to the start of the transition period, only identifying off of changes in private plan enrollment occurring after July 2008.

fixed effects illustrated in the right panel of Figure 1, and $\epsilon_{it}$ represents a random error term. We also include a full set of individual fixed effects, $\gamma_i$ to ensure that our estimates are not driven by differential changes in the composition of Medicaid enrollees over time in treatment vs. control counties. For our primary outcomes, we also include estimates from regressions without individual fixed effects.

We also estimate reduced form results pooled over the pre period (Q1_2004-Q4_2006) and post period (Q1_2007-Q2_2010) using the regression

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} \times Post_t + \alpha_{st} + \gamma_i + \epsilon_{it} \tag{2}$$

where $Post_t$ is an indicator equal to 1 for any quarter during the post period (Q1_2007-Q4_2010). Here, $\beta_1$ represents the differential change in the outcome in treatment vs. control counties averaged across the entire post-period.

Our IV specification uses the county-level mandates as an instrument for enrollment in a private plan. The first stage regression is:

$$Private_{it} = \delta_0 + \delta_1 Treat_{it} \times Post_t + \alpha_{st} + \gamma_i + \eta_{it} \tag{3}$$

where $Private_{it}$ is equal to the portion of quarter $t$ that person $i$ is enrolled in a private plan, $Post_t$ is an indicator equal to 1 for any quarter during the post period (Q1_2007-Q4_2010), and $\eta$ is a random error term. Here, $\delta_1$ represents the portion of person-quarters spent in a private plan during the post-mandate period in treatment counties relative to control counties. The IV regression specification is:

$$Y_{it} = \theta_0 + \theta_1 \widehat{Private}_{it} + \alpha_{st} + \gamma_i + \psi_{it} \tag{4}$$

where $\widehat{Private}_{it}$ represents the predicted values from Equation 3 and $\psi_{it}$ is a random error term. Here, $\theta_1$ is a LATE, representing the average difference in the outcome between public and private Medicaid plans for the 70% of the disabled population who comply with the private-plan enrollment mandate.

### 4.3 Identification

In order for $\theta_1$ to represent the causal effect of enrollment in a private Medicaid plan vs. the public program, it must be the case that there was no other change in the treatment counties between the pre- and post-STAR+Plus periods that did not also occur in the control counties. Because there was no other contemporaneous change in Texas's Medicaid program that only affected treatment counties and not the controls, the main potential threat to identification is spurious differential trends in outcomes across the treatment and control counties. To ensure that differential trends do not explain our results, we first include service area grouping-by-quarter fixed effects to account for any local shocks affecting healthcare utilization patterns. Second, for all outcomes, we present event study graphs showing how the difference in the outcome between the treatment and control counties changes over time. This offers a visual test of whether differential pre-trends exist over the time period preceding the introduction of private provision. Finally, in Appendix C we replicate all analyses restricting to border zip codes within 25 miles of each other to further ensure that the control group represents a valid counterfactual for the treatment group.

A more subtle threat to identification is the potential for private provision to impact the underlying composition of Medicaid enrollees. Private Medicaid plans benefit financially from increasing take-up among Medicaid eligible individuals and from decreasing the rate at which their enrollees disenroll from the program. Not all individuals are profitable, however, implying that private plans may be incentivized to increase enrollment among some (healthier) groups while decreasing enrollment among other (sicker) groups. While there is some evidence of plans engaging in this type of selection behavior for the mainstream Medicaid population (Currie and Fahr, 2005), such behavior is unlikely when it comes to the disabled, as Medicaid eligibility for SSI beneficiaries is typically determined indirectly by the Social Security Administration rather than by state Medicaid programs.

The possibility of differential shifts in the composition of disabled Medicaid beneficiaries in treatment vs. control counties motivates our inclusion of individual fixed-effects. We also provide results of our primary analyses restricting to a balanced panel of Medicaid beneficiarie for similar reasons. This ameliorates any problems stemming from composition changes, though it also causes our estimates to reflect the effects of within-person changes in private provision rather than the more general consequences of private provision. Overall effects of private provision combine the effects on individuals forced to actually switch from public to private plans with the effects on individuals newly enrolling in Medicaid after the introduction of private provision. These two effects may be different, as the first may entail potential disruption to a beneficiary's care while the second may not entail any such disruption. Because of this, we include results with and without individual fixed effects for all of our primary outcomes, always with the caveat that the results from regressions excluding individual fixed effects are potentially vulnerable to differential shifts in the composition of enrollees in treatment vs. control counties.

Finally, as evidence that these types of compositional shifts do not explain our results, Appendix Table A2 shows that there is no significant effect of MMC on the number of adults with disabilities entering or exiting Medicaid. Alongside our use of individual fixed effects, these results provide strong evidence that our main estimates are not driven by differential shifts in the composition of Medicaid enrollment.

# 5 Main Results - Texas

We start by reporting the effects of private provision on healthcare spending and utilization in Texas, beginning with overall healthcare spending and then drilling down on utilization by type. We then proceed to assess effects on fiscal/program spending. Next, we describe the effects of the roll-out of private provision in New York and contrast those effects with the Texas results. We then focus on marginal inpatient admissions and drugs and make conclusions about the effects of the shift to private provision on quality of care and quality of life for our study sample in both states. Finally, in Section 8 we study the mechanisms behind the utilization effects of the shift to private plans.

## 5.1 Healthcare Spending

Main results for Texas are reported in Table 2. For each primary outcome (log total realized healthcare spending, log inpatient spending, log drug spending, log outpatient spending), we report coefficients from four regressions. The first two regressions include individual fixed effects while the second two regressions do not. The first and third regressions include an interaction between an indicator for residing in a treatment county ("Treatment") and an indicator for the quarter being after February 2007 ("Post"), the month in which mandated enrollment in private Medicaid plans began in Texas. The second and fourth columns break the "post" period into two periods, an "early-post" period (2007-2008) and a "late-post" period (2009-2010). For each regression specification we report both reduced form and IV coefficients. Reduced form coefficients should be interpreted as the effect of a county-level private-plan enrollment mandate on the outcome, allowing take-up of private plans to be incomplete even under mandated enrollment. IV coefficients should be interpreted as the difference in the outcome in the public Medicaid program vs. in a private plan for the average beneficiary who was induced by the mandate to enroll in a private plan. For all primary outcomes, we also present event study figures (Figure 3) showing the evolution of the reduced form difference in the outcome between the treatment and control counties over time. In Appendix Table A1 we also present regression results where the outcome is spending in levels rather than in logs.

The first outcome we investigate is log total realized healthcare spending. This is not a measure of total fiscal or program spending, but instead the sum of total payments made by either the public or private plans to providers or drug manufacturers for actual healthcare services or drugs. Panel (a) of Figure 3 presents graphical evidence for the effects of private provision on this outcome, reporting event study regression coefficients describing how the difference in log total spending between treatment and control counties changed over time relative to the difference in the last quarter of 2006 (the quarter prior to the introduction of the private plan enrollment mandate). The difference is relatively stable prior to the introduction of the mandate, providing graphical evidence that the treatment and control counties had parallel trends for the outcome during the pre-treatment period. Immediately following the introduction of the mandate, there is a notable drop in spending in treatment counties relative to control counties, reaching about 8% by the second quarter of 2007. This initial drop in spending is short-lived, however, with the treatment vs. control difference returning to its pre-mandate level by the first quarter of 2008. After that time, the spending differential between the treatment and control groups grows markedly, reaching almost 20% by the end of our sample period in the last quarter of 2010. Regression results in Table 2 confirm the results presented in Figure 3. When all post-mandate quarters are pooled, the effect of private provision is positive but statistically insignificant. However, when the post-period is divided into early and late periods, we observe an insignificant negative effect in the early period followed by a significant positive effect (10.3%) in the late period. IV coefficients are positive and significant, indicating a spending increase of 7% among compliers averaged across the entire post-period. Results in Appendix Table A1 where spending is defined in levels instead of logs also confirm these results, with a long-run increase in spending of $535 per quarter over a baseline mean of $3,332, though here there is no initial decline in spending. These results indicate that while there may be an initial decline in spending of Medicaid beneficiaries immediately following their shift to private Medicaid plans (potentially due to care disruption), the bulk of the evidence suggests that the shift to private provision leads to spending *increases* in the longer-run.

### 5.2 Prescription drugs

Panel (b) of Figure 3 shows the effects of the private plan enrollment mandate on log drug spending in Texas. Again, the difference in drug spending between treatment and control counties is stable prior to the mandate. Immediately following the mandate, however, drug spending begins to increase in treatment counties relative to control counties. By the end of our sample period, the effect of the mandate reaches 30%. Here, there is no clear initial drop in spending, implying that the immediate "disruption" effect we observed in total healthcare spending is not coming through prescription drugs. IV regression results in Table 2 indicate that the private plan enrollment mandate led to an increase in individual drug spending of around 20% among compliers, over the full post-mandate period, again with the increase building over time.

Table 3 presents regression results for additional prescription drug outcomes in Texas. Specifically, we show that private plans induce beneficiaries to increase days supply of drugs by 41.7% by the end of the sample period, suggesting that the spending increase is driven by increased drug use rather than shifts to higher-priced drugs. Additionally, we show that there are spending increases for both generic (29.8%) and branded (40.4%) drugs, suggesting that the spending increase is not entirely due to a shift from generic to branded drugs but instead results from overall quantity increases among both types of drugs.

Although the spending increases under private provision appear to come from quantity increases, we find no accompanying extensive margin effects on drug utilization (columns 3-4). In other words, the shift to private plans appears to affect the quantity of drugs an individual consumes, but not whether she consumes *any* drugs in a given quarter. This result rules out the story that private plans increase drug consumption by getting people who are disconnected from the healthcare system in to see a doctor for the first time. This is not surprising, given high baseline levels of drug utilization (73% of beneficiaries taking any drug). We do find, however, that enrollment in private plans produces strong extensive margin effects at the level of the therapeutic category. Panel (b) of Appendix Table A3 presents results from regressions where the outcome is any spending *in a particular therapeutic class*. Enrollment in a private plan led to significant increases in every category except for

Immunosuppressants. These results suggest that while enrollment in a private plan does not affect whether you take *any* drugs, it clearly causes beneficiaries to start taking *new* drugs that they were not previously taking.

### 5.3 Outpatient services

Figure 3 plots event study coefficients describing the effects of private provision on the number of outpatient days in Panel (c) and log realized outpatient spending in Panel (d) in Texas. For both outcomes, the difference between treatment and control counties is relatively stable throughout the pre-mandate period, again indicating parallel pre-trends. Immediately following the introduction of the private plan enrollment mandate, both spending and days drop, with the spending decrease reaching almost 20% by the second quarter of 2007. After the initial quarters under private provision, however, the effect of mandated enrollment in private plans switches from negative to positive. By the end of our sample period, outpatient spending in treatment counties has increased by almost 20% relative to control counties. These results are confirmed by the regression estimates presented in Table 2, where we estimate a statistically insignificant negative effect of private provision in the early part and a significant positive effect in the late part of the post-mandate period. Results in Table A1 where outcomes are measured in levels instead of logs provide further support for the hypothesis that the long run effect of private provision on outpatient spending is positive, with statistically significant positive effects in both the short and long run.

This pattern of an initial drop followed by a long-run increase in outpatient spending under private provision could be due to immediate "disruption" to beneficiaries' healthcare (caused by the shift to private provision) followed by long-run higher levels of outpatient spending under private plans. However, it could also be due to differential reporting. Recall that this outcome represents the only outcome in Texas where there was a shift in the data source (from the public plan to the private plans) pre- vs. post-mandated private plan enrollment. This shift in the source of the data implies that an alternative explanation for this initial drop could be differential reporting between the public plan and the private plans. Importantly, however, under both interpretations, these results indicate long-run higher levels of outpatient spending under private vs. public provision, and, in the case of under-reporting by private plans, our estimates represent a lower-bound of the size of those increases.

Table 4 provides regression estimates for additional outpatient outcomes. Again, it is clear that the effect of the shift to private provision on outpatient days grows over time, similar to the effect on spending. Additionally, we observe that, similar to drugs, there is no extensive margin effect of private provision on outpatient utilization. Again, this is not particularly surprising given that 74% of beneficiaries are using some outpatient care during the pre-mandate period. Table 4 also shows the effects of private provision on ED visits. We find a statistically significant decrease in ED visits in the short run, but while the long-run coefficient is larger it is no longer statistically significant. We take these results as evidence that the transition to private provision did not increase rates of ED use and may have even lowered them.

### 5.4 Inpatient services

Panel (e) of Figure 3 plots the event study coefficients describing the effects of mandated enrollment in private plans on log inpatient spending. While this outcome is noisier than other outcomes, we again observe that the difference between treatment and control counties is relatively stable pre-mandate. Post-mandate, however, inpatient spending in treatment counties clearly falls relative to control counties, with the IV regression coefficients in Table 2 indicating a decrease of 7.6%. Appendix Table A1 shows that these results are even stronger when focusing on spending in levels rather than logs.

Table 5 provides regression estimates for additional inpatient outcomes. Unlike with other outcomes, there is a strong extensive margin ("any admissions") effect of private provision, where the shift to private plans decreased the probability of having any inpatient admission in a quarter by 0.6 percentage points or 8% of the baseline probability. Table 5 also reveals that there is no effect of private provision on inpatient admissions related to surgery, suggesting that private plans did not reduce inpatient admissions by simply shifting beneficiaries from inpatient to outpatient surgeries. Instead, the entire effect comes through non-surgery admissions which are less likely to be viewed as "discretionary" but more likely to be deemed responsive to preventive measures (i.e. signals of low-quality care).

### 5.5 Heterogeneity

In Figure 4 and Appendix Table A5 we explore heterogeneity by health status in the effects of the shift to private provision. For this analysis, we divide the population into three groups based on their pre-mandate Elixhauser comorbidities: the top group has no comorbidities, the middle group has 1-3 comorbidities, and the bottom group (the sickest) has 4+ comorbidities.[13] Figure 4 shows IV coefficients from our primary regression specification for our three primary outcomes: inpatient spending, outpatient spending, and drug spending. The figure shows that the reduction in inpatient spending and the increase in drug spending appear to be driven largely by the sickest beneficiaries. For the sickest group the shift to private provision decreased inpatient spending by 27.5%, compared to a decrease of only 5.8% for the healthiest group. For drug use, the shift to private provision increased spending by 33.3% for the sickest group vs. a statistically insignificant 6.5% increase for the healthiest group. These results are consistent with private health plans targeting their efforts to beneficiaries with conditions that can be managed using the tools of managed care.

In Appendix Table A6 we stratify the sample by age instead of health status. Younger and older SSI beneficiaries are likely to be quite different. Duggan, Kearney and Rennane (2015) show that over 70% of younger SSI beneficiaries (ages 18-40) qualified for SSI due to a mental disability compared to fewer than 50% of older SSI beneficiaries (ages 50-64). The stratified results indicate that the effects of privatization on drug spending are clearly increasing in age. We estimate a statistically significant 11.9% effect on drug spending for the youngest group (ages 20-34) and a significant 25.4% effect for the oldest group (ages 50-64). The effects on inpatient spending, on the other hand, appear to be driven primarily by the middle age group (35-49), with a highly significant 13.3% decrease. While this result may seem counterintuitive, it is likely that the types of inpatient admissions one would typically consider to be "marginal" are concentrated among this group: The younger group has very low levels of inpatient use, suggesting that it may be difficult to further decrease use of this type of care for that population, while the older group has much higher levels of inpatient use, indicating increased severity of illness and suggesting greater difficulty in effectuating health improvements that would translate to lower use of inpatient care.

The treatment effect heterogenetiy we document raises the possibility that the changing effects

[13] We use pre-privatization data to construct comorbidity measures in order to avoid contamination by the causal effects of MMC on the probability of being diagnosed with chronic conditions.

over the post-period (immediate disruption plus long-run effects being larger than short-run effects) are due to changes in the composition of the sample over time. To address this possibility, in Appendix Table A10 we present our main results using a balanced panel of Medicaid beneficiaries. Panel (a) uses a short panel (2005-2008) while Panel (b) requires enrollment for the entire study period (2004-2010). While the balanced panel restriction clearly hurts statistical power, our key results are robust to the use of this balanced sample.

### 5.6 Fiscal Costs of Medicaid and Pass-Through

We now turn to the fiscal costs of private provision. Thus far, all spending outcomes have been based on payments from insurers (either private plans or the government) to healthcare providers. We now ask how private provision affects the total cost of Medicaid for the government (state and federal). As discussed in Section 3.3, fiscal spending consists of two components. The first component is any fee-for-service healthcare spending paid directly from the government to healthcare providers. This includes all spending for beneficiaries enrolled in the Texas public plan as well as drug spending and inpatient spending for beneficiaries enrolled in private plans in Texas. The second component is any premium payments from the government to private health plans. This component is equal to zero for all beneficiaries enrolled in the public plan and equal to the monthly premium payments paid to private health plans for beneficiaries enrolled in private plans.

We report regression estimates for fiscal spending outcomes in Table 6.[14] The key outcomes of interest are log Medicaid spending and Medicaid spending. The results provide clear evidence that the shift to private provision led to an *increase* in Medicaid spending, with fiscal costs increasing by 7.3% in treatment counties relative to control counties.[15] This increase appears to come from both

[14]These regressions differ from all previous regressions in that they are run at the county rather than the individual level. Logged spending outcomes in this table are the log of average spending for the county rather than the average of log spending. We do this because typical log transformations are problematic for this particular analysis. Under the public plan, there are many individuals with zero fiscal spending in a given year. Typically, this would only be a minor problem for conventional log transformations such as $\log(x + 1)$ or the inverse hyperbolic sine transformation. Here, however, it presents a problem that is more severe than usual because under private provision *no* individual has zero spending in a year (due to positive premium payments for all private plan enrollees). This causes any transformation to affect the public plan more than the private plans and generates results of the effect of the shift to private provision on fiscal spending that are severely biased by the transformation. For example, when using the $\log(x + 1)$ transformation, we obtain IV coefficients larger than 1.0, implying enormous effects of private provision on fiscal spending, while when we estimate the same regression using spending levels we obtain IV coefficients equal to about 20% of pre-period spending. Log spending outcomes are important here, however, as visual analysis of treatment and control county trends reveal that control county log fiscal spending represents a better counterfactual for treatment county log fiscal spending than control county fiscal spending in levels as a counterfactual for treatment county fiscal spending in levels.

[15]A subtlety about assessing the effects of the shift to private provision on fiscal spending has to do with administrative costs. Premium payments to private plans include $50 per person per month that is meant to cover administrative costs.

spending on services that are covered by the private plan contract ("covered spending") and services that are carved-out of the private plan contract ("not covered spending"). The coefficients from the regressions where spending levels are the dependent variable (columns 5-8) indicate that about one-third of the spending increase (\$113.17) comes from increases in spending on carved-out services while the other two-thirds (\$242.00) comes from premium payments to private plans that are set higher than counterfactual public plan spending for covered services. However, the "not covered" effects are also clearly larger as a percent of baseline as indicated by the strong log spending results in Column 3.

Comparing the effects of the shift to private provision on fiscal spending to the effects on realized spending provides evidence of the extent of "pass-through" of the additional spending to providers and patients (vs. private insurers). The coefficients in Columns 1 and 4 and in Columns 5 and 8 of Table 6 indicate that increases in realized spending were slightly smaller than increases in fiscal spending. Specifically, the results from regressions using spending levels indicate that the increase in realized spending (\$273.43) was about 77% of the increase in fiscal spending (\$355.17), providing suggestive evidence that the vast majority of additional Medicaid spending went to providers and patients rather than to private insurers.

### 5.7 Robustness

As described above, graphical evidence from analyses of the effects of the shift to private provision indicates that all outcomes were trending similarly in treatment and control counties prior to the roll-out of the private plan enrollment mandate. This suggests that post-mandate trends of outcomes in control counties are likely to be good counterfactuals for post-mandate trends in treatment counties in the absence of the shift to private provision. However, parallel pre-trends need not necessitate parallel post-trends in the absence of the treatment. If treatment and control counties are hit with a shock that affects these counties differently, the effects of this shock, despite occurring in both treatment and control counties, could confound the effects of the shift to private provision.

Of particular concern in our setting are the facts that (1) treatment counties are more urban than control counties and (2) the treatment occurred in early 2007, not long before the start of the Great

We do not observe administrative costs under the public plan, however, so we remove this \$50 per person per month from our measures of fiscal spending under private provision. This implies that we are estimating the fiscal costs of private provision, *except for any change in spending on administrative costs.*

Recession. If the recession affected more-urban vs. less-urban counties in different ways, this might confound the effects of the shift to private provision. To test whether this is a problem, in Appendix C we present results where we only include beneficiaries in treatment zip codes within 25 miles of a control zip code and beneficiaries in control zip codes within 25 miles of a treatment zip code. Appendix Figure C1 shows the included and excluded zipcodes. This effectively eliminates urban centers and rural outlying areas, causing treatment and control areas to be more similar on some measures than in the case where we use all zip codes in treatment and control counties. Appendix Table C1 shows summary statistics for the included control and treatment zipcodes.

With these restrictions, our results are virtually identical to the baseline results. Regression estimates in Appendix Table C2 indicate that the shift to private provision caused a statistically significant increase in total realized healthcare spending, spending on prescription drugs, and outpatient spending. Again, we also find a statistically significant decrease in inpatient spending.

Another potential concern might be that there are spillovers between treatment and control counties. For example, if all beneficiaries living in control counties see doctors practicing in treatment counties, and these doctors also treat a substantial number of beneficiaries living in treatment counties, the control beneficiaries may be impacted by the treatment. In the presence of this type of spillover, our estimates would represent a lower bound of the overall effect of the effects of private provision. This type of spillover is of particular concern when we focus on county borders as in the analysis in Appendix C.

To explore the extent to which spillovers may occur in our setting, we determine the extent to which control county beneficiaries see doctors with high numbers of treatment county patients. Appendix Figure A1 is a histogram showing the percent of claims from treatment county patients for each provider in the data. It is clear that the distribution is bi-modal, with most providers either treating only control-county beneficiaries or treatment-county beneficiaries and few providers treating patients from both treatment and control counties.

To further guage the robustness of our findings, in Appendix D we break down our difference-in-difference estimates by service area, finding in Appendix Tables D1-D4 that our key results hold in each service area in the state (though with some loss of statistical power). This shows that our results are not driven by one particular service area. Taken together, these results provide additional confidence that the effects we estimate stem from the shift to private provision rather than some other confounding factor.

## 6 Main Results - New York

We now describe results for analyses focusing on the effects of the shift to private provision in New York. Appendix Table B1 shows summary statistics for control and treatment counties in New York. Coefficients from regressions estimating the effects of private provision on our main outcomes are found in Appendix Table B2. Corresponding event study plots are found in Appendix Figure B2. We do not present analyses focusing on total realized healthcare spending for New York because our data does not include payments from private plans to providers. This also motivates our focus on quantity rather than spending measures for outpatient and inpatient utilization. We do observe drug spending for beneficiaries in private plans, however, so we report spending effects for that outcome.

Regression coefficients in Column 1 of Appendix Table B2 and Columns 1 to 5 of Appendix Table B3 indicate no effect of private provision on drug spending in New York.[16] The event study plot in Panel (a) of Appendix Figure B2 provides further evidence of a null finding for this outcome, with the difference in drug spending between treatment vs. control counties remaining stable through the pre-period, the transition period, and into the post-period. These results contrast with the large positive effects of private provision on drug spending in Texas. In Section 8 we provide suggestive evidence that this difference between the two states is explained by the strict rationing of drugs in Texas's public program.

Turning to outpatient utilization, our results in Columns 3 and 4 of Appendix Table B2 and Columns 6 and 7 of Appendix Table B3 again indicate no effect of the shift to private provision. Panel (b) of Appendix Figure B2 shows that changes over time in the difference between treatment and control counties in this outcome are somewhat noisy. There is some indication of a drop in outpatient days around the start of the post-period, but only after an increase in outpatient days that started around the start of the transition period. We conclude that our estimates provide little guidance as to the effects of private provision on outpatient use in New York.

Column 5 of Appendix Table B2 provides regression estimates for the effect of the shift to private provision on inpatient admissions in New York. These results indicate that the shift to private

[16] Column 2 shows that without fixed effects there appears to be a positive effect of private provision on drug utilization. However, the possibility of differential changes in the composition of Medicaid enrollees raises concerns about this specification.

provision resulted in a decrease in inpatient admissions, with a marginally significant decrease of 0.01 admissions per quarter, or 11% of the baseline level of admissions between the pre- and post-periods. IV results indicate that for the average beneficiary induced to enroll in a private plan by the enrollment mandate, the shift to private plans decreased the number of inpatient admissions by a statistically significant 0.05 admissions, or 50% of the baseline number of admissions. This is a huge decrease, though it is consistent with prior work on the effects of private provision in Medicare (Duggan, Gruber and Vabson, 2018). Additional results, using data on inpatient admissions from the New York SPARCS hospital discharge dataset instead of from the MAX claims files, are found in Panel (b) of Appendix Table B4. These estimates have the advantage that they use data consistently reported (by hospitals) for beneficiaries in public and private Medicaid plans, and they corroborate our findings from the analyses using MAX data with IV coefficients indicating a 33% drop in inpatient admissions. These results contrast with the smaller (though much more precisely estimated) decrease in inpatient admissions we observe in Texas where inpatient spending was carved out of private plan contracts, suggesting that contract design influences private plan behavior.

Finally, we turn to the fiscal consequences of private provision in NY. These estimates are found in Appendix Table B6. We find no evidence of any effect of the shift to private provision on fiscal spending in New York. This is true for both "covered" and "not covered" spending. The 95% confidence interval for the effect of private provision on log Medicaid spending (Column 1) allows us to reject savings larger than 5.6% and spending increases larger than 7.8%. This stands in contrast to Texas where private provision clearly led to increased Medicaid spending.

## 7 Quality and Beneficiary Health

Thus far, we have assessed the effects of the shift to private provision on healthcare spending and utilization patterns. We now turn to the question of how these shifts in utilization patterns affected the quality of care received by and, ultimately, the health of SSI beneficiaries.

To assess the effects of private provision on quality and health, we first focus our attention on the marginal drugs and marginal inpatient admissions that are affected by the shift to private plans. For drugs, we assess whether the marginal drugs are "high value" and have a high likelihood of positively impacting the lives of chronically ill beneficiaries. For inpatient admissions, we assess whether the marginal admissions fall into categories that are typically deemed potentially "avoidable" given appropriate management of chronic diseases.[17] We then turn to measures of beneficiary health and and functional capacity. Specifically, we analyze the effects of the shift to private provision on mortality, employment, and exit from the SSI program using administrative data from the Social Security Administration.

### 7.1 Drug Outcomes

Because drugs are carved out of private plan contracts in both New York and Texas, we have detailed data on drug utilization that is consistently reported pre- vs. post-mandate. This allows us to further investigate the effects of private provision on patterns of drug utilization in order to assess whether the shifts in utilization are consistent with quality improvements. Figure 5 and Appendix Table A3 present the effects of the shift to private provision on log spending and "any spending" by therapeutic category for the ten largest categories. The large increase in drug spending we observe under private provision is driven by six categories: Anti-infective agents, autonomic drugs, cardiovascular agents, central nervous system, hormones and synthetic substitutes, and gastrointestinal drugs. The central nervous system class is the largest class in this population, and further results by drug within the class (Appendix Table A7) reveal that the largest effects are observed for anti-depressants, anti-psychotics, and drugs used to treat pain. These drugs, especially the anti-psychotics, are critical drugs in this population where there is a high prevalence of mental illness (see Section 2.3). The large increase in utilization of these drugs suggests severe undertreatment of these conditions under the public Medicaid plan in Texas.

The detailed results for cardiovascular agents, the second largest class in this population, are reported in Appendix Table A8. Here, the effects are driven by ACE Inhibitors, Beta Blockers, and Anti-hyperlipidemic Drugs (i.e. statins). All of these drugs are considered "high value" drugs that are highly effective at treating heart disease, a common condition in this population (see Table 1), again suggesting potential improvements to health and quality of life. The detailed results for the hormones and synthetic substitutes class, the third largest class in this population, are reported in

[17] Our data do not allow for the generation of many conventional quality measures. For example, we generated measures of the Prevention Quality Indicators (PQI) developed by the Agency for Healthcare Research and Quality (AHRQ) for assessing quality in Medicaid. We discovered that many of these measures, such as breast cancer screenings and smoking/tobacco cessation treatments, were highly sensitive to coding practices that were changing over our sample period. For these two measures in particular, we find no instances of the codes used to identify these procedures/treatments in any part of the state during our pre-period, with rapid increases in use during the post-period in both treatment and control counties, making it difficult to assess the effects of the shift to private provision on these outcomes. Other outcomes such as flu vaccinations also have unrealistically low baseline means, suggesting measurement problems.

Appendix Table A9. Here, the effects are driven by Adrenals and anti-diabetic agents. Adrenal drugs are used to treat asthma and COPD, two common ailments in this population. Anti-diabetic agents consist of insulins and sulfonylureas, both used to manage diabetes.

Thus, the drugs driving most of the large positive effect of private provision on prescription drug utilization are all drugs used to treat chronic conditions that are highly prevalent in this population. Unlike some drugs, the value of these drugs for patients is well-established. These drugs are also highly unlikely to be prescribed to patients who would not benefit from them. All of these factors combine to provide strong suggestive evidence that private provision led to important improvements in quality of care, and likely quality of life, for this population.

### 7.2 Inpatient Outcomes

Like for drugs, our data on inpatient utilization is detailed and consistently reported pre- vs. post-mandate, allowing us to perform a "deep dive" into the effects of private provision on inpatient outcomes in both states. Specifically, we can assess whether the shift to private provision led to reductions in potentially avoidable inpatient admissions. Figure 6 and Appendix Table A4 break down the effects of private provision on inpatient spending in Texas by the Clinical Classifications Software (CCS) category of the principal diagnosis for the admission.[18] The strongest effect is observed for inpatient admissions related to mental illness, where the shift to private provision decreased spending by 13.4%. Three other categories saw statistically and clinically significant decreases of around 5%: Endocrine, nutritional, and metabolic diseases and immunity disorders (where the most common disease is diabetes); diseases of the respiratory system (including pneumonia, asthma, and COPD); and diseases of the digestive system (including gastro-intestinal and liver disorders).

Inpatient stays across all four of these categories are often considered avoidable via appropriate management of underlying chronic conditions such as bipolar disorder, schizophrenia, depression, diabetes, asthma, and COPD. The conditions associated with these categories are also highly prevalent in this population. Reductions in inpatient spending in these areas thus provide suggestive evidence that the shift to private provision led to important improvements in quality of care, and,

[18]The Clinical Classifications Software (CCS) is a classification developed as part of the Healthcare Cost and Utilization Project (HCUP) by the Agency for Healthcare Research Quality (AHRQ). It groups diagnosis codes into clinically meaningful categories. For our analysis, we used the highest level of aggregation with 18 groups and present results for the 10 most common categories. The CCS classification is available online at https://www.hcup-us.ahrq.gov/toolssoftware/ccs/ccs.jsp

potentially, quality of life for adults with disabilities suffering from these conditions. An alternative explanation for these results is that private plans were stinting on access to necessary inpatient care in these categories. Recall, however, that in Texas inpatient care was carved out of private plan contracts so that private plans do not benefit financially from limiting inpatient admissions.[19] Further, the tight link between the conditions associated with the CCS categories with the largest decreases in inpatient admissions and the conditions associated with the therapeutic classes of drugs with the largest increases in utilization suggest (1) a mechanism for the avoided inpatient admissions (discussed further in Section 8) and (2) are consistent with the shift to private plans leading to important care improvements where the key observable outcome was a reduction in avoidable inpatient admissions. This link, when combined with the fact that private plans had little to gain by limiting access to necessary inpatient care, causes us to conclude that the effects of private provision on inpatient utilization that we observe in Texas are more consistent with improvements in the quality of care received by and health of disabled Medicaid beneficiaries than with stinting by private plans.

Appendix Table B5 shows that in New York, the much larger immediate reduction in inpatient admissions was also driven by admissions related to mental illness. However, in New York we do not observe any reduction in inpatient admissions related to diabetes, asthma, or COPD as we do in Texas. Recall that in New York, inpatient spending was carved in to private plan contracts, so plans had a direct incentive to limit access to inpatient care. Here, it is thus difficult to determine whether the reductions in inpatient admissions reflect improvements in healthcare (as we argue they do in Texas) vs. barriers to inefficient (low-value) inpatient care vs. barriers to both efficient (high-value) and inefficient (low-value) inpatient care.

### 7.3 SSA Outcomes

We now turn to indicators of beneficiary health and functional capacity, including death, employment, and the suspension of SSI benefits. We focus on three measures derived from SSA's Disability Analysis File (DAF). The DAF contains monthly administrative records on the universe of SSI and SSDI beneficiaries. We isolate adults (21-64) enrolled only in the SSI program during our sample period. Regression specifications follow Equation 1 (intent-to-treat estimator), as we do not observe

[19] While there is no direct financial benefit to private plans for stinting on inpatient care, there could be an indirect benefit in the form of deterring enrollment from beneficiary types who are likely to use need inpatient treatment. See Geruso and Layton (2017) for a detailed treatment of these types of contract distortions.

private plan enrollment in the SSA data and therefore cannot account for incomplete take-up of private provision in an instrumental variables framework.[20] Mortality is defined as a binary indicator for whether a beneficiary died in a given quarter. Employment is defined as a binary indicator for whether the beneficiary had positive earnings in a given quarter. SSI suspension is defined as a binary indicator for whether a beneficiary's SSI benefits were suspended due to work in a given quarter. Mortality provides a direct measure of beneficiary health. Employment and SSI suspensions provide indirect measures of functional capacity, with the assumption being that take-up of employment or the suspension of benefits due to work indicate improvements in functional capacity and overall well-being.

Regression results for Texas are presented in Table 7 and Appendix Figure A3. Odd columns pool all years in the post-period, and even columns split the post-period into an early and a late period. Coefficients generally go in a direction consistent with overall improvements in health and functional capacity, with private provision leading to long-run reductions in mortality, increases in employment, and more suspensions of benefits due to work. However, none of the coefficients are statistically significantly different from zero, and confidence intervals are quite wide. For mortality, we get a point estimate of -0.06 percentage points, or a reduction of 6% relative to the baseline mean quarterly mortality rate of 1%. However, the 95% confidence interval ranges from a mortality reduction of 0.18 percentage points (18%) to a mortality increase of 0.05 percentage points (5%), implying that we can only rule out mortality increases larger than 5%. For employment, we can only rule out reductions larger than 0.32 percentage points (6%), and for suspensions we can only rule out reductions larger than 0.21 percentage points (15%). Results from New York, found in Appendix Table B7, exhibit similar signs and similarly wide confidence intervals. We thus conclude that while the signs on these coefficients are all consistent with improvements in health and functional capacity under private provision, they are too noisy to lead to any firm conclusions.

## 8 Mechanisms

Our main analyses have provided evidence that private provision (1) increased prescription drug utilization and spending in Texas, (2) increased outpatient spending and outpatient utilization in

[20] We also do not include individual fixed effects, as this is not appropriate with the mortality and suspension outcomes, which are absorbing states.

Texas, (3) decreased inpatient utilization in both New York and Texas, (4) increased overall spending on healthcare in Texas, and (5) increased fiscal spending in Texas. In this section, in an effort to unpack the "black box" of managed care, we empirically explore the mechanisms behind results (1), (2), and (3). We then draw on the government procurement literature in economics to rationalize results (4) and (5) and provide insights into the contracting problem between states and private plans.

### 8.1 Prescription Drug Mechanisms

There are three features of the STAR+Plus program in Texas that could explain the increase in drug utilization under private provision: (1) Strict rationing of drugs in the Texas public program that is relaxed under private provision, (2) the "carve-out" of prescription drugs from the private plan contracts, and (3) the shift to private provision of medical benefits. We discuss each of these in turn, noting that only (1) differs between Texas and New York, and that only Texas appears to see an increase in drug use.

#### 8.1.1 Drug caps

Drugs are strictly rationed under Texas's public Medicaid program. Individuals enrolled in the public Texas program can only fill three prescriptions per month. There are few exceptions to this rule, making it likely to be highly binding for adults with disabilities. To underscore the stringency of this rule, given typical levels of drug utilization, 35% of adults with disabilities enrolled in both Medicaid and Medicare (similar to, but *not* our sample) would have exceeded this cap in a typical month during the 2006-2010 period. As a consequence, the relaxation of this cap for those enrolling in a private plan is likely to explain much of the private vs. public difference in drug utilization.

To understand how much of the increase in drug use under private provision comes from the relaxation of the drug cap, we extend the regressions used as part of our primary analyses. In these new regressions, the outcomes are indicators for the number of months in the year in which the individual filled more than 0 prescriptions, more than 1 prescription, more than 2 prescriptions, etc. up to more than 6 prescriptions. If we see small or no effects for "more than 0", "more than 1", and "more than 2" but large effects for "more than 3", "more than 4", "more than 5", etc. this will provide strong evidence that much of the effect on drug utilization is coming from the relaxation of the drug cap as opposed to the drug carve out or the shift to private provision for medical services because both of those features would be expected to shift all parts of the distribution of drug utilization rather than only shifting people to take more than 3 drugs.

The event study coefficients from each of these regressions are plotted in Figure 7. The left panel shows results for Texas, and the right panel shows results for New York. First, the results again clearly indicate no effect of private provision on drug utilization in New York. In Texas, however, there are large effects. The dotted lines show effects for changes in drug utilization below the 3-drug cap, while the solid lines show effects for drug utilization above the 3-drug cap. There is essentially no effect for "more than 0" or "more than 1". There is a small effect for "more than 2". The largest effect, however, is for "more than 3". There are also large effects for "more than 4", "more than 5", and "more than 6". This, combined with the absence of any effect of private provision on drug utilization in New York (where there was no drug cap), suggests that much of the effect of private provision on drug utilization in Texas was from beneficiaries starting to fill more than 3 prescriptions in a month. This provides strong suggestive evidence that the relaxation of the drug cap was responsible for much of the overall drug effect.

### 8.1.2 Carve-out of prescription drugs

Even though the relaxation of the drug cap appears to be the main mechanism through which private provision impacts drug utilization, the fact that drugs were carved out of private plan contracts could also play a role; recall that drugs were paid for by the public program for all beneficiaries in all years, even for beneficiaries enrolled in a private plan. With this carve-out, plans had no incentive to reduce drug spending, and may have instead been incentivized to drive up drug utilization, given potential drug-driven medical offsets (Chandra, Gruber and McKnight, 2010; Starc and Town, 2015). Indeed, if drugs had been "carved-in" or included in private plan contracts, plans may have chosen to ration access to drugs more aggressively than they did in the presence of the carve-out, possibly limiting the effect of relaxing the public drug cap. This suggests that our interpretation of the results in Section 6 as the difference between public vs. private rationing of prescription drugs may not apply when drugs are included in private plan contracts.

To investigate this possibility, we leverage the fact that drugs were carved in to private plan contracts in Texas starting in 2012. Our detailed claims and enrollment data ends in 2010, so we cannot use it to study the effects of the carve-in of prescription drugs. Instead, we follow Dranove, Ody and Starc (2017) and use publicly available aggregate data describing prescription drug utilization and spending in Texas's Medicaid program (both public and private plans) over time.[21] In Appendix Figure A2, we document per-enrollee prescription drug utilization and expenditure levels in Texas Medicaid around the 2012 integration of drug services into private Medicaid contracts. The figures show no meaningful change in any of these measures of drug use within Texas Medicaid, following the carve-in. In the figure, we also show the same set of outcomes for Arkansas as a reference and control, as it is the neighboring state with the most similar pre-2012 trends in drug utilization.

These results provide suggestive evidence that the prescription drug carve-out is relatively inconsequential for patterns of drug utilization in Texas. This is consistent with results from Dranove, Ody and Starc (2017) showing that when a large set of states carve in prescription drug benefits to private plan contracts, there is no change in patterns of utilization; while they do find changes overall spending, these appear driven by changes in unit prices rather than by changes in utilization. The implication of this body of evidence is that private plans in Texas would have behaved similarly with respect to drug utilization had drugs been carved into their contracts in 2007, when the shift to private provision and the relaxation of the drug cap for enrollees of private plans occurred. This implies that drug spending differences between public and private plans are not sensitive to whether drugs are carved-in: In Texas, the public plan rations access to drugs much more aggressively than private plans, a key difference between public and private provision.

#### 8.1.3 Shift to private provision of medical benefits

While the analysis in Section 8.1.1 suggests that private provision's effect on drug utilization in Texas came partly through the accompanying relaxation of drug caps, we cannot completely rule out the alternative mechanism of the drug effect instead coming through private provision's effect on patterns of medical care. For example, it is possible that the activities of the private Medicaid plans related to outpatient care (i.e. care management) naturally led to increased levels of drug utilization. Specifically, we showed that private provision led to increased use of outpatient care in Texas and it is possible just seeing the doctor more could lead to higher levels of drug utilization.

However, it seems unlikely that any care management activities would *only* affect utilization on the margin of taking three or more drugs, the margin we showed to be by far the most important for

[21]The Medicaid State Drug Utilization Data is available online from https://www.medicaid.gov/medicaid/prescription-drugs/state-drug-utilization-data/index.html.

the drug effect we estimate. That said, the analysis in Section 8.1.1 cannot entirely rule out comparable drug effects, even absent the lifting of the public drug cap under privatization. To make this point, we must instead rely on the null result for drugs in New York, which had no drug cap.

Based on this evidence, we argue that the relaxation of the drug cap serves as the primary mechanism through which privatization produced the observed increase in drug utilization. This provides a peek into the black box of healthcare production under private as well as public provision: Differences in outcomes between the programs are as much a function of the public program's design as they are of the design of and incentives embedded in the private program.

### 8.2 Outpatient Utilization Mechanisms

To unpack the increase in outpatient spending in Texas, we start by decomposing the spending increase into changes in price and quantity. Recall that outpatient spending in Texas shifted from public to private provision, so changes in spending could be due to either changes in quantities or to differences between the rates paid to providers for a given service by public vs. private plans.

We start by providing descriptive comparisons of prices in Texas's public program vs. prices paid by Texas's private plans. These descriptive analyses are found in Appendix E. For all analyses, we classify outpatient claims according to the procedure code listed on the claim. We then compare public and private payments for each procedure code. Appendix Figure E1 provides scatterplots and histograms comparing public and private prices. All figures suggest that there is some variation in prices between the public and private plans, but that overall prices appear fairly similar.

Next, we use a regression to estimate price differences between public and private plans. Specifically, we estimate a regression of the following form:

$$log(Payment_{cp}) = \beta private_c + \gamma_p + \eta_c \tag{5}$$

The unit of analysis is the claim line, and we regress the log payment on a full set of procedure fixed effects ($\gamma_p$) and an indicator for whether the claim is a private plan claim vs. a public plan claim. $\beta$ represents the average difference in payment for private vs. public plans, conditional on procedure, which we interpret as the public vs. private price difference. We estimate this difference to be 8.4%, as indicated in Panel (b) of Appendix Table E1. We also perform a version of this regression where we allow the price difference to vary by procedure. The distribution of public vs. private price differences estimated by this regression is presented in Appendix Figure E2. The median price difference is 4%. Taken together, these analyses suggest that private prices are slightly higher than public prices.

This raises the question of whether the increase in outpatient spending reflects more money being transferred to providers for providing the same set of outpatient services or instead represents a combination of higher prices and increased access and utilization achieved via those higher prices. To answer this question, we first point to Panel (c) of Figure 3 and Table 4 where, as discussed in Section 5.3, we show that the quantity of outpatient care (as measured by the number of days with an outpatient claim) increases under private provision in Texas. This result indicates that private provision resulted in higher prices *and* higher quantities.

Next, we use the estimates from Equation 5 to "re-price" private plan claims to be based on public plan prices, by removing the estimated private plan price effect (either with or without heterogeneity). We use these re-priced claims to build measures of price-equivalent "plan outpatient spending" for each individual, which only reflects differences in utilization and not in prices. We then run our primary regression specification using these outcomes. Results from these regressions are found in Appendix Table E2. Columns 1-2 show results for actual outpatient spending, columns 3-4 show results for spending that is adjusted to be price equivalent (using public plan procedure-specific prices), and columns 5-6 show results for spending that is adjusted using a fixed homogeneous public plan price. Comparing columns 1-2 to columns 3-4 reveals that differences between public and private prices account for less than 20% of the $488.51 increase in outpatient spending under private provision. Columns 5-6 show that when we impose a constant public vs. private price difference, prices explain a larger portion of the outpatient spending increase (72%), but that a significant increase in outpatient spending remains even with the adjustment.

We interpret these results as suggestive evidence that in Texas (1) private plans pay higher prices to healthcare providers than do public plans and (2) utilization of outpatient care increases under private provision. These results are consistent with an upward-sloping supply curve for healthcare, with private plans paying higher prices for healthcare services and providers responding by increasing their supply of those services to Medicaid beneficiaries. Thus, a key difference between public and private plans in Texas seems to be the level of payments to physicians: Private plans pay more, but those higher payments come with better access to care.

### 8.3 Inpatient Utilization Mechanisms

We showed in Section 6 that private provision led to a decrease in inpatient admissions in both New York and Texas. In Section 7 we showed that the decrease in admissions is concentrated in admissions related to mental illness in both states, with some effect on admissions related to diabetes and respiratory conditions such as asthma and COPD in Texas. Interestingly, in Section 7 we also showed that the increase in drug utilization we observe under private provision is largely driven by drugs used to treat mental illness, with important effects also observed for drugs used to treat diabetes and asthma. This raises the question as to whether the increase in drug utilization *caused* the decrease in inpatient admissions.

In our setting, it is not possible to disentangle the independent effects of the increase in drug utilization from the effects of other actions on the part of the private plans, given that private plans differed from public ones along many dimensions. That said, we provide suggestive evidence to support the hypothesis that the increase in drug utilization was an important factor contributing to the decrease in inpatient admissions.

First, in Figure 4 we showed that the same groups (the sickest beneficiaries) see both the largest increases in drug utilization and the largest decreases in inpatient utilization under private provision. These results are consistent with the increase in drug utilization causing the decrease in inpatient utilization. Second, in Figures 5 and 6 we showed that the conditions associated with the inpatient admissions where we observe large reductions under private provision (mental illness, diabetes, asthma, COPD) are the same conditions where we observe the largest increases in drug utilization. These two results combined provide strong suggestive evidence that the difference in public vs. private rationing of drugs may be at least partially responsible for the reduction in inpatient spending. This suggests that the relaxation of the drug cap may be the primary mechanism behind many of our results. We emphasize, however, that the relaxation of the drug cap should not be viewed as *confounding* the effects of the shift to private provision but instead as the primary *mechanism* behind the consequences of the shift to private provision. This claim is bolstered by discussions with Texas Medicaid officials who confirmed that there is no realistic counterfactual world where Texas relaxed the drug cap without shifting Medicaid beneficiaries to private plans; instead, these two seemingly distinct policy changes were inseparably linked.

We point out, however, that in New York we also observed large decreases in inpatient utilization related to mental health conditions, despite no change in drug utilization. This raises the possibility that the increase in drug utilization in Texas is not the only mechanism by which the shift to private provision led to decreased inpatient use. However, recall that in New York inpatient spending was carved in to private plan contracts while in Texas it was carved out so that private plans were not responsible for inpatient spending. This key difference in private plan incentives in New York and Texas could easily have led private plans in New York to use additional tools, such as care management, offsetting use of non-inpatient psychiatric services, or just strict rationing of access to inpatient care to reduce inpatient utilization related to mental illness.

### 8.4 Contracting and Observed Increases in Healthcare and Fiscal Spending

We now turn to our results establishing that in Texas the transition to private provision led to (1) an overall increase in healthcare spending and (2) an increase in fiscal spending. Considering that many states cite lowering fiscal spending as a primary motivation for switching to private provision, it is valid to ask why this goal was not achieved in either New York nor Texas. We start by considering the state's procurement problem and the contracting tools available to it to accomplish its goals.

The state has two levers to achieve its desired contracting outcomes: exclusion and payment. Exclusion refers to the state's ability to choose which insurers will participate in its Medicaid program. Payment refers to the method by which the state sets payments to the chosen insurers. While payment can be part of the exclusion process (as would be the case in a first-price auction or other auction-like procurement method), this need not be the case. Instead, states can set payments via formula and select insurers based on proposed non-payment plan characteristics. Indeed, this is the form of Medicaid procurement in both New York and Texas as well as most (but not all) other states.

Given these two levers, the state has roughly five options to consider when designing its procurement process. First, the state could tie exclusion and payment together and choose plans in an auction, awarding contracts to the $J$ insurers with the lowest price offers that also meet the minimum requirements set forth by the state (i.e. a first-price auction). Second, the state could pay plans "cost-plus", reimbursing each insurer for its incurred costs plus a mark-up to provide a profit margin, while selecting plans based on their predicted costs and other non-cost plan characteristics (i.e. provider network, use of value-based payment, etc.). Third, the state could set payments via "yardstick competition" (Shleifer, 1985), where the payment to Plan $j$ is equal to realized costs among all other plans, while again selecting plans based on their predicted costs and other non-cost plan characteristics. Fourth, the state could set payments to Plan $j$ equal to average costs across all plans in the market, including Plan $j$, a hybrid of the cost-plus and yardstick competition options, while still selecting plans based on their predicted costs and other non-cost plan characteristics. Finally, the state could just set payments based on some external benchmark, unrelated to plan costs, and again select plans based on predicted costs and non-cost plan characteristics.

In practice, all of these procurement methods are used across state Medicaid programs, with the exception of yardstick competition. Texas and New York use the hybrid of cost-plus and yardstick competition, setting payments to plans equal to a projection of past costs across all plans in the market to the current payment period. It is straightforward to see why such a model might lead to increased healthcare and fiscal spending (or at least no decrease in fiscal spending as observed in New York). In practice, plan payments are set by trending forward past spending in a given service-area. In Texas, most service areas have just 2 insurers, implying a relatively tight link between plan healthcare spending in year $t$ and plan payments in year $t+1$, limiting the incentive for the plan to exert costly effort to reduce healthcare spending. In New York, on the other hand, many counties have 4 or 5 insurers, implying a looser link between plan healthcare spending and plan payments and a stronger incentive to limit healthcare spending. It is possible that this difference in the number of plans and thus the strength of the incentive contributed to our finding of spending increases in Texas vs. no change in spending in New York.

Why would states choose such an arrangement, when they could choose other options with stronger incentives to reduce healthcare spending such as a first-price auction or yardstick competition? One reason may be to induce higher quality plans to enter. If quality is difficult to observe, strong incentives to restrain healthcare spending may induce a "race to the bottom" in terms of quality where insurers compete on price to win the contract with the state at the cost of quality. States may weaken insurer incentives to restrain spending in order to avoid such a race to the bottom.

Another more subtle reason to avoid strong incentives to restrain spending may be that such incentives are not as strong as they appear. The theoretical literature on government procurement has suggested the possibility that strong incentives for cost control may lead to *ex-post* payments from the state to firms in the presence of unexpected cost shocks (Bajari and Tadelis, 2001). Such *ex-post* renegotiation may result in transaction costs states wish to avoid. Additionally, the possibility for this type of *ex-post* contract renegotiation may weaken the incentives of winning insurers to actually engage in costly effort to reduce healthcare spending, because winning insurers know that if their costs are high they will be able to renegotiate their contracts with the state and recoup losses. Indeed, there is empirical evidence from state procurement in other non-healthcare sectors suggesting that contracts with strong incentives to reduce costs (i.e. contracts chosen via first-price auction) often result in large *ex-post* payments (Decarolis, 2014). Similarly, anecdotal evidence from state Medicaid contracting suggests that in cases where states used contracts with strong incentives to reduce costs, similar *ex-post* contract renegotiation resulted either in large *ex-post* payments to insurers and/or premature insurer exit from the program.[22]

Providing healthcare to hundreds of thousands (or sometimes millions) of households clearly qualifies as a complex contracting problem due to many components of healthcare provision that are non-contractable. Medicaid contracts also tend to be 3-5 years in length, further complicating the problem due to unpredictable year-to-year fluctuations in the evolution of healthcare spending. In such a setting, contracts that involve some form of link between costs and payment may be optimal, even acknowledging the possibility that such contracts weaken insurer incentives to restrain spending (Bajari and Tadelis, 2001). This is especially likely to be the case when the cost to an insurer of severing the contract is much smaller than the cost to the state, due to political or social consequences of insurer exit. Indeed, similar links between realized cost and insurer payment exist in the large-group health insurance market, the largest sector of the U.S. health insurance market. Thus, while the procurement and payment systems may lead to higher levels of healthcare and fiscal spending in the Medicaid program, these systems may be preferable to the counterfactual world where private plans are contracted to provide Medicaid services but with strong cost-reduction incentives.

# 9 Conclusion

An understanding of the distinctions and relative trade-offs between public and private provision of social insurance benefits is critical to the future of social insurance programs in the U.S. and around

[22] In Kentucky, payment rates from the state to MCOs were included in insurer proposals and were part of the plan selection process, providing strong incentives for insurers to design plans that produced low spending levels. One of three chosen MCOs exited after the first year after sustaining large losses and after the state declined to provide *ex-post* payments (Marton et al., 2017). Iowa also provided strong incentives for insurers to reduce spending, setting payments to insurers below expected fee-for-service costs. This resulted in the exit of one plan and large payments in later years to other plans to compensate them for losses (Forsgren, 2017).

the world. Previous work studying this question has reached mixed conclusions. In this paper, we add to this literature by looking specifically at Medicaid, the largest social insurance program in terms of beneficiaries covered in the U.S. We examine the full array of services under the program (including prescriptions and outpatient care), leverage clear and transparent identification to distinguish between public and private program effects, decompose the mechanisms through which private provision impacts outcomes, and use natural experiments from multiple states to identify how effects are also contingent on public and private program features.

We find that the effects of shifting from public to private provision of Medicaid benefits are nuanced. In Texas, private provision clearly improves access to healthcare as well as the quality of care received by beneficiaries. Private provision also appears to improve beneficiary health in that state, through decreased rates of avoidable hospitalizations. These improvements, however, come at the cost of higher spending levels for the Medicaid program. These results suggest that the trade-off between spending and quality is real and is not broken by the shift to private provision. In New York, on the other hand, we find that the shift to private provision led to reduced inpatient utilization, with minimal effects on prescription drug use, outpatient utilization, or fiscal spending.

Why do the effects of private provision differ so much across these two states? We provide suggestive evidence that it is largely due to differences in the generosity and overall design of each state's *public* program. Texas strictly rations access to prescription drugs in its public plan, while New York's public plan allows relatively liberal use of drugs. The dramatic increase in drug utilization under private provision can largely be attributed to the accompanying relaxation of a 3 drug cap, which applies under Texas's public program but not for beneficiaries enrolled in private plans. We also provide suggestive evidence that the relaxation of this cap at least partially explains the concurrent quality improvements (i.e. decrease in avoidable hospitalizations) we observe.

It is thus tempting to interpret our results as an endorsement of relaxing rationing under the public program, rather than shifting entirely to private provision. However, because these two approaches (relaxed rationing and the shift to private provision) were undertaken simultaneously, it is impossible to conclude which ultimately led to the effects we observe. More importantly, it is not clear that it is useful to separate them. The argument that all improvements from private provision could have also been accomplished through reduced public rationing presumes that such a reform is possible. However, it is likely that one cannot be undertaken without the other, given that conservative legislatures may only be willing to relax rationing under their existing public programs through a shift to private provision, as those legislatures believe that private provision will result in both marginal and inframarginal dollars being spent more efficiently. Indeed, interviews with Texas Medicaid officials suggest that this is precisely the political economy problem that is reality in that state. The key implication of this framing of the public vs. private problem is that differences between public and private provision of social insurance benefits will differ greatly across states and depend critically on the design of both the public and the private programs.

In the course of answering how public vs. private provision of social insurance impacts beneficiary outcomes and public budgets, our work also raises a number of important questions. First, while privatization alters patterns of utilization in a manner generally consistent with improvements in health (lower inpatient use, higher use of primary care and "high-value" prescription drugs), we have not been able to examine its effects on broader measures such as disability status, functional limitations, or mortality. Future work that links clean variation in private plan enrollment to these outcomes is critical for understanding the full impact of private provision. In addition, while our work looks at multiple state Medicaid programs to obtain a result that is more nationally representative, states' private Medicaid managed care programs are clearly unique and not created equal (Layton, Ndikumana and Shepard, 2018). Moreover, shifts in key program features, such as the carving in of drug benefits (Dranove, Ody and Starc, 2017; Vabson, 2017), the carving out of behavioral health benefits (Richards and Tello-Trillo, 2019), and changes in procurement rules around plan competition (Duggan, Starc and Vabson, 2016) or rate setting could significantly alter the consequences of privatization. Future work should aim to better understand the contributions of specific program features to the effects of privatization.

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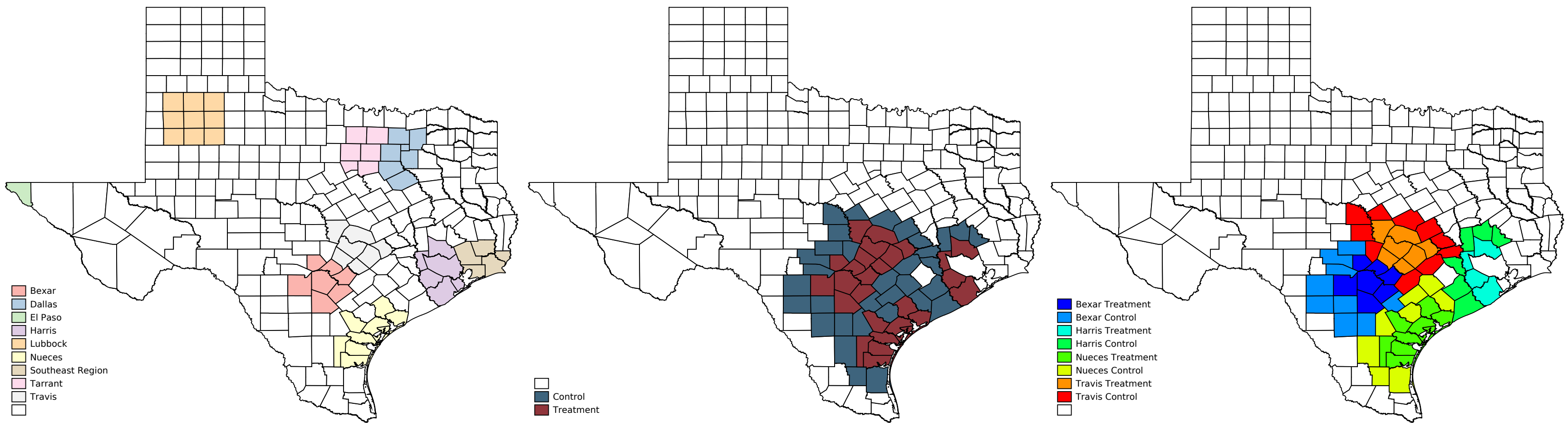
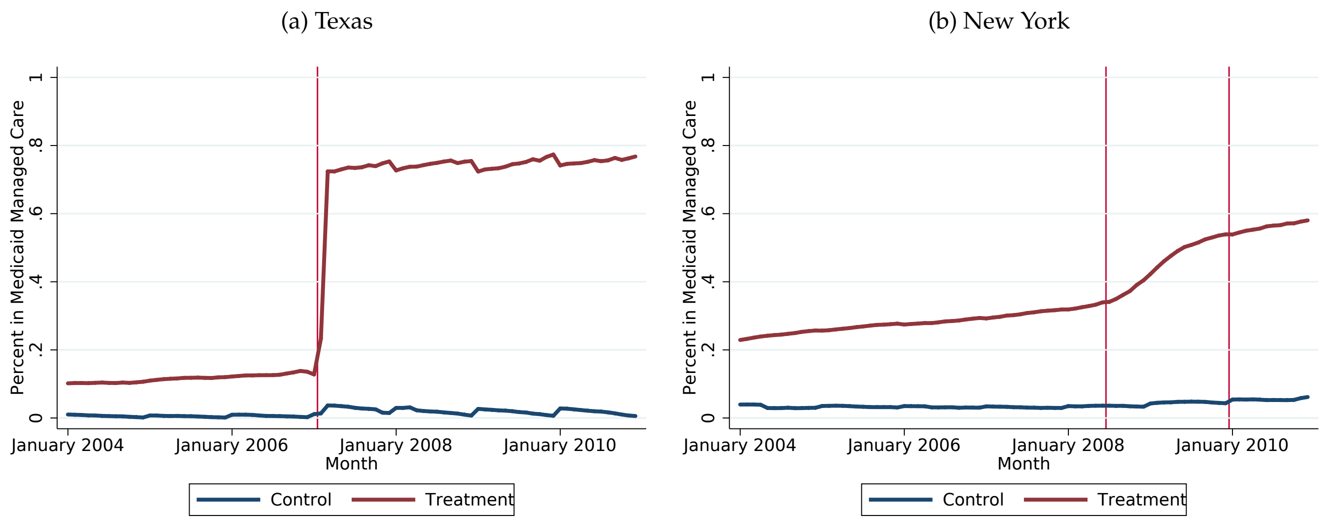
Figure 1: Texas Counties

(a) Medicaid Service Areas | (b) Treatment and Control Counties | (c) Treatment and Control Counties by Service Area




**Note:** Figure shows Medicaid service areas and the treatment and control counties we define based on these service areas in Texas. Panel (a) shows all ten of the Medicaid service areas designated by the Texas Health and Human Services Commission in April 2004. Panel (b) shows the Texas counties that we include in our sample as treatment and control counties. Treatment counties are counties where Medicaid managed care was expanded that are contiguous with at least one county where Medicaid managed care was not expanded. Control counties are counties where Medicaid managed care was not expanded that are contiguous with at least one county where Medicaid managed care was expanded. Panel (c) shows treatment and control counties by service area. For more details, see Section 4.1.

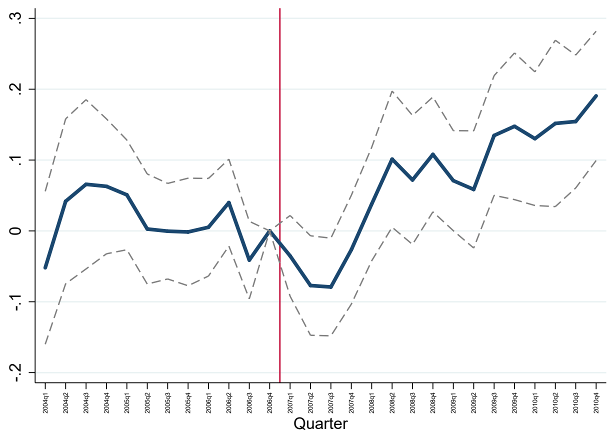
Figure 2: First Stage



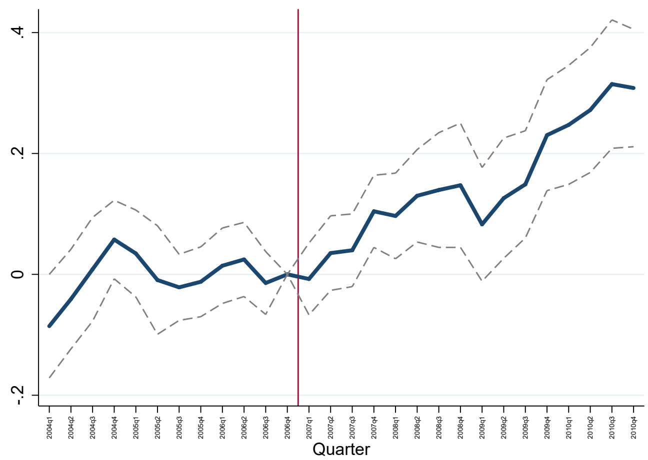

**Note:** Figure shows Medicaid managed care enrollment in treatment and control counties in Texas and New York. Panel (a) shows Medicaid managed care enrollment in treatment and control counties in Texas. The red vertical line between January and February 2007 corresponds to the date of the introduction of the STAR+Plus Medicaid managed care program in the treatment counties. Panel (b) shows Medicaid managed care enrollment in treatment and control counties in New York. The red vertical line between June and July 2008 indicates the beginning of a period to transition to Medicaid managed care, and the red vertical line between December 2009 and January 2010 indicates the end of this transition period. For more details, see Section 4.1.

Figure 3: Main Outcomes

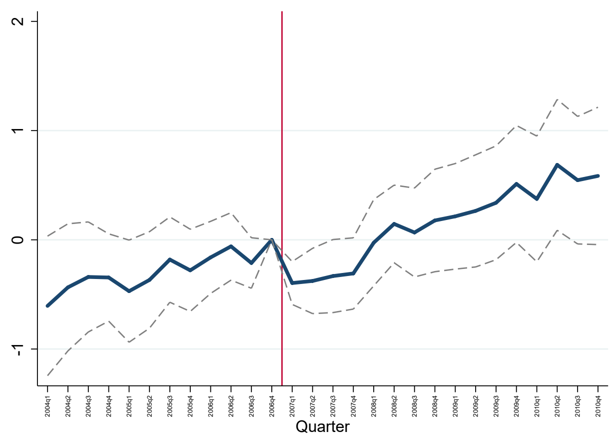
(a) Log Realized Spending



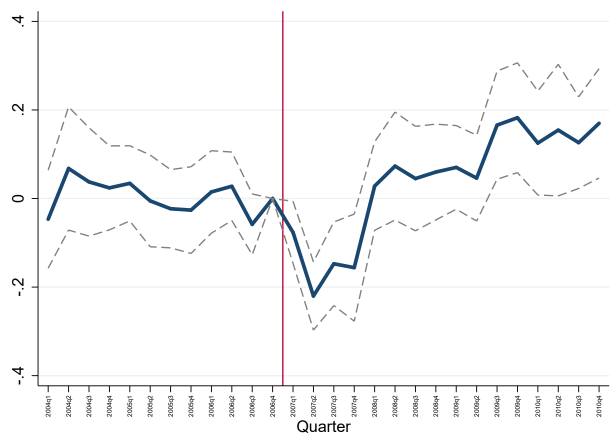

(b) Log Rx Spending



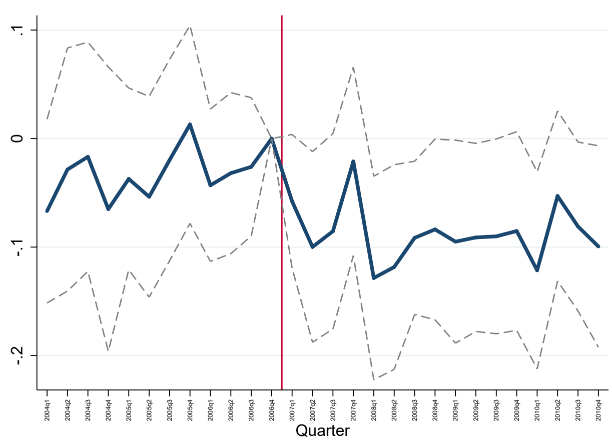

(c) Outpatient Days




(d) Log Realized Outpatient Cost

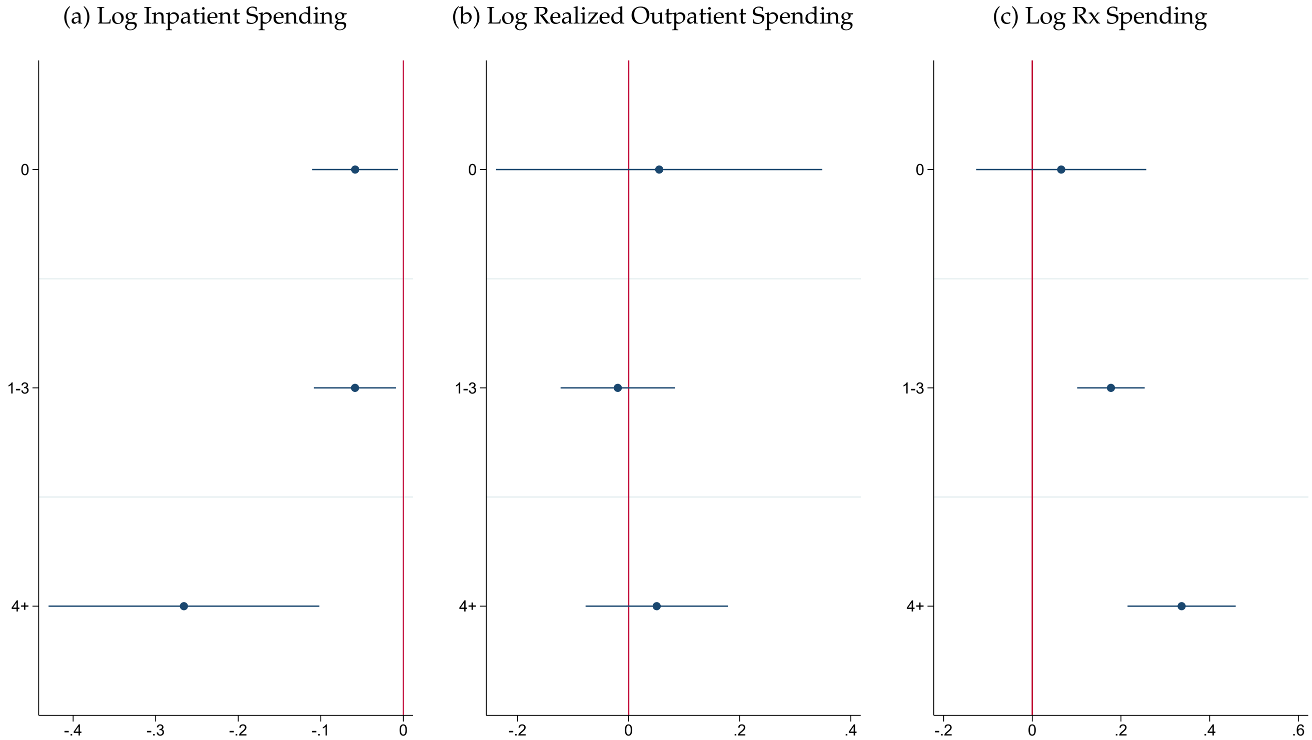



(e) Log Inpatient Spending



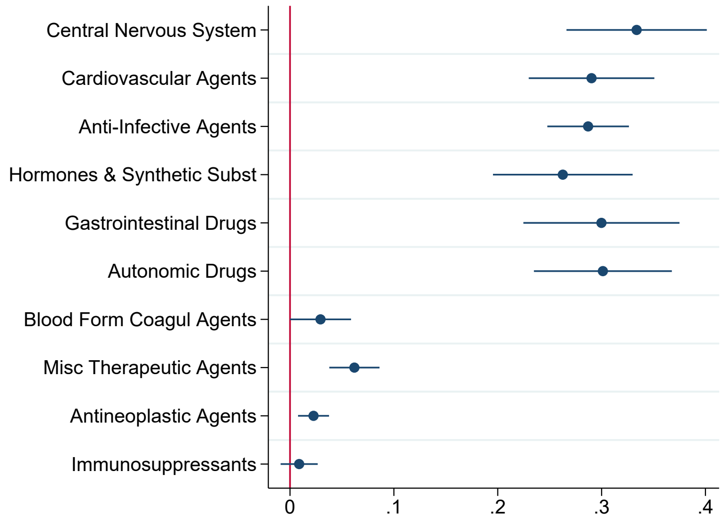

**Note:** Figure shows control-treatment differences in the main outcomes in Texas. These coefficients are from estimating the event study difference-in-differences specification in Equation 1. For more details, see Section 4.2.

Figure 4: Heterogeneity by Health Status



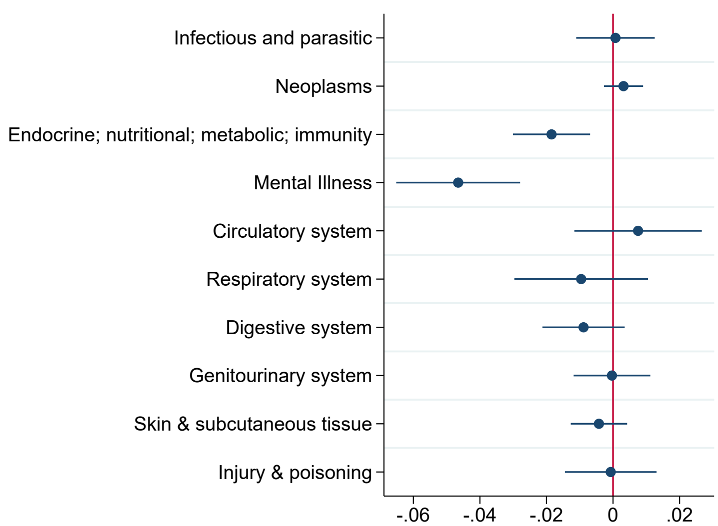

**Note:** Figure shows the impact of Medicaid managed care on log inpatient spending, log realized outpatient spending, and log prescription drug spending by health status (measured as number of preperiod comorbidities) in Texas. These coefficients are from estimating the instrumental variable specification in Equation 4 separately for each comorbidity group (no comorbidities, 1 to 3 comorbidities, and at least 4 comorbidities). For more details, see Section 4.2.

Figure 5: Log Rx Spending by Therapeutic Category




**Note:** Figure shows the impact of Medicaid managed care on log prescription drug spending by therapeutic category in Texas. These coefficients are from estimating the instrumental variable specification in Equation 4 separately for each of the therapeutic categories. For more details, see Section 4.2.

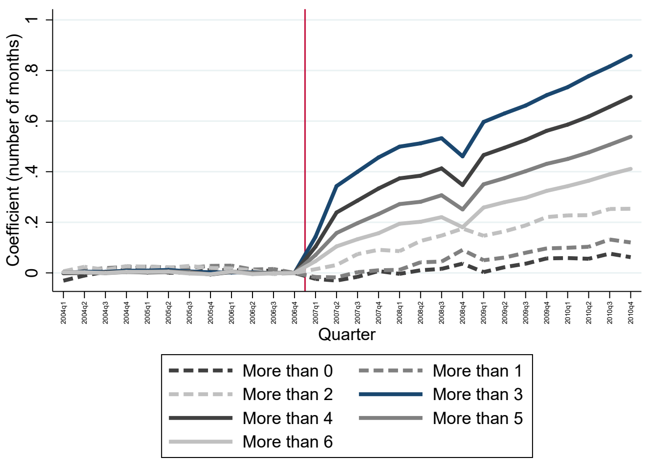
Figure 6: Log Inpatient Spending by CCS Category



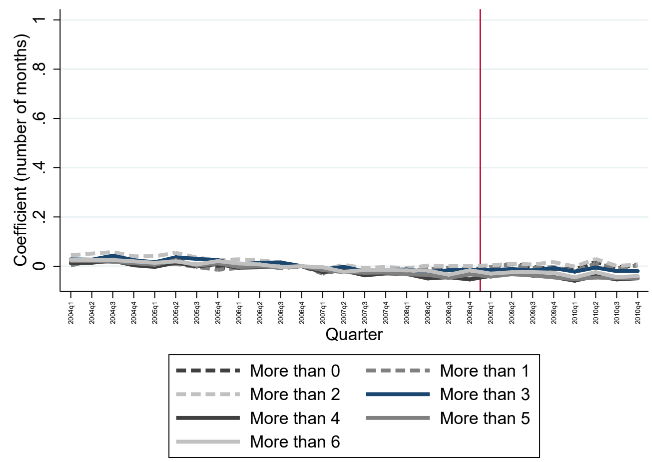

**Note:** Figure shows the impact of Medicaid managed care on log inpatient spending by CCS category in Texas. These coefficients are from estimating the instrumental variable specification in Equation 4 separately for each of the spending categories. For more details, see Section 4.2.

Figure 7: Number of Months With More Than a Given Number of Unique Drugs

(a) Texas




(b) New York




**Note:** Figure shows control-treatment differences in the number of months during which more than a given number of unique drugs was prescribed in Texas and New York. These coefficients are from estimating the event study difference-in-differences specification in Equation 1. For more details, see Section 4.2.

Table 1: Summary Statistics

| | Contiguous Control | Treatment | Non-Contiguous Control |
|---|---|---|---|
| Average Total spending 2004 | 10,729 | 11,404 | 11,621 |
| Average Inpatient spending 2004 | 2,929 | 3,050 | 2,810 |
| Average Outpatient spending 2004 | 5,499 | 5,868 | 6,368 |
| Average Rx spending 2004 | 2,302 | 2,486 | 2,444 |
| Age 20 to 24 | .09083 | .1037 | .09585 |
| Age 25 to 29 | .07696 | .08478 | .07654** |
| Age 30 to 34 | .0778 | .08149 | .07306*** |
| Age 35 to 39 | .0817 | .08782 | .0791*** |
| Age 40 to 44 | .1013** | .111 | .1029* |
| Age 45 to 49 | .1294 | .1281 | .1213 |
| Age 50 to 54 | .1415 | .1362 | .1399 |
| Age 55 to 59 | .1636 | .1457 | .1636** |
| Age 60 to 64 | .1369* | .1212 | .1478*** |
| Female | .5782 | .5556 | .5739 |
| Male | .4218 | .4444 | .4261 |
| Heart Disease | .348* | .3125 | .3559** |
| Diabetes | .2146** | .2085 | .2226 |
| HIV/AIDS | .008895 | .01526 | .0107* |
| Cancer | .05177 | .04644 | .0473 |
| Rheumatoid Arthritis | .03595 | .03406 | .04246 |
| Obesity | .02805 | .03109 | .02828 |
| Substance Use | .0509*** | .06205 | .04668*** |
| Mental Illness | .21 | .2345 | .1968*** |
| N recipients Jan 2004 | 7,401 | 30,510 | 76,210 |
| N recipients Dec 2010 | 9,206 | 42,210 | 106,562 |
| N pre-period recipient months | 289,353 | 1,202,845 | 2,976,227 |
| N post-period recipient months | 405,188 | 1,824,141 | 4,594,026 |

**Note:** Table shows summary statistics for control and treatment counties in Texas. In our analysis, treatment counties are counties where Medicaid managed care was expanded that are contiguous with at least one county where Medicaid managed care was not expanded. In our analysis, control counties are counties where Medicaid managed care was not expanded that are contiguous with at least one county where Medicaid managed care was expanded. However, here we also show summary statistics for all counties in Texas where Medicaid managed care was not expanded. For more details, see Section 4.1.

Table 2: Main Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Realized Spending | | | | Log Inpatient Spending | | | | Log Rx Spending | | | | Log Realized Outpatient Spending | | | |
| Treatment $\times$ *Post* | 0.053 | | 0.112*** | | -0.056*** | | -0.027 | | 0.146*** | | 0.183*** | | 0.027 | | 0.126*** | |
| | (0.034) | | (0.038) | | (0.019) | | (0.018) | | (0.033) | | (0.044) | | (0.042) | | (0.047) | |
| Treatment $\times$ *Post* (2007-2008) | | -0.001 | | 0.048 | | -0.056*** | | -0.031* | | 0.087*** | | 0.142*** | | -0.054 | | 0.018 |
| | | (0.030) | | (0.034) | | (0.020) | | (0.017) | | (0.031) | | (0.041) | | (0.040) | | (0.042) |
| Treatment $\times$ *Post* (2009-2010) | | 0.103** | | 0.155*** | | -0.058*** | | -0.021 | | 0.202*** | | 0.209*** | | 0.111** | | 0.203*** |
| | | (0.042) | | (0.049) | | (0.020) | | (0.022) | | (0.041) | | (0.053) | | (0.051) | | (0.056) |
| IV Coefficient | 0.072* | 0.075* | 0.178*** | 0.175*** | -0.076*** | -0.075*** | -0.043 | -0.040 | 0.197*** | 0.199*** | 0.291*** | 0.287*** | 0.037 | 0.051 | 0.200*** | 0.200** |
| | (0.043) | (0.042) | (0.060) | (0.062) | (0.024) | (0.021) | (0.028) | (0.028) | (0.039) | (0.040) | (0.073) | (0.076) | (0.053) | (0.054) | (0.076) | (0.078) |
| Baseline Mean | 5.825 | 5.825 | 5.825 | 5.825 | .657 | .657 | .657 | .657 | 4.096 | 4.096 | 4.096 | 4.096 | 4.59 | 4.59 | 4.59 | 4.59 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for the main outcomes in Texas. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second and fourth columns show reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Table 3: Rx Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Log Spending | Log Spending | Any Prescriptions | Any Prescriptions | Days Supply | Days Supply | Log Spending Branded Drugs | Log Spending Branded Drugs | Log Spending Generic Drugs | Log Spending Generic Drugs |
| Treatment $\times$ *Post* | 0.146*** | | 0.003 | | 55.723*** | | 0.244*** | | 0.214*** | |
| | (0.033) | | (0.004) | | (6.089) | | (0.037) | | (0.025) | |
| Treatment $\times$ *Post* (2007-2008) | | 0.087*** | | -0.001 | | 37.250*** | | 0.142*** | | 0.160*** |
| | | (0.031) | | (0.004) | | (4.867) | | (0.033) | | (0.025) |
| Treatment $\times$ *Post* (2009-2010) | | 0.202*** | | 0.003 | | 76.604*** | | 0.358*** | | 0.270*** |
| | | (0.041) | | (0.005) | | (8.286) | | (0.051) | | (0.031) |
| IV Coefficient | 0.197*** | 0.199*** | 0.004 | 0.002 | 75.168*** | 78.093*** | 0.329*** | 0.346*** | 0.288*** | 0.292*** |
| | (0.039) | (0.040) | (0.005) | (0.005) | (6.101) | (6.421) | (0.042) | (0.044) | (0.031) | (0.030) |
| Baseline Mean | 4.096 | 4.096 | .676 | .676 | 186.653 | 186.653 | 3.491 | 3.491 | 2.525 | 2.525 |
| Individual Fixed Effects | X | X | X | X | X | X | X | X | X | X |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for prescription drug outcomes in Texas. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second column shows reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Table 4: Outpatient Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Log Realized Cost | Log Realized Cost | Number of Outpatient Days | Number of Outpatient Days | Any Use | Any Use | ED Visits | ED Visits |
| Treatment x Post | 0.027 | | 0.391 | | -0.009** | | -0.072 | |
| | (0.042) | | (0.274) | | (0.004) | | (0.123) | |
| Treatment $\times Post$ (2007-2008) | | -0.054 | | 0.121 | | -0.013*** | | -0.087 |
| | | (0.040) | | (0.239) | | (0.004) | | (0.075) |
| Treatment $\times Post$ (2009-2010) | | 0.111** | | 0.653* | | -0.003 | | -0.219 |
| | | (0.051) | | (0.359) | | (0.005) | | (0.216) |
| IV Coefficient | 0.037 | 0.051 | 0.528 | 0.551 | -0.013** | -0.010* | -0.097 | -0.212 |
| | (0.053) | (0.054) | (0.350) | (0.372) | (0.005) | (0.005) | (0.157) | (0.178) |
| Baseline Mean | 4.59 | 4.59 | 8.201 | 8.201 | .717 | .717 | 2.167 | 2.167 |
| Individual Fixed Effects | X | X | X | X | X | X | X | X |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for outpatient outcomes in Texas. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second column shows reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Table 5: Inpatient Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Spending | | Any Admissions | | Admissions | | Surgery Admissions | | Non-Surgery Admissions | | Length of Stay | |
| Treatment $\times$ *Post* | -0.056*** | | -0.005** | | -0.006** | | 0.000 | | -0.007** | | -0.061* | |
| | (0.019) | | (0.002) | | (0.003) | | (0.001) | | (0.003) | | (0.031) | |
| Treatment $\times$ *Post* (2007-2008) | | -0.056*** | | -0.005** | | -0.006** | | -0.001 | | -0.005** | | -0.019 |
| | | (0.020) | | (0.002) | | (0.003) | | (0.002) | | (0.002) | | (0.037) |
| Treatment $\times$ *Post* (2009-2010) | | -0.058*** | | -0.004* | | -0.005 | | 0.003* | | -0.008*** | | -0.041 |
| | | (0.020) | | (0.002) | | (0.003) | | (0.002) | | (0.003) | | (0.031) |
| IV Coefficient | -0.076*** | -0.075*** | -0.006** | -0.006** | -0.008** | -0.008** | 0.001 | 0.001 | -0.009*** | -0.009*** | -0.082** | -0.041 |
| | (0.024) | (0.021) | (0.003) | (0.002) | (0.004) | (0.003) | (0.002) | (0.002) | (0.003) | (0.003) | (0.040) | (0.039) |
| Baseline Mean | .657 | .657 | .075 | .075 | .096 | .096 | .039 | .039 | .057 | .057 | .698 | .698 |
| Individual Fixed Effects | X | X | X | X | X | X | X | X | X | X | X | X |

Standard errors in parentheses

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for inpatient outcomes in Texas. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second column shows reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Table 6: Medicaid Spending

| | (1) Log Medicaid Spending | (2) Log Covered Spending | (3) Log Not Covered Spending | (4) Log Realized Spending | (5) Medicaid Spending | (6) Covered Spending | (7) Not Covered Spending | (8) Realized Spending |
|---|---|---|---|---|---|---|---|---|
| Treatment $\times$ *Post* | 0.073*** | 0.061* | 0.090*** | 0.054** | 355.169*** | 242.004* | 113.165*** | 273.429** |
| | (0.026) | (0.036) | (0.018) | (0.026) | (124.489) | (125.237) | (25.077) | (125.030) |
| IV Coefficient | 0.117*** | 0.098* | 0.143*** | 0.087** | 565.455*** | 385.288** | 180.167*** | 435.318** |
| | (0.040) | (0.055) | (0.027) | (0.040) | (191.790) | (192.763) | (38.400) | (192.451) |
| Baseline Mean | 8.192 | 7.737 | 7.129 | 8.19 | 3711.76 | 2444.256 | 1267.504 | 3702.188 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for Medicaid spending outcomes in Texas. For each outcome, county-level estimates of control-treatment differences are from the pooled reduced form specification in Equation 2 and county-level estimates of the impact of Medicaid managed care are from the instrumental variable specification in Equation 4. For more details, see Section 4.2.

Table 7: Other Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Mortality | | Employment | | SSI Suspensions | |
| Treatment $\times Post$ | -0.00029 | | 0.0028 | | 0.000082 | |
| | (0.00054) | | (0.0036) | | (0.0011) | |
| Treatment $\times Post(2007-2008)$ | | 0.000081 | | 0.00034 | | -0.00037 |
| | | (0.00066) | | (0.0033) | | (0.0011) |
| Treatment $\times Post(2009-2010)$ | | -0.00063 | | 0.0050 | | 0.00049 |
| | | (0.00060) | | (0.0042) | | (0.0013) |
| Baseline Mean | 0.010 | 0.010 | 0.051 | 0.051 | 0.014 | 0.014 |

Standard errors in parentheses

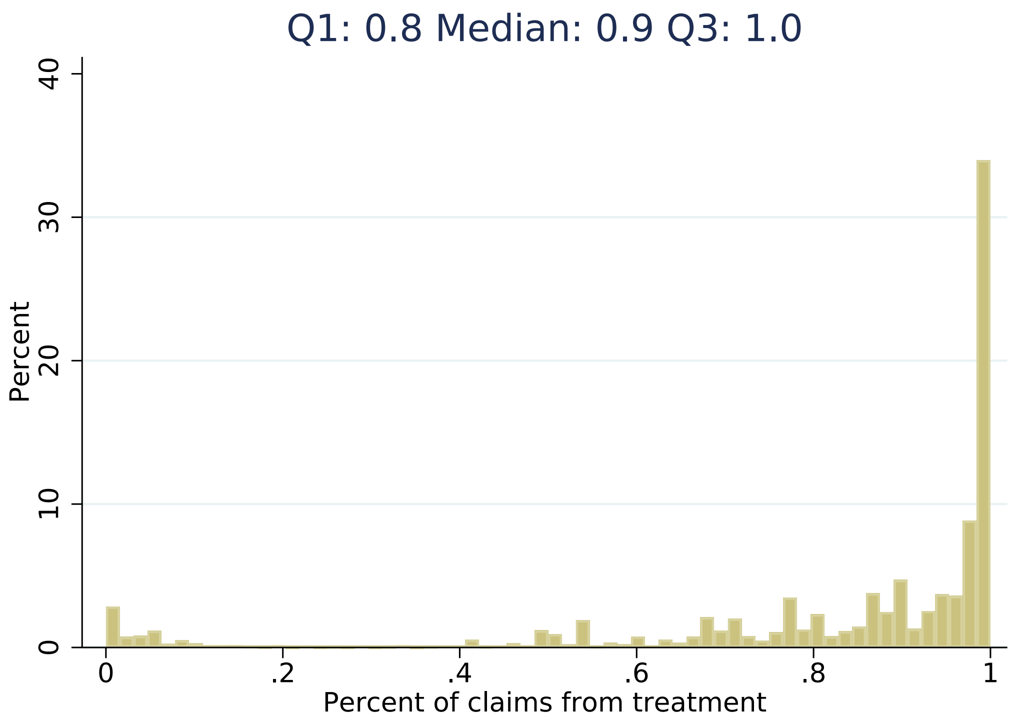
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form estimates for mortality, employment, and SSI suspension in Texas. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2, pooling over the entire post period. The second column shows reduced form estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Online Appendix for:
**Private vs. Public Provision of Social Insurance: Evidence from Medicaid**

# A Additional Texas Results

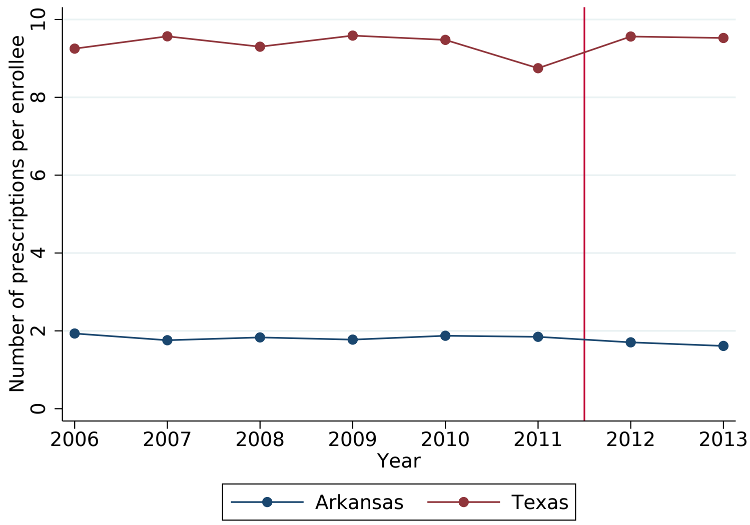
Appendix Figure A1: Provider Overlap



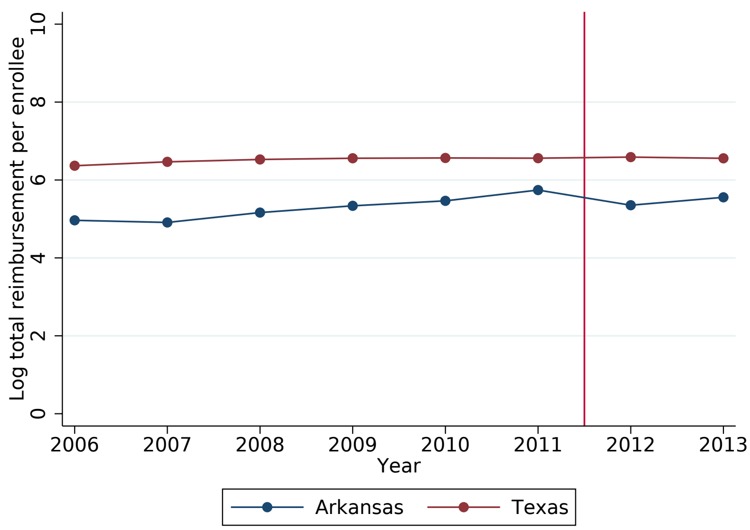

**Note:** Figure shows the distribution of the percent of claims at a provider that come from patients who live in treatment counties.

Appendix Figure A2: Impact of the Prescription Drug Carve-in in Texas

(a) Number of prescriptions



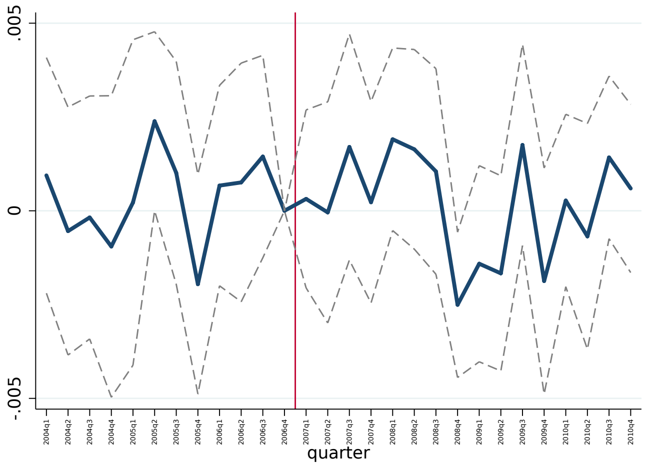

(b) Spending



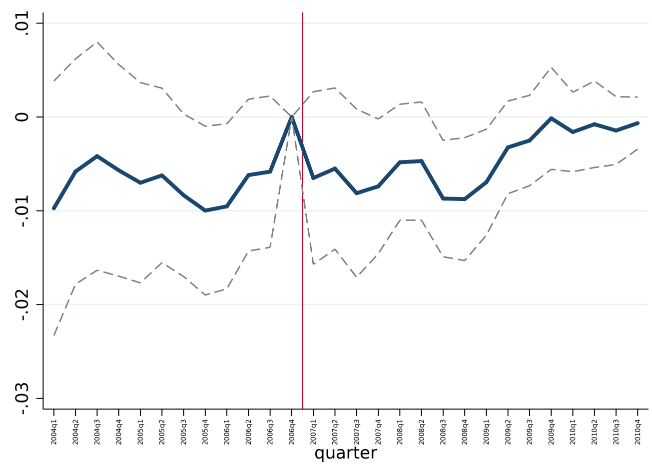

**Note:** Figure shows the number of prescriptions and the amount of spending per enrollee in Texas and Arkansas before and after Texas carved prescription drugs into its managed care contracts in 2012. The data displayed here come from the publicly available Medicaid State Drug Utilization Data. For more details, see Section 8.1.2.

Appendix Figure A3: Other Outcomes

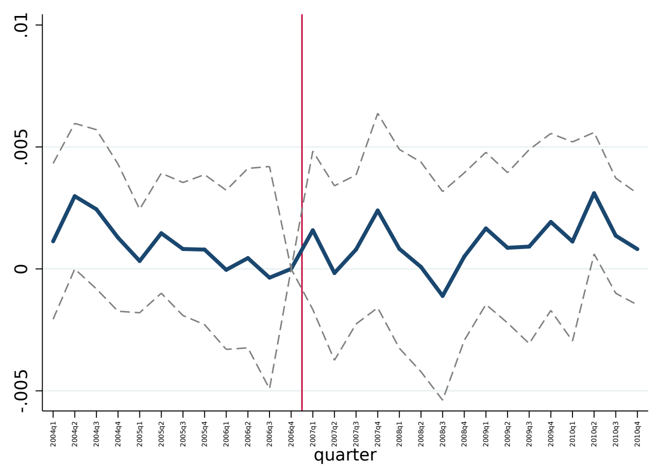
(a) Mortality




(b) Employment




(c) SSI Suspensions




**Note:** Figure shows control-treatment differences in mortality, employment, and SSI suspensions in Texas. These coefficients are from estimating the event study difference-in-differences specification in Equation 1. For more details, see Section 4.2.

Appendix Table A1: Main outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Realized Spending | | | | Inpatient Spending | | | | Rx Spending | | | | Realized Outpatient Spending | | | |
| Treatment | 365$^{***}$ | | 214$^{**}$ | | -47$^{**}$ | | -18 | | 118$^{***}$ | | 117$^{***}$ | | 312$^{***}$ | | 274$^{***}$ | |
| $\times$*Post* | (111) | | (92) | | (21) | | (21) | | (16) | | (17) | | (38) | | (37) | |
| Treatment | | 188$^{*}$ | | 124$^{**}$ | | -33 | | -7 | | 66$^{***}$ | | 68$^{***}$ | | 170$^{***}$ | | 178$^{***}$ |
| $\times$*Post* | | (108) | | (56) | | (24) | | (22) | | (13) | | (12) | | (31) | | (29) |
| (2007-2008) | | | | | | | | | | | | | | | | |
| Treatment | | 535$^{***}$ | | 249$^{**}$ | | -45$^{*}$ | | -18 | | 178$^{***}$ | | 149$^{***}$ | | 427$^{***}$ | | 322$^{***}$ |
| x Post | | (151) | | (122) | | (26) | | (25) | | (22) | | (22) | | (54) | | (52) |
| (2009-2010) | | | | | | | | | | | | | | | | |
| IV Coefficient | 572$^{***}$ | 585$^{***}$ | 393$^{**}$ | 361$^{**}$ | -74$^{**}$ | -61$^{**}$ | -33 | -25 | 185$^{***}$ | 197$^{***}$ | 214$^{***}$ | 212$^{***}$ | 489$^{***}$ | 480$^{***}$ | 503$^{***}$ | 479$^{***}$ |
| | (170) | (192) | (163) | (159) | (31) | (28) | (37) | (39) | (23) | (23) | (31) | (31) | (57) | (60) | (64) | (67) |
| Baseline Mean | 3,332 | 3,332 | 3,332 | 3,332 | 557.743 | 557.743 | 557.743 | 557.743 | 539.576 | 539.576 | 539.576 | 539.576 | 1,343 | 1,343 | 1,343 | 1,343 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for the main outcomes in Texas. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second and fourth columns show reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Appendix Table A2: Medicaid Entry and Exit

| | (1) Enter | (2) Exit |
|---|---|---|
| Treatment $\times Post$ | -0.004 | -0.001 |
| | (0.004) | (0.003) |
| IV Coefficient | -0.026 | -0.005 |
| | (0.028) | (0.019) |
| Treatment | $0.017^{**}$ | -0.003 |
| | (0.008) | (0.006) |
| Baseline Mean | .148 | .101 |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows control-treatment differences in Medicaid entry and exit in Texas. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table A3: Therapeutic Classes

(a) Log Spending

| | (1) Anti-Infective Agents | (2) Anti-neoplastic Agents | (3) Autonomic Drugs | (4) Blood Form/ Coagul Agents | (5) Cardio-vascular Agents | (6) Central Nervous System | (7) Gastro-intestinal Drugs | (8) Hormones & Synthetic Subst | (9) Immuno-suppressants | (10) Misc Thera-peutic Agents |
|---|---|---|---|---|---|---|---|---|---|---|
| Treatment $\times Post$ | 0.334*** | 0.033** | 0.365*** | 0.034* | 0.238*** | 0.300*** | 0.317*** | 0.256*** | 0.009 | 0.088*** |
| | (0.041) | (0.014) | (0.060) | (0.019) | (0.046) | (0.048) | (0.056) | (0.048) | (0.011) | (0.020) |
| IV Coefficient | 0.471*** | 0.047*** | 0.515*** | 0.048** | 0.336*** | 0.423*** | 0.448*** | 0.362*** | 0.012 | 0.124*** |
| | (0.044) | (0.015) | (0.060) | (0.022) | (0.047) | (0.053) | (0.056) | (0.048) | (0.012) | (0.021) |
| Baseline Mean | 1.774 | .113 | 1.052 | .378 | 1.834 | 3.639 | 1.13 | 1.625 | .083 | .221 |

(b) Any Spending

| | (1) Anti-Infective Agents | (2) Anti-neoplastic Agents | (3) Autonomic Drugs | (4) Blood Form/ Coagul Agents | (5) Cardio-vascular Agents | (6) Central Nervous System | (7) Gastro-intestinal Drugs | (8) Hormones & Synthetic Subst | (9) Immuno-suppressants | (10) Misc Thera-peutic Agents |
|---|---|---|---|---|---|---|---|---|---|---|
| Treatment $\times Post$ | 0.059*** | 0.006*** | 0.069*** | 0.008** | 0.025*** | 0.028*** | 0.053*** | 0.043*** | 0.001 | 0.020*** |
| | (0.009) | (0.002) | (0.010) | (0.004) | (0.008) | (0.008) | (0.009) | (0.007) | (0.001) | (0.004) |
| IV Coefficient | 0.084*** | 0.009*** | 0.098*** | 0.011*** | 0.036*** | 0.039*** | 0.075*** | 0.060*** | 0.002 | 0.028*** |
| | (0.009) | (0.002) | (0.010) | (0.004) | (0.009) | (0.009) | (0.009) | (0.007) | (0.001) | (0.004) |
| Baseline Mean | .411 | .02 | .246 | .072 | .32 | .61 | .215 | .303 | .011 | .046 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows control-treatment differences spending for the most common therapeutic classes in Texas. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table A4: Inpatient spending on the Top 10 CCS categories

| | (1) Infectious and parasitic Diseases | (2) Neoplasms | (3) Endocrine; nutritional; and metabolic Diseases and immunity disorders | (4) Mental Illness | (5) Diseases of the circulatory system | (6) Diseases of the respiratory system | (7) Diseases of the digestive system | (8) Diseases of the genitourinary system | (9) Diseases of the skin and subcutaneous tissue | (10) Injury and poisoning |
|---|---|---|---|---|---|---|---|---|---|---|
| Treatment $\times Post$ | 0.001 | 0.007 | -0.040** | -0.093*** | 0.028 | -0.036* | -0.033* | -0.006 | -0.008 | 0.023 |
| | (0.019) | (0.009) | (0.019) | (0.025) | (0.025) | (0.020) | (0.019) | (0.018) | (0.014) | (0.018) |
| IV Coefficient | 0.001 | 0.010 | -0.058*** | -0.134*** | 0.041 | -0.052** | -0.048** | -0.009 | -0.012 | 0.033 |
| | (0.022) | (0.011) | (0.021) | (0.026) | (0.029) | (0.023) | (0.021) | (0.021) | (0.016) | (0.021) |
| Baseline Mean | .114 | .128 | .144 | .198 | .39 | .249 | .264 | .124 | .1 | .181 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows control-treatment differences in the top 10 most common Clinical Classification Software (CCS) groups of diagnoses in Texas, separately for inpatient and outpatient. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table A5: Outcomes by Pre-period Health Status

(a) No Comorbidities

| | (1) Log Inpatient Spending | (2) Log Realized Outpatient Cost | (3) Log Rx Spending | (4) ED Visits |
|---|---|---|---|---|
| Treatment $\times Post$ | -0.042** | 0.039 | 0.047 | -0.049 |
| | (0.020) | (0.114) | (0.075) | (0.113) |
| IV Coefficient | -0.058** | 0.055 | 0.065 | -0.069 |
| | (0.027) | (0.150) | (0.098) | (0.150) |
| Baseline Mean | .054 | 1.867 | 1.482 | .423 |

(b) 1-3 Comorbidites

| | (1) Log Inpatient Spending | (2) Log Realized Outpatient Cost | (3) Log Rx Spending | (4) ED Visits |
|---|---|---|---|---|
| Treatment $\times Post$ | -0.056 | -0.047 | 0.143*** | -0.216 |
| | (0.036) | (0.049) | (0.039) | (0.185) |
| IV Coefficient | -0.075 | -0.064 | 0.192*** | -0.291 |
| | (0.046) | (0.064) | (0.049) | (0.240) |
| Baseline Mean | .487 | 5.289 | 4.763 | 1.793 |

(c) More Than 3 Comorbidites

| | (1) Log Inpatient Spending | (2) Log Realized Outpatient Cost | (3) Log Rx Spending | (4) ED Visits |
|---|---|---|---|---|
| Treatment $\times Post$ | -0.207* | 0.035 | 0.251** | 0.172 |
| | (0.104) | (0.094) | (0.101) | (0.496) |
| IV Coefficient | -0.275** | 0.046 | 0.333*** | 0.228 |
| | (0.126) | (0.116) | (0.121) | (0.613) |
| Baseline Mean | 2.153 | 6.597 | 5.362 | 5.045 |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows control-treatment differences in the main outcomes broken down by pre-period health in Texas. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table A6: Outcomes by Age

(a) 20-34

| | (1) Log Inpatient Spending | (2) Log Realized Outpatient Cost | (3) Log Rx Spending | (4) ED Visits |
|---|---|---|---|---|
| Treatment $\times$ *Post* | -0.035 | 0.032 | 0.074 | -0.284* |
| | (0.025) | (0.087) | (0.045) | (0.148) |
| IV Coefficient | -0.056 | 0.052 | 0.119* | -0.458** |
| | (0.038) | (0.130) | (0.067) | (0.232) |
| Baseline Mean | .35 | 4.272 | 3.513 | 1.491 |

(b) 35-49

| | (1) Log Inpatient Spending | (2) Log Realized Outpatient Cost | (3) Log Rx Spending | (4) ED Visits |
|---|---|---|---|---|
| Treatment $\times$ *Post* | -0.100*** | 0.010 | 0.166*** | 0.009 |
| | (0.036) | (0.053) | (0.058) | (0.186) |
| IV Coefficient | -0.133*** | 0.014 | 0.220*** | 0.012 |
| | (0.045) | (0.065) | (0.067) | (0.229) |
| Baseline Mean | .685 | 4.506 | 4.014 | 2.548 |

(c) 50-64

| | (1) Log Inpatient Spending | (2) Log Realized Outpatient Cost | (3) Log Rx Spending | (4) ED Visits |
|---|---|---|---|---|
| Treatment $\times$ *Post* | -0.023 | 0.049 | 0.204*** | 0.098 |
| | (0.032) | (0.049) | (0.045) | (0.144) |
| IV Coefficient | -0.029 | 0.061 | 0.254*** | 0.122 |
| | (0.036) | (0.057) | (0.053) | (0.166) |
| Baseline Mean | .839 | 4.86 | 4.543 | 2.335 |

Standard errors in parentheses

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows control-treatment differences in the main outcomes broken down by age in Texas. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table A7: Central Nervous System Classes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Analg/ Antipyr, Nonstr/ Antiinflm | Analg/ Antipyr, Opiate Agonists | Analg/ Antipyr, NEC | Anti-convulsant, Benzo-diazepine | Anti-conv, Hydan-toin Deri-vative | Anti-conv, Misc | Psych other, Anti-depres-sants | Psychother, Tranq/ Anti-psy-chotic | ASH, Benzo-dia-zepines | ASH, NEC |
| Treatment $\times$ *Post* | 0.307*** | 0.189*** | 0.124*** | 0.027*** | 0.011 | 0.118*** | 0.247*** | 0.219*** | 0.106*** | 0.137*** |
| | (0.031) | (0.039) | (0.023) | (0.009) | (0.012) | (0.032) | (0.046) | (0.037) | (0.027) | (0.018) |
| IV Coefficient | 0.434*** | 0.266*** | 0.175*** | 0.038*** | 0.016 | 0.167*** | 0.349*** | 0.309*** | 0.150*** | 0.193*** |
| | (0.036) | (0.041) | (0.028) | (0.010) | (0.013) | (0.035) | (0.048) | (0.041) | (0.028) | (0.023) |
| Baseline Mean | .778 | 1.139 | .324 | .182 | .177 | .99 | 1.453 | 1.118 | .749 | .606 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows control-treatment differences spending for the most common subclasses of the central nervous system therapeutic class in Texas. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table A8: Cardiovascular Classes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | NEC | ACE Inhibitors | Cardiac Glycosides | Antiarrhythmic Agents | Alpha-Beta Blockers | Beta Blockers | Calcium Channel | Antihyperlipidemic Drugs, NEC | Hypotensive Agents, NEC | Vasodilating Agents, NEC |
| Treatment $\times$ *Post* | 0.031 | 0.156*** | 0.013** | -0.002 | 0.002 | 0.114*** | 0.082*** | 0.168*** | 0.039*** | 0.046*** |
| | (0.031) | (0.022) | (0.006) | (0.004) | (0.004) | (0.027) | (0.017) | (0.057) | (0.011) | (0.009) |
| IV Coefficient | 0.044 | 0.221*** | 0.019*** | -0.002 | 0.002 | 0.160*** | 0.115*** | 0.237*** | 0.055*** | 0.065*** |
| | (0.034) | (0.023) | (0.006) | (0.005) | (0.005) | (0.029) | (0.019) | (0.061) | (0.013) | (0.009) |
| Baseline Mean | .361 | .509 | .038 | .015 | .013 | .427 | .482 | .898 | .124 | .102 |

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows control-treatment differences spending for the most common subclasses of the cardiovascular agents therapeutic class in Texas. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table A9: Hormones Classes

| | (1) Adrenals & Comb, NEC | (2) Contra-ceptive, Oral Comb, NEC | (3) Estrogens & Comb, NEC | (4) Anti-diabetic Agents, Insulins | (5) Anti-diabetic Agents, Sulfo nylureas | (6) Anti-diabetic Agents, Misc | (7) Para-thyroid Hor-mones, NEC | (8) Pituitary Hor-mones, NEC | (9) Pro-gestins, NEC | (10) Thy /Antithy, Thyroid/ Hor-mones |
|---|---|---|---|---|---|---|---|---|---|---|
| Treatment $\times$ *Post* | 0.244*** | -0.009 | 0.026* | 0.006 | 0.075*** | 0.084*** | 0.002 | -0.003 | 0.001 | 0.072*** |
| | (0.041) | (0.010) | (0.014) | (0.027) | (0.017) | (0.022) | (0.004) | (0.003) | (0.006) | (0.015) |
| IV Coefficient | 0.344*** | -0.013 | 0.037** | 0.009 | 0.106*** | 0.119*** | 0.003 | -0.004 | 0.002 | 0.102*** |
| | (0.040) | (0.011) | (0.016) | (0.030) | (0.019) | (0.024) | (0.005) | (0.003) | (0.006) | (0.016) |
| Baseline Mean | .39 | .112 | .15 | .399 | .326 | .58 | .018 | .018 | .022 | .245 |

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows control-treatment differences spending for the most common subclasses of the hormones and synthetic substances therapeutic class in Texas. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table A10: Main outcomes: balanced panel

(a) 2005-2008

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Realized Spending | | | | Log Inpatient Spending | | | | Log Rx Spending | | | | Log Realized Outpatient Spending | | | |
| Treatment x Post | 0.013 | | 0.013 | | -0.059$^{***}$ | | -0.059$^{***}$ | | 0.083$^{***}$ | | 0.083$^{***}$ | | -0.058 | | -0.058 | |
| | (0.030) | | (0.029) | | (0.021) | | (0.020) | | (0.028) | | (0.027) | | (0.042) | | (0.041) | |
| Treatment x Post (2007-2008) | | 0.007 | | 0.007 | | -0.059$^{***}$ | | -0.059$^{***}$ | | 0.078$^{***}$ | | 0.078$^{***}$ | | -0.073$^{*}$ | | -0.073$^{*}$ |
| | | (0.030) | | (0.029) | | (0.019) | | (0.018) | | (0.028) | | (0.027) | | (0.043) | | (0.042) |
| IV Coefficient | 0.017 | 0.010 | 0.017 | 0.010 | -0.078$^{***}$ | -0.080$^{***}$ | -0.078$^{***}$ | -0.080$^{***}$ | 0.111$^{***}$ | 0.106$^{***}$ | 0.111$^{***}$ | 0.106$^{***}$ | -0.077 | -0.099$^{*}$ | -0.077 | -0.099$^{*}$ |
| | (0.038) | (0.039) | (0.038) | (0.039) | (0.026) | (0.024) | (0.026) | (0.024) | (0.034) | (0.035) | (0.034) | (0.035) | (0.054) | (0.057) | (0.054) | (0.057) |
| Baseline Mean | 6.336 | 6.336 | 6.336 | 6.336 | .456 | .456 | .456 | .456 | 4.753 | 4.753 | 4.753 | 4.753 | 4.98 | 4.98 | 4.98 | 4.98 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

(b) 2004-2010

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Realized Spending | | | | Log Inpatient Spending | | | | Log Rx Spending | | | | Log Realized Outpatient Spending | | | |
| Treatment x Post | 0.050 | | 0.050 | | -0.045$^{**}$ | | -0.045$^{**}$ | | 0.102$^{***}$ | | 0.102$^{***}$ | | 0.040 | | 0.040 | |
| | (0.050) | | (0.049) | | (0.018) | | (0.017) | | (0.038) | | (0.037) | | (0.058) | | (0.057) | |
| Treatment x Post (2007-2008) | | -0.016 | | -0.016 | | -0.053$^{**}$ | | -0.053$^{***}$ | | 0.067$^{**}$ | | 0.067$^{**}$ | | -0.077 | | -0.077 |
| | | (0.050) | | (0.049) | | (0.020) | | (0.020) | | (0.032) | | (0.032) | | (0.060) | | (0.059) |
| Treatment x Post (2009-2010) | | 0.093 | | 0.093$^{*}$ | | -0.038 | | -0.038 | | 0.127$^{***}$ | | 0.127$^{***}$ | | 0.122$^{*}$ | | 0.122$^{*}$ |
| | | (0.056) | | (0.055) | | (0.024) | | (0.023) | | (0.047) | | (0.047) | | (0.065) | | (0.064) |
| IV Coefficient | 0.066 | 0.056 | 0.066 | 0.056 | -0.060$^{***}$ | -0.059$^{***}$ | -0.060$^{***}$ | -0.059$^{***}$ | 0.134$^{***}$ | 0.131$^{***}$ | 0.134$^{***}$ | 0.131$^{***}$ | 0.053 | 0.041 | 0.053 | 0.041 |
| | (0.063) | (0.065) | (0.063) | (0.065) | (0.023) | (0.023) | (0.023) | (0.023) | (0.046) | (0.046) | (0.046) | (0.046) | (0.073) | (0.077) | (0.073) | (0.077) |
| Baseline Mean | 6.305 | 6.305 | 6.305 | 6.305 | .419 | .419 | .419 | .419 | 4.722 | 4.722 | 4.722 | 4.722 | 4.945 | 4.945 | 4.945 | 4.945 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

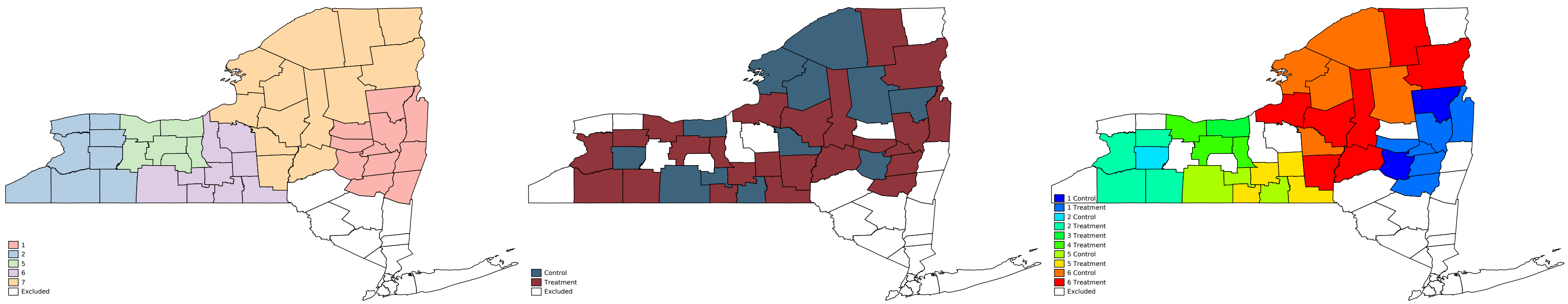
Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows control-treatment differences in the main outcomes in a balanced panel. Panel (a) shows a shorter panel, for 2005-2008 and Panel (b) shows all years, 2004-2010. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

## B New York results

Appendix Figure B1: New York Counties

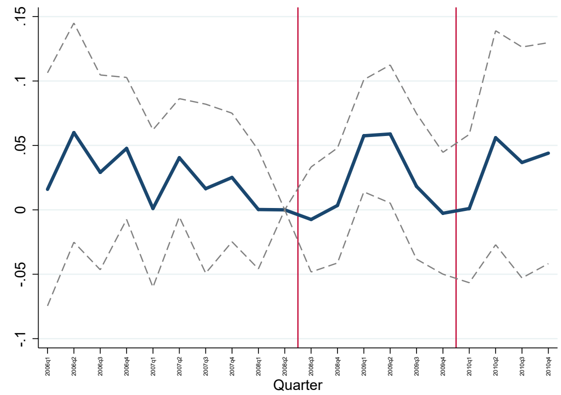
(a) Medicaid Service Areas | (b) Treatment and Control Counties | (c) Treatment and Control Counties by Service Area



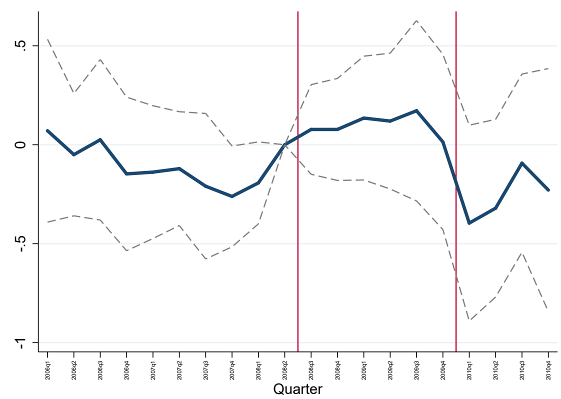

**Note:** Figure shows Medicaid service areas and the treatment and control counties we define based on these service areas in New York. Panel (a) shows the five geographic rating areas (defined by the Centers for Medicare and Medicaid Services) that we include in our analysis. (We exclude the other three geographic rating areas because there is no variation *within* these areas in Medicaid managed care status.) Panel (b) shows the New York counties that we include in our sample as treatment and control counties. Treatment counties are counties where Medicaid managed care was expanded that are contiguous with at least one county (within their own geographic rating area) where Medicaid managed care was not expanded. Control counties are counties where Medicaid managed care was not expanded that are contiguous with at least one county (within their own geographic rating area) where Medicaid managed care was expanded. Panel (c) shows treatment and control counties by service area. For more details, see Section 4.1.

Appendix Figure B2: Main outcomes (New York)

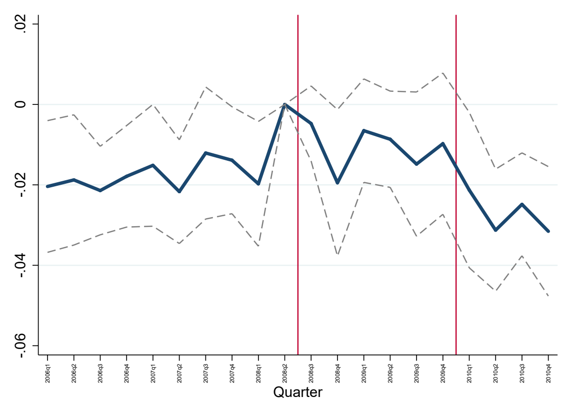
(a) Log Rx spending




(b) Outpatient days




(c) Inpatient admissions




**Note:** Figure shows control-treatment differences in the main outcomes in New York. These coefficients are from estimating the event study difference-in-differences specification in Equation 1. For more details, see Section 4.2.

Appendix Table B1: Summary Statistics (New York)

| | Control | Treatment |
|---|---|---|
| Average Total spending 2006 | 15,750 | 19,305 |
| Average Inpatient spending 2006 | 2,813 | 3,711 |
| Average Outpatient spending 2006 | 8,385 | 11,566 |
| Average Rx spending 2006 | 4,552 | 4,028 |
| Age 20 to 24 | .09985 | .1085 |
| Age 25 to 29 | .08856 | .09062 |
| Age 30 to 34 | .08276 | .07978 |
| Age 35 to 39 | .09316 | .09147 |
| Age 40 to 44 | .1271 | .1139 |
| Age 45 to 49 | .1352 | .1374 |
| Age 50 to 54 | .1421 | .1388 |
| Age 55 to 59 | .1375 | .1387 |
| Age 60 to 64 | .09374 | .1008 |
| Female | .5758 | .5217 |
| Male | .4242 | .4783 |
| Heart Disease | .291 | .2811 |
| Diabetes | .1699 | .1575 |
| HIV/AIDS | .008069 | .03101 |
| Cancer | .04236 | .03912 |
| Rheumatoid Arthritis | .02446 | .01894 |
| Obesity | .05282 | .04514 |
| Substance Use | .07388 | .115 |
| Mental Illness | .2677 | .3033 |
| N recipients Jan 2006 | 7,753 | 27,689 |
| N recipients Dec 2010 | 8,083 | 30,259 |
| N pre-period recipient months | 227,519 | 807,600 |
| N post-period recipient months | 230,955 | 836,391 |

**Note:** Table shows summary statistics for control and treatment counties in New York. In our analysis, treatment counties are counties where Medicaid managed care was expanded that are contiguous with at least one county where Medicaid managed care was not expanded. In our analysis, control counties are counties where Medicaid managed care was not expanded that are contiguous with at least one county where Medicaid managed care was expanded. For more details, see Section 4.1.
**Note:** Table shows control-treatment differences in the main outcomes in New York. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table B2: Main Outcomes (New York)

| | (1) Log Rx Spending | (2) Log Rx Spending | (3) Outpatient Days | (4) Outpatient Days | (5) Inpatient Admissions | (6) Inpatient Admissions |
|---|---|---|---|---|---|---|
| Treatment $\times$ *Transition* | -0.002 | 0.026 | 0.204 | 0.445** | 0.005 | 0.004 |
| | (0.017) | (0.021) | (0.137) | (0.191) | (0.005) | (0.004) |
| Treatment $\times$ *Post* | 0.011 | 0.124** | -0.157 | -0.060 | -0.012* | -0.012* |
| | (0.036) | (0.053) | (0.235) | (0.305) | (0.007) | (0.007) |
| IV Coefficient | 0.049 | 0.496*** | -0.622 | -0.117 | -0.050** | -0.048* |
| | (0.161) | (0.171) | (1.101) | (1.232) | (0.025) | (0.029) |
| Baseline Mean | 4.633 | 4.633 | 12.304 | 12.304 | .101 | .101 |
| Individual Fixed Effects | X | | X | | X | |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table B3: Rx and Outpatient Outcomes (New York)

| | (1) Rx Log Spending | (2) Rx Any Prescriptions | (3) Rx Log Days Supply | (4) Rx Log Spending Branded Drugs | (5) Rx Log Spending Generic Drugs | (6) Outpatient Number of Days | (7) Outpatient Any Use |
|---|---|---|---|---|---|---|---|
| Treatment $\times$ *Transition* | -0.002 | 0.003 | -0.006 | -0.047* | 0.055 | 0.204 | 0.002 |
| | (0.017) | (0.002) | (0.014) | (0.028) | (0.034) | (0.137) | (0.003) |
| Treatment $\times$ *Post* | 0.011 | 0.005 | 0.002 | -0.019 | 0.036 | -0.157 | 0.006 |
| | (0.036) | (0.005) | (0.034) | (0.038) | (0.051) | (0.235) | (0.005) |
| IV Coefficient | 0.049 | 0.023 | 0.008 | -0.109 | 0.188 | -0.622 | 0.027 |
| | (0.161) | (0.023) | (0.148) | (0.158) | (0.228) | (1.101) | (0.022) |
| Baseline Mean | 4.633 | .728 | 4.042 | 3.859 | 3.253 | 12.304 | .776 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows control-treatment differences in various prescription drug and outpatient outcomes in New York. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table B4: Inpatient Outcomes (New York)

(a) MAX Data

| | (1) Any Admissions | (2) Admissions Conditional | (3) Surgery | (4) Non-Surgery | (5) Length |
|---|---|---|---|---|---|
| Treatment | 0.001 | 0.062 | 0.001 | 0.004 | 0.124* |
| ×*Transition* | (0.003) | (0.052) | (0.001) | (0.004) | (0.061) |
| Treatment | -0.008* | 0.034 | 0.001 | -0.012** | -0.020 |
| ×*Post* | (0.004) | (0.043) | (0.002) | (0.006) | (0.101) |
| IV Coefficient | -0.036** | 0.252 | 0.004 | -0.054** | -0.035 |
| | (0.016) | (0.202) | (0.009) | (0.021) | (0.447) |
| Baseline Mean | .072 | 1.403 | .025 | .076 | .922 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) SPARCS Data

| | (1) Any Admissions | (2) Admissions Conditional | (3) Surgery | (4) Non-Surgery | (5) Length | (6) Charges |
|---|---|---|---|---|---|---|
| Treatment | -0.002 | 0.085 | -0.001 | 0.002 | -0.018 | -30.414 |
| ×*Transition* | (0.003) | (0.058) | (0.001) | (0.005) | (0.059) | (226.678) |
| Treatment | -0.006** | 0.009 | -0.000 | -0.009** | -0.037 | 68.240 |
| ×*Post* | (0.003) | (0.063) | (0.001) | (0.004) | (0.046) | (189.720) |
| [IV] Portion of | -0.027* | 0.253 | -0.003 | -0.033 | -0.172 | 219.809 |
| quarter in MMC | (0.014) | (0.274) | (0.004) | (0.021) | (0.244) | (960.348) |
| Baseline Mean | .076 | 1.351 | .014 | .088 | .789 | 2061.24 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows control-treatment differences in various inpatient outcomes in New York. Panel (a) shows estimates using the MAX data that we use for all of our analyses. Panel (b) shows estimates using the New York Statewide Planning and Research Cooperative System (SPARCS) which contains inpatient hospital discharge data. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table B5: Top 10 Inpatient CCS (New York)

| | (1) Infectious and parasitic Diseases | (2) Neoplasms | (3) Endocrine; nutritional; and metabolic Diseases and immunity disorders | (4) Mental Illness | (5) Diseases of the circulatory system | (6) Diseases of the respiratory system | (7) Diseases of the digestive system | (8) Diseases of the genitourinary system | (9) Diseases of the skin and subcutaneous tissue | (10) Injury and poisoning |
|---|---|---|---|---|---|---|---|---|---|---|
| Treatment ×*Transition* | 0.000 | 0.000 | -0.000 | 0.001 | 0.001 | 0.001 | 0.001 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.000) | (0.001) |
| Treatment ×*Post* | -0.001 | 0.000 | -0.000 | -0.007*** | -0.001 | -0.000 | 0.002* | 0.000 | -0.000 | -0.000 |
| | (0.001) | (0.001) | (0.001) | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.001) | (0.001) |
| [IV] Portion of quarter in MMC | -0.003 | 0.001 | -0.000 | -0.033*** | -0.004 | -0.000 | 0.011* | 0.002 | -0.002 | -0.001 |
| | (0.003) | (0.003) | (0.005) | (0.011) | (0.008) | (0.009) | (0.007) | (0.003) | (0.003) | (0.004) |
| Baseline Mean | .003 | .003 | .005 | .029 | .013 | .011 | .009 | .003 | .005 | .007 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Table shows control-treatment differences in the top 10 most common inpatient Clinical Classification Software (CCS) groups of diagnoses in New York. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects and individual fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

Appendix Table B6: Medicaid Spending (New York)

| | (1) Log Medicaid Spending | (2) Log Covered Spending | (3) Log Not Covered Spending | (4) Medicaid Spending | (5) Covered Spending | (6) Not Covered Spending |
|---|---|---|---|---|---|---|
| Treatment $\times$ *Transition* | 0.002 | 0.010 | -0.024 | 21.354 | 77.921 | -56.567 |
| | (0.064) | (0.081) | (0.067) | (378.551) | (342.292) | (119.958) |
| Treatment $\times$ *Post* | -0.023 | -0.020 | -0.025 | -115.028 | -104.201 | -10.827 |
| | (0.095) | (0.121) | (0.100) | (562.018) | (508.186) | (178.096) |
| Baseline Mean | 8.662 | 8.327 | 7.389 | 5895.387 | 4258.477 | 1636.91 |

Standard errors in parentheses

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for Medicaid spending outcomes in New York. For each outcome, county-level estimates of control-treatment differences are from the pooled reduced form specification in Equation 2 and county-level estimates of the impact of Medicaid managed care are from the instrumental variable specification in Equation 4. For more details, see Section 4.2.

Appendix Table B7: Other Outcomes (New York)

| | (1) Mortality | (2) Employment | (3) SSI Suspensions |
|---|---|---|---|
| Treatment $\times$ *Transition* | -0.00026 | -0.0059 | -0.0026 |
| | (0.00083) | (0.0025) | (0.0014) |
| Treatment $\times$ *Post* | 0.00057 | -0.0030 | -0.0017 |
| | (0.00078) | (0.0048) | (0.00130) |
| Baseline Mean | 0.007 | 0.111 | 0.020 |

Standard errors in parentheses

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form estimates for mortality, employment, and SSI suspension in Texas. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2, pooling over the entire post period. The second column shows reduced form estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

# C Border zip code analysis

In this appendix we replicate our main results limiting to border zip codes. The motivation for this analysis is that one might be concerned that our treatment counties are more urban than control counties and urban and rural counties may have been differntially impacted by potential shocks that occurred around the time of our treatment (February 2007). Focusing on border zipcodes may make control and treatment counties even more similar. Border zipcodes are defined as zipcodes in a control county that are within 25 miles of a treatment county and zipcodes in a treatment county that are within 25 miles of a control county. Distance is measured as great-circle distance calculated using the Haversine formula based on internal points in zipcodes.[23]

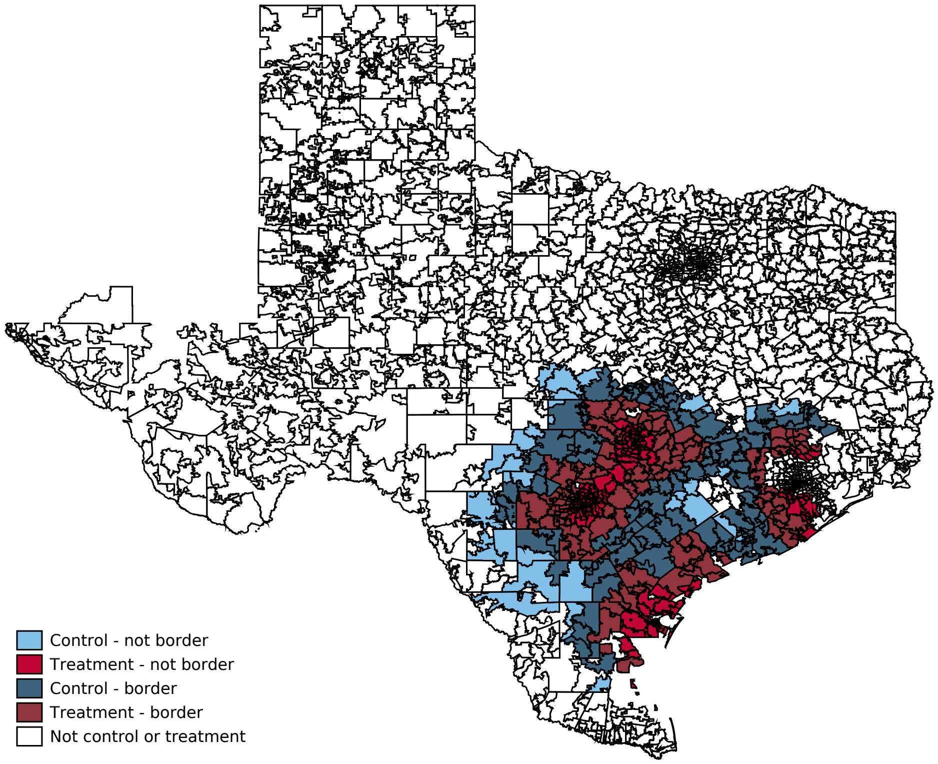
Appendix Figure C1 shows a map of zipcodes in Texas. Control and treatment counties are highlighted in shades of blue and shades of red, respectively, separating border and non-border zipcodes.

Appendix Table C1 replicates Table 1 limiting to the border zipcodes.

Appendix Table C2 replicates Table 2 limiting to the border zipcodes. For each primary outcome (log total realized healthcare spending, log inpatient spending, log drug spending, log outpatient spending), we report coefficients from four regressions. The first two regressions include individual fixed effects while the second two regressions do not. The first and third regressions include an interaction between an indicator for residing in a treatment county ("Treatment") and an indicator for the quarter being after February 2007 ("Post"), the month in which mandated enrollment in private Medicaid plans began in Texas. The second and fourth columns break the "post" period into two periods, an "early-post" period (2007-2008) and a "late-post" period (2009-2010). For each regression specification we report both reduced form and IV coefficients. Reduced form coefficients should be interpreted as the effect of a county-level private-plan enrollment mandate on the outcome, allowing take-up of private plans to be incomplete even under mandated enrollment. IV coefficients should be interpreted as the difference in the outcome in the public Medicaid program vs. in a private plan for the average beneficiary who was induced by the mandate to enroll in a private plan. We highlight that our main results remain quite similar on this restricted sample.

[23] Files with distances between zipcodes are available at https://www.nber.org/data/zip-code-distance-database.html.

Appendix Figure C1: Texas Zipcodes




**Note:** Figure shows the map of zipcodes in Texas. For our analysis of zipcodes we classify zipcodes within the control and treatment counties into border and not border zipcodes. Border zipcodes are zipcodes in control counties within 25 miles of a treatment zipcode and zipcodes in treatment counties within 25 miles of a control zipcode. Not border zipcodes are all the other zipcodes in control and treatment counties. Distance is measured using as great-circle distance calculated using the Haversine formula based on internal points in the zipcode.

Appendix Table C1: Summary statistics

| | Control | Treatment |
|---|---|---|
| Average Total spending 2004 | 10,648 | 11,649 |
| Average Inpatient spending 2004 | 2,888 | 2,981 |
| Average Outpatient spending 2004 | 5,439 | 6,123 |
| Average Rx spending 2004 | 2,321 | 2,545 |
| Age 20 to 24 | .09529 | .1107 |
| Age 25 to 29 | .07768 | .0825 |
| Age 30 to 34 | .08013 | .08016 |
| Age 35 to 39 | .084 | .08626 |
| Age 40 to 44 | .09945 | .1106 |
| Age 45 to 49 | .13 | .1237 |
| Age 50 to 54 | .1418 | .1304 |
| Age 55 to 59 | .162 | .1493 |
| Age 60 to 64 | .1297 | .1263 |
| Female | .5776 | .5595 |
| Male | .4224 | .4405 |
| Heart Disease | .3388 | .3146 |
| Diabetes | .1979 | .2061 |
| HIV/AIDS | .009941 | .008996 |
| Cancer | .05182 | .04958 |
| Rheumatoid Arthritis | .03555 | .0369 |
| Obesity | .02802 | .02873 |
| Substance Use | .05242 | .05091 |
| Mental Illness | .2121 | .2132 |
| N recipients Jan 2004 | 6,092 | 8,710 |
| N recipients Dec 2010 | 7,191 | 11,548 |
| N pre-period recipient months | 234,355 | 339,409 |
| N post-period recipient months | 315,790 | 503,044 |

**Note:** Table shows summary statistics for border zipcodes in control and treatment counties in Texas. For our analysis of zipcodes we classify zipcodes within the control and treatment counties into border and not border zipcodes. Border zipcodes are zipcodes in control counties within 25 miles of a treatment zipcode and zipcodes in treatment counties within 25 miles of a control zipcode. Not border zipcodes are all the other zipcodes in control and treatment counties. Distance is measured using as great-circle distance calculated using the Haversine formula based on internal points in the zipcode.

Appendix Table C2: Main Outcomes (Border Zipcodes)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Realized Spending | | | | Log Inpatient Spending | | | | Log Rx Spending | | | | Log Realized Outpatient Spending | | | |
| Treatment x Post | 0.087$^{**}$ | | 0.110$^{**}$ | | -0.070$^{***}$ | | -0.036 | | 0.179$^{***}$ | | 0.192$^{***}$ | | 0.068 | | 0.133$^{**}$ | |
| | (0.035) | | (0.052) | | (0.026) | | (0.023) | | (0.038) | | (0.058) | | (0.046) | | (0.061) | |
| Treatment x Post (2007-2008) | | 0.036 | | 0.069 | | -0.062$^{**}$ | | -0.028 | | 0.130$^{***}$ | | 0.162$^{***}$ | | -0.016 | | 0.042 |
| | | (0.034) | | (0.046) | | (0.026) | | (0.021) | | (0.036) | | (0.056) | | (0.048) | | (0.054) |
| Treatment x Post (2009-2010) | | 0.133$^{***}$ | | 0.138$^{**}$ | | -0.072$^{***}$ | | -0.035 | | 0.219$^{***}$ | | 0.210$^{***}$ | | 0.158$^{***}$ | | 0.201$^{***}$ |
| | | (0.047) | | (0.067) | | (0.027) | | (0.028) | | (0.053) | | (0.070) | | (0.058) | | (0.072) |
| IV Coefficient | 0.113$^{***}$ | 0.112$^{**}$ | 0.168$^{**}$ | 0.165$^{**}$ | -0.090$^{***}$ | -0.086$^{***}$ | -0.055 | -0.049 | 0.231$^{***}$ | 0.226$^{***}$ | 0.292$^{***}$ | 0.287$^{***}$ | 0.088 | 0.098 | 0.203$^{**}$ | 0.202$^{**}$ |
| | (0.042) | (0.044) | (0.079) | (0.081) | (0.032) | (0.028) | (0.034) | (0.031) | (0.046) | (0.049) | (0.091) | (0.095) | (0.056) | (0.060) | (0.095) | (0.096) |
| Baseline Mean | 5.99 | 5.99 | 5.99 | 5.99 | .703 | .703 | .703 | .703 | 4.298 | 4.298 | 4.298 | 4.298 | 4.76 | 4.76 | 4.76 | 4.76 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for the main outcomes in Texas using only border zipcodes. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second and fourth columns show reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

# D Results by service area

Appendix Table D1: Main Outcomes, Bexar Service Area

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Realized Spending | | | | Log Inpatient Spending | | | | Log Rx Spending | | | | Log Realized Outpatient Spending | | | |
| Treatment x Post | 0.019 | | 0.132 | | -0.140 | | -0.071 | | 0.093 | | 0.167 | | 0.006 | | 0.160 | |
| | (0.067) | | (0.095) | | (0.097) | | (0.112) | | (0.107) | | (0.107) | | (0.065) | | (0.109) | |
| Treatment x Post (2007-2008) | | 0.016 | | 0.063 | | -0.088 | | -0.041 | | 0.017 | | 0.052 | | -0.007 | | 0.080 |
| | | (0.071) | | (0.092) | | (0.093) | | (0.091) | | (0.097) | | (0.107) | | (0.072) | | (0.090) |
| Treatment x Post (2009-2010) | | 0.024 | | 0.196* | | -0.223 | | -0.098 | | 0.214 | | 0.275** | | 0.027 | | 0.235* |
| | | (0.085) | | (0.104) | | (0.143) | | (0.154) | | (0.138) | | (0.112) | | (0.086) | | (0.133) |
| IV Coefficient | 0.025 | 0.026 | 0.225 | 0.233 | -0.187* | -0.209** | -0.121 | -0.124 | 0.124 | 0.160 | 0.285 | 0.300* | 0.008 | 0.015 | 0.273 | 0.283 |
| | (0.070) | (0.070) | (0.162) | (0.163) | (0.100) | (0.104) | (0.182) | (0.186) | (0.113) | (0.115) | (0.178) | (0.178) | (0.069) | (0.068) | (0.190) | (0.193) |
| Baseline Mean | 7.232 | 7.232 | 7.232 | 7.232 | 1.663 | 1.663 | 1.663 | 1.663 | 5.521 | 5.521 | 5.521 | 5.521 | 6.198 | 6.198 | 6.198 | 6.198 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for the main outcomes in Texas's Bexar Service Area. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second and fourth columns show reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Appendix Table D2: Main Outcomes, Harris Service Area

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Realized Spending | | | | Log Inpatient Spending | | | | Log Rx Spending | | | | Log Realized Outpatient Spending | | | |
| Treatment x Post | 0.100 | | 0.000 | | -0.176 | | -0.088 | | 0.110 | | 0.172 | | 0.147 | | 0.131 | |
| | (0.060) | | (0.102) | | (0.122) | | (0.100) | | (0.089) | | (0.098) | | (0.097) | | (0.097) | |
| Treatment x Post (2007-2008) | | 0.039 | | -0.014 | | -0.271$^{*}$ | | -0.204$^{*}$ | | 0.071 | | 0.142 | | 0.079 | | 0.075 |
| | | (0.080) | | (0.094) | | (0.131) | | (0.106) | | (0.076) | | (0.109) | | (0.139) | | (0.127) |
| Treatment x Post (2009-2010) | | 0.197$^{*}$ | | 0.014 | | -0.022 | | 0.017 | | 0.173 | | 0.200 | | 0.255$^{***}$ | | 0.181 |
| | | (0.084) | | (0.145) | | (0.153) | | (0.105) | | (0.150) | | (0.109) | | (0.068) | | (0.106) |
| IV Coefficient | 0.141$^{**}$ | 0.177$^{***}$ | 0.001 | 0.004 | -0.249$^{*}$ | -0.170 | -0.166 | -0.138 | 0.156$^{*}$ | 0.176$^{*}$ | 0.324$^{*}$ | 0.329$^{*}$ | 0.208$^{**}$ | 0.244$^{***}$ | 0.246 | 0.258 |
| | (0.063) | (0.058) | (0.180) | (0.186) | (0.130) | (0.129) | (0.177) | (0.176) | (0.089) | (0.099) | (0.178) | (0.178) | (0.101) | (0.078) | (0.174) | (0.172) |
| Baseline Mean | 7.184 | 7.184 | 7.184 | 7.184 | 1.888 | 1.888 | 1.888 | 1.888 | 5.041 | 5.041 | 5.041 | 5.041 | 5.933 | 5.933 | 5.933 | 5.933 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for the main outcomes in Texas's Harris Service Area. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second and fourth columns show reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Appendix Table D3: Main Outcomes, Nueces Service Area

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Realized Spending | | | | Log Inpatient Spending | | | | Log Rx Spending | | | | Log Realized Outpatient Spending | | | |
| Treatment x Post | $0.159^{*}$ | | 0.085 | | $-0.243^{*}$ | | $-0.204^{**}$ | | $0.260^{***}$ | | 0.155 | | $0.199^{*}$ | | 0.093 | |
| | (0.085) | | (0.106) | | (0.134) | | (0.075) | | (0.064) | | (0.102) | | (0.109) | | (0.114) | |
| Treatment x Post (2007-2008) | | $0.133^{*}$ | | 0.124 | | -0.228 | | -0.132 | | $0.253^{***}$ | | $0.188^{*}$ | | 0.140 | | 0.113 |
| | | (0.073) | | (0.109) | | (0.170) | | (0.087) | | (0.084) | | (0.103) | | (0.092) | | (0.115) |
| Treatment x Post (2009-2010) | | 0.200 | | 0.049 | | -0.266 | | $-0.269^{*}$ | | $0.271^{***}$ | | 0.124 | | $0.289^{*}$ | | 0.075 |
| | | (0.120) | | (0.112) | | (0.153) | | (0.128) | | (0.078) | | (0.111) | | (0.149) | | (0.119) |
| IV Coefficient | $0.194^{**}$ | $0.200^{**}$ | 0.131 | 0.129 | $-0.296^{**}$ | $-0.295^{**}$ | $-0.313^{***}$ | $-0.317^{***}$ | $0.317^{***}$ | $0.313^{***}$ | 0.238 | 0.236 | $0.242^{**}$ | $0.260^{**}$ | 0.144 | 0.143 |
| | (0.082) | (0.086) | (0.158) | (0.158) | (0.130) | (0.123) | (0.109) | (0.111) | (0.062) | (0.058) | (0.149) | (0.149) | (0.106) | (0.110) | (0.169) | (0.169) |
| Baseline Mean | 7.514 | 7.514 | 7.514 | 7.514 | 1.997 | 1.997 | 1.997 | 1.997 | 5.726 | 5.726 | 5.726 | 5.726 | 6.676 | 6.676 | 6.676 | 6.676 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for the main outcomes in Texas's Nueces Service Area. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second and fourth columns show reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

Appendix Table D4: Main Outcomes, Travis Service Area

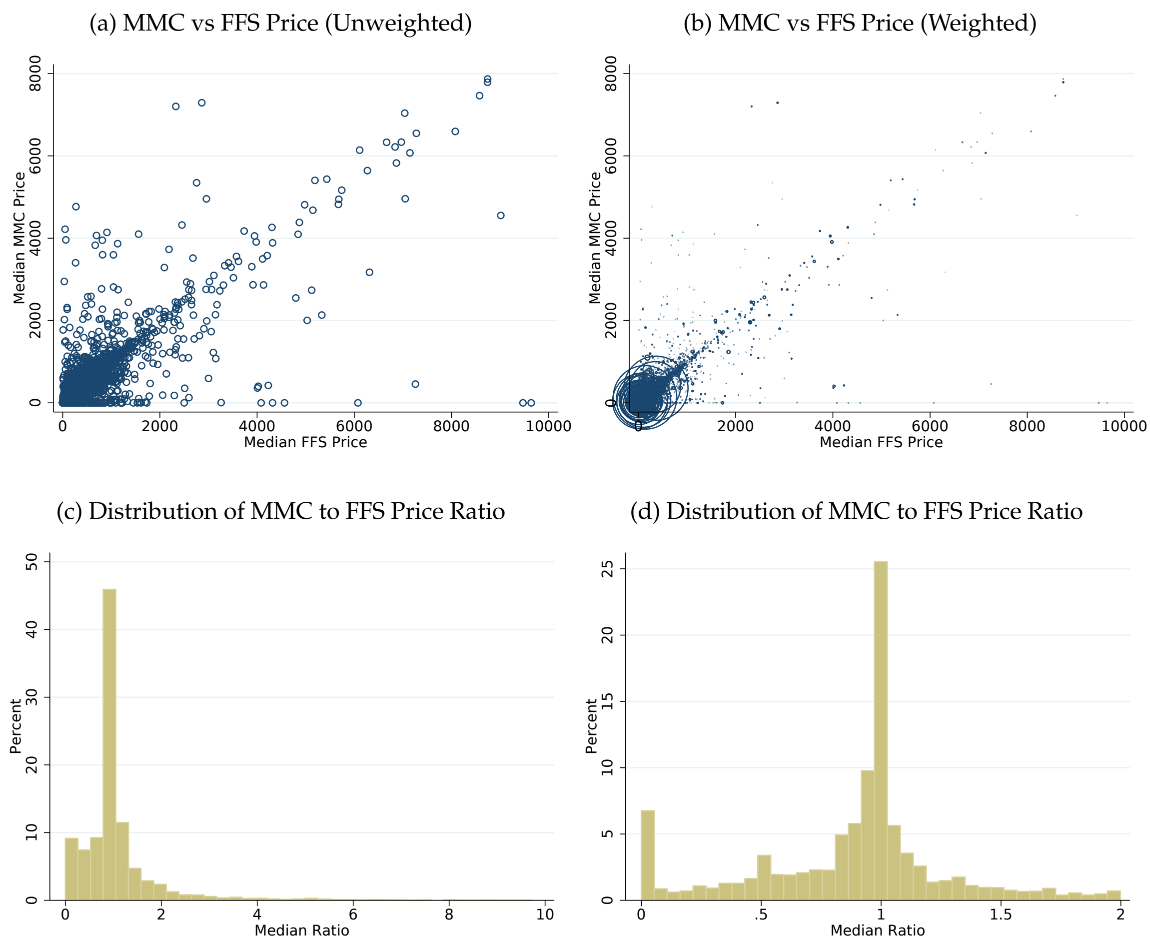
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Log Realized Spending | | | | Log Inpatient Spending | | | | Log Rx Spending | | | | Log Realized Outpatient Spending | | | |
| Treatment x Post | 0.035 | | 0.383$^{***}$ | | -0.057 | | 0.048 | | 0.130 | | 0.356$^{***}$ | | 0.062 | | 0.361$^{***}$ | |
| | (0.070) | | (0.059) | | (0.079) | | (0.036) | | (0.081) | | (0.094) | | (0.085) | | (0.062) | |
| Treatment x Post (2007-2008) | | -0.018 | | 0.240$^{***}$ | | -0.059 | | 0.000 | | 0.067 | | 0.305$^{***}$ | | 0.004 | | 0.230$^{***}$ |
| | | (0.065) | | (0.051) | | (0.075) | | (0.036) | | (0.079) | | (0.080) | | (0.081) | | (0.051) |
| Treatment x Post (2009-2010) | | 0.118 | | 0.505$^{***}$ | | -0.055 | | 0.089$^{**}$ | | 0.229$^{**}$ | | 0.401$^{***}$ | | 0.154 | | 0.474$^{***}$ |
| | | (0.096) | | (0.077) | | (0.089) | | (0.041) | | (0.091) | | (0.112) | | (0.107) | | (0.076) |
| IV Coefficient | 0.056 | 0.102 | 0.812$^{***}$ | 0.849$^{***}$ | -0.093 | -0.085 | 0.101 | 0.115 | 0.211$^{**}$ | 0.256$^{***}$ | 0.756$^{***}$ | 0.765$^{***}$ | 0.101 | 0.149 | 0.766$^{***}$ | 0.800$^{***}$ |
| | (0.087) | (0.091) | (0.125) | (0.128) | (0.097) | (0.094) | (0.074) | (0.075) | (0.098) | (0.096) | (0.193) | (0.197) | (0.104) | (0.105) | (0.127) | (0.130) |
| Baseline Mean | 6.621 | 6.621 | 6.621 | 6.621 | 1.365 | 1.365 | 1.365 | 1.365 | 4.394 | 4.394 | 4.394 | 4.394 | 5.506 | 5.506 | 5.506 | 5.506 |
| Individual Fixed Effects | X | X | | | X | X | | | X | X | | | X | X | | |

Standard errors in parentheses
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

**Note:** Table shows reduced form and instrumental variable estimates for the main outcomes in Texas's Travis Service Area. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled reduced form specification in Equation 2 and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation 4, pooling over the entire post period. The second and fourth columns show reduced form and instrumental variable estimates, when the post period is broken into two separate periods, 2007-2008 and 2009-2010. For more details, see Section 4.2.

# E Price analysis

Appendix Figure E1: Prices Under FFS and MMC




(a) MMC vs FFS Price (Unweighted)

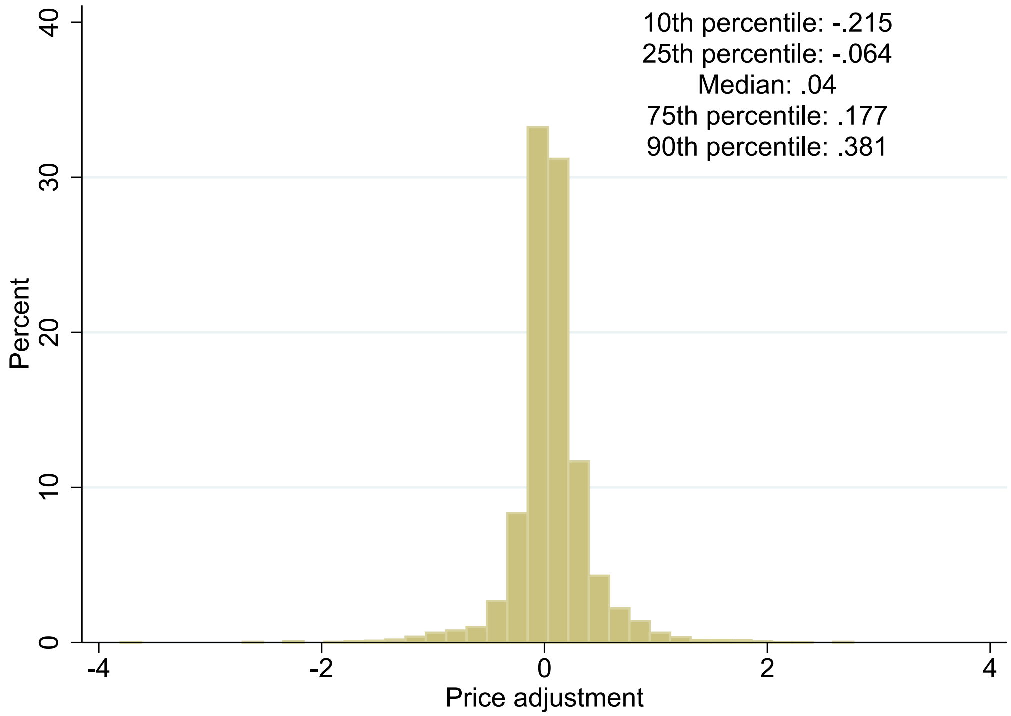
(b) MMC vs FFS Price (Weighted)

(c) Distribution of MMC to FFS Price Ratio

(d) Distribution of MMC to FFS Price Ratio

**Note:** Figure shows how MMC and FFS prices compare in 2010, the final year in our data. For each procedure that we observe both under fee-for-service (FFS) Medicaid and Medicaid managed care (MMC), we compute the median price under FFS and under MMC. Panel (a) shows an unweighted scatterplot of the median MMC price vs the median FFS price, censored at $10,000 for readability. Panel (b) shows a weighted (by frequency under FFS) scatterplot of the median MMC price vs the median FFS price, censored at $10,000 for readability. Panel (c) shows a histogram of the distribution of the ratio of the median MMC price to the median FFS price, censored at 10 for readability. Panel (d) shows a histogram of the distribution of the ratio of the median MMC price to the median FFS price, censored at 2 for readability. For more details, see Section 8.2.

Appendix Figure E2: Distribution of Procedure-specific Price Differences




**Note:** Figure shows the distribution of procedure specific price differences. We estimate Equation 5 on the sample of procedures that we observe both under fee-for-service (FFS) Medicaid and Medicaid managed care, allowing the price difference to vary by procedure. We then plot the distribution of the estimated price differences. For more details, see Section 8.2.

Appendix Table E1: Per day payments and price adjustment coefficient

(a) Per day payments

| | (1) Realized Cost Per Day (Median) | (2) Realized Cost Per Day (Mean) | (3) Log Realized Cost Per Day (Median) | (4) Log Realized Cost Per Day (Mean) |
|---|---|---|---|---|
| Treatment $\times$ *Post* | 53.529*** | 72.112*** | 0.170*** | 0.208*** |
| | (4.969) | (6.023) | (0.015) | (0.015) |
| IV Coefficient | 73.353*** | 98.818*** | 0.232*** | 0.285*** |
| | (6.604) | (8.377) | (0.016) | (0.015) |
| Baseline Mean | 179.622 | 179.622 | 4.78 | 4.78 |

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) Price adjustment coefficient

| | (1) Log Medicaid Payment |
|---|---|
| Medicaid Managed Care | 0.084*** |
| | (0.000) |

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Note:** Panel (a) shows control-treatment differences in per-day outpatient spending in Texas. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.Panel (b) shows the estimated difference in log Medicaid payments between Medicaid managed care and fee-for-service Medicaid. The results are from estimating Equation 5. For more details, see Section 8.2.

Appendix Table E2: Price-adjusted Outpatient Spending Outcomes

| | (1) Realized Cost | (2) Realized Cost | (3) Adjusted Cost | (4) Adjusted Cost | (5) Adjusted (No Heterogeneity) Cost | (6) Adjusted (No Heterogeneity) Cost |
|---|---|---|---|---|---|---|
| Treatment x Post | 312.056*** | | 250.863*** | | 90.129** | |
| | (38.158) | | (44.157) | | (39.189) | |
| Treatment x Post (2007-2008) | | 169.799*** | | 121.225*** | | -6.897 |
| | | (31.471) | | (37.113) | | (31.876) |
| Treatment x Post (2009-2010) | | 426.839*** | | 353.162*** | | 159.304*** |
| | | (53.595) | | (58.401) | | (55.494) |
| IV Coefficient | 488.510*** | 479.858*** | 392.715*** | 384.017*** | 141.093** | 131.628** |
| | (56.500) | (60.172) | (64.262) | (65.891) | (58.343) | (62.858) |
| Baseline Mean | 1342.537 | 1342.537 | 1339.75 | 1339.75 | 1337.574 | 1337.574 |
| Individual Fixed Effects | X | X | X | X | X | X |

| | (1) Realized Cost | (2) Realized Cost | (3) Adjusted Cost | (4) Adjusted Cost | (5) Adjusted (No Heterogeneity) Cost | (6) Adjusted (No Heterogeneity) Cost |
|---|---|---|---|---|---|---|
| Treatment $\times Post$ | 0.027 | | -0.008 | | -0.014 | |
| | (0.042) | | (0.041) | | (0.041) | |
| Treatment $\times Post(2007-2008)$ | | -0.054 | | -0.083** | | -0.089** |
| | | (0.040) | | (0.040) | | (0.039) |
| Treatment $\times Post(2009-2010)$ | | 0.111** | | 0.070 | | 0.064 |
| | | (0.051) | | (0.050) | | (0.050) |
| IV Coefficient | 0.037 | 0.051 | -0.010 | 0.003 | -0.018 | -0.004 |
| | (0.053) | (0.054) | (0.052) | (0.054) | (0.052) | (0.053) |
| Baseline Mean | 4.59 | 4.59 | 4.589 | 4.589 | 4.589 | 4.589 |
| Individual Fixed Effects | X | X | X | X | X | X |

Standard errors in parentheses
$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

**Note:** Table shows control-treatment differences in price adjusted outpatient spending outcomes. The first row shows reduced form results from estimating Equation 2 and the second row shows instrumental variables estimates from estimating Equation 4. We control for service area by year fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.