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, despite mixed evidence on welfare effects. We investigate the impact of privatization in Medicaid by exploiting the staggered introduction of county-level mandates in Texas that required disabled beneficiaries to switch from public to private plans. Compared to the public program, which used blunt rationing to control costs, we find privatization led to improvements in healthcare—including increased consumption of high-value drug treatments and fewer avoidable hospitalizations—but also higher Medicaid spending. We conclude that private provision can be beneficial when constraints in the public setting limit efficiency.

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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 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 enrolled in a private medical plan, while all Medicare beneficiaries who elect Part D prescription drug coverage obtain it through private 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 plans desire to keep spending low because they are often the residual claimants on any savings generated, while 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 of care (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 investigate the effects of shifting Medicaid coverage from public to private provision in Texas, the second largest American state by population. During the period we study, Texas had the third largest number of Medicaid enrollees in the U.S., with over 4.2 million enrollees or a full 7.5% of national Medicaid enrollment, making it important to the Medicaid program in particular and the U.S. safety net more generally. There are several advantages to using the Medicaid setting to study the more general question of private vs. public provision of health insurance. First, Medicaid is the setting where this question is most policy 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 in this setting due to frequent use of mandates to shift targeted groups of Medicaid beneficiaries from public to private plans. Third, unlike Medicare, Medicaid healthcare utilization data is often available for both public and private plan enrollees, enabling a detailed examination of how these two forms of coverage differ.

To leverage these advantages, we make use of a natural experiment in Texas in the mid-2000s, when the state 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 and abrupt; private enrollment among adults with disabilities rose from around 10% to almost 80% instantaneously. Moreover, Texas 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 novel and important. 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*). While most states have already shifted healthier Medicaid populations to private plans, the transition of individuals with disabilities to private plans is either recent, ongoing, or currently under consideration. Despite this, we know relatively little about the effects of private provision on this population.[1]

We find clear evidence that private plans rationed healthcare services to a *lesser* degree than the public fee-for-service Medicaid program. Specifically, private provision increased outpatient medical spending and prescription drug spending. 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). This suggests the supply curve for outpatient services in Medicaid is upward-sloping, consistent with previous evidence from Medicaid (Chen, 2017) and Medicare (Clemens and Gottlieb, 2014), and that private plans relax rationing of access to care by paying providers higher rates for the same services.

The mechanism behind the increase in prescription drug spending was different. We find that blunt rationing in Texas's *public* program was responsible for the increased spending under *private* provision. Prior to privatization, the state imposed strict rationing of drugs among public plan enrollees through a monthly limit of three prescriptions, while not imposing this

[1]Prior work on private provision in Medicaid has largely focused on infants and non-disabled adults and children. Exceptions are Vabson (2017) and an earlier version of this paper (Layton et al., 2019), both of which analyze a similar natural experiment in New York.

limit on private plan enrollees and instead allowing the private plans to use their own utilization management methods.[2] Although not widely known, strict rationing of prescription drugs using *ad hoc* quantity controls is a common feature of public Medicaid plans (Council of State Governments Midwest, 2013). Importantly, these limits 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 most prevalent in this population. For example, we find strong extensive margin responses to the removal 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 removal may have led to fairly large improvements in quality of life for many 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.

As rationing was relaxed for drugs and outpatient care in Texas, we find clear evidence that inpatient spending *decreased* by at least 8%, consistent with other work on private provision in Medicaid and elsewhere (Van Parys, 2015; Vabson, 2017). Importantly, this reduction is concentrated in inpatient admissions related to mental illness, diabetes, and respiratory conditions (such as asthma and COPD). While we cannot rule out increased rationing of inpatient services by private plans (i.e., "stinting"), there was little direct incentive for plans to stint since plans were not liable for these services; somewhat uniquely, inpatient care for the disabled population was "carved out" of private plan contracts and directly financed by the state. 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, 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 concurrent 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 under private provision reflects an improvement in the quality of healthcare received by

[2]Under Texas's private Medicaid program drug plans were not financially responsible for drug spending, as it was "carved out" of the private plan contracts. With no direct financial incentives to limit drug consumption, one might argue that comparing consumption under the strict drug limit imposed in the public plan to the unconstrained consumption allowed under the private plan (but financed by the state) provides few lessons for differences between public and private rationing of prescription drugs. However, in a later year, Texas "carved in" drugs to the private plan contracts. In Section 7, 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 were carved-out at the time of the transition, the shift in drug utilization under private provision reflects differences in public vs. private rationing of drugs rather than in financial incentives.

Medicaid beneficiaries—as well as their actual health—rather than stinting by private health plans. Complementary analyses of the effects of private provision on other outcomes such as mortality, employment, and exit from the SSI program also suggest improvements in health and functional capacity, although the associated results are not statistically significant.

Finally, we show that the reduced rationing and improved quality under private provision in Texas came at a cost: Fiscal (i.e., program) spending increased by 12% under private provision. This increase was mostly due to the fact that capitation payments to private plans were set 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 enrolled in private plans (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 services.

To summarize, we find that private provision leads to higher spending for the state and weaker rationing of healthcare services in Texas. These results are contrary to the conventional wisdom among policymakers that private provision saves money (Lewin Group, 2004), though they are in line with previous findings of cost increases in the economics literature (Duggan and Hayford, 2013). Furthermore, our strong evidence that Medicaid enrollees in Texas were better off in private plans, is contrary to 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).

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, our analysis unpacks the "black box" of managed care by revealing mechanisms behind the estimated increases in drug and outpatient spending and the decrease in inpatient utilization in Texas. Specifically, we provide evidence that the relaxation of a public plan drug cap and increases in payments to providers led to increases in drug and outpatient utilization, which in turn decreased inpatient admissions. Second, our results suggest that the impacts 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, a comparison of our results from Texas to results from our work on the roll-out of private provision in New York (Vabson, 2017; Layton et al., 2019) is suggestive of when private provision might be more vs. less beneficial: The shift to private provision led to a much more pronounced relaxation of rationing (along with a corresponding greater 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.[3] 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 have been 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 relate our results to the economics literature on government contracting with private firms. We focus our attention on two tools available to states when contracting with private Medicaid insurers: exclusion and payment. Most state Medicaid programs (including 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 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 while this procurement system may in some cases lead to higher levels of healthcare and fiscal spending (which we observe in Texas), it may also be the optimal system in this complex contracting environment.

Lastly, 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. The lack of work on this population's medical outcomes is surprising, given that its Medicaid expenditures ($187 billion in 2014) dwarf 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

[3]This interpretation is supported by interviews with current and former Medicaid officials in Texas, North Carolina, and Massachusetts.

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 likely benefits adults with disabilities in some settings.

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.[4] 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, and even participating Medicaid providers may treat Medicaid patients less intensively than others (Medicaid and CHIP Payment and Access Commission, 2019). 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. Providers appear to be responsive to Medicaid payments on various margins, including appointment availability (Polsky et al., 2015), waiting times (Oostrom, Einav and Finkelstein, 2017), and other measures of access (Alexander and Schnell, 2017; Chen, 2017). Low fees may also cause providers to offer lower-quality care (Hackmann, 2019). Texas pays particularly low fees, ranked 37th 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 external formulas

[4] 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*).

(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 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 in 2001 to 20 in 2010 (Lieberman et al., 2016). The caps vary in their scope, from general ones 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 in their stringency, 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. Texas imposes a particularly restrictive cap of 3 drug fills per person per month, which applies to nearly all Medicaid enrollees, including enrollees with multiple chronic conditions.[5]

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 (though not necessarily more restrictive) than those available to public Medicaid programs. On the medical side, private plans can construct customized provider networks that can include providers participating in the public program as well as providers not accepting public Medicaid. Private plans can independently negotiate payment rates with these providers and sometimes impose additional prior authorization requirements on certain services or on certain higher-cost doctors. In addition, private plans often use care managers to ensure that patients get needed treatment in order to prevent potentially costly complications or hospitalizations. With respect to drugs, private plans may have more scope to ration care than the public program, such as by instituting prior authorization requirements 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 (Man-

[5]In Texas, "[a]dults enrolled in traditional fee-for-service (FFS) Medicaid are limited to three prescriptions per month. All other Medicaid-eligible individuals are allowed an unlimited amount of prescriptions." (Texas Health and Human Services Commission, 2017) Those exempted from the Medicaid three-prescription limit are children under the age of 21, people enrolled in private Medicaid plans, and people enrolled in eligibility waiver programs. "Drugs and products that are not counted towards the three-prescription limit include family planning drugs, diabetic supplies, smoking cessation products, home health supplies, and mosquito repellent." (Texas Health and Human Services Commission, 2018)

att, 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.

The greater availability of these tools to private plans, combined with the sharper incentives private plans have for applying them, can potentially allow the 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 7.4 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 Managed Care Program

Texas transitioned a subset of adults with disabilities out of their publicly managed fee-for-service Medicaid program and into private Medicaid managed care plans during the mid- to late-2000s. We now describe the institutional details of the Texas managed care program.

Texas's Medicaid program is divided into ten service areas, shown in the left panel of Appendix Figure A1 (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 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 the 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.[6] 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 the imposition of mandatory enrollment, 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

[6] 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.

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 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.

Under the Texas STAR+Plus program, inpatient services were also carved out of private plan contracts for adults with disabilities (but not for other Medicaid populations).[7] 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 the disabled population. 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.[8]

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. 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

[7]This was done to retain eligibility for federal matching of supplemental payments made to hospitals under Upper Payment Limit (UPL) regulations (Medicaid and CHIP Payment and Access Commission, 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.

[8]STAR+Plus also differs from other state Medicaid managed care programs in covering various forms of behavioral health and long-term care, whereas other states often 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 us to differentiate these services from other non-inpatient services, we do not explore the effects of private provision on these services.

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, 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. Finally, the data also tracks specific plan of enrollment, for those in private plans.

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.

Prescription Drug Data As discussed in Section 2, prescription drugs in Texas 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.

Inpatient Data Inpatient services are also carved out of private plan contracts in Texas, implying that, like prescription drugs, there is again no concern about differential reporting confounding our estimates of the effects of private provision. 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. Using this information, it is possible to classify hospitalizations into various relevant categories, including elective, emergency, and surgical admissions.

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, which the plans then report 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 (negative) 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 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, as well as for about 80% of all private Medicaid plan claims. For the 20% of 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.[9]

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 fiscal 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 prescription 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.[10]

3.4 Mortality and Employment Data

We also examine indicators of beneficiary health and functional capacity, including death, employment, and the suspension of SSI benefits using the 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. We only observe Medicaid and Medicare eligibility and county of residence but not private vs.

[9] In Appendix E we provide some evidence that the private plan payment amounts reflect actual payments rather than fee-for-service fees from the public program. Specifically, we document cross-carrier variation in prices. There are four carriers (EverCare, Amerigroup, Molina, and Superior HealthPlan) contracted by Texas Medicaid. We estimate their impact on the negotiated price after accounting for provider, procedure, and time fixed effects. The distribution of the estimated carrier fixed effects appears plausible, suggesting that the observed negotiated rate information is high-quality and reflects actual payments. If the payment amounts were just fee-for-service amounts or charges, we would not expect to observe the amount of cross-carrier variation in prices *within a provider* that we estimate.

[10] Private plan premium payments include $50 per person per month in administrative costs (Texas Health and Human Services Commission, 2007). 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.

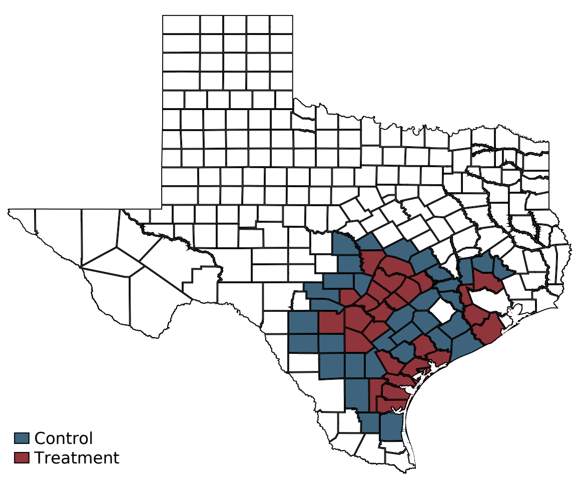
public plan enrollment in the SSA data. Mortality is defined as a binary indicator for whether a beneficiary died in a given quarter. Employment is defined as a binary indicator for whether the beneficiary had positive earnings in a given quarter. SSI suspension is defined as a binary indicator for whether 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.

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, 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.

Figure 1: Treatment and Control Counties



Note: Figure shows the Texas counties that we include in our sample as treatment and control counties.

Treatment (red) and control (blue) counties are shown in 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

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 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.

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 more comparable.[11] To further ensure comparable treatment and control groups, in Appendix B we provide additional results where we zoom in 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

[11] An alternative strategy would be to use urban counties where STAR+Plus was not rolled out as control counties. These counties would potentially include the El Paso and Houston areas. Unfortunately, the state rolled out other programs in these cities around this time, making this approach infeasible.

the right panel of Appendix Figure A1. 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 Appendix Figure A1. 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.

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%.[12] 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 directly identify these exempted groups. Second, upon enrolling in Medicaid, new beneficiaries 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, meaning that some new Medicaid beneficiaries (including those in private Medicaid) may be denoted as having at some point been covered under the public program.

4.2 Regression Framework

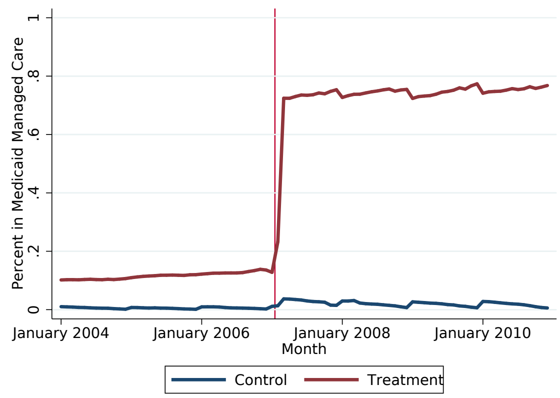
Because take-up is incomplete, we present reduced form estimates as well as instrumental variable (IV) estimates. The IV estimates are local average treatment effects (LATEs) specific to the population of disabled beneficiaries who complied with the private plan enrollment mandate (70% of the population). 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, α_{st} represents the full set of service area-by-

[12] In our analyses we drop all beneficiaries who are enrolled in a private plan at any point before February 2007. These individuals are not excluded from Figure 2.

Figure 2: First Stage



Note: Figure shows Medicaid managed care enrollment in treatment and control counties. 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.

quarter fixed effects illustrated in the right panel of Appendix Figure A1, and ϵ_{it} represents a random error term. We also include a full set of individual fixed effects, γ_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-Q4_2010) using a modified version of the regression described in Equation (1) where we replace the quarter-by-quarter interactions between quarter dummies and $Treat_{it}$ with a single indicator equal to 1 for any quarter during the post period, $Post_t$. In this regression the coefficient on $Post_t$ 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:

$$Private_{it} = \delta_0 + \delta_1 Treat_{it} \times Post_t + \alpha_{st} + \gamma_i + \eta_{it} \tag{2}$$

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 η is a random error term. Here, δ_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:

$$Y_{it} = \theta_0 + \theta_1 \widehat{Private}_{it} + \alpha_{st} + \gamma_i + \psi_{it} \tag{3}$$

where $\widehat{Private}_{it}$ represents the predicted values from Equation (2) and ψ_{it} is a random error term. Here, θ_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 θ_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 B 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 beneficiaries 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 A6 shows that there is no significant effect of private provision 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

We start by reporting the effects of private provision on healthcare spending and utilization, beginning with overall healthcare spending and then drilling down on utilization by type. Next, we proceed to assess effects on fiscal/program spending. 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 7 we study the mechanisms behind the utilization effects of the shift to private plans.

5.1 Healthcare Spending

Main results 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

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 × 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 × 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 × 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. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. We control for service area by year fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

the treatment and control counties over time. In Appendix Table A5 we also present regression results where the outcome is spending in levels rather than in logs.

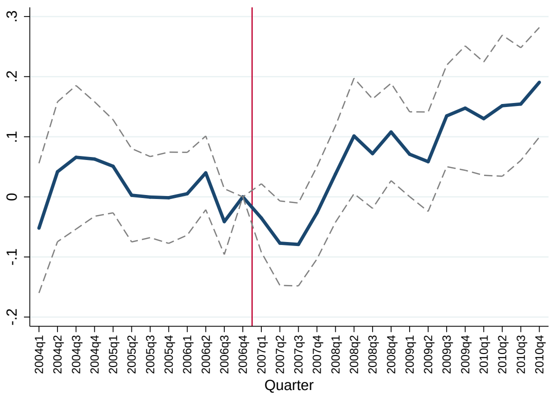
The first outcome we investigate is log total realized healthcare spending. This is not a measure of total fiscal or program spending, but instead the sum of total payments made by either the public or private plans to providers or drug manufacturers for actual healthcare services or drugs. Panel (a) 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 A5 where spending is defined in levels instead of logs also confirm these findings, 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 healthcare 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

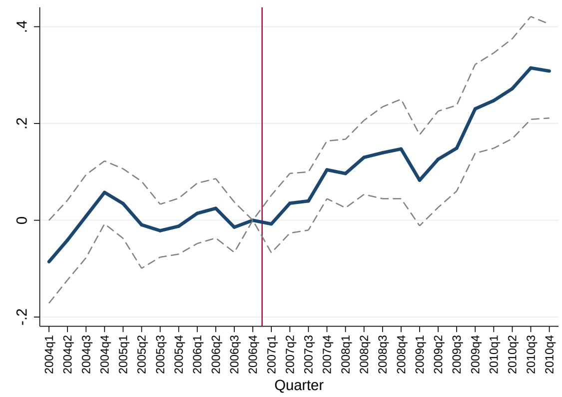
Panel (b) of Figure 3 shows the effects of the private plan enrollment mandate on log drug spending. Again, the difference in drug spending between treatment and control counties is stable prior to the mandate. Immediately following the mandate, however, drug spending begins to increase in treatment 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

Figure 3: Main Outcomes

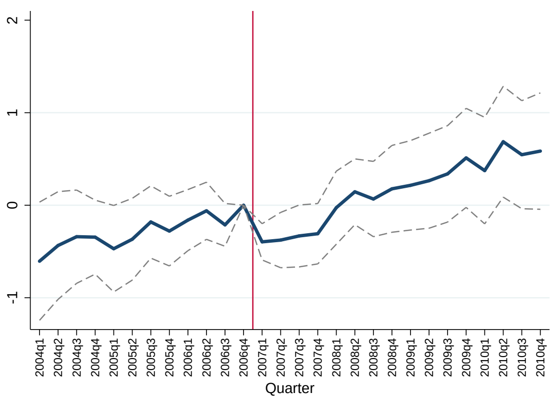
(a) Log Realized Spending



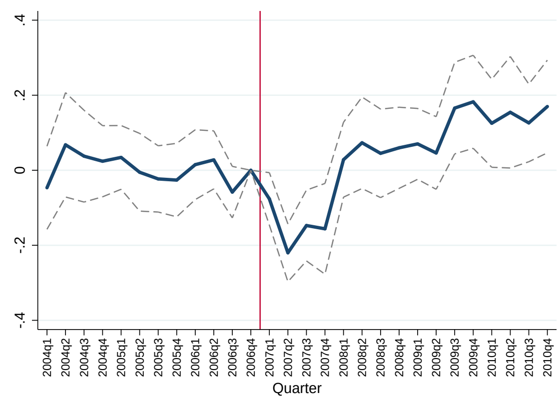
(b) Log Rx Spending



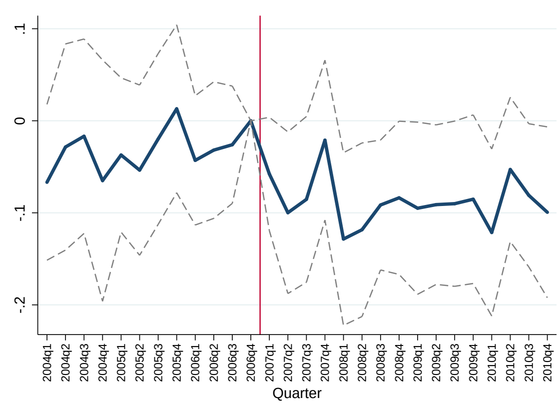
(c) Outpatient Days



(d) Log Realized Outpatient Cost



(e) Log Inpatient Spending



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

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.

Appendix Table A1 presents regression results for additional prescription drug outcomes. Specifically, we show that private plans induce beneficiaries to increase days supply of drugs by 76.6 days (41.7% of the baseline mean) 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 for this population (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 A7 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

Panels (c) and (d) of Figure 3 plot event study coefficients describing the effects of private provision on the number of outpatient days (Panel c) and log realized outpatient spending in (Panel d). 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 Appendix Table A5 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 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, especially during the first year of the STAR+Plus program (2007). 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 long-run increases.

Appendix Table A2 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. Appendix Table A2 also shows the effects of private provision on ED visits. We find a statistically insignificant decrease in ED visits in the short run, and a larger, but still insignificant, decrease in the long-run. 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, especially given that the direction of the effect on ED visits is the opposite of the direction of the effect on other types of outpatient care.

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%. Unlike for outpatient utilization, the decrease in inpatient spending is permanent and persists through the end of our sample period. Appendix Table A5 shows that these results are even stronger when focusing on spending in levels rather than logs.

Appendix Table A3 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. Appendix Table A3 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).

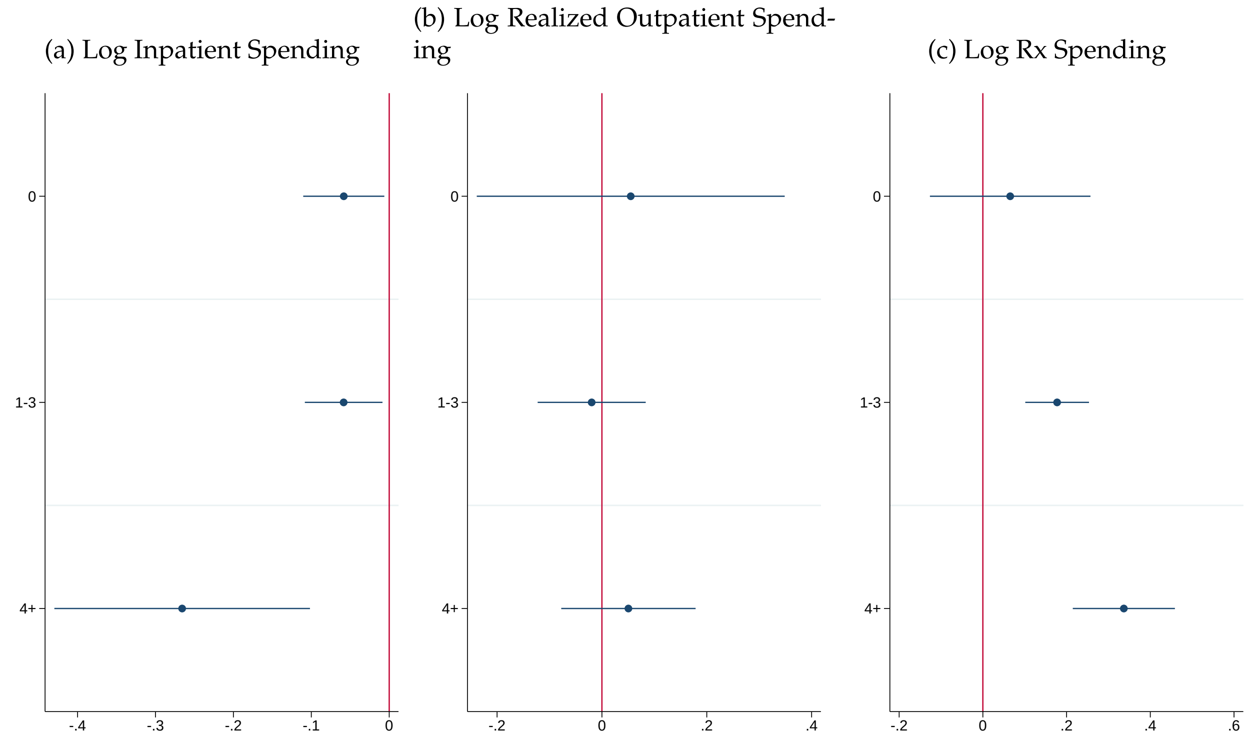
5.5 Heterogeneity

In Figure 4 and Appendix Table A9 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 A10 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 the shift to private provision 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

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

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). These coefficients are from estimating the instrumental variable specification in Equation (3) separately for each comorbidity group (no comorbidities, 1 to 3 comorbidities, and at least 4 comorbidities). For more details, see Section 4.2.

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 heterogeneity we document raises the possibility that the changing effects 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 A14 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.

Table 3: 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 × 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. For each outcome, county-level estimates of control-treatment differences are from estimating the pooled version of the reduced form specification in Equation (1) and county-level estimates of the impact of Medicaid managed care are from estimating the instrumental variable specification in Equation (3). For more details, see Section 4.2.

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 Medicaid insurers (either private plans or the government) to healthcare providers. We now ask how private provision affects the total fiscal 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 by 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. 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 3.[14] The key outcomes

[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 reveals 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.

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 spending increase appears to come both from 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 being set higher than counterfactual public plan spending for covered services. However, the "not covered" effects are also clearly larger as a percent of the baseline mean as indicated by the strong log spending results in Column 3.

The extent to which increases in fiscal spending under private provision translate into increases in realized spending indicates the degree of marginal spending "pass-through" to providers and patients (vs. private insurers). The coefficients in Columns 1 and 4 and in Columns 5 and 8 of Table 3 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.

[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 (Texas Health and Human Services Commission, 2007). 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 our estimate will capture increases in non-administrative fiscal costs under private relative to public provision, *excluding differences in spending on administrative costs.*

Border zip code analysis Concerns about the validity of our empirical approach could include potential confounding differences between treatment and control counties, given: (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 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 B 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 B1 shows the included and excluded zipcodes. This restriction effectively excludes urban centers and rural outlying areas, leading to greater similarity between treatment and control groups on some measures relative to the case where we use all zip codes in treatment and control counties. Appendix Table B1 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 B2 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.

Potential spillovers 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 B.

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 A2 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. This suggests that spillovers of the treatment onto control county patients is unlikely.

Analysis by service area To further gauge the robustness of our findings, in Appendix Tables A15-A18 we break down our difference-in-difference estimates by service area, finding 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 we are capturing the effect of shifting to private provision, rather than some other confounding factor.

6 Quality and Beneficiary Health

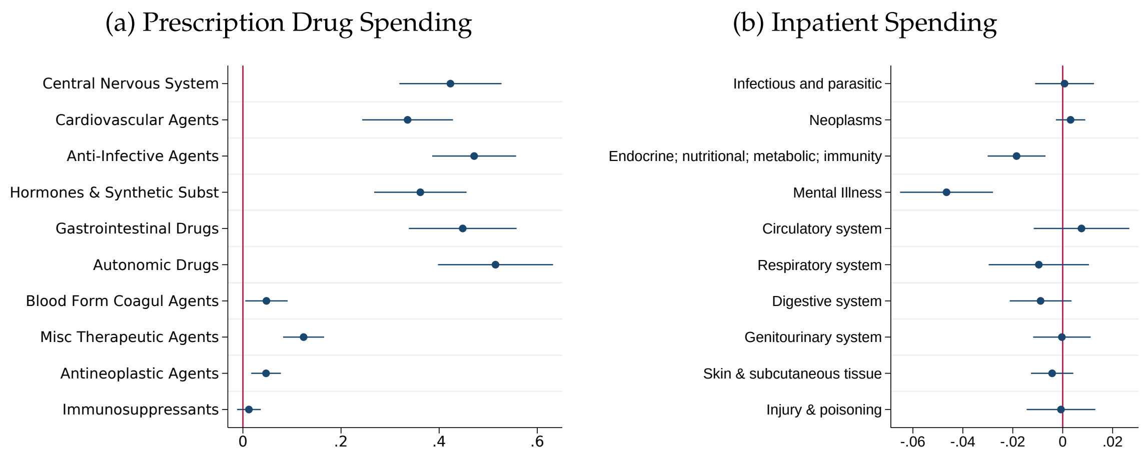
Thus far, we have assessed the effects of the shift to private provision on healthcare spending and utilization patterns. We now turn to the question of how these shifts in utilization patterns affected the quality of care received by and, ultimately, the health of SSI beneficiaries.

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 provision. 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.[16] 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.

Drug Outcomes Because drugs are carved out of private plan contracts in 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. Panel (a) of Figure 5 and Appendix Table A7 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 using narrower classes (Appendix Table A11) 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 for this population given its high rate of mental illness (see Section D). The large increase in utilization of these drugs suggests severe undertreatment of these conditions under the public Medicaid plan in

[16] 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.

Figure 5: Prescription Drug and Inpatient Spending by Category



Note: Figure shows the impact of Medicaid managed care on prescription drug spending and inpatient spending. Panel (a) shows estimates for log prescription drug spending by Redbook therapeutic category. Panel (b) shows estimates for log inpatient spending by CCS category. These coefficients are from estimating the instrumental variable specification in Equation (3) separately for each of the categories. For more details, see Section 4.2.

Texas.

The detailed results for cardiovascular agents, the second largest class in this population, are reported in Appendix Table A12. Here, the effects are driven by ACE Inhibitors, Beta Blockers, and Anti-hyperlipidemic Drugs (i.e. statins). All of these medications 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 Appendix Table A13. 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, most of the large positive effect of private provision on prescription drug utilization comes from drugs that are used to treat chronic conditions 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.

Inpatient Outcomes As with prescription 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. Panel (b) of Figure 5 and Appendix Table A8 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.[17] The strongest effect is observed for inpatient admissions related to mental illness, for which 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, potentially, quality of life. 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.[18] 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 (1) suggest a mechanism for the avoided inpatient admissions (discussed further in Section 7) and (2) are consistent with important care improvements under private provision, for example in terms of the key observable outcome of reduced 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.

Mortality and Employment Outcomes We now turn to indicators of beneficiary health and functional capacity, including death, employment, and the suspension of SSI benefits. Regres-

[17]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 and 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

[18]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.

sion specifications follow Equation (1) (intent-to-treat estimator), as we do not observe private plan enrollment in the SSA data and therefore cannot account for incomplete take-up of private provision in an instrumental variables framework.[19] Regression results are presented in Appendix Table A4 and Appendix Figure A4. 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%). 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.

7 Mechanisms

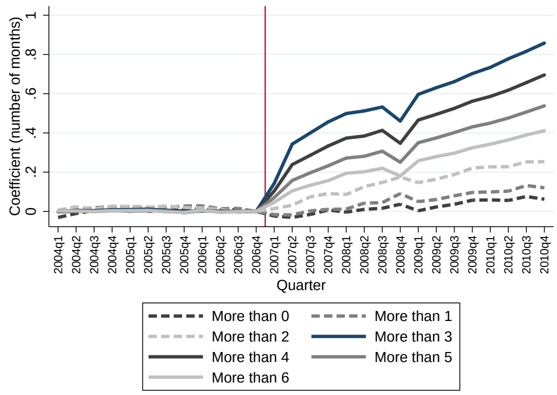
Our main analyses have provided evidence that in Texas private provision (1) increased prescription drug utilization and spending, (2) increased outpatient utilization and spending, (3) decreased inpatient utilization, (4) increased overall spending on healthcare, and (5) increased fiscal spending. In 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.

7.1 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, in some cases comparing our results from Texas to results from our work on private provision in New York, where (2) and (3) held but (1) did not (Layton et al., 2019).

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

Figure 6: Number of Months With More Than a Given Number of Unique Drugs



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

Drug caps Drugs are strictly rationed under Texas's public Medicaid program. Individuals enrolled in the public 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 6. 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) from our prior work, 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.

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), including the inpatient offsets we document in Sections 6 and 7.3. 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 5 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.[20] In Appendix Figure A3, 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) who show that when a large set of states carve prescription drug benefits into private plan contracts, patterns of utilization change in ways that are similar to states which privatized medical and drug benefits at the same time. While they do find changes overall spending, these appear driven by changes in unit prices rather than by changes in

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

utilization. The implication of this body of evidence is that there seems to be little consequence of including or excluding drugs from private plan contracts. In other words, the evidence suggests that the effect of shifting to private provision on drug utilization would have been comparable irrespective of whether drugs were carved in or carved out, at the time of the shift.

Shift to private provision of medical benefits While the analysis in Section 7.1 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 that 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 the drug effect we estimate. That said, the analysis in Section 7.1 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 our work studying private provision in New York, which had no drug cap (Layton et al., 2019).

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.

7.2 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 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 versus 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 C. 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 C1 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:

$$log(Payment_{cp}) = \beta private_c + \gamma_p + \eta_c \tag{4}$$

The unit of analysis is the claim line, and we regress the log payment on a full set of procedure fixed effects (γ_p) and an indicator for whether the claim is a private plan claim vs. a public plan claim. β 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 C1. 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 C2. 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 Appendix Table A2 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 (4) 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 C2. 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.

7.3 Inpatient Utilization Mechanisms

We showed in Section 5 that private provision led to a decrease in inpatient admissions in Texas. In Section 6 we showed that the decrease in admissions is concentrated in admissions related to mental illness, with additional significant effects on admissions related to diabetes and respiratory conditions such as asthma and COPD. Interestingly, in Section 6 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 how much of the reduction in inpatient admissions comes from increased drug utilization versus other potential mechanisms, 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. Second, in Figure 5 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 together 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.[21]

7.4 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 Texas. We start by considering the state's procurement problem and the contracting tools available to it for accomplishing 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 Texas as well as most (but not all) other states.

These two levers give the state roughly five different 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 markup 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 realized plan costs, and again select plans based on

[21] In our work studying the effects of private provision 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, 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.

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 uses 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.[22]

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 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 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.[23]

[22]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.

[23]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

Providing healthcare to hundreds of thousands (or sometimes millions) of households clearly qualifies as a complex contracting problem given that many components of healthcare provision 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 (Craig, Ericson and Starc, 2018). 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.

8 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 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. Our focus is on individuals with disabilities enrolled in the Medicaid program: the population where private provision is most likely to have an impact because enrollees are relatively sick, where the public-private transition is currently most policy relevant, and where little evidence exists on healthcare spending and outcomes despite their importance for government spending. 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 and decompose the mechanisms through which private provision impacts outcomes.

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

and large payments in later years to other plans to compensate them for losses (Forsgren, 2017).

shift to private provision.

Why do the effects of private provision differ from our prior work studying the effects of private provision in New York? 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, informal interviews with Medicaid officials from Texas, North Carolina, and Massachusetts suggest that this is precisely the political economy problem that is reality in many states. 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 are unable to precisely estimate its effects on broader measures such as disability status, functional limitations, or mortality. Future work that is sufficiently powered to link variation in private plan enrollment to these outcomes is critical for understanding the full impact of private provision. In addition, while Texas represents a large and important state, 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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Appendix for:

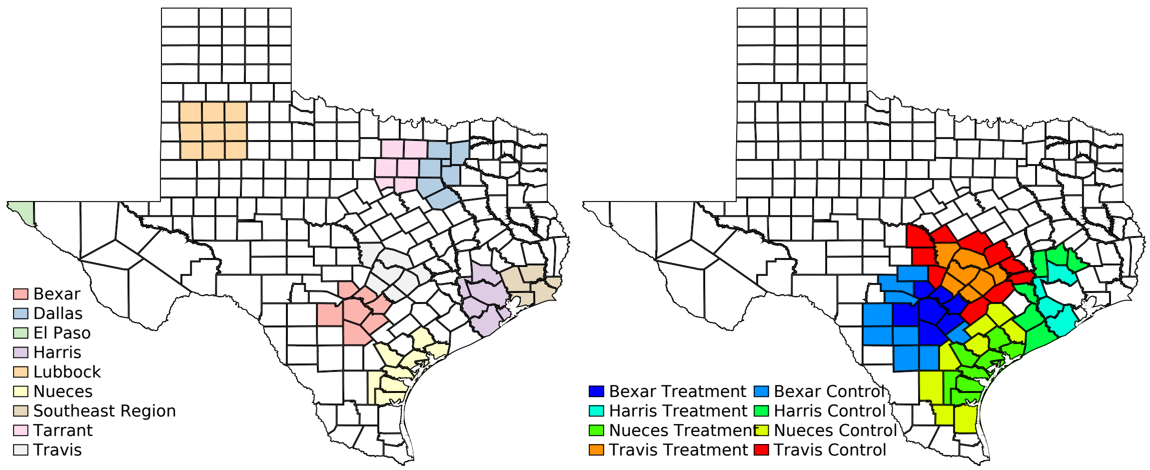
Private vs. Public Provision of Social Insurance: Evidence from Medicaid

A Additional Figures and Tables

Appendix Figure A1: Texas Counties

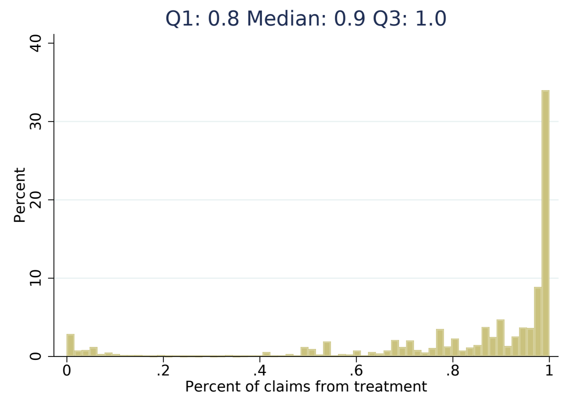
(a) Medicaid Service Areas

(b) Treatment and Control Counties by Service Area



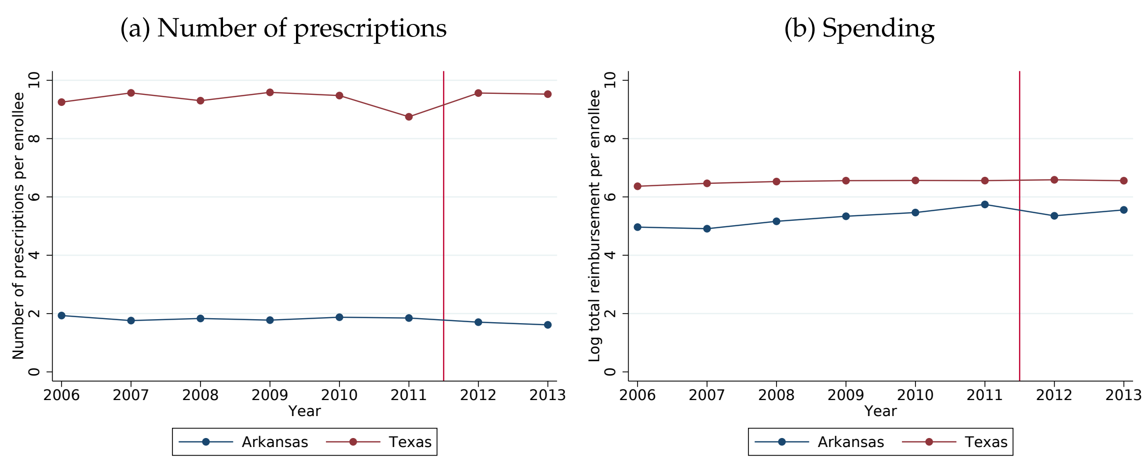
Note: Figure shows Medicaid service areas and the treatment and control counties we define based on these service areas. Panel (a) shows all ten of the Medicaid service areas designated by the Texas Health and Human Services Commission in April 2004. Panel (b) shows treatment and control counties by service area. For more details, see Section 4.1.

Appendix Figure A2: Provider Overlap



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

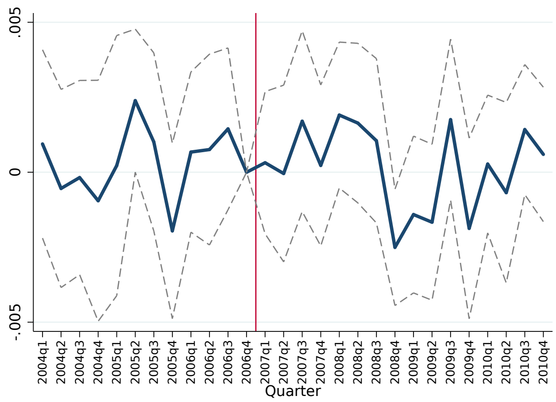
Appendix Figure A3: Impact of the Prescription Drug Carve-in



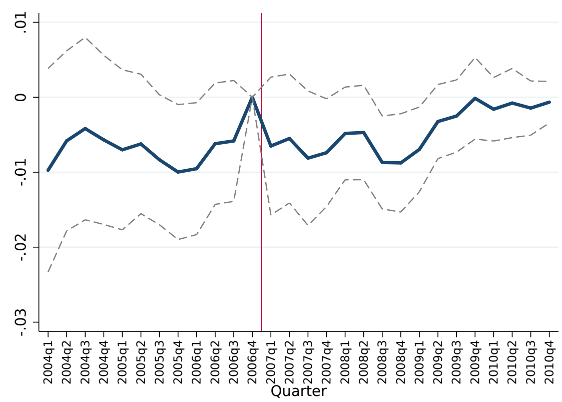
Note: Figure shows the number of prescriptions and the amount of spending per enrollee in Texas and Arkansas before and after Texas carved prescription drugs into its managed care contracts in 2012. The data displayed here come from the publicly available Medicaid State Drug Utilization Data. For more details, see Section 7.1.

Appendix Figure A4: Other Outcomes

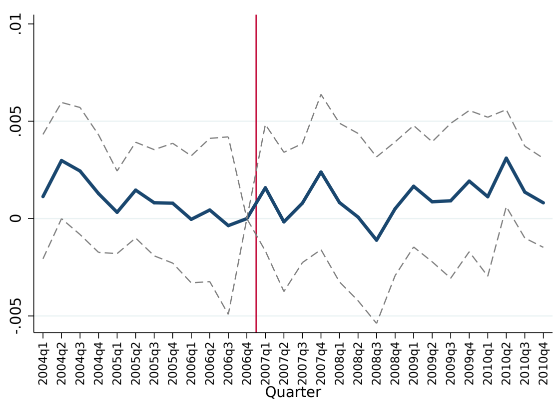
(a) Mortality



(b) Employment



(c) SSI Suspensions



Note: Figure shows control-treatment differences in mortality, employment, and SSI suspensions. 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: Rx Outcomes

	(1) Log Spending	(2) Log Spending	(3) Any Prescriptions	(4) Any Prescriptions	(5) Days Supply	(6) Days Supply	(7) Log Spending Branded Drugs	(8) Log Spending Branded Drugs	(9) Log Spending Generic Drugs	(10) Log Spending Generic Drugs
Treatment × Post	0.146***		0.003		55.723***		0.244***		0.214***	
	(0.033)		(0.004)		(6.089)		(0.037)		(0.025)	
Treatment × Post (2007-2008)		0.087***		-0.001		37.250***		0.142***		0.160***
		(0.031)		(0.004)		(4.867)		(0.033)		(0.025)
Treatment × 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. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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 A2: 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 × Post (2007-2008)		-0.054		0.121		-0.013***		-0.087
		(0.040)		(0.239)		(0.004)		(0.075)
Treatment × 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. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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: 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 × 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 × 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 × 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. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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 A4: Other Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Mortality		Employment		SSI Suspensions	
Treatment × Post	-0.00029		0.0028		0.000082	
	(0.00054)		(0.0036)		(0.0011)	
Treatment × Post (2007-2008)		0.000081		0.00034		-0.00037
		(0.00066)		(0.0033)		(0.0011)
Treatment × 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. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1), 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. 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 A5: Main Outcomes (Levels)

	(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 × Post	365^{***}		214^{**}		-47^{**}		-18		118^{***}		117^{***}		312^{***}		274^{***}	
	(111)		(92)		(21)		(21)		(16)		(17)		(38)		(37)	
Treatment × Post (2007-2008)		188^{*}		124^{**}		-33		-7		66^{***}		68^{***}		170^{***}		178^{***}
		(108)		(56)		(24)		(22)		(13)		(12)		(31)		(29)
Treatment x Post (2009-2010)		535^{***}		249^{**}		-45^{*}		-18		178^{***}		149^{***}		427^{***}		322^{***}
		(151)		(122)		(26)		(25)		(22)		(22)		(54)		(52)
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. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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 A6: Medicaid Entry and Exit

	(1) Enter	(2) Exit
Treatment × 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 reduced form and instrumental variable estimates for Medicaid entry and exit. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 A7: 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-suppres-sants	(10) Misc Thera-peutic Agents
Treatment × 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-suppres-sants	(10) Misc Thera-peutic Agents
Treatment × 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 reduced form and instrumental variable estimates for the most common therapeutic classes. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 A8: 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 × 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 reduced form and instrumental variable estimates for the top 10 most common Clinical Classification Software (CCS) groups of diagnoses. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 A9: 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 × 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 × 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 × 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 reduced form and instrumental variable estimates for the main outcomes broken down by pre-period health status. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 A10: Outcomes by Age

(a) 20-34

	(1) Log Inpatient Spending	(2) Log Realized Outpatient Cost	(3) Log Rx Spending	(4) ED Visits
Treatment × 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 × 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 × 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 reduced form and instrumental variable estimates for the main outcomes broken down by age. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 A11: 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 × 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 reduced form and instrumental variable estimates for the most common subclasses of the central nervous system therapeutic class. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 A12: Cardiovascular Classes

	(1) NEC	(2) ACE Inhibitors	(3) Cardiac Glycosides	(4) Anti-arrhythmic Agents	(5) Alpha-Beta Blockers	(6) Beta Blockers	(7) Calcium Channel	(8) Anti-hyper-lipidemic Drugs, NEC	(9) Hypotensive Agents, NEC	(10) Vasodilating Agents, NEC
Treatment × 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 reduced form and instrumental variable estimates for the most common subclasses of the cardiovascular agents therapeutic class. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 A13: 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 × 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 reduced form and instrumental variable estimates for the most common subclasses of the hormones and synthetic substances therapeutic class. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 A14: 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 × 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 × 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 × 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 × 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 × 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 reduced form and instrumental variable estimates for the main outcomes in a balanced panel. Panel (a) shows a shorter panel, for 2005-2008 and Panel (b) shows all years, 2004-2010. For each outcome, the first and third columns show estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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 A15: 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 × 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 × 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 × 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 version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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 A16: 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 × 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 × 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 × 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 version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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 A17: 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 × 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 × 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 × 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 version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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 A18: 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 × Post	0.035 (0.070)		0.383*** (0.059)		-0.057 (0.079)		0.048 (0.036)		0.130 (0.081)		0.356*** (0.094)		0.062 (0.085)		0.361*** (0.062)	
Treatment × Post (2007-2008)		-0.018 (0.065)		0.240*** (0.051)		-0.059 (0.075)		0.000 (0.036)		0.067 (0.079)		0.305*** (0.080)		0.004 (0.081)		0.230*** (0.051)
Treatment × Post (2009-2010)		0.118 (0.096)		0.505*** (0.077)		-0.055 (0.089)		0.089** (0.041)		0.229** (0.091)		0.401*** (0.112)		0.154 (0.107)		0.474*** (0.076)
IV Coefficient	0.056 (0.087)	0.102 (0.091)	0.812*** (0.125)	0.849*** (0.128)	-0.093 (0.097)	-0.085 (0.094)	0.101 (0.074)	0.115 (0.075)	0.211** (0.098)	0.256*** (0.096)	0.756*** (0.193)	0.765*** (0.197)	0.101 (0.104)	0.149 (0.105)	0.766*** (0.127)	0.800*** (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 version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. 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 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 deferentially 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.[24]

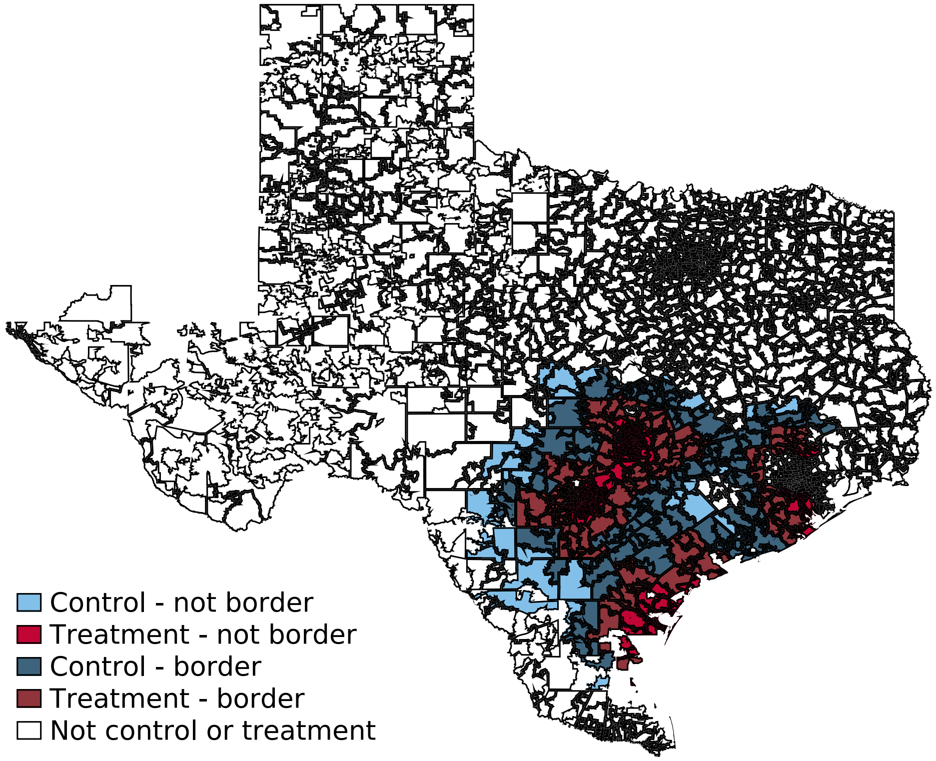
Appendix Figure B1 shows a map of zipcodes in Texas. Control and treatment counties are highlighted in shades of blue and shades of red, respectively, separating border and non-border zipcodes.

Appendix Table B1 replicates Table 1, limiting to the border zipcodes.

Appendix Table B2 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.

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

Appendix Figure B1: 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 as great-circle distance calculated using the Haversine formula based on internal points in zipcodes.

Appendix Table B1: Summary Statistics (Zipcodes)

	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. 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 as great-circle distance calculated using the Haversine formula based on internal points in zipcodes.

Appendix Table B2: 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 × Post	0.087** (0.035)		0.110** (0.052)		-0.070*** (0.026)		-0.036 (0.023)		0.179*** (0.038)		0.192*** (0.058)		0.068 (0.046)		0.133** (0.061)	
Treatment × Post (2007-2008)		0.036 (0.034)		0.069 (0.046)		-0.062** (0.026)		-0.028 (0.021)		0.130*** (0.036)		0.162*** (0.056)		-0.016 (0.048)		0.042 (0.054)
Treatment × Post (2009-2010)		0.133*** (0.047)		0.138** (0.067)		-0.072*** (0.027)		-0.035 (0.028)		0.219*** (0.053)		0.210*** (0.070)		0.158*** (0.058)		0.201*** (0.072)
IV Coefficient	0.113*** (0.042)	0.112** (0.044)	0.168** (0.079)	0.165** (0.081)	-0.090*** (0.032)	-0.086*** (0.028)	-0.055 (0.034)	-0.049 (0.031)	0.231*** (0.046)	0.226*** (0.049)	0.292*** (0.091)	0.287*** (0.095)	0.088 (0.056)	0.098 (0.060)	0.203** (0.095)	0.202** (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		

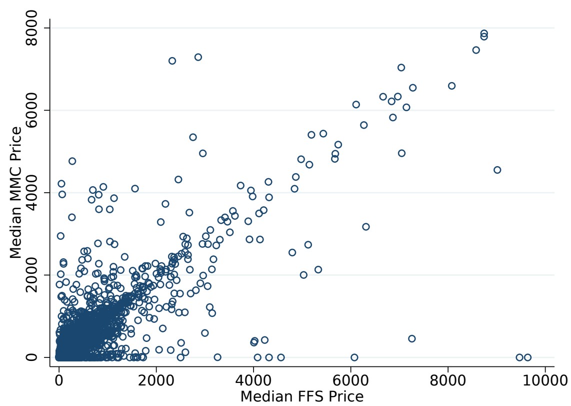
Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows reduced form and instrumental variable estimates for the main outcomes 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 (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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.

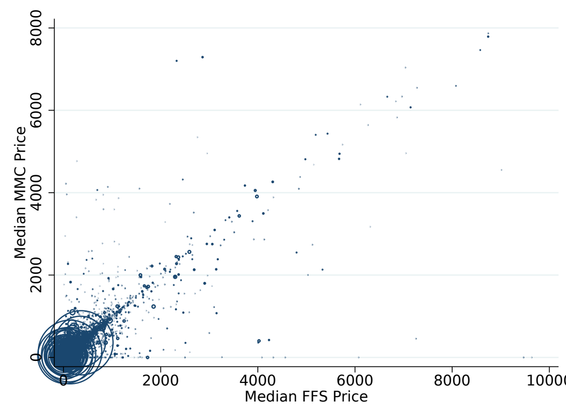
C Price Analysis

Appendix Figure C1: Prices Under FFS and MMC

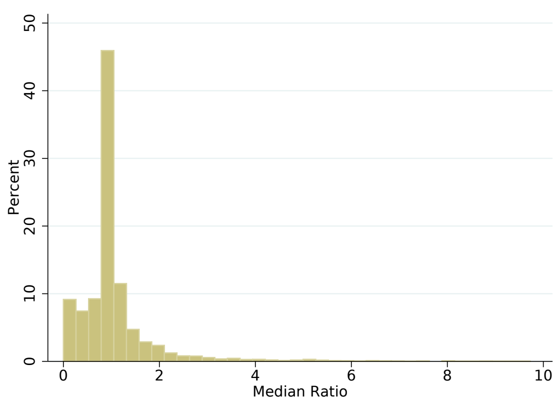
(a) MMC vs FFS Price (Unweighted)



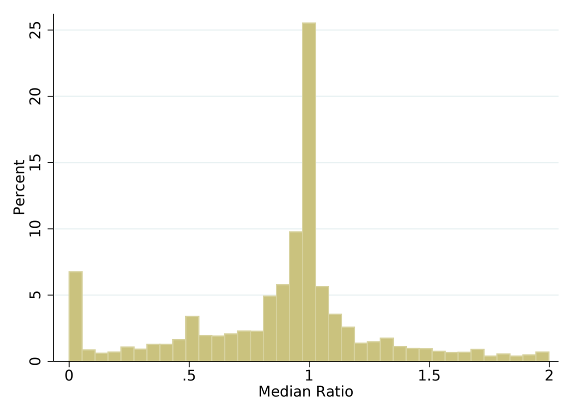
(b) MMC vs FFS Price (Weighted)



(c) Distribution of MMC to FFS Price Ratio

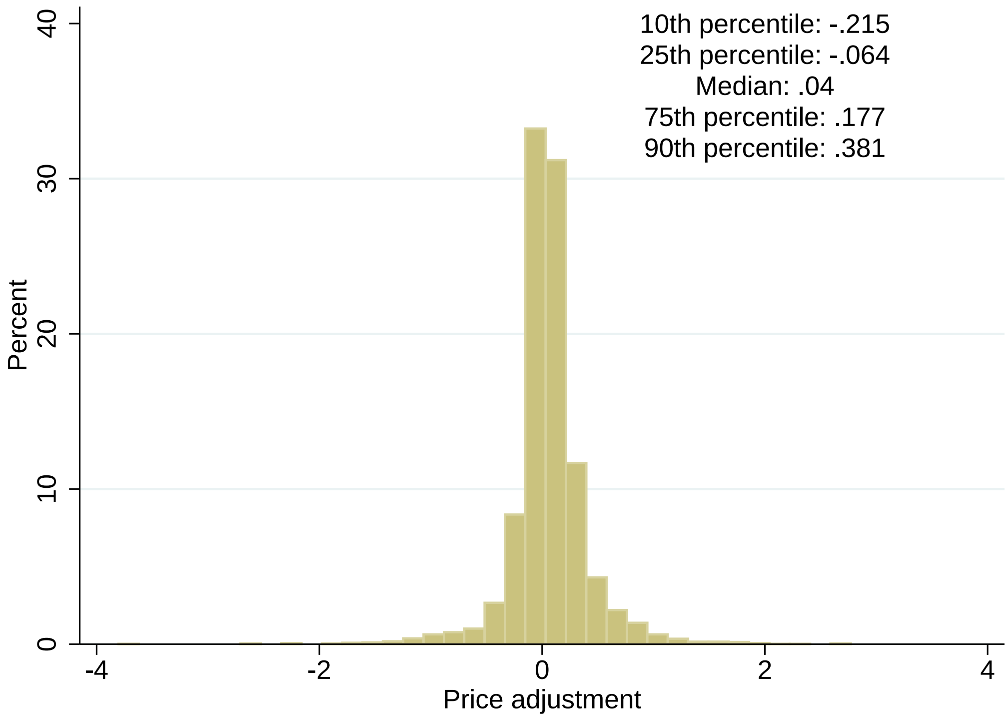


(d) Distribution of MMC to FFS Price Ratio



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 7.2.

Appendix Figure C2: Distribution of Procedure-Specific Price Differences



Note: Figure shows the distribution of procedure specific price differences. We estimate Equation (4) 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 7.2.

Appendix Table C1: 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 × 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 reduced form and instrumental variable estimates for per-day outpatient spending. The first row shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and the second row shows estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), pooling over the entire post-period. 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 (4). For more details, see Section 7.2.

Appendix Table C2: 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 × Post	312.056***		250.863***		90.129**	
	(38.158)		(44.157)		(39.189)	
Treatment × Post (2007-2008)		169.799***		121.225***		-6.897
		(31.471)		(37.113)		(31.876)
Treatment × 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) Log Realized Cost	(2) Log Realized Cost	(3) Log Adjusted Cost	(4) Log Adjusted Cost	(5) Log Adjusted (No Heterogeneity) Cost	(6) Log Adjusted (No Heterogeneity) Cost
Treatment × Post	0.027		-0.008		-0.014	
	(0.042)		(0.041)		(0.041)	
Treatment × Post (2007-2008)		-0.054		-0.083**		-0.089**
		(0.040)		(0.040)		(0.039)
Treatment × 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 reduced form and instrumental variable estimates for price-adjusted outpatient spending outcomes. For each outcome, the first column shows estimates of control-treatment differences from estimating the pooled version of the reduced form specification in Equation (1) and estimates of the impact of Medicaid managed care from estimating the instrumental variable specification in Equation (3), 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. We control for service area by year fixed effects. Standard errors are clustered at the county level. For more details, see Section 4.2.

D 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 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.[25] SSDI beneficiaries are categorically eligible for Medicare, making those SSI beneficiaries who also qualify for SSDI dually eligible for both Medicaid and Medicare. In Texas (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 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.

[25] 10 states have stricter criteria, while 7 states require a separate application but have no additional criteria. In Texas, Medicaid eligibility is automatic for SSI beneficiaries

E Price Variation Across Managed Care Plan Carriers

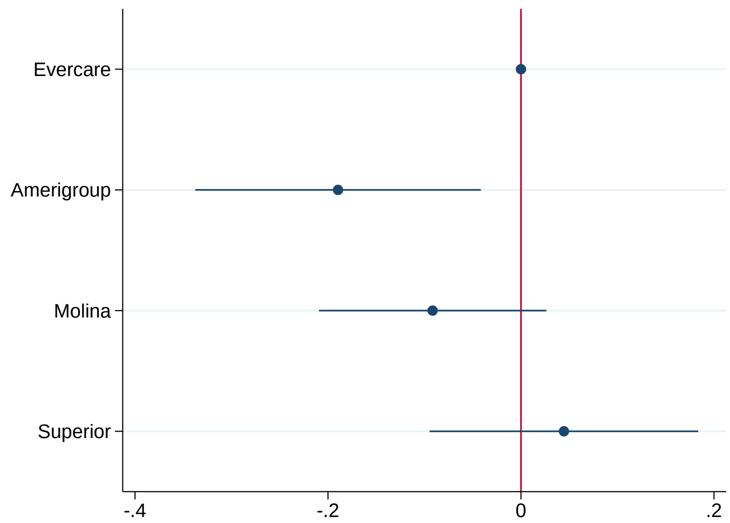
The Texas outpatient data includes information on actual cost amounts for both the public *and the private* programs. Specifically, the data contains the negotiated amounts actually paid to providers by the public or private plans at the claim-line level. These actual provider payment amounts are available for all public Medicaid claims in both states, as well as for about 80% of all private Medicaid plan claims in Texas. In this section, we examine the variation in the observed rates across the four carriers (EverCare, Amerigroup, Molina, and Superior HealthPlan) that the state contracted with in 2009 and 2010, the years during which Medicaid managed care was already rolled out and for which we have a crosswalk from plan identifiers in the data to carrier names.

We examine the sample of outpatient managed care claims for 2009 and 2010 for which the payment from plans to providers is available, which comprises 80% of managed care claims. Furthermore, we restrict to the 99.9% of claims that are associated with a plan and carrier that we observe as a plan contracted by the state of Texas in the actuarial reports. To decrease noise in prices, we exclude claims that have a quantity of service provided different from 1 and claims that have a procedure modifier code. On this final sample of outpatient managed care claims, we estimate the following regression:

$$log\left(p_{ichpt}\right) = \gamma_c + \delta_h + \psi_p + \tau_t + \varepsilon_{ichpt} \qquad (5)$$

where i indexes individuals, c indexes carriers, h indexes providers, p indexes procedures, and t indexes time; γ_c is a set of carrier fixed effects, δ_h is a set of provider fixed effects, ψ_p is a set of procedure fixed effects, and τ_t is a set of year fixed effects. We define procedures as unique combinations of procedure codes and place of service codes. We cluster standard errors at the carrier level. Figure E1 shows the estimated carrier fixed effects. Relative to the omitted carrier, Evercare, Amerigroup has on average 19% lower prices, Molina has on average 9% lower prices, and Superior has on average 4% higher prices, though these latter two differences are not statistically significant.

Appendix Figure E1: Distribution of Estimated Carrier Fixed Effects



Note: Figure shows the distribution of estimated carrier fixed effects from estimating Equation (5). Standard errors are clustered at the carrier level.