A Dose of Managed Care: Controlling Drug Spending in Medicaid

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Abstract

We study the effect of privatizing Medicaid drug benefits on drug prices and utilization. Drug spending would fall by 21.3 percent if private insurers administered all drug benefits. One-third of the decrease is driven by private insurers’ ability to negotiate prices with pharmacies. The remaining two-thirds are driven by the greater use of lower cost drugs, such as generics, and are only realized in states that give private insurers the flexibility to design drug benefits. Privatization does not decrease prescriptions per enrollee and spending cuts are smaller for drugs that lower medical spending.

JEL classification: I11, I13, L10

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1 Introduction

Rising healthcare spending – a key concern for both American households and policymakers – is driven by both prices and use. While there is no consensus about how best to control spending, rising drug prices are of particular concern. In this paper, we document the causal effect of drug procurement institutions on outcomes, including prices. We compare public, state administered programs and private insurers. The comparison is policy-relevant: public insurance programs increasingly finance and regulate competing private insurance plans rather than providing coverage directly.

Advocates of public administration argue that public insurers can unilaterally set low prices. However, public insurers often use cost plus pricing and cover nearly all services. As a result, public insurers often set prices too high for low value services and do not encourage the use of high value services. By contrast, innovation in the private insurance market has increased use of “managed care.” Advocates of privatization therefore argue that private insurers can selectively cover services and negotiate lower prices as a result. We look inside the “black box” of managed care to understand what drives potential savings from privatization. Private insurers may be able to steer demand to higher value care, negotiate lower prices, or both.

Our setting is the Medicaid program, which covers low-income Americans. Many state Medicaid programs contract with managed care organizations (MCOs) to provide health insurance benefits to program enrollees. However, state Medicaid programs historically reimbursed pharmacies for drugs on a cost-plus basis. States managed drug benefits separately from medical benefits because they were entitled to large discounts from drug manufacturers for beneficiaries enrolled in publicly administered plans.

In March 2010, the Affordable Care Act (ACA) required drug manufacturers to provide identical discounts for MCO enrollees, making it weakly better for states to privatize Medicaid drug benefits post-ACA. Many states responded by privatizing the drug benefit: the share of prescription spending that was privately administered increased from 11 percent as of the second quarter of 2010 to 58 percent as of the third quarter of 2016. We argue that privatization was a response to the ACA rule change, and exploit variation in whether and when states privatized to identify the causal effect of privatization on prescription drug spending.

Full privatization of a state’s Medicaid drug benefit decreases drug spending by 21.3 percent. MCOs negotiate point-of-sale prices with pharmacies for identical drugs that are 8.8 percent lower than pre-privatization levels. There is no change in prescriptions per enrollee. Instead, the remaining reduction in spending is due to greater use of low cost drugs, such as generics. These savings are larger in states that
allow MCOs create their own formularies – lists of favored drugs – for Medicaid patients. Privatization in our 13 treatment states, which collectively enrolled roughly a third of Medicaid beneficiaries, lowered annual Medicaid prescription drug spending by over 1.2 billion dollars.

We provide evidence against a number of alternative explanations of our results. Crucially, we find no evidence to suggest that MCOs lower spending by skimping on non-contractible product attributes; the spending changes are smaller for “high value” drugs that prevent unnecessary hospitalizations. Furthermore, our results are not driven by enrollee selection: the timing of privatization is not coincident with large changes in aggregate enrollment or Medicaid beneficiary characteristics.

While a number of studies contrast public and private provision in Medicare (Town and Liu [2003], Curto et al. [Forthcoming]) and Medicaid (Duggan [2004], Duggan and Hayford [2013], Aizer et al. [2007], Currie and Fahr [2005], Vabson [2017]), researchers rarely observe both prices and quantities of care under both systems. Yet the seminal studies documenting variation in healthcare utilization (e.g., Finkelstein et al. [2016], Fisher et al. [2003]) and prices (e.g., Anderson et al. [2003], Cooper et al. [2019]) rarely observe variation in procurement institutions. Our paper contributes to the intersection of these literatures. We illustrate how drug procurement institutions affect prices and quantities. Cutler et al. [2000] (in privately funded healthcare) and Duggan and Scott Morton [2010] (in the Medicare program) document that MCOs obtain lower prices than alternative healthcare purchasers. The magnitudes in our analyses are similar to those in Duggan and Scott Morton [2010], despite differences in the institutional setting.

Both reductions in point-of-sale prices for identical drugs and a shift to generic drugs drive our results. Consistent with Curto et al. [Forthcoming], who study privatized Medicare, we document meaningful substitution to lower cost alternatives. Taken together, our results show that private insurers lower the cost of providing prescription drugs. They are better able to steer demand away from high cost drugs and are able to negotiate lower prices with pharmacies. The results illustrate that the private sector provides one type of social insurance at lower cost than the public sector.

The paper is organized as follows. In Section 2, we provide an overview of the relevant features of the Medicaid program, particularly the role of managed care in Medicaid and the structure of drug benefits. Section 3 provides evidence on the quasi-experimental variation that underlies our analysis, and describes the data. Section 4.1 describes our empirical strategy. Section 5 details the paper’s central results measuring the effect of privatization on drug spending, quantities, point-of-sale price, and rebates. Section 7 explores the robustness of our results and includes extensions. Section 8 concludes.
2 Setting

2.1 Medicaid Drug Benefits

Medicaid is the chief health insurer for low-income families in the United States, and is funded with a combination of state and federal dollars. Some aspects of Medicaid are federally regulated, but states have considerable latitude in designing the program. Historically, eligibility was tied to income and/or assets, but also typically limited to infants, children, pregnant women, and the disabled. The ACA expanded eligibility to all low-income adults. As of December 2016, Medicaid enrolled over 68 million individuals – roughly one in five Americans. As of 2010, roughly 50 percent of Medicaid enrollees were covered by a MCO responsible for medical spending.\(^1\) Privatization of Medicaid benefits can vary based on an enrollee’s location, reason for eligibility (e.g., pregnancy or disability), and benefit type (e.g., primary care or prescription drugs). Existing research suggests that Medicaid MCOs enroll a healthier subset of the Medicaid population (Duggan and Hayford [2013]).

Figure 1: Share of drug spending administered by MCOs, privatization dates, and Medicaid expansion dates for 13 treatment states

Notes: Sample is restricted to 13 treatment states. Dashed red lines and state abbreviations give state privatization dates. The y-axis shows the share of drug spending by MCOs.

\(^a\) These treatment states expanded Medicaid in 2014Q1.

\(^b\) These treatment states expanded Medicaid and privatized drug coverage simultaneously.

\(^c\) These treatment states expanded Medicaid at a time that is neither 2014Q1, nor the same time they privatized drug coverage.

Drugs are a small but growing share of Medicaid spending, increasing from roughly 6 percent of spending to roughly 9 percent from 2010 to 2015. Medicaid enrollees in all states receive drug coverage, and

\(^1\)One third of Medicare enrollees are in a MCO (Duggan et al. [2016]).
pay very little for their drugs regardless of whether their benefit is publicly or privately administered (Medicaid and CHIP Payment and Access Commission [2017]). Drug manufacturers are obligated to provide discounts known as “rebates” to state Medicaid programs according to statutory formulas. The formula essentially dictates that the per prescription rebates that states receive be higher than the rebates received by any non-publicly funded drug purchaser. Historically, drug manufacturers were not required to provide rebates to states or MCOs for drugs purchased by MCOs for Medicaid patients. The presence of mandatory rebates for state drug purchases, but not MCO purchases, provided a strong incentive for states to retain control of drug spending. In 2010, under 15 percent of Medicaid drug spending was administered by MCOs. The ACA mandated that drug manufacturers provide states with rebates on drugs purchased by MCOs for Medicaid patients. As a result, privatizing drug benefits became weakly more attractive post-ACA than it was pre-ACA.

The result was a dramatic shift of drug benefits administration away from state governments to MCOs. By 2016, 13 states transitioned from public provision to MCO provision of Medicaid drug benefits, and MCOs managed over 55 percent of Medicaid drug benefits. The increase in the share of Medicaid drug benefits administered by MCOs is more dramatic in the 13 treatment states. As Figure 1 illustrates, in the second quarter of 2010 (the first quarter in our analyses), effectively no drug spending was administered by MCOs in our treatment states. By the third quarter of 2016 (the final quarter of our analyses), over 80 percent of drug spending was administered by MCOs. Figure 1 also presents each state’s privatization date, and notes the relationship between a state’s privatization date and whether and when the state expanded Medicaid. States privatized in a staggered way, with only North Dakota and Indiana privatizing Medicaid drug benefits and expanding Medicaid concurrently.

2.2 Conceptual Framework

In this subsection, we describe how privatization could lower drug spending. We define drug spending $c_{it}$ for enrollee $i$ in quarter $t$ as:

$$c_{it} = \sum_h \sum_d p_{hdt} q_{ihdt},$$

(1)

where $d$ represents an individual drug and $h$ represents an individual pharmacy. MCOs could alter quantities $q_{ihdt}$ and/or prices $p_{hdt}$, which we address in turn.

2States and MCOs are free to negotiate supplemental rebates with drug manufacturers both pre and post-ACA; empirically supplemental rebates are minimal.
MCOs could lower drug prices; Figure A.10 provides a graphical explanation of how prices are determined for both public and private Medicaid drug administration. The actual amount that a state or MCO spends on a drug is $p_{hdt}$, and is equal to the price paid to pharmacies ($\varphi_{hdt}$) minus any rebates from drug manufacturers ($\rho_{dt}$): $p_{hdt} = \varphi_{hdt} - \rho_{dt}$. Pharmacy prices are also known as “point-of-sale prices,” and are the primary focus of our analysis. Point-of-sale prices and rebates are set separately using different mechanisms. Under public administration, federal regulations mandate that point-of-sale prices equal a pharmacy’s expected average cost. States reimburse pharmacies the sum of an estimated ingredient cost and a dispensing fee, both of which are administratively determined.

In contrast to the highly regulated government procurement system, MCOs can selectively contract with pharmacies and bargain over point-of-sale prices. Ingredient costs and dispensing fees are fungible from the perspective of both the MCO and the pharmacy, so we consider the overall point-of-sale price. The MCO’s bargaining position depends on how hard it is to exclude pharmacies from their network and is largely determined by competition for enrollees (Ho and Lee [2017]) and political constraints, including advocacy and legal action by excluded pharmacies. The pharmacy’s bargaining position depends on the ability of the MCO to steer consumers away to (potentially lower cost) competitor pharmacies if an agreement is not reached.³ Anecdotally, exclusion of high cost independent pharmacies is an important cost control mechanism (Ross [2017]).⁴

Two institutional factors suggest that MCOs will be able to negotiate lower point-of-sale prices. First, the per prescription dispensing fees are typically set by law to be higher for Medicaid than for private insurers (Cardona [2011]). Second, Medicaid’s estimated ingredient costs are often inflated well above actual drug acquisition costs (Alpert et al. [2013]).⁵ As a result, reimbursed point-of-sale prices may greatly exceed pharmacies’ marginal costs, and a MCO may be able to negotiate point-of-sale prices lower than the previously administered rates. While these two facts are specific to Medicaid, the incentives that contribute to them are likely of broader relevance to public payers.⁶ Finally, total prices also depend on rebates from

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³MCOs often set binding point-of-sale price ceilings above which they will not reimburse a pharmacy, leaving the pharmacy to decide between accepting the price ceiling as reimbursement and filling the prescription or not filling the prescription.

⁴This could harm enrollee health if reduced access to pharmacies reduced adherence; we will explore the effect of privatization on quantities in our empirical work.

⁵Within the context of Medicaid drug benefits, they show that cost-based reimbursement has led to “perverse competition,” in which manufacturers inflate estimated ingredient costs and pharmacies fill prescriptions for drugs with particularly inflated estimated costs.

⁶When public payers reimburse based on a drug (or procedure’s) expected average costs and do not steer consumers, they eliminate provider incentives to offer high value drugs. Furthermore, the average cost of drugs is often driven to some extent by reimbursement levels, which can distort average costs away from competitive levels.
drug manufacturers ($\rho_{dt}$). Drug manufacturers are required to pay state governments large, administratively determined rebates on all Medicaid drug spending. In addition, both MCOs and states have the option to negotiate with drug manufacturers to obtain additional rebates. Given the change in the underlying price setting mechanism, both rebates and point-of-sale prices could increase or decrease post-privatization.

MCOs could also reduce the quantity of drugs consumed; we consider three factors affecting quantities. First, MCOs could reduce the provision of drugs overall, leading to a lower $q_{ihdt}$ for all $d$. Second, MCOs could alter the mix of drugs consumed, favoring more cost-effective drugs such as generics. Although Medicaid rules limit enrollee cost-sharing and require some degree of access to almost all drugs, MCOs can affect overall drug usage and the mix of drugs consumed through the use of drug formularies.\(^7\) Third, lower cost, healthy enrollees could select into MCOs.\(^8\) We perform our analyses at the state level; by aggregating over MCO and non-MCO enrollees within a state, we eliminate concerns that the subset of enrollees within a state that are enrolled in managed care may be non-random. To alleviate concerns that privatization might lead to changes in the composition of a state’s Medicaid population, we show that there are no changes in aggregate enrollment or enrollees’ characteristics that are coincident with privatization.\(^9\)

3 Data

3.1 Drug Benefit Privatization Decisions

We divide states into three categories at any given point in time: (1) state-administered medical, state-administered drug (public/public); (2) private MCO-administered medical, state-administered drug (private/public); and (3) private MCO-administered medical, private MCO-administered drug (private/private). There are no instances of state-administered medical benefits and MCO-administered drug benefits; in private/private states, medical and drug benefits are both administered by the same MCO. After the implementation of the ACA, there are nine potential transitions as described in Table 1.\(^10\)

In our baseline analysis, we use the 16 states that are public/public or private/public in both the pre-period and the post-period as control states, and compare them against the 13 states that transitioned to

\(^7\)Formularies enumerate the drugs that Medicaid enrollees can take without restrictions, and the drugs that are only available in limited quantities or with documentation of medical necessity.

\(^8\)Private insurers have incentives to design contracts to cream-skim healthy enrollees. Screening incentives affect contract design in Medicare Part D (Lavetti and Simon [2018]; Carey [2017]), Medicaid (Kuziemko et al. [2018]), and the health insurance marketplaces (Geruso et al. [Forthcoming]).

\(^9\)We test for, but do not find, heterogeneity in the main effect by risk adjustment regime in Table 7.

\(^10\)Table A.9 identifies which states undergo each of the nine transition types.
Table 1: State transition types

<table>
<thead>
<tr>
<th>(medical / drug)</th>
<th>t=T</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>public/public</td>
<td>private/public</td>
</tr>
<tr>
<td>t=0</td>
<td>11</td>
<td>0</td>
</tr>
<tr>
<td>private/public</td>
<td>1</td>
<td>4</td>
</tr>
<tr>
<td>private/private</td>
<td>0</td>
<td>0</td>
</tr>
</tbody>
</table>

Notes: States are classified as private if a significant number of Medicaid enrollees are in MCOs that bear financial risk for enrollees’ medical spending. The cells for treatment states contain **bold numbers**. The cells for control states contain *italicized numbers*.

private/private in the post-period. As of 2010, 16 percent of Medicaid enrollees were in the control states and 33 percent were in the treatment states. We exclude the 22 states that remained private/private throughout our time period.\(^{11}\) By the end of our sample period, 47 of 51 states are either public/public or private/private. States rarely exclude drugs from MCO contracts in the post-period, and therefore a state’s decision about whether to privatize medical benefit predicts the state’s decision for a drug benefit.

Eight of the 13 states in our sample transitioned from private/public to private/private, likely reflecting a response to the ACA rule change, rather than a sudden change in preferences about drug benefit privatization. Only five of the 16 states that began as public/public privatized their drug benefits, and all of those states did so as a part of a much larger decision to privatize medical benefits. For these states, drug privatization is likely incidental to the medical privatization decision, alleviating concerns that the transition might be a function of trends in drug costs unobservable to the econometrician.

3.2 Data

For each National Drug Code (NDC) in each state and in each quarter, the Medicaid State Drug Utilization Data contain the number of prescriptions filled as well as point-of-sale (i.e., pre-rebate) prices. Since the passage of the ACA, MCO drug purchases have been collected and reported separately within the same dataset. Therefore, our analysis covers the second quarter of 2010 (the first quarter for which MCO drug utilization was reported), through the third quarter of 2016. Given that reimbursements and rebates rely on these data, state governments, the federal government, and drug manufacturers have strong incentives to ensure that they are accurate.

We obtain additional information from a number of sources. We collect data on the timing of Medicaid expansions and obtain enrollment from the Medicaid Enrollment Reports. We merge the Medicaid State

\(^{11}\)As Figure A.11 illustrates, the percent of drug benefits privately administered in these states grows from 26 percent to 64 percent over the course of our sample period.
Drug Utilization data with current and historical versions of the Drugs @ FDA database and the FDA’s National Drug Code (NDC) Directory, which enables us to determine each NDC’s active ingredient and whether it is branded or generic.\textsuperscript{12}

We construct a number of dependent variables: \textit{share MCO} is the share of Medicaid drug spending in a state-quarter that is administered by MCOs. \textit{Drug spending per enrollee} is drug spending per quarter divided by Medicaid enrollment, and is equal to\textit{ prescriptions per enrollee} times (point-of-sale) \textit{price per prescription}.\textsuperscript{13} \textit{Drug utilization per enrollee} holds point-of-sale prices fixed; specifically, we replace the NDC-state-quarter point-of-sale price per prescription with the average point-of-sale price per prescription for that NDC, and then recalculate spending at the new (average) prices. In order to measure the use of inexpensive generics, we create three variables: \textit{generic penetration} is the share of prescriptions that are filled with a generic; \textit{generic efficiency} is the share of prescriptions filled with a generic when a generic is available; and \textit{generic accessibility} is the share of prescriptions that are for drugs for which a generic is available. \textit{Generic penetration} is equal to \textit{generic efficiency} times \textit{generic accessibility}. Finally, we create \textit{share high offset} which is the share of drug spending on “Category 1” drugs, as classified by Chandra et al. [2010].\textsuperscript{14} If these drugs are not taken, a serious event, such as hospitalization, is likely to occur within the next six months.

Table 2 compares summary statistics for the second quarter of 2010 in treatment and control states.\textsuperscript{15} \textit{Drug spending per enrollee} and \textit{drug utilization per enrollee} are economically similar and statistically indistinguishable for the treatment and control states. However, there is evidence that the treatment and control states differ along several dimensions. For example, utilization of high offset drugs is 3.6 percentage points lower in treatment states than in control states. These differences suggest more room to increase this measure of efficiency in the states that privatized drug benefits. Finally, treatment states were more likely than control states to expand Medicaid under the ACA. Section 7.2 describes additional robustness checks showing that the results are not driven by the Medicaid expansions; we also present all analyses both excluding

\textsuperscript{12}In analyses that rely upon FDA data, we exclude non-matching NDCs, which account for roughly 3 percent of revenues and often are non-prescription drugs.

\textsuperscript{13}Medicare-Medicaid dual eligibles receive drug coverage from Medicare Part D, and therefore their prescription drug use is not included in the Medicaid State Utilization Data. When constructing Medicaid drug spending per enrollee, we exclude Medicare-Medicaid dual eligibles from the denominator. The exact timing of the historical reports that we were able to locate varies across years. We interpolate the enrollment data to make it available for each quarter.

\textsuperscript{14}To ensure this measure captures differences in utilization rather than differences in pricing, we replace the NDC-state-quarter point-of-sale price per prescription with the average point-of-sale price per prescription for that NDC, and then recalculates spending at the new (average) prices.

\textsuperscript{15}Figure A.12 presents histograms of our key dependent variables for treatment and control states. Figure A.13 illustrates there is no geographic clustering of our treatment and control states.
### Table 2: Summary statistics

<table>
<thead>
<tr>
<th></th>
<th>Treatment</th>
<th>Control</th>
<th>Difference</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Mean</td>
</tr>
<tr>
<td><strong>Share MCO</strong></td>
<td>0.0016</td>
<td>0.0051</td>
<td>0.0000</td>
</tr>
<tr>
<td><strong>Drug spending per enrollee</strong></td>
<td>209</td>
<td>57</td>
<td>212</td>
</tr>
<tr>
<td><strong>Prescriptions per enrollee</strong></td>
<td>3.162</td>
<td>0.892</td>
<td>2.947</td>
</tr>
<tr>
<td><strong>Price per prescription</strong></td>
<td>66.44</td>
<td>8.22</td>
<td>72.53</td>
</tr>
<tr>
<td><strong>Price per generic prescription</strong></td>
<td>19.59</td>
<td>4.86</td>
<td>22.26</td>
</tr>
<tr>
<td><strong>Price per branded (no gen comp) prescription</strong></td>
<td>248.48</td>
<td>25.62</td>
<td>265.18</td>
</tr>
<tr>
<td><strong>Price per branded (w/ gen comp) prescription</strong></td>
<td>102.45</td>
<td>14.15</td>
<td>114.37</td>
</tr>
<tr>
<td><strong>Drug utilization per enrollee</strong></td>
<td>251.84</td>
<td>74.21</td>
<td>239.00</td>
</tr>
<tr>
<td><strong>Generic penetration</strong></td>
<td>0.6611</td>
<td>0.0375</td>
<td>0.6679</td>
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<tr>
<td><strong>Generic efficiency</strong></td>
<td>0.7883</td>
<td>0.0249</td>
<td>0.7930</td>
</tr>
<tr>
<td><strong>Generic accessibility</strong></td>
<td>0.8381</td>
<td>0.0284</td>
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<td><strong>Share high offset</strong></td>
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<td><strong>Share with Medicaid</strong></td>
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<td>0.0544</td>
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<td><strong>Share black</strong></td>
<td>0.2110</td>
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<td>0.1422</td>
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<td><strong>Share Hispanic</strong></td>
<td>0.1264</td>
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<td>0.0752</td>
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<tr>
<td><strong>Share disabled</strong></td>
<td>0.1496</td>
<td>0.0422</td>
<td>0.1526</td>
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<tr>
<td><strong>Share dual</strong></td>
<td>0.1589</td>
<td>0.0378</td>
<td>0.1748</td>
</tr>
<tr>
<td><strong>Share in medical MCO</strong></td>
<td>0.3069</td>
<td>0.3194</td>
<td>0.1785</td>
</tr>
<tr>
<td><strong>Expands Medicaid?</strong></td>
<td>0.7692</td>
<td>0.4385</td>
<td>0.3125</td>
</tr>
<tr>
<td><strong>Has Republican governor</strong></td>
<td>0.4615</td>
<td>0.5189</td>
<td>0.4375</td>
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<tr>
<td><strong>Republican share of legislature</strong></td>
<td>0.4682</td>
<td>0.1220</td>
<td>0.5065</td>
</tr>
</tbody>
</table>

Notes: Unit of observation is a state in the second quarter of 2010. Differences between the treatment and control states that are statistically significant are noted as:* 0.10 ** 0.05 *** 0.01.
4 Empirical Strategy

We conduct most of the paper’s analyses at the state-quarter level. For state \( s \) in quarter \( t \) (e.g., quarter 2 of 2010), let \( Y_{st} \) be one of the dependent variables of interest, \( \alpha_s \) be a state-specific fixed effect, \( \tau_t \) be a quarter fixed effect, \( priv_{st} \) be an indicator for whether a state has begun to privatize its Medicaid drug benefit, and \( Medicaid \text{ expansion}_{st} \) be an indicator for whether the state has expanded Medicaid under the ACA. Our estimating equation is:

\[
Y_{st} = \alpha_s + \tau_t + \beta priv_{st} + \lambda Medicaid \text{ expansion}_{st} + \varepsilon_{st}.
\] (2)

We replace \( priv \) in equation (1) with indicators for the number of quarters since a state began privatizing drug benefits. We plot these lag/lead coefficients to illustrate the extent of privatization over time and the resulting effect on outcomes of interest. We weigh observations by each state’s Medicaid drug spending in the second quarter of 2010, the first quarter of our analysis, and cluster standard errors by state.

The coefficient on \( priv \) is the difference-in-differences (DID) effect of privatizing some drug benefits. We also refer to this as the “reduced form” analysis. When states privatize some drug benefits, they do not switch all drug spending into MCOs.\(^{16}\) We also create instrumental variable (IV) estimates of the effect of full privatization of drug benefits. To do this, we use equation (1) to estimate the “first stage” relationship between privatizing some drug benefits and \( share \ MCO \). The IV estimates of the effect of \( share \ MCO \) on our outcomes of interest re-scales the reduced form estimates by dividing them by the first stage estimates.

4.1 First Stage

Figure 2 plots the coefficients on lags and leads of privatization using \( share \ MCO \) as the dependent variable. The share of drug spending administered by MCOs is flat prior to privatization, jumps by nearly 60 percent in the quarter after privatization begins, and reaches nearly 80 percent within two years.\(^{17}\) When states

\(^{16}\)The enrollees that switch into MCOs are likely a non-random subset of enrollees. Specifically, many states move healthier enrollees into MCOs. For this reason, we do not compare spending within a state of those with MCO drug benefits relative to those with state administered drug benefits. Our estimates will represent a local average treatment effect for those switching into MCOs and therefore our results might not generalize to other groups of enrollees.

\(^{17}\)The estimated coefficients are all near zero prior to privatization, but are not identically zero for two reasons. First, even if \( share \ MCO \) was identically zero in the pre-period, the coefficient would not be zero because of a general time trend towards greater privatization. Second, states such as Delaware, Utah, and Iowa had very limited experiences with privatized drug benefits prior to the broader adoption decision that we study.
begin privatizing, not all drug spending shifts to MCOs. However, the large point estimate in the first stage regression suggests that the local average treatment effect that we estimate will be identified off of privatization-induced spending changes from a large share of Medicaid enrollees.

Panel A of Table 3 presents estimates of a regression in which we collapse the lags and leads into a pre-post indicator variable. Column (1) indicates that the share of drug spending administered by MCOs in the privatization states rose by 61.8 percentage points. The IV estimates will effectively re-scale the reduced form DID estimates by these first stage estimates (by approximately $1/0.618 = 1.62$). In column (2) we include pre-post indicators for states that expanded Medicaid, using the appropriate timing for each state, to control for any changes in the composition of enrollees. The coefficient on medicaid expansion is close to zero, and the coefficient on priv is unchanged and still highly significant.

5 Impact on Drug Use

5.1 Impact on prescriptions per enrollee and price per prescription

In this section, we estimate the effect of privatization on drug consumption, as measured by prescriptions per enrollee. Figure 3 shows that drug consumption is unchanged in the post-period.

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Figure A.14 plots the share of drug spending by MCOs in each of our 13 treatment states over time; the vertical dashed line denotes the privatization date. For most states, there is a sharp break in the share of drug spending administered by MCOs at the time of privatization.
By contrast, Figure 4 shows that the average point-of-sale price per prescription is flat before privatization and falls immediately following privatization. This reduction in average price could be due to lower prices for the same drugs or a shift towards less expensive drugs, or both. We will decompose the point-of-sale price decrease into these two sources.

Our figures show that the effect of privatization grows over time, which is sensible given market institutions. Patients must visit physicians and fill prescriptions; inertia in drug choice among consumers may also delay the effect of the policy change. Furthermore, prices are typically renegotiated on an annual or biannual basis, while we use the quarter as unit of analysis in our study.

### 5.2 Impact on drug spending per enrollee

We next present the net effect of flat drug consumption and lower point-of-sale drug prices on drug spending per enrollee. Figure 5 plots the coefficients of lags and leads of privatization on the log of drug spending per enrollee. While drug spending per enrollee fluctuates prior to privatization, it drops following privatization. Column (1) in Panel B of Table 3 shows the reduced form estimate of the effect of privatization on logged spending, where the predictor variable is the post-privatization indicator variable, \( priv \). Column (2) presents the IV estimate, while Columns (3) and (4) include a post-Medicaid expansion indicator as a robustness check. The estimate in our preferred specification, column (4), suggests that full privatization of drug
Figure 4: Lags and leads of relationship between privatization and $\ln(\text{price per prescription})$

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line, and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

Figure 5: Lags and leads of relationship between privatization and $\ln(\text{drug spending per enrollee})$

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line, and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.
benefits would reduce drug spending by 21.4 percent.\textsuperscript{19} In our 13 treated states, which account for roughly one-third of Medicaid enrollees, privatization of roughly 80 percent of Medicaid drug spending lowers annual drug spending by roughly 1.2 billion dollars.

Echoing the graphical presentation above, we decompose the effect of privatization on (pre-rebate) spending into its effects on prices and quantities. Panels C, D, and E of Table 3 are structured identically to Panel B, except that the dependent variables are prescriptions per enrollee, price per prescription, and drug utilization per enrollee, respectively.\textsuperscript{20} Focusing on column (4), Panels C and D further illustrate that the decrease in drug spending per enrollee is driven by a shift toward lower priced prescriptions, rather than by a decrease in the number of prescriptions per enrollee. Full privatization decreases Price per prescription by a statistically significant 27.8 percent, more than offsetting a statistically insignificant 9 percent increase in prescriptions per enrollee. The point estimate in Panel E implies that full privatization reduces drug utilization per enrollee (spending holding prices fixed at their national averages) by 14.1 percent but is statistically indistinguishable from zero. The effect of full privatization on drug utilization per enrollee is about two thirds of the effect on drug spending per enrollee, suggesting that roughly one third of the decrease in spending is due to price changes. We explore the effect of privatization on within-drug point-of-sale prices in Section 6.

5.3 Impact on generic penetration

Here, we estimate the effect of privatization on the composition of consumed drugs. Figure 6 highlights one potential driver of the decline in point-of-sale prices: substitution from branded to generic drugs. We plot the coefficients on lags and leads of the privatization decision on generic penetration (share of prescriptions that are filled with a generic). After a decrease six quarters prior to privatization, generic penetration remains flat for the next six quarters. Generic penetration increases steadily beginning at the time of privatization; two years after privatization, it has risen by roughly 5 percentage points. Panel A of Table 4 presents pooled difference-in-differences and corresponding IV estimates. According to column (4), full privatization causes generic penetration to increase by 7.73 percentage points: an 11.7 percent increase on a base of 66 percent.

Panel B of Table 4 presents estimates from a model with generic efficiency (share of prescriptions that are filled generic when a generic is available) as the dependent variable. Panel C of Table 4 presents estimates

\textsuperscript{19}If the effect of privatization on the logged dependent variable is $\beta$, then privatization causes a $100 \times (e^\beta - 1)$ percent change in the dependent variable.

\textsuperscript{20}The effect of lags and leads of the privatization on drug utilization per enrollee is presented in Figure A.15.
### Table 3: Effect of privatization on per enrollee spending

#### Panel A: Dep var = Share MCO

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<thead>
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<td></td>
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<td>[0.0758]**</td>
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<tr>
<td>Medicaid expansion</td>
<td>0.0351</td>
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<tr>
<td>R-sq</td>
<td>0.933</td>
<td>0.935</td>
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<tr>
<td>Kleibergen-Paap rk Wald F statistic</td>
<td>38.46</td>
<td>38.93</td>
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<tr>
<td>Share MCO mean</td>
<td>0.148 (0.28)</td>
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#### Panel B: Dep var = ln(drug spending per enrollee)

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<tr>
<td></td>
<td>[0.0549]**</td>
<td>[0.0682]**</td>
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<td>-0.205</td>
<td>-0.239</td>
<td></td>
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</tr>
<tr>
<td></td>
<td>[0.0884]**</td>
<td>[0.115]**</td>
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<tr>
<td>Medicaid expansion</td>
<td>0.0637</td>
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<tr>
<td></td>
<td>[0.0582]</td>
<td>[0.0651]</td>
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<tr>
<td>Spending per enrollee mean</td>
<td>221.79 (75.62)</td>
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#### Panel C: Dep var = ln(prescriptions per enrollee)

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<td></td>
<td>[0.0355]**</td>
<td>[0.0416]</td>
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<td>Share MCO</td>
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<td>[0.0494]**</td>
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<td>[0.0410]**</td>
<td>[0.0405]**</td>
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<tr>
<td>Prescriptions per enrollee mean</td>
<td>2.92 (0.86)</td>
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#### Panel D: Dep var = ln(price per prescription)

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<td>[0.0480]**</td>
<td>[0.0514]**</td>
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<td>-0.335</td>
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<td>[0.0608]**</td>
<td>[0.0719]**</td>
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<td>-0.0312</td>
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<tr>
<td></td>
<td>[0.0339]</td>
<td>[0.0369]</td>
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<tr>
<td>Price per prescription mean</td>
<td>76.76 (18.9)</td>
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#### Panel E: Dep var = ln(utilization per enrollee)

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<td>[0.0552]</td>
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<td>Share MCO</td>
<td>-0.119</td>
<td>-0.152</td>
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<td></td>
<td>[0.0896]</td>
<td>[0.117]</td>
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<tr>
<td>Medicaid expansion</td>
<td>0.0648</td>
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<tr>
<td></td>
<td>[0.0585]</td>
<td>[0.0644]</td>
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<tr>
<td>Utilization per enrollee mean</td>
<td>223.23 (72.89)</td>
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</table>

N | 741 | 741 | 741 | 741

Notes: Unreported controls include state fixed effects and quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Figure 6: Lags and leads of relationship between privatization and *generic penetration*

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line, and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

from a model with *generic accessibility* (share of prescriptions that are for drugs for which a generic is available) as the dependent variable.\(^{21}\) The coefficients in column (4) for *generic efficiency* and *generic accessibility* are both statistically significant, meaning that both within- and across-molecule substitution contribute to the increase in *generic penetration*. Full privatization would decrease the share of prescriptions filled for molecules for which no generic is available by 5.26 percentage points, or 29 percent (on a pre-privatization mean of 18.6 percent). These results imply that roughly two thirds of the increase in generic penetration results from across-molecule shifts and roughly one third is a result of within-molecule shifts from branded to generic.\(^{22}\)

A back-of-the-envelope calculation, described in Table A.10, suggests that increased use of generic and generic accessible drugs decreases drug spending by 8.9 percent. Table A.11 presents an alternative decomposition of the ways in which MCOs decrease point-of-sale prices per prescription. Price decreases for identical 9 digit NDCs and across-molecule but within drug class substitution from branded drugs to generics are the two most important mechanisms driving our results.

One concern is that increases in generic accessibility could be driven by patent expiration, rather than molecular substitution: differences across states in the types of drugs used by Medicaid patients combined

\(^{21}\)The reduced form relationships for these two variables are presented in Figure A.16 and Figure A.17.

\(^{22}\)The share of prescriptions for branded drugs with no generic equivalent available decreased by 5.26 percentage points. The share of prescriptions for generics increased by 7.73 percentage points. It follows that the share of prescriptions for branded drugs for which a generic is available decreased by 2.47 percentage points.
Table 4: Effect of MCOs on generic penetration

Panel A: Dep var = Generic penetration

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<td>0.0470 [0.0171]**</td>
<td>0.0872 [0.0265]***</td>
<td>0.0773 [0.0249]***</td>
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<td>share MCO</td>
<td>0.0236 [0.0102]**</td>
<td>0.0209 [0.00994]***</td>
<td>0.0236 [0.0102]**</td>
<td>0.0209 [0.00994]***</td>
</tr>
<tr>
<td>Medicaid expansion</td>
<td>0.0202 [0.00715]***</td>
<td>0.0188 [0.00776]***</td>
<td>0.0202 [0.00715]***</td>
<td>0.0188 [0.00776]***</td>
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<td>Generic penetration mean</td>
<td>0.733 (0.046)</td>
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Panel B: Dep var = Generic efficiency

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<td>0.0248 [0.0102]**</td>
<td>0.0497 [0.0155]***</td>
<td>0.0408 [0.0161]***</td>
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<tr>
<td>share MCO</td>
<td>0.0202 [0.00715]***</td>
<td>0.0188 [0.00776]***</td>
<td>0.0202 [0.00715]***</td>
<td>0.0188 [0.00776]***</td>
</tr>
<tr>
<td>Medicaid expansion</td>
<td>0.00662 [0.00835]</td>
<td>0.00478 [0.00743]</td>
<td>0.00662 [0.00835]</td>
<td>0.00478 [0.00743]</td>
</tr>
<tr>
<td>Generic efficiency mean</td>
<td>0.823 (0.026)</td>
<td>0.823 (0.026)</td>
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Panel C: Dep var = Generic accessibility

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<td>0.0339 [0.0146]**</td>
<td>0.0320 [0.0123]**</td>
<td>0.0549 [0.0194]***</td>
<td>0.0526 [0.0168]***</td>
</tr>
<tr>
<td>share MCO</td>
<td>0.00662 [0.00835]</td>
<td>0.00478 [0.00743]</td>
<td>0.00662 [0.00835]</td>
<td>0.00478 [0.00743]</td>
</tr>
<tr>
<td>Medicaid expansion</td>
<td>0.889 (0.035)</td>
<td>0.889 (0.035)</td>
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</tbody>
</table>

N 741 741 741 741

Notes: Unreported controls include state fixed effects and quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
with patent expiration could lead to changes in generic accessibility. To rule out this concern, we create *simulated generic accessibility*, which is how generic accessibility would have changed over time (as a result of patent expiration) if molecule-level drug shares remained fixed at the second quarter of 2010 levels. Figure A.18 illustrates that privatization does not affect this variable.

### 5.4 Impact on use of high offset drugs

![Graph showing the impact of privatization on share high offset](image)

**Figure 7: Lags and leads of relationship between privatization and share high offset**

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line, and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

MCOs reduce drug spending; however, if this reduction is attributable to high-value drugs that offset other spending, overall medical costs could increase. Figure 7 plots the effect of lags and leads of privatization on *share high offset* (share of drug utilization on drugs likely to prevent immediate adverse health consequences). There is an immediate jump of roughly 3 percent in the share of spending for high offset drugs post-privatization. In Panel C of Table 5, we present pooled versions of this analysis; we find that full privatization causes the share of utilization for high offset drugs to increase by 5.44 percentage points. Prior to privatization of drug benefits, MCOs had incentives to promote the use of high offset drugs and would have been relatively indifferent about the use of low offset drugs. Privatization might enhance the MCO’s ability to promote high offset drugs while discouraging use of low-value drugs. Panels A and B of Table 5 illustrate that there is no evidence of a change in the per enrollee utilization of high offset drugs after privatization. Instead, utilization of all other drugs decreases. Lower utilization – which holds prices...
constant – of non-high offset drugs decreased spending by roughly 12.9 percent. Table A.12, Figure A.19, and Figure A.20 re-estimate many of the paper’s central analyses separately for high offset and non-high offset drugs.

MCOs have strong incentives to keep enrollees using high offset drugs. In Table A.13, we find more muted changes for drugs in Medicare’s six protected drug classes and among drugs with orphan drug status. This assuages concerns that MCOs are not providing sufficient coverage of drugs deemed to be clinically valuable or drugs targeting vulnerable populations. Finally, a related concern is that MCOs may be exposing enrollees to substantially more financial risk through higher cost sharing; Section A.1 and A.6 do not find any evidence of increased cost sharing, either in general or for specific types of drugs that are clinically beneficial or for which cost sharing might be particularly likely to deter use.

### 6 Impact on Point-of-Sale Prices

In this section, we estimate the effect of privatization on point-of-sale prices for identical drugs, as measured by a 9-digit NDC code.

For NDC $d$ in state $s$ in quarter $t$, let $\alpha_{ds}$ be an NDC-state-specific fixed effect, $\tau_{dt}$ be an NDC-quarter fixed effect, $\text{priv}_{st}$ be an indicator for whether a state has begun to privatize its Medicaid drug benefit, and $\text{Medicaid expansion}_{st}$ be a post-Medicaid expansion indicator. Our estimating equation is given by:

$$
\ln(\text{price per prescription}_{dst}) = \alpha_{ds} + \tau_{dt} + \beta \text{priv}_{st} + \lambda \text{Medicaid expansion}_{st} + \epsilon_{dst}.
$$

Drug and time fixed effects control for pricing differentials at a very granular level.

Figure 8 presents leads and lags showing the movement of prices before and after privatization: prices are relatively flat until privatization and then drop by 5.3 percent after privatization, consistent with our previous results. Analogous regressions are presented in Table 6. Columns (1) and (2) exclude all NDC-specific fixed effects. Consistent with previous results, the estimates are meaningfully different once these fixed effects are included. The IV regression with all controls in column (6) indicates that full privatization

---

23 Privatization decreases utilization for non-high offset drugs, which account for roughly 60 percent of spending, by 21.5 percent.

24 In unreported results, we replicate the paper’s central analyses for each of 55 drug classes, and specifically examine the effect of privatization on each of Medicare’s 6 protected classes. There is substitution towards generics and therefore spending decreases for three of the six protected classes (antidepressants, antipsychotics, and immunosuppressants). By contrast, there is no evidence of changes for the other three protected classes. Medicare has flagged antidepressants, antipsychotics, and immunosuppressants as classes for which protected status may no longer be appropriate.
Table 5: Effect of privatization on use of high offset drugs

Panel A: Dep var = $ln(\text{utilization per enrollee, high offset drugs})$

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<td>$Medicaid \ expansion$</td>
<td>-0.0286</td>
<td>-0.0283</td>
<td>[0.0570]</td>
<td>[0.0601]</td>
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</table>

Panel B: Dep var = $ln(\text{utilization per enrollee, not high offset drugs})$

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<td>[0.101]*</td>
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<td>$Medicaid \ expansion$</td>
<td>0.103</td>
<td>0.112</td>
<td>[0.0591]*</td>
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Panel C: Dep var = $Share \ high \ offset$

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<td>[0.00461]***</td>
<td>[0.00488]***</td>
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Notes: Unreported controls include state fixed effects and quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Figure 8: Lags and leads of relationship between privatization and 
\( \ln(\text{price per prescription}) \)

Notes: Unit of observation is the NDC-state-quarter. Includes NDC-state fixed effects, NDC-quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by drug spending for each NDC-state combination in the first quarter in which it appears in the data. Point estimates for the effect of quarter pre-post privatization are presented in the dark line, and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

Decreases prices by 8.4 percent.\(^{25}\) Because the states’ administratively set prices do not vary by pharmacy, the results imply that bargaining lowers prices for at least some pharmacies. We find that pharmacy reimbursements from Medicaid decrease substantially for drugs that are neither generics nor high offset, while reimbursements for generics remain similar. The effect of privatization on the prices of non-generic drugs that are high offset is more mixed.

\(^{25}\)This is similar but not identical to the decrease in spending from price changes in Section 5.2. Factors such as differences in weighting, the level of aggregation, and the controls included likely contribute to the (small) difference.
Table 6: Effect of privatization on ln(price per prescription)

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<td>[0.0176]***</td>
<td>[0.0214]**</td>
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Unreported Controls

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Notes: Unit of observation is the NDC-state-quarter. The transition quarter in which a state privatizes is omitted. Observations are weighted by drug spending for each NDC-state combination in the first quarter in which it appears in the data. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01

Our data does not allow us to track utilization at the individual pharmacy location level. As a result, one concern could be that lower point-of-sale reimbursements are associated with reduced access to pharmacies or pharmacy exit (Starc and Swanson [2018]). We note that Medicaid network adequacy rules require that MCOs maintain “sufficient” networks with respect to both distance and driving time for beneficiaries. Furthermore, in unreported specifications we find no evidence of systematic pharmacy exit in response to the policy change.

6.1 Rebates

Total Medicaid spending on drugs depends on both point-of-sale prices and rebates from drug manufacturers. To examine whether privatization affects rebates, we collected supplemental data from the Medicaid Financial Management Report, which provide state-year level summary information on all Medicaid expenditures. On average, rebates reduce Medicaid drug spending by 50 percent. Given that the statutory rebates in the Medicaid program exceed those negotiated by private payers in Medicare Part D (Department of Health and Human Services [2015]), we expect the magnitude of any additional rebates to be small. In fact, only 8 percent of rebates received by our sample states in 2010 resulted from negotiations with drug manufacturers; the other 92 percent were classified as federally mandated.
We define *rebate share* as the share of point-of-sale drug spending that is returned in rebates. The numerator is constructed from the Medicaid Financial Management Reports, while the denominator is constructed from the Medicaid State Drug Utilization Data. Figure 9 and Table A.14 illustrate that privatization does not affect rebates. Because *rebate share* is constant over time and the dependent variable is in logs rather than levels, any proportional adjustments do not affect our analyses. While we cannot rule out the possibility that privatization affects rebates for some specific drugs, these results suggest that lower point-of-sale prices are not, on average, being offset by lower rebates.

**Figure 9: Lags and leads of relationship between privatization and rebate share**

Notes: Includes state fixed effects, year fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in quarters 2 through 4 of 2010. Point estimates for the effect of year pre-post privatization are presented in the dark line, and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

### 7 Robustness and Extensions

#### 7.1 Heterogeneity

This section examines three potential sources of heterogeneity in the effect of privatization. First, we collect data from the Medicaid Drug Utilization Review (MDUR) to determine whether MCOs are required to use the state’s formulary or whether MCOs design their own formularies. Formularies are an important tool for steering consumers, so we anticipate larger effects in the states in which MCOs have some control over formularies. Second, we consider whether a state risk adjusts based upon enrollee health. Without perfect risk-adjustment, MCOs may have an incentive to under-provide coverage that will be particularly attractive for sick enrollees, which could in turn affect utilization. Finally, we estimate the differential
effect of privatizing both the medical and drug benefit at the same time. We replicate our central analysis, adding interactions between privatization and indicators for simultaneous medical and drug privatization, formulary requirements, and risk-adjustment.\textsuperscript{26} Table 7 presents the results of these analyses. In Panel A, we interact \textit{priv} and the three indicators. In Panel B, we perform an IV specification in which the endogenous variables are interactions between \textit{share MCO} and the three indicators, and the instruments are the interactions between \textit{priv} and the three binary variables. Column (1) of Panel A presents the effect of the privatization when interacted with the three categorizations on \textit{share MCO}.\textsuperscript{27}

In Panel B, the coefficient on \textit{share MCO*1(Same formulary)} has the opposite sign of the effect of share MCO in many of the columns, implying that effect of privatization is stronger in states that allow their MCOs to design their own formularies. Column (2) shows that the effect of full privatization on \textit{Drug spending per enrollee} is effectively zero in states in which MCOs cannot design their own formulary. Consistent with our hypotheses in Section 2.2, Columns (6) through (8) show that the shift towards generics is stronger in states where the MCO designs the formulary.\textsuperscript{28} Figure 10 further illustrates that the effect of privatization is concentrated in states where MCOs can design their own formularies. However, Table A.16 shows that differences in point-of-sale prices are not driven by states that allow their MCOs to design their own formularies. Therefore, we conclude that bargaining with pharmacies is critical for achieving lower point-of-sale prices, and restricting formularies is critical for shifting prescriptions to generics.

By contrast, we do not find statistically significant differences between the effect of privatization in the five states that privatized medical and drug benefits concurrently and the effect of privatization in the eight states that added drug benefits into existing MCO contracts. Finally, we examine the coefficient on \textit{share MCO*1(no risk adj)}, which gives the differential effect of full privatization for the three states that do not employ health based risk-adjustment. In these states, we observe larger decreases in \textit{drug spending per enrollee}, driven by decreases in the number of prescriptions written per enrollee. Additional analyses show no differences in the effect of full privatization based on the competitiveness of a state’s insurance market or whether the state includes some of the largest Medicaid MCOs.\textsuperscript{29} Overall, the results of this section suggest

\begin{itemize}
\item \textsuperscript{26}Table A.15 presents each state’s categorization for each of the binary variables and details on data sources.
\item \textsuperscript{27}Table A.17 contains the first stages for the four endogenous variables, which mirror Column (1) of Panel A of Table 7. In the states that do not risk adjust based upon health the effect of privatization on the share of spending by MCOs is 0.27 rather than 0.69. The smaller first stage complicates inference about the effect of full privatization of drug benefits in states that do not risk adjust.
\item \textsuperscript{28}Even in the states in which the MCOs must use the state’s formulary, MCOs shifted utilization to high offset drugs (Column (9)) and substituted non-bio-equivalent generics for branded drugs; such shifts could not be achieved through formulary design alone.
\item \textsuperscript{29}These results are presented in Table A.18. Unreported results are qualitatively similar across markets with greater degrees of insurer concentration, as measured by Herfindal indices of Part D carriers.
\end{itemize}
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<th>Share MCO</th>
<th>ln(drug spending per enrollee)</th>
<th>ln(prescriptions per enrollee)</th>
<th>ln(price per prescription)</th>
<th>ln(drug utilization per enrollee)</th>
<th>Generic accessibility</th>
<th>Generic efficiency</th>
<th>Generic penetration</th>
<th>Share high offset</th>
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<td>0.0597</td>
<td>-0.231</td>
<td>-0.122</td>
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<td>0.0377</td>
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<td>0.0935</td>
<td>0.0862</td>
<td>0.202</td>
<td>-0.0196</td>
<td>-0.0390</td>
<td>-0.0494</td>
<td>-0.00211</td>
</tr>
<tr>
<td>priv*1(same formulary)</td>
<td>0.0563</td>
<td>0.180</td>
<td>0.0935</td>
<td>0.0862</td>
<td>0.202</td>
<td>-0.0196</td>
<td>-0.0390</td>
<td>-0.0494</td>
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<td>-0.0196</td>
<td>-0.0390</td>
<td>-0.0494</td>
<td>-0.00211</td>
</tr>
<tr>
<td>priv*1(no prior MCO)</td>
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Panel B: IV

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<th>ln(prescriptions per enrollee)</th>
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Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Figure 10: Heterogeneity in effect of privatization by whether MCOs can design formularies

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. The lags and leads for the two groups of states are from separate regressions; each regression controls for the privatization in the other group of states with a post-privatization indicator. Point estimates for the effect of quarter pre-post privatization by drug type are presented as lines and 95 percent confidence intervals for those coefficients, based upon standard errors that are clustered by state, are presented with dotted lines/shading.

that formulary design is an important tool with which MCOs affect drug spending.

7.2 Robustness

The most serious concern with our analyses is that they may not control adequately for the (potentially heterogeneous) effects of Medicaid expansions and differential selection into Medicaid across states and over time. Table 8 shows that the results in Sections 5.2, 5.3, and 5.4 are unchanged when we include a variety of alternative controls for the Medicaid expansion. Table A.19 shows that the results are robust to excluding states based upon whether they expanded Medicaid. The robustness of these results is unsurprising; privatization is not concurrent with changes in Medicaid enrollment (see Figure A.2). Figures 11 and 12, which are discussed in detail in Section A.2, document that Medicaid enrollee demographics do not change concurrently with privatization.
Table 8: IV Estimate of effect of full privatization (Robustness to alternative ways of controlling for Medicaid expansion)

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Unreported Controls

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Notes: Specifications are limited to treatment states that privatize for drug benefits and and control states with no MCO drug benefits. Unreported controls includes state FE, quarter FE, and an indicator for Medicaid expansion. Additional controls are indicated in each column. Enrollment is non-dual eligible Medicaid enrollment. 2013 uninsured rates are for the 18-64 population and are from Kaiser Family Foundation’s analysis of the March Current Population Survey. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. * 0.10 ** 0.05 *** 0.01
Figure 11: Lags and leads of relationship between privatization and logged expected Medicaid enrollee drug spending based on age-sex-race/Hispanic status

Notes: Includes state FEs, year FEs, and a post-Medicaid expansion indicator. The dependent variable is the log of expected Medicaid enrollee drug spending based on age-sex-race/Hispanic status. The average is calculated for individuals in the American Community Survey (ACS) with Medicaid and without Medicare for each state-year. Observations are weighted by the ACS respondents underlying these calculations in 2010 for each state. Further detail on this analysis is in Section A.2. Point estimates for the effect of year pre-post privatization is presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

Table A.20 illustrates that the results are robust to a number of alternative methods of restricting the sample of analysis states. Table A.21 confirms that the results are unchanged by winsorization, controlling for linear time trends, and equalizing the number of pre and post period quarters for each treatment state. Figure A.21 illustrates that the lags and leads specifications remain unchanged when we restrict the treatment states’ data so that the same sample of states identifies all lag and lead parameters. Table A.22 demonstrates that the results are robust to excluding individual treatment states. Table A.23 illustrates that the results are robust to utilizing variation across all states and replacing the privatization variable with share MCO (lagged by one quarter).

Finally, one concern with privatization is that private firms may profit because of selection. Private firms may skimp on product quality to cream-skim profitable patients; no similar incentive exists when a public insurer serves the whole market. The consequences of privatization - lower point-of-sale prices, a shift in spending to high offset drugs, and increased use of generics - are not naturally explained by insurer attempts to offer less generous coverage to cream-skim. We circumvent concerns that private plans are cream skimming healthy enrollees from public plans by performing our analyses at the state-level. Furthermore, we test for and fail to find changes in enrollee characteristics that are concurrent with privatization.
Figure 12: Lags and leads of relationship between privatization and Medicaid enrollee characteristics

Notes: Includes state FEs, year FEs, and a post-Medicaid expansion indicator. The average is calculated for individuals in the ACS with Medicaid and without Medicare for each state-year. Observations are weighted by the ACS respondents underlying these calculations in 2010 for each state. Point estimates for the effect of year pre-post privatization is presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

7.3 Alternative Empirical Strategies

This section presents two alternative empirical strategies.

First, one concern with our staggered difference-in-differences framework is that post-privatization changes in relative outcomes could be driven by changes in outcomes in control states. We evaluate this possibility by asking how likely it would be to observe breaks from trend in the control states. We create a placebo privatization date for each of the 16 control states by sampling with replacement from the weighted empirical distribution of privatization dates in the treatment states. We then estimate a regression that includes state fixed effects, and separate lags and leads for actual and placebo privatization. We repeat this exercise 1,000 times. Figure 13 plots the point estimates and 95 percent confidence interval of the effect of lags and leads of the actual privatization decision, the average of the 1,000 point estimates of the lags and leads of placebo treatment, and the 5th and 95th percentile of the 1,000 point estimates. The results of these analyses further illustrate that the estimated effects of privatization are driven by treatment states rather than by control states.

Second, a related concern is that the effect sizes that we observe may be more likely than it would
Figure 13: Lags and leads of relationship between actual and placebo Medicaid privatization and drug usage

Notes: Unreported controls include state fixed effects. Observations are weighted by state Medicaid drug spending the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization for actual privatization are presented as thin lines and 95 percent confidence intervals for those coefficients, based upon standard errors that are clustered by state, are presented with shading. Placebo effects are presented with a thicker line and their 95 percent confidence interval is presented with the dashed lines.
appear based on traditional statistical testing because of a small number of clusters.\footnote{Table A.25 illustrates that there are only small changes to statistical significance if we calculate \( p \)-values following the suggestions in Cameron et al. [2008].} Figure 14 presents a permutation test in which we randomly create 13 placebo privatization events in both treatment and control states. We then estimate the effect of placebo privatization on outcomes. We repeat this process 1,000 times. Figure 14 plots the distribution of the \( \beta \) coefficients, compares it to the central estimate, and lists how often the placebo treatments from the permutation tests result in an estimate more extreme than the one we actually observe. The distributions suggest that it is very unlikely that one would observe more extreme outcomes by chance.

### 7.4 Medicare Falsification Exercise

To show that changes in drug usage are not driven by contemporaneous state level factors, we present an additional placebo test using Medicare (Part D) data.\footnote{This “placebo” group could be treated if privatization led to spillovers to other insurance segments, as identified by Lakdawalla and Yin [2015] for price negotiations in Part D.} More specifically, we begin with a 10 percent sample of Medicare beneficiaries for 2010 through 2014. We restrict the sample to beneficiaries over 65 who never receive low-income subsidies. We recalculate each of our central measures for this sample.\footnote{The measures that we construct for Medicare and Medicaid patients differ slightly because in the Medicaid data quantities are measured as prescriptions whereas in the Medicare data quantities are measured as days of drugs supplied.}

Figure 15 presents the relationship between lags and leads of the Medicaid privatization decision and Medicare Part D drug usage. Table A.24 presents pooled versions of this relationship. There is no evidence of any relationship between Medicaid privatization and \textit{drug spending per enrollee}, days supply per enrollee (the analog of \textit{prescriptions per enrollee}), point-of-sale price per days supply (the analog of \textit{price per prescription}), \textit{drug utilization per enrollee} or \textit{generic efficiency} for Medicare Part D enrollees. The pooled estimate for \textit{share high offset} is statistically significant, but the point estimate is small and results from a pre-trend. For \textit{generic accessibility} (and therefore also \textit{generic penetration}), there is a noticeable break from trend at the time of privatization. The pooled estimate is statistically significant with a magnitude that is roughly one-fourth as large as that for the Medicaid drug sample. However, privatization’s effect on \textit{simulated generic accessibility} is nearly identical to its effect on \textit{generic accessibility}, which means the increased use of generics in the Medicare Part D sample is driven exclusively by patent expiration.\footnote{Figure A.18 rules out this explanation for the Medicaid sample. The entry of generic Lipitor – used heavily in the Medicare population but not the Medicaid population of interest – explains a substantial share of increased generic accessibility among Medicare Part D patients.} Overall, the falsification exercise suggests that our results are not driven by concurrent state-level shocks.
Figure 14: Distribution of permutation test effect sizes vs. actual coefficient estimates

Notes: We generated a new dataset under the null hypothesis that privatization had no effect. We re-estimated the central regression for each variable, and generated a new dataset in which we alter the data for the treatment states, so as to take out the estimated actual effect of privatization and proceed as described in the text.
Figure 15: Lags and leads of relationship between Medicaid privatization and Medicare drug usage

Notes: Unreported controls include state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line, and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines. Spending is computed for Medicare enrollees 65+ years old who never receive a low income subsidy. Data covers the second quarter of 2010 through the fourth quarter of 2014.
7.5 Effect on Overall State Spending

This section examines how privatization affects overall healthcare spending, and how states and MCOs split any savings. The overall effect of privatization will depend on the nature of procurement, the incentives the state creates for MCOs, and the level of competition in the insurance market.

We present multiple pieces of suggestive evidence on how Medicaid drug privatization affects overall healthcare spending of Medicaid patients. First, an issue with interpreting our results is that they may not represent gains from privatization, but instead gains from having the same provider of medical and drug benefits. If this was the case, we would observe larger effects in the states that began with private medical benefits. We do not. A separate concern is that decreases in drug spending may lead to increases in medical spending. Three facts should alleviate this concern. First, in all of the states that privatize drug benefits, medical benefits are managed by the same MCO, so the MCO’s incentives should be well-aligned. Second, privatization does not lower use of drugs with high medical offsets. Finally, Section A.4 does not find any evidence that privatization affects Emergency Department use by Medicaid patients, although the estimates are imprecise.

Next, we present suggestive evidence that states capture at least part of the savings from lower drug spending. First, Section A.3 examines the effect of privatization on total state spending on Medicaid, which depends on both drug spending and any spillover effects on other types of spending, as well as on how MCOs and states split any surplus that MCOs create. The estimates are noisy, but we are able to rule out large increases in spending. While we do not find evidence of changes in overall spending, the literature on offsets and recent work by Lavetti and Simon [2018] and Starc and Town [2015] highlight externalities to non-drug medical utilization. Next, Section A.5 examines the effect of privatization on the difference between MCO premium revenues and claims costs. There is suggestive evidence (although not statistically significant at conventional levels) that privatization did increase MCO profits, but that this was driven by the expansion in demand (quantity/covered lives), rather than by higher margins. While we are unable to rule out the possibility that either the MCO or the state capture the full cost savings, we note that if MCOs were capturing the full surplus, then the difference between their premium revenues and costs would be higher.

A number of pieces of anecdotal evidence support this interpretation. First, bidding for MCO contracts tends to be competitive: in Illinois, state officials only accepted six of nine bids. When Iowa expanded its

34See Illinois Department of Healthcare and Family Services [2017]. Nearly all states currently have multiple carriers.
program from a single carrier to multiple carriers, it gave contracts to only four of 10 bidders.\textsuperscript{35} Second, states increasingly require plans to submit utilization information and incorporate that information into future reimbursements (Medicaid and CHIP Payment and Access Commission [2011]); political scientists note that we should expect a ratchet effect compressing reimbursements each time the states rebid the contract (Thompson and Dilulio [1998]).\textsuperscript{36} Finally, minimum loss ratio regulations now require Medicaid MCOs to spend at least 85 percent of premium dollars on medical expenses, ensuring that the surplus is split between the parties.

8 Conclusions

State and federal officials have proposed a range of policies to combat rising drug prices. Many advocate for direct price negotiation between the Medicare program and drug manufacturers. This debate overlooks the fact that private MCOs administer a large percentage of both Medicare and Medicaid drug benefits; it is important to know whether outsourcing to MCOs has been successful. The policy change we study could save billions of dollars: privatizing 100 percent of the Medicaid drug benefits would lower drug spending 21.3 percent. In the 13 treated states, privatization lowered annual Medicaid drug spending by 1.2 billion dollars.

Several prior studies compare the performance of publicly versus privately administered Medicaid benefits (Duggan [2004], Duggan and Hayford [2013], Aizer et al. [2007], Currie and Fahr [2005], Vabson [2017]). Our approach goes inside the “black box,” identifies the sources of MCO savings, and considers possible harmful consequences. MCOs achieve one third of savings by negotiating lower prices with pharmacies, and achieve two thirds of savings by restricting formularies to steer demand to generics and drugs with high medical offsets. These results suggest that MCOs cut spending with a scalpel rather than a hatchet.

The pricing results are also consistent with those of Duggan and Hayford [2013]. They find that privatization only lowers spending in states where administered prices are high. Pre-ACA, public insurers received large, mandated rebates unattainable by private plans. As a result, privatization was not attractive. By contrast, public insurers pay pharmacies higher prices than the ones that private plans negotiated. Once the ACA equalized rebates, privatization became attractive. Public insurers do not face competitive constraints

\textsuperscript{35}See Iowa Department of Human Services [2015].

\textsuperscript{36}Our effects are consistent with Al-Ississ and Miller [2013], who find evidence of abnormal returns in the pharmaceutical sector after Scott Brown’s election endangered passage of the ACA.
on provider reimbursement rates. Therefore, the impact of privatization on prices depends on how both market and public reimbursement rates are set.

In our setting, states calculate pharmacy reimbursements on a cost-plus basis. This type of reimbursement is typical of public insurers. Furthermore, both Medicare and Medicaid cover a wide range of care, without regard to cost effectiveness. In these ways, our setting is a microcosm of publicly administered healthcare in the United States. Public coverage and reimbursement rules often avoid steering patients, even based on cost effectiveness.

Is broad coverage and cost-plus reimbursement an immutable feature of publicly administered care? The federal government recently denied the Massachusetts Medicaid program’s request to restrict the state administered formulary and pharmacy network (McCluskey (2018)). However, Medicare is experimenting with value-based payments reforms. Public systems in other countries make coverage decisions based on cost effectiveness. Thus, the performance of publicly administered care in the United States will remain a fluid issue and empirical question.

9 Bibliography

References


A Appendices (For Online Publication Only)

A.1 Point-of-Sale Drug Prices

This section examines whether the effect of privatization on point-of-sale prices varies based on whether a drug is high offset, generic, or neither. It also examines whether privatization affects cost sharing. We begin by estimating a variant of the regression in equation 3, including interactions between privatization and whether the NDC was high offset, generic, or neither. We plot the coefficients from these regressions in Figure A.1. Panels A and C plot coefficients from the regression in which the dependent variable is Medicaid price per prescription, which we define to be what Medicaid (rather than the beneficiary) pays per prescription. Panels B and D plot the coefficients from the same regression in which Cost sharing per prescription is the dependent variable, which we define to be what the beneficiary pays per prescription. In Panel A, the red line plots the coefficients on lags and leads of the privatization decision interacted with a drug being a generic, whereas the black line plots the coefficients on lags and leads of the privatization decision interacted with a drug being neither a generic nor a high offset drug. In Panel C, the red line plots out the coefficients on lags and leads of the privatization interacted with a drug being high offset, whereas the black line plots out the coefficients on lags and leads of the privatization decision interacted with a drug being neither a generic nor a high offset drug. Thus, the black lines in Panel A and Panel C are identical. Based on Panel A, pharmacy reimbursements from Medicaid are decreasing substantially for drugs that are neither generics nor high offset. By contrast, reimbursements for generics remain similar. Based upon Panel C, the drop in Medicaid per prescription reimbursements is smaller for high offset drugs than for drugs that are neither generic nor high offset.

We present a pooled versions of the differential effect of privatization on generics and high offset drugs in Table A.1. The decrease in point-of-sale prices to pharmacies is $6 less for generics than for other drugs. When pooled, there is no evidence of differential point-of-sale prices post-privatization for high offset drugs.
Table A.1: Reduced Form relationship between privatization and point-of-sale prices

Panel A: Dep var = Medicaid price per prescription

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Panel B: Dep var = Cost sharing per prescription

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Notes: Unit of observation is the NDC-state-quarter. Unreported controls include NDC-state fixed effects and NDC-quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by drug spending for each NDC-state combination in the first quarter in which it appears in the data. Standard errors are clustered by state. * 0.10 ** 0.05 *** 0.01
Figure A.1: Lags and leads of relationship between privatization and point-of-sale Medicaid reimbursement and cost-sharing

Notes: Unit of observation is the NDC-state-quarter. Includes NDC-state fixed effects, NDC-quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by drug spending for each NDC-state combination in the first quarter in which it appears in the data. Point estimates for the effect of quarter pre-post privatization by drug type are presented as lines and 95 percent confidence intervals for those coefficients, based upon standard errors that are clustered by state, are presented with dotted lines/shading.

A.2 Medicaid Expansion and Program Demographics

One concern is that our results could be driven by changes in the composition of the Medicaid population. There are no changes in aggregate Medicaid enrollment that are concurrent with privatization (Figure A.2). However, this section presents a number of analyses that examine whether the Medicaid population is changing post-privatization.

We illustrate that there is no relationship between privatization and expected drug spending based on Medicaid enrollee characteristics. We use data from both the American Community Survey (ACS) and the Medical Expenditure Panel Survey (MEPS) and restrict to the sample of enrollees that are not Medicare dual eligibles. Medicare-Medicaid dual eligibles receive drug coverage through Medicare rather than Medicaid, their drug spending is not included in the Medicaid State Drug Utilization database, and therefore they are excluded from the paper’s central analyses; Figure A.3 illustrates that the share of Medicaid enrollees who are dually eligible does not change around the time of privatization.
Figure A.2: Lags and leads of relationship between privatization and ln(Medicaid enrollment)

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

The MEPS and the ACS contain the age, sex, and race/Hispanic status of individuals. The MEPS also contains drug spending, but lacks state of residence and is based on a relatively small sample size. By contrast, the ACS lacks drug spending, but includes state of residence and is based on a relatively large sample size. Therefore, we estimate the relationship between age-sex-race/Hispanic status and drug spending with the MEPS, and then apply those estimates to the sample of individuals in the ACS to construct expected drug spending of Medicaid enrollees based on age-sex-race/Hispanic status for each state-year.\footnote{We aggregate race/Hispanic status into four groups: (1) anyone who is Hispanic, (2) non-Hispanic blacks, (3) non-Hispanic whites, (4) other. We model the relationship between age and spending separately for each sex-race/Hispanic group using a non-parametric local linear kernel regression where optimal bandwidth is chosen based on cross-validation.}

Figure 11 illustrates that there are no changes in the expected spending of Medicaid enrollees based on demographics around the time of privatization. However, we may lack the power to detect changes in enrollee composition or there may be measurement error in the reporting of health insurance status in government surveys. Figure A.4 confirms that the ACS can detect changes in enrollee composition: it documents a large increase in the risk profile of Medicaid enrollees that is concurrent with the ACA’s Medicaid expansions. Pooled results presented in Table A.2 confirm that privatization had no effect on expected spending based on patient demographics, but that the Medicaid expansion did.

Figure 12 illustrates that other Medicaid enrollee characteristics are not changing concurrently with privatization. By contrast, Figure A.5 illustrates that the Medicaid expansions did affect enrollee character-
Figure A.3: Lags and leads of relationship between privatization and the share of Medicaid enrollees who are dual eligible

Notes: Share of Medicaid enrollees who are dual eligible is calculated from administrative reports. Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line, and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.
Figure A.4: Lags and leads of relationship between privatization and logged Expected Medicaid enrollee drug spending based on age-sex-race/Hispanic status

Notes: Includes state FEs, year FEs, and an indicator for whether the state has privatized its drug benefit. The dependent variable is an average drug risk score based on age, sex, and race/Hispanic status. The average is calculated for individuals in the ACS with Medicaid and without Medicare for each state-year. Observations are weighted by the ACS respondents underlying these calculations in 2010 for each state. Point estimates for the effect of years pre-post Medicaid expansion is presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

Table A.2: Effect of privatization on ln(Expected Medicaid enrollee drug spending based on age-sex-race/Hispanic status)

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Notes: Includes state FEs, year FEs, and a post-Medicaid expansion indicator. The transition year in which a state privatizes is omitted. The dependent variable is an average drug risk score based on age and sex. The average is calculated for individuals in the ACS with Medicaid and without Medicare for each state-year. Observations are weighted by the ACS respondents underlying these calculations in 2010 for each state. Standard errors are clustered by state. * 0.10 ** 0.05 *** 0.01
Figure A.5: Lags and leads of relationship between Medicaid expansion and Medicaid enrollee characteristics

Notes: Includes state FE, year FE, and an indicator for whether the state has privatized drug benefits. The average is calculated for individuals in the ACS with Medicaid and without Medicare for each state-year. Observations are weighted by the ACS respondents underlying these calculations in 2010 for each state. Point estimates for the effect of year pre-post Medicaid expansion is presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.
A.3 Impact on overall state spending on Medicaid

We examine how privatization affects overall Medicaid spending using the Medicaid Financial Management Reports. Because this analysis does not rely on the MCO drug claims, we extend the sample period to 2007 through 2015. Medicare-Medicaid dual eligible enrollees receive drug coverage from Medicare Part D. These individuals are not affected by drug privatization and are not included in the paper’s central analyses. Dual eligibles account for only 15 percent of Medicaid enrollment but account for 39 percent of Medicaid spending. We cannot separate premiums or spending for dual eligibles from spending of other Medicaid enrollees. This decreases the anticipated magnitude of any effect of drug spending on overall spending, and adds imprecision to our estimates. To partially address this, we add the share of Medicaid enrollees that are dual eligible as an optional control in the analyses in this section.

Figure A.6 presents the relationship between privatization and total spending per enrollee. There is evidence that spending is lower after privatization than beforehand, although it is difficult to ascertain whether this represents a real break from trend. Unsurprisingly, given that drug spending is only a small share of overall Medicaid spending and the aggregated nature of our data, we are unable to rule out very economically large total spending increases or total spending decreases. Table A.3 confirms this fact. The point estimate in column (6) suggests full privatization lowers spending by a (statistically insignificant) 8.1 percent. The upper bound of the 95 percent confidence interval is 1.5 percent. To place these numbers in context, we calculate that if states were to capture all of the savings from privatization, that spending per Medicaid enrollee would decrease by roughly 85 dollars or by 1.2 percent. 38 Thus, while the evidence is suggestive that state spending decreased, we cannot rule out that MCOs captured the entire surplus.39

38 This calculation relies on a number of facts: (1) pre-rebate quarterly drug spending per non-dual eligible enrollee is roughly 210 dollars in this sample, (2) rebates to states average roughly 47 percent of pre-rebate drug spending, (3) dual eligibles are roughly 15 percent of enrollees, and (4) full privatization lower spending by 21.3 percent, and (5) non-dual prescription spending is roughly 5.5 percent of total Medicaid spending in 2015.

39 The large point estimate for the effect of privatization on spending may be a result of statistical imprecision or could be suggestive of broader consequences of drug and/or medical privatization.
Figure A.6: Lags and leads of relationship between privatization and logged total spending per enrollee

Notes: Sample years are 2007-2015. Includes state fixed effects, year fixed effects, an indicator for whether the state has expanded Medicaid under the ACA, and the share of a state’s Medicaid enrollees that are dual eligible. Observations are weighted by drug spending for the second to fourth quarters of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

Table A.3: Effect of privatization on logged total spending per enrollee

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Notes: Sample years are 2007-2015. Includes state fixed effects, year fixed effects, and a post-Medicaid expansion indicator. The transition year in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in quarter 2 through 4 of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
A.4 Impact on Emergency Department use

To investigate whether there were spillover effects of drug privatization on enrollee health or non-drug spending, we study the effect of privatization on Emergency Department (ED) usage. We downloaded quarterly data on the number of ED visits by Medicaid enrollees per quarter from the Healthcare Cost and Utilization Project (HCUP). These data are only available until the fourth quarter of 2015 and only covers four treatment states (IL, NY, ND, UT) and eight control states (ME, MO, NC, NE, SD, TN, VT, WI). Figure A.7 presents lags and leads of the privatization decision on Medicaid ED utilization. ED use is lower after privatization than beforehand, although it is unclear whether this is due to a break from or continuation of trend. The pooled point estimate, presented in Table A.4, is negative and marginally statistically significant, and the magnitude of the point estimate is economically sensitive to controlling for the Medicaid expansion. The mean of ED visits per enrollee is 0.185 in our sample, suggesting we can rule out large increases in ED use from privatization.

Figure A.7: Lags and leads of relationship between privatization and Medicaid ED visits per enrollee

Notes: Unit of observation is the state x quarter. Specification is restricted to the four treatment states (IL, NY, ND, UT) and eight control states (ME, MO, NC, NE, SD, TN, VT, WI) for which ED data was available. Includes state fixed effects and quarter fixed effects, and a post-Medicaid expansion indicator. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

40 HCUP reports more limited data from a number of other states that we exclude. We excluded IN and IA, both of which are treatment states but are treated after the end of the data. We also excluded AR, MT, and WY, all of which are control states and only had data available for very limited time periods. Finally, we exclude NY prior to the first quarter of 2011 because of a change in how Medicaid managed care enrollees are coded. Other than excluding the coding changes in NY, these restrictions do not affect the results.
Table A.4: Effect of Privatization on Medicaid ED visits per enrollee

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Notes: Unit of observation is the state x quarter. Specification is restricted to the four treatment states (IL, NY, ND, UT) and eight control states (ME, MO, NC, NE, SD, TN, VT, WI) for which ED data was available. Unreported controls includes state fixed effects and quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01

A.5 MCO Profitability

We investigate the effect of privatization on MCO profits using data from the National Association of Insurance Commissioners (NAIC). These data include enrollee months, claims expenses, and premiums for Medicaid MCO enrollees. The NAIC data includes medical spending on dual eligibles in MCOs, even though these enrollees were not affected by drug privatization, which limits our power to detect any effects of drug benefit privatization. NAIC does not include data on enrollees in less comprehensive privately administered benefits, such as PCCMs. Such revenue sources could be meaningful relative to our magnitudes - PCCMs are often reimbursed roughly three dollars per member month to cover administration (Kaiser Family Foundation [2012]). Relatedly, the data do not include administrative costs and therefore does not allow us to calculate MCO margins inclusive of these costs. We eliminate Delaware because the regulators of Medicaid managed care in Delaware does not submit data to NAIC.41 Due to data availability, we limit the sample to 2007 through 2014.

As a first exercise, we examine the Medicaid MCO margins, defined as the difference between premiums and claims expenses as a percent of claims expenses. Figure A.8 illustrates that these move over time, but are typically between 0.10 and 0.20 in a year. In the future, federal regulations will mandate that if Medicaid margins are above 0.15, then MCOs will have to return the difference to states. Thus, it is likely that states have captured or will capture at least some of the savings generated by MCOs.

As discussed in the prior section, our central point estimate suggests that MCOs lower drug spending by

41 A similar issue affects California and Arizona, two states that are not in sample. We also eliminate years of data for Connecticut and for Utah in which changes in the use of MCOs seem to lead to outliers caused by the timing in which data elements are recorded; for example, when Connecticut stopped using MCOs, there is a year in which claims are reported, but no revenues or enrollees.
roughly 85 dollars per Medicaid enrollee per year or by roughly 1.2 percent. Finally, note that drug benefit privatization could affect MCO profitability for a number of reasons, such as spillovers between medical and drug benefits. Furthermore, drug benefit privatization is sometimes concurrent with medical benefit privatization.

With these numbers in mind, we estimated the relationship between MCO profitability and privatization. We considered three measures of profitability. First, MCO profits per Medicaid enrollee in state gives the ratio of revenues minus claims and total state Medicaid enrollment (i.e., regardless of dual eligibility or enrollment in a MCO). We use this measure because it is comparable to state spending per enrollee measures. Note that states without MCOs will have zeros for this measure, and if MCOs are on average profitable, this measure will increase when states switch from public to private administration of medical benefits. We therefore also perform a second analysis where MCO profits per enrollee remains the dependent variable, but where we restrict attention to states that always have privately administered medical benefits. Finally, we consider the effect on MCO margins. Note that all except the first of these three measures are defined only for states with Medicaid MCOs and therefore analyses with those dependent variables are restricted to the states that always have privately administered medical benefits.
Figure A.9: Effect of rebate law change on states already privatizing drug benefits

Notes: Specification is limited to states which had already privatized a substantial share of drug benefits prior to 2010 and to control states with no MCO drug benefits, but excludes Delaware. Sample years are 2007 through 2014. Includes state fixed effects and year fixed effects, an indicator for whether the state has expanded Medicaid under the ACA, and the share of a state’s Medicaid enrollees that are dual eligible. Observations are weighted by 2007 Medicaid enrollment. Point estimates for the time varying effect of each year for treatment and control states are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, are presented with the dotted lines.

Figure A.9 presents the effect of lags and leads of privatization on these variables. Table A.5 presents pooled estimates. There is suggestive evidence that MCO profits per Medicaid enrollee increase following privatization, although the pooled estimate in Panel A is not statistically significant at conventional levels. Panel B limits the sample to just those states with privately administered medical benefits prior to drug privatization, and the point estimate shrinks meaningfully. The point estimate in Panel B of 44 dollars is roughly half of the decrease in drug spending, and therefore implies that MCOs are capturing roughly half of the decrease and that states are capturing the other half. The point estimate is not, however, statistically significant: we cannot rule out the possibility that the MCO captures none of the decrease in spending or all of the decrease in spending. In Panel C, there is no evidence that margins increase post Medicaid drug benefit privatization, although the estimates are again imprecise. The point estimate suggests margins decrease by roughly 3 percent following drug privatization, although at the top of the 95 percent confidence interval we are unable to rule out a 3 percent increase.
Table A.5: Relationship between privatization and MCO profitability

Panel A: Dep var = MCO profits per Medicaid enrollee in state

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Panel B: Dep var = MCO profits per Medicaid enrollee in state
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Panel C: Dep var = MCO margins
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Notes: Specification is limited to states which had already privatized a substantial share of drug benefits prior to 2010 and to states that that privatize for drug benefits and control states with no MCO drug benefits, but excludes Delaware. Sample years are 2007 through 2014. Unreported controls includes state FE's and year FE's. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. * 0.10 ** 0.05 *** 0.01
A.6 Moral Hazard

We obtain data on 160 molecule-generic combinations for which Einav et al. [2018] published their price elasticities. We create a sample where the unit of observation is the state-molecule-generic status-quarter for these drugs. Drug utilization patterns are quite different for Medicare and Medicaid patients. For example, within the sample of 160 drugs, Atorvastatin Calcium (a statin) is roughly 10 percent of Medicare spending, but only 1 percent of Medicaid spending. By contrast, Quetiapine Fumarate (an antipsychotic) is nearly 9 percent of Medicaid spending within this sample, but is only 1 percent of Medicare spending. The correlation in spending shares is under 0.06, and only roughly a quarter of Medicaid spending is on these drugs. As a result, the estimates on this subsample of Medicaid spending will not perfectly match our central estimates. Furthermore, demand elasticities could differ across these two populations. We proceed nonetheless.

We split drugs into “more elastic” and “less elastic” samples by the revenue weighted median of these drugs (the results are similar when we use a continuous measure of elasticity). For each observation, we calculate the number of prescriptions per Medicaid enrollee, the Medicaid price per prescription, and cost sharing per prescription. We examine the effect of privatization on each of these dependent variables. We also include interactions between privatization and four drug categories: (i) whether the drug is a generic, (ii) whether the drug is relatively price elastic, (iii) whether it is a maintenance drug, and (iv) whether the drug treats a chronic condition.

Tables A.6-A.8 present results from this analysis. Table A.6 presents additional evidence that the point-of-sale price per prescription decreases after privatization, but that prices for generics do not decrease. More elastic drugs have slightly larger price decreases. There is little heterogeneity in the effect of privatization on point-of-sale price per prescription for the remaining drug categories.

Turning to Table A.7, there is no relationship between privatization and cost sharing per prescription, either on average or for any of these specific drug types. In particular, it does not appear that Medicaid MCOs are raising cost sharing on drugs for which demand is particularly price elastic. Given the limited role of cost sharing, this is unsurprising. In Table A.8, we examine the relationship between privatization and logged prescriptions per enrollee for each of these drugs. The interaction term between privatization and being elastic is near zero and statistically insignificant. There is some evidence (although no longer statistically significant) that use of generics increases after privatization. Furthermore, there is marginally
Table A.6: Reduced Form relationship between *Price per prescription*, privatization, and Einav, Finkelstein, Polykova’s drug elasticities

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Notes: Unit of observation is the NDC-state-quarter. Unreported controls include NDC-state fixed effects and NDC-quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by drug spending for each NDC-state combination in the first quarter in which it appears in the data. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Table A.7: Reduced Form relationship between Cost sharing per prescription, privatization, and Einav, Finkelstein, Polykova’s drug elasticities

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Notes: Unit of observation is the NDC-state-quarter. Unreported controls include NDC-state fixed effects and NDC-quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by drug spending for each NDC-state combination in the first quarter in which it appears in the data. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Table A.8: Reduced Form relationship between logged *Prescriptions per enrollee*, privatization, and Einav, Finkelstein, Polykova’s drug elasticities

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<td>0.0352</td>
<td>[0.0401]</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>[0.0584]</td>
</tr>
<tr>
<td>(<em>Medicaid expansion</em>)1(<em>Maintenance</em>)</td>
<td></td>
<td></td>
<td>0.0283</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>[0.0878]**</td>
</tr>
<tr>
<td>(<em>Medicaid expansion</em>)1(<em>Chronic</em>)</td>
<td></td>
<td></td>
<td>0.204</td>
<td></td>
</tr>
</tbody>
</table>

N  67,527  67,527  67,527  67,527

Notes: Unit of observation is the NDC-state-quarter. Unreported controls include NDC-state fixed effects and NDC-quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by drug spending for each NDC-state combination in the first quarter in which it appears in the data. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
statistically significant evidence that use of maintenance drugs (which likely overlap with our high offset sample) increases after privatization.

Overall, we conclude that Medicaid MCOs do not increase cost sharing of price elastic drugs to control spending; the mechanism underlying the spending reductions that we observe differ from the one identified in Einav et al. [2018].
### Table A.9: State identities by transition type

<table>
<thead>
<tr>
<th>(medical / drug)</th>
<th>t=T Public/Public</th>
<th>Private/Public</th>
<th>Private/Private</th>
</tr>
</thead>
<tbody>
<tr>
<td>t=0 Public/Public</td>
<td>AK, AL, AR, ID, ME, MT, NC, OK, SD, VT, WY</td>
<td>-</td>
<td>IA, LA, MS, NH, ND</td>
</tr>
<tr>
<td>Private/Public</td>
<td>CT</td>
<td>NE, MO, WI, TN</td>
<td>IL, UT, TX, WV, NY, IN, DE, OH</td>
</tr>
<tr>
<td>Private/Private</td>
<td>-</td>
<td>-</td>
<td>AZ, CA, CO, DC, FL, GA, HI, KS, KY, MA, MD, MI, MN, NJ, NM, NV, OR, PA, RI, SC, VA, WA</td>
</tr>
</tbody>
</table>

Notes: States are classified as private if a significant number of Medicaid enrollees are in MCOs that bear financial risk for enrollees’ medical spending. VT is classified as Public/Public, because enrollees are in a state run MCO.
Table A.10: Effect of increased generic utilization on drug spending per enrollee

<table>
<thead>
<tr>
<th>Drug type</th>
<th>$P$</th>
<th>$Q$</th>
<th>$\triangle Q$</th>
<th>$%\Delta spend per enrollee$ (Version 1)</th>
<th>$%\Delta spend per enrollee$ (Version 2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>branded with no generic equivalent</td>
<td>248</td>
<td>0.162</td>
<td>-0.0526</td>
<td>-18.26%</td>
<td>-7.51%</td>
</tr>
<tr>
<td>branded with a generic equivalent</td>
<td>102</td>
<td>0.177</td>
<td>-0.0247</td>
<td>-3.53%</td>
<td>-3.53%</td>
</tr>
<tr>
<td>generic</td>
<td>20</td>
<td>0.661</td>
<td>0.0773</td>
<td>2.16%</td>
<td>2.16%</td>
</tr>
<tr>
<td>Overall</td>
<td></td>
<td></td>
<td></td>
<td>-19.62%</td>
<td>-8.87%</td>
</tr>
</tbody>
</table>

Notes: The average point-of-sale prices ($P$), average quantity shares ($Q$), and changes in quantities ($\triangle Q$) are based upon the summary statistics for treatment states in Table 2 and the point estimates in Table 4. Version 1 of the calculation assumes that shifts in demand from privatization are for average priced drugs within three groups of drugs: (i) branded drugs with no generic equivalents, (ii) branded drugs with a generic equivalent, and (iii) generic drugs. Increased use of generics has a large effect on spending because of the large point-of-sale price differentials between branded and generic drugs. As a result, the calculate is sensitive to assumptions about the prices of the branded drugs that are being shifted to generic. Version 2, our preferred calculation, assumes that branded drugs with no generic equivalent that are substituted for with a generic are priced like branded drugs that have a generic.
Table A.11: Decomposition of source of decreases in point-of-sale price per prescription

<table>
<thead>
<tr>
<th></th>
<th>NDC 9 (1)</th>
<th>molecule-generic (2)</th>
<th>molecule (3)</th>
<th>VA class-generic (4)</th>
<th>VA class (5)</th>
<th>All prescriptions (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share MCO</td>
<td>-0.0835</td>
<td>-0.0178</td>
<td>-0.0255</td>
<td>-0.0398</td>
<td>-0.107</td>
<td>-0.0553</td>
</tr>
<tr>
<td></td>
<td>[0.0247]***</td>
<td>[0.0105]</td>
<td>[0.0171]</td>
<td>[0.0371]</td>
<td>[0.0503]**</td>
<td>[0.0289]*</td>
</tr>
</tbody>
</table>

Notes: To determine why point-of-sale price per prescription decreases, we reprice the prescriptions using coarser and coarser drug classifications. Each column presents the contribution of further repricing to the overall effect of full privatization on point-of-sale price per prescription. For example, column (1) implies that the effect of full privatization on prices would have been 8.4 percentage points smaller if there was no price variation within 9 digit NDC. The coefficient in column (2) implies that the effect of full privatization on prices would have been 1.8 percentage points smaller if there was no price variation within molecule-generic versus molecule. The large coefficients in columns (1) and (5) imply that privatization is lowering point-of-sale prices by lowering prices for identical 9 digit NDCs and by substituting generics within VA class but across molecule. Drug sample is limited to drugs matched to both the FDA and VA data. Unit of observation is the state-quarter. Unreported controls includes state fixed effects and quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. * 0.10 ** 0.05 *** 0.01

Table A.12: Heterogeneity in IV effect of full privatization on high offset vs non-high offset drugs

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Non-high offset (1)</th>
<th>High offset (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(Spending per enrollee)</td>
<td>-0.304</td>
<td>-0.126</td>
</tr>
<tr>
<td></td>
<td>[0.129]**</td>
<td>[0.100]</td>
</tr>
<tr>
<td>ln(Prescriptions per enrollee)</td>
<td>0.0642</td>
<td>0.155</td>
</tr>
<tr>
<td></td>
<td>[0.0651]</td>
<td>[0.0688]**</td>
</tr>
<tr>
<td>ln(Price per prescription)</td>
<td>-0.368</td>
<td>-0.281</td>
</tr>
<tr>
<td></td>
<td>[0.0870]**</td>
<td>[0.0574]**</td>
</tr>
<tr>
<td>ln(Utilization per enrollee)</td>
<td>-0.242</td>
<td>-0.00827</td>
</tr>
<tr>
<td></td>
<td>[0.130]*</td>
<td>[0.107]</td>
</tr>
<tr>
<td>Generic accessibility</td>
<td>0.0589</td>
<td>0.0487</td>
</tr>
<tr>
<td></td>
<td>[0.0247]**</td>
<td>[0.0120]**</td>
</tr>
<tr>
<td>Generic efficiency</td>
<td>0.0506</td>
<td>0.0189</td>
</tr>
<tr>
<td></td>
<td>[0.0187]**</td>
<td>[0.0139]</td>
</tr>
<tr>
<td>Generic penetration</td>
<td>0.0918</td>
<td>0.0556</td>
</tr>
<tr>
<td></td>
<td>[0.0342]**</td>
<td>[0.0118]**</td>
</tr>
</tbody>
</table>

Notes: Specifications are limited to treatment states that privatize drug benefits and control states with no MCO drug benefits. Unreported controls includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Table A.13: Heterogeneity in IV effect of full privatization by whether a drug is in one of Medicare’s 6 protected class and by orphan drug status

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Any protected class</th>
<th>Not protected class</th>
<th>Orphan</th>
<th>Not Orphan</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>( \ln(\text{drug spending per enrollee}) )</td>
<td>-0.0328</td>
<td>-0.320</td>
<td>-0.117</td>
<td>-0.269</td>
</tr>
<tr>
<td></td>
<td>[0.101]</td>
<td>[0.124]**</td>
<td>[0.119]</td>
<td>[0.117]**</td>
</tr>
<tr>
<td>( \ln(\text{prescriptions per enrollee}) )</td>
<td>0.138</td>
<td>0.0816</td>
<td>0.163</td>
<td>0.0781</td>
</tr>
<tr>
<td></td>
<td>[0.0782]*</td>
<td>[0.0639]</td>
<td>[0.0564]**</td>
<td>[0.0643]</td>
</tr>
<tr>
<td>( \ln(\text{price per prescription}) )</td>
<td>-0.171</td>
<td>-0.402</td>
<td>-0.280</td>
<td>-0.347</td>
</tr>
<tr>
<td></td>
<td>[0.0639]**</td>
<td>[0.0821]***</td>
<td>[0.0930]***</td>
<td>[0.0710]***</td>
</tr>
<tr>
<td>( \ln(\text{drug utilization per enrollee}) )</td>
<td>0.0258</td>
<td>-0.223</td>
<td>-0.0528</td>
<td>-0.178</td>
</tr>
<tr>
<td></td>
<td>[0.104]</td>
<td>[0.124]*</td>
<td>[0.104]</td>
<td>[0.123]</td>
</tr>
<tr>
<td>Generic accessibility</td>
<td>0.0532</td>
<td>0.0558</td>
<td>0.0141</td>
<td>0.0580</td>
</tr>
<tr>
<td></td>
<td>[0.0181]***</td>
<td>[0.0160]***</td>
<td>[0.0118]</td>
<td>[0.0179]***</td>
</tr>
<tr>
<td>Generic efficiency</td>
<td>0.0332</td>
<td>0.0434</td>
<td>0.0204</td>
<td>0.0439</td>
</tr>
<tr>
<td></td>
<td>[0.0135]**</td>
<td>[0.0168]**</td>
<td>[0.0143]</td>
<td>[0.0168]**</td>
</tr>
<tr>
<td>Generic penetration</td>
<td>0.0668</td>
<td>0.0825</td>
<td>0.0274</td>
<td>0.0848</td>
</tr>
<tr>
<td></td>
<td>[0.0206]***</td>
<td>[0.0249]***</td>
<td>[0.0185]</td>
<td>[0.0262]***</td>
</tr>
</tbody>
</table>

Notes: Unreported controls includes state fixed effects and quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010.
Standard errors are clustered by state. * 0.10 ** 0.05 *** 0.01

Table A.14: Effect of privatization on rebate share

<table>
<thead>
<tr>
<th></th>
<th>Reduced Form</th>
<th>IV</th>
<th>Reduced Form</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>( \text{Priv} )</td>
<td>-0.0107</td>
<td>0.0106</td>
<td>[0.0503]</td>
<td>[0.0361]</td>
</tr>
<tr>
<td>( \text{Share MCO} )</td>
<td>-0.0174</td>
<td>0.0175</td>
<td>[0.0838]</td>
<td>[0.0578]</td>
</tr>
<tr>
<td>( \text{Medicaid expansion} )</td>
<td>-0.116</td>
<td>-0.117</td>
<td>[0.0372]***</td>
<td>[0.0362]***</td>
</tr>
<tr>
<td>N</td>
<td>162</td>
<td>162</td>
<td>162</td>
<td>162</td>
</tr>
</tbody>
</table>

Notes: Unreported controls include state fixed effects and year fixed effects. The transition year in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in quarter 2 through 4 of 2010.
Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Table A.15: Privatizing State Characteristics for Heterogeneity Analysis

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>MS</td>
<td>2011Q2</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>NY</td>
<td>2011Q4</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>1</td>
</tr>
<tr>
<td>OH</td>
<td>2011Q4</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>2</td>
</tr>
<tr>
<td>TX</td>
<td>2012Q1</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>2</td>
</tr>
<tr>
<td>IL</td>
<td>2012Q3</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>LA</td>
<td>2012Q3</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>1</td>
</tr>
<tr>
<td>UT</td>
<td>2013Q1</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>WV</td>
<td>2013Q2</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>0</td>
</tr>
<tr>
<td>NH</td>
<td>2013Q4</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>ND</td>
<td>2014Q1</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>0</td>
</tr>
<tr>
<td>DE</td>
<td>2015Q1</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>0</td>
</tr>
<tr>
<td>IN</td>
<td>2015Q1</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>IA</td>
<td>2016Q2</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>0</td>
</tr>
</tbody>
</table>

Notes: States with Centene and Molina are determined by examining company annual reports. Same formulary is an indicator for whether MCOs are required to use the state’s formulary; MCOs in other states may face oversight in formulary design. We obtain this variable from the 2015 Medicaid Drug Utilization Review (MDUR) for all states, except Iowa, which had not privatized the drug benefit at that time. Based on more recent information from the Kaiser Family Foundation, Iowa requires MCOs to follow the state formulary. No prior MCO is an indicator for whether a state concurrently privatized medical and drug benefits or already had privatized medical benefits prior to privatizing drug benefits. No risk adj is an indicator for whether a state risk adjusts MCO payments based upon enrollee health status is based using responses in Gifford et al. (2011). Based on supplemental internet, all that privatized medical benefits after 2010 risk adjust based upon health. We measure the presence of an any willing pharmacy law as data from the National Conference of State Legislatures.
Table A.16: Reduced Form relationship between privatization and point-of-sale prices
(heterogeneity by whether MCOs must use state formulary)

<table>
<thead>
<tr>
<th></th>
<th>Dep var = Medicaid price per prescription</th>
<th>Dep var = Cost sharing per prescription</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>$Priv$</td>
<td>-11.46</td>
<td>2.846</td>
</tr>
<tr>
<td></td>
<td>[2.808]**</td>
<td>[1.998]</td>
</tr>
<tr>
<td>$Priv \times 1(\text{High offset})$</td>
<td>0.108</td>
<td>1.559</td>
</tr>
<tr>
<td></td>
<td>[3.029]</td>
<td>[1.759]</td>
</tr>
<tr>
<td>$Priv \times 1(\text{Generic})$</td>
<td>10.21</td>
<td>-3.069</td>
</tr>
<tr>
<td></td>
<td>[3.417]**</td>
<td>[2.149]</td>
</tr>
<tr>
<td>$\text{Medicaid expansion}$</td>
<td>-0.593</td>
<td>1.945</td>
</tr>
<tr>
<td></td>
<td>[2.350]</td>
<td>[1.946]</td>
</tr>
<tr>
<td>$(\text{Medicaid expansion}) \times 1(\text{High offset})$</td>
<td>-0.0728</td>
<td>-1.310</td>
</tr>
<tr>
<td></td>
<td>[1.789]</td>
<td>[1.234]</td>
</tr>
<tr>
<td>$(\text{Medicaid expansion}) \times 1(\text{Generic})$</td>
<td>0.247</td>
<td>-1.413</td>
</tr>
<tr>
<td></td>
<td>[2.369]</td>
<td>[1.816]</td>
</tr>
<tr>
<td>$Priv \times 1(\text{Same formulary})$</td>
<td>1.383</td>
<td>-5.605</td>
</tr>
<tr>
<td></td>
<td>[4.517]**</td>
<td>[1.545]**</td>
</tr>
<tr>
<td>$Priv \times 1(\text{Same formulary}) \times 1(\text{High offset})$</td>
<td>-0.989</td>
<td>-1.420</td>
</tr>
<tr>
<td></td>
<td>[4.313]**</td>
<td>[1.530]**</td>
</tr>
<tr>
<td>$Priv \times 1(\text{Same formulary}) \times 1(\text{Generic})$</td>
<td>-3.295</td>
<td>5.530</td>
</tr>
<tr>
<td></td>
<td>[4.917]**</td>
<td>[1.748]**</td>
</tr>
<tr>
<td>N</td>
<td>3,220,610</td>
<td>3,220,610</td>
</tr>
</tbody>
</table>

Notes: Unit of observation is the NDC-state-quarter. Includes NDC-state fixed effects, NDC-quarter fixed effects, and a post-Medicaid expansion indicator. The transition quarter in which a state privatizes is omitted. Observations are weighted by drug spending for each NDC-state combination in the first quarter in which it appears in the data. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
<table>
<thead>
<tr>
<th></th>
<th>Share MCO</th>
<th>Share MCO</th>
<th>Share MCO</th>
<th>Share MCO</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>*1(Same formulary)</td>
<td>*1(No prior MCO)</td>
<td>*1(No risk adj)</td>
<td></td>
</tr>
<tr>
<td>Priv</td>
<td>0.685</td>
<td>0.0212</td>
<td>-0.00730</td>
<td>-0.0228</td>
</tr>
<tr>
<td></td>
<td>[0.0654]***</td>
<td>[0.0191]</td>
<td>[0.0113]</td>
<td>[0.0205]</td>
</tr>
<tr>
<td>Priv*1(Same formulary)</td>
<td>0.0563</td>
<td>0.685</td>
<td>0.0110</td>
<td>0.00647</td>
</tr>
<tr>
<td></td>
<td>[0.0516]</td>
<td>[0.0609]***</td>
<td>[0.0224]</td>
<td>[0.0181]</td>
</tr>
<tr>
<td>Priv*1(No prior MCO)</td>
<td>-0.0898</td>
<td>-0.0200</td>
<td>0.587</td>
<td>0.0276</td>
</tr>
<tr>
<td></td>
<td>[0.0724]</td>
<td>[0.0415]</td>
<td>[0.0472]***</td>
<td>[0.0202]</td>
</tr>
<tr>
<td>Priv*1(No risk adj)</td>
<td>-0.412</td>
<td>-0.114</td>
<td>-0.0128</td>
<td>0.350</td>
</tr>
<tr>
<td></td>
<td>[0.0507]***</td>
<td>[0.0882]</td>
<td>[0.0166]</td>
<td>[0.0165]***</td>
</tr>
<tr>
<td>Medicaid expansion</td>
<td>0.0629</td>
<td>-0.0200</td>
<td>-0.00566</td>
<td>0.0200</td>
</tr>
<tr>
<td></td>
<td>[0.0343]*</td>
<td>[0.0181]</td>
<td>[0.0189]</td>
<td>[0.0334]</td>
</tr>
<tr>
<td>N</td>
<td>741</td>
<td>741</td>
<td>741</td>
<td>741</td>
</tr>
</tbody>
</table>

Notes: Includes state fixed effects and quarter fixed effects. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Table A.18: Heterogeneity in effect of privatization based on presence of Molina and Centene

Panel A: Reduced Form

<table>
<thead>
<tr>
<th></th>
<th>share MCO</th>
<th>ln(drug spending per enrollee)</th>
<th>ln(Prescriptions per enrollee)</th>
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Panel B: IV

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Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
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Notes: Unreported controls includes state FEs and quarter FEs. Column (2) excludes Indiana and North Dakota because they privatized drug benefits and expanded Medicaid at the same time. Column (5) includes both expansion and non-expansion states, but excludes states from the sample after they expand Medicaid. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. * 0.10 ** 0.05 *** 0.01
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<th>Limit to states with private medical at start of sample</th>
<th>Limit to states that risk adjust above median drug spending in 2010Q2</th>
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Notes: Unreported controls include state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
### Table A.21: IV estimate of effect of full privatization (further robustness checks)

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<td>[0.0378]**</td>
<td>[0.0269]**</td>
</tr>
<tr>
<td>( \text{Share high offset} )</td>
<td>0.0544</td>
<td>0.0539</td>
<td>0.0663</td>
<td>0.0493</td>
</tr>
<tr>
<td></td>
<td>[0.00927]***</td>
<td>[0.00989]***</td>
<td>[0.0117]***</td>
<td>[0.0130]***</td>
</tr>
</tbody>
</table>

Notes: Unreported controls includes state FEs and quarter FEs. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. In column (2), we run a regression with state and year fixed effects on the dependent variable, and obtain predicted residuals. We then replace any residuals below the first percentile of residuals with the first percentile and any residuals above the 99th percentile of residuals with the 99th percentile of residuals. The winsorized version of our variable is then the predicted values from this regression plus these winsorized residuals. * 0.10 ** 0.05 *** 0.01
### Table A.22: IV estimate of effect of full privatization (robustness of results to excluding states one at a time)

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Full sample (1)</th>
<th>DE (2)</th>
<th>IA (3)</th>
<th>IL (4)</th>
<th>IN (5)</th>
<th>LA (6)</th>
<th>MS (7)</th>
<th>ND (8)</th>
<th>NH (9)</th>
<th>NY (10)</th>
<th>OH (11)</th>
<th>TX (12)</th>
<th>UT (13)</th>
<th>WV (14)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln(Spending)</td>
<td>-0.239</td>
<td>-0.242</td>
<td>-0.230</td>
<td>-0.218</td>
<td>-0.231</td>
<td>-0.258</td>
<td>-0.240</td>
<td>-0.233</td>
<td>-0.174</td>
<td>-0.257</td>
<td>-0.340</td>
<td>-0.238</td>
<td>-0.226</td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.117]**</td>
<td>[0.117]**</td>
<td>[0.117]**</td>
<td>[0.117]**</td>
<td>[0.117]**</td>
<td>[0.116]**</td>
<td>[0.117]**</td>
<td>[0.115]**</td>
<td>[0.124]**</td>
<td>[0.101]*****</td>
<td>[0.117]**</td>
<td>[0.115]**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ln(Prescriptions)</td>
<td>0.0865</td>
<td>0.0872</td>
<td>0.0951</td>
<td>0.120</td>
<td>0.0920</td>
<td>0.103</td>
<td>0.0721</td>
<td>0.0863</td>
<td>0.0929</td>
<td>0.108</td>
<td>0.0865</td>
<td>0.0388</td>
<td>0.0853</td>
<td>0.0917</td>
</tr>
<tr>
<td></td>
<td>[0.0626]</td>
<td>[0.0631]</td>
<td>[0.0625]</td>
<td>[0.0535]**</td>
<td>[0.0641]</td>
<td>[0.0634]</td>
<td>[0.0641]</td>
<td>[0.0626]</td>
<td>[0.0633]</td>
<td>[0.0876]</td>
<td>[0.0676]</td>
<td>[0.0594]</td>
<td>[0.0633]</td>
<td>[0.0823]</td>
</tr>
<tr>
<td>ln(Price per</td>
<td>-0.325</td>
<td>-0.329</td>
<td>-0.325</td>
<td>-0.338</td>
<td>-0.323</td>
<td>-0.327</td>
<td>-0.330</td>
<td>-0.326</td>
<td>-0.326</td>
<td>-0.343</td>
<td>-0.379</td>
<td>-0.323</td>
<td>-0.317</td>
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</tr>
<tr>
<td>prescription)</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
<td>[0.0789]**</td>
</tr>
<tr>
<td>ln(Utilization)</td>
<td>-0.152</td>
<td>-0.154</td>
<td>-0.147</td>
<td>-0.130</td>
<td>-0.147</td>
<td>-0.0984</td>
<td>-0.169</td>
<td>-0.152</td>
<td>-0.147</td>
<td>-0.0793</td>
<td>-0.161</td>
<td>-0.268</td>
<td>-0.155</td>
<td>-0.136</td>
</tr>
<tr>
<td>per enrollee)</td>
<td>[0.117]</td>
<td>[0.119]</td>
<td>[0.119]</td>
<td>[0.120]</td>
<td>[0.118]</td>
<td>[0.111]</td>
<td>[0.119]</td>
<td>[0.117]</td>
<td>[0.119]</td>
<td>[0.135]</td>
<td>[0.128]</td>
<td>[0.0855]**</td>
<td>[0.119]</td>
<td>[0.116]</td>
</tr>
<tr>
<td>Generic</td>
<td>0.0526</td>
<td>0.0535</td>
<td>0.0522</td>
<td>0.0553</td>
<td>0.0555</td>
<td>0.0497</td>
<td>0.0530</td>
<td>0.0528</td>
<td>0.0537</td>
<td>0.0302</td>
<td>0.0572</td>
<td>0.0606</td>
<td>0.0535</td>
<td>0.0514</td>
</tr>
<tr>
<td>accessibility</td>
<td>[0.0408]</td>
<td>[0.0411]</td>
<td>[0.0415]</td>
<td>[0.0401]</td>
<td>[0.0424]</td>
<td>[0.0345]</td>
<td>[0.0411]</td>
<td>[0.0407]</td>
<td>[0.0411]</td>
<td>[0.0272]</td>
<td>0.0419</td>
<td>0.0581</td>
<td>0.0416</td>
<td>0.0407</td>
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<tr>
<td>Generic</td>
<td>0.152</td>
<td>0.154</td>
<td>0.147</td>
<td>0.130</td>
<td>0.147</td>
<td>-0.0984</td>
<td>-0.169</td>
<td>-0.152</td>
<td>-0.147</td>
<td>-0.0793</td>
<td>-0.161</td>
<td>-0.268</td>
<td>-0.155</td>
<td>-0.136</td>
</tr>
<tr>
<td>efficiency</td>
<td>[0.0861]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
<td>[0.0868]**</td>
</tr>
<tr>
<td>Generic</td>
<td>0.0773</td>
<td>0.0782</td>
<td>0.0776</td>
<td>0.0783</td>
<td>0.0811</td>
<td>0.0695</td>
<td>0.0778</td>
<td>0.0773</td>
<td>0.0784</td>
<td>0.0492</td>
<td>0.0816</td>
<td>0.0988</td>
<td>0.0787</td>
<td>0.0761</td>
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<tr>
<td>penetration</td>
<td>[0.0249]**</td>
<td>[0.0253]**</td>
<td>[0.0258]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
<td>[0.0254]**</td>
</tr>
<tr>
<td>State high offset</td>
<td>0.0544</td>
<td>0.0559</td>
<td>0.0548</td>
<td>0.0552</td>
<td>0.0525</td>
<td>0.0541</td>
<td>0.0538</td>
<td>0.0542</td>
<td>0.0544</td>
<td>0.0489</td>
<td>0.0556</td>
<td>0.0604</td>
<td>0.0554</td>
<td>0.0526</td>
</tr>
<tr>
<td></td>
<td>[0.0027]**</td>
<td>[0.0031]**</td>
<td>[0.0029]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
<td>[0.0032]**</td>
</tr>
</tbody>
</table>

Notes: Unreported controls includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Table A.23: Robustness to including states with drug MCOs prior to 2010Q2 and to using all variation in share MCO

<table>
<thead>
<tr>
<th></th>
<th>ln(drug spending per enrollee)</th>
<th>ln(Prescriptions per enrollee)</th>
<th>ln(Price per prescription)</th>
<th>ln(drug utilization per enrollee)</th>
<th>Generic accessibility</th>
<th>Generic efficiency</th>
<th>Generic penetration</th>
<th>Share high offset</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lagged share MCO</td>
<td>-0.121</td>
<td>0.133</td>
<td>-0.255</td>
<td>-0.0528</td>
<td>0.0450</td>
<td>0.0336</td>
<td>0.0647</td>
<td>0.0291</td>
</tr>
<tr>
<td></td>
<td>[0.0858]</td>
<td>[0.0468]***</td>
<td>[0.0515]***</td>
<td>[0.0853]</td>
<td>[0.0186]**</td>
<td>[0.0112]***</td>
<td>[0.0230]***</td>
<td>[0.00915]***</td>
</tr>
<tr>
<td>Medicaid expansion</td>
<td>0.0485</td>
<td>0.0826</td>
<td>-0.0340</td>
<td>0.0588</td>
<td>0.00766</td>
<td>0.0112</td>
<td>0.0169</td>
<td>-0.0247</td>
</tr>
<tr>
<td></td>
<td>[0.0569]</td>
<td>[0.0473]*</td>
<td>[0.0242]</td>
<td>[0.0510]</td>
<td>[0.00604]</td>
<td>[0.00453]**</td>
<td>[0.00605]***</td>
<td>[0.00456]***</td>
</tr>
<tr>
<td>N</td>
<td>1,174</td>
<td>1,174</td>
<td>1,174</td>
<td>1,174</td>
<td>1,174</td>
<td>1,174</td>
<td>1,174</td>
<td>1,174</td>
</tr>
</tbody>
</table>

Notes: Specification includes all states, except for DC, RI, HI, KS, and the second quarter of 2014 for VA. These are excluded because of implausible spending patterns. Unit of observation is the state-quarter. Unreported controls are state fixed effects and quarter fixed effects. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Significance levels: * 0.10 ** 0.05 *** 0.01
Table A.24: IV estimate of effect of full Medicaid privatization on Medicare
drug spending (falsification)

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>$\ln(\text{Spending per enrollee})$</td>
<td>-0.00919</td>
<td>[0.0167]</td>
</tr>
<tr>
<td>$\ln(\text{Days supply per enrollee})$</td>
<td>-0.00795</td>
<td>[0.00643]</td>
</tr>
<tr>
<td>$\ln(\text{Price per days supply})$</td>
<td>-0.00123</td>
<td>[0.0135]</td>
</tr>
<tr>
<td>$\ln(\text{Utilization per enrollee})$</td>
<td>-0.0141</td>
<td>[0.0139]</td>
</tr>
<tr>
<td>Generic accessibility</td>
<td>0.0110</td>
<td>[0.00496]**</td>
</tr>
<tr>
<td>Simulated generic accessibility</td>
<td>0.00943</td>
<td>[0.00432]**</td>
</tr>
<tr>
<td>Generic efficiency</td>
<td>-0.00146</td>
<td>[0.00137]</td>
</tr>
<tr>
<td>Generic penetration</td>
<td>0.00837</td>
<td></td>
</tr>
<tr>
<td>Share high offset</td>
<td>0.00430</td>
<td>[0.00196]**</td>
</tr>
</tbody>
</table>

Notes: Specifications are limited to treatment states that privatize drug benefits and control states with no MCO drug benefits. Unreported controls include state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Standard errors are clustered by state. Spending is computed for Medicare enrollees 65+ years old who never receive a low income subsidy during the years for which we have data. Data covers the second quarter of 2010 through the fourth quarter of 2014. Significance levels: *0.10 **0.05 ***0.01
Table A.25: Reduced Form Estimate of Effect of Privatization  
(Robustness of Results to calculating p-values with Wild Cluster Bootstrap)

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Original (1)</th>
<th>Wild Cluster Bootstrap (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \ln(\text{drug spending per enrollee}) )</td>
<td>-0.145</td>
<td>-0.145</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.114)</td>
</tr>
<tr>
<td>( \ln(\text{price per prescription}) )</td>
<td>-0.198</td>
<td>-0.198</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>( \ln(\text{drug utilization per enrollee}) )</td>
<td>-0.0925</td>
<td>-0.0925</td>
</tr>
<tr>
<td></td>
<td>(0.194)</td>
<td>(0.270)</td>
</tr>
<tr>
<td>\text{Generic Accessibility}</td>
<td>0.0320</td>
<td>0.0320</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>\text{Generic Efficiency}</td>
<td>0.0248</td>
<td>0.0248</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.116)</td>
</tr>
<tr>
<td>\text{Generic Penetration}</td>
<td>0.0470</td>
<td>0.0470</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>\text{Share high offset}</td>
<td>0.0331</td>
<td>0.0331</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
</tbody>
</table>

Notes: Unreported controls includes state FEs and quarter FEs. The transition quarter in which a state privatizes is omitted. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. In column (1), p-values, reported in (), are computed from standard errors clustered by state. In column (2), p-values, reported in () are computed using a wild-cluster bootstrap with 1000 repetitions, and using the null hypothesis that privatization has no effect on outcomes when resampling errors. These decisions are implemented using the Stata command cgmwildboot and mirror the baseline suggestions in Cameron, Gelbach, and Miller (2008).
Figure A.10: Price setting mechanism under public vs private administration

Public

Private

* High in Medicaid/Phm, 2017

* Moving from an administered "price" to a negotiated rate

Figure A.11: Share MCO for states beginning with some private Medicaid drug administration

Notes: Excludes DC, MA, and RI because of data limitations.
Figure A.12: Histogram of summary statistics

Notes: Unit of observation is the state-quarter for states in the second quarter of 2010.

Figure A.13: Map of states by Medicaid drug MCO status, 2010-2016

Notes: Excludes Alaska (Control) and Hawaii (Excluded).
Figure A.14: Percent of Medicaid drug spending by MCOs, privatization date, and Medicaid expansion date for treatment states

Notes: For each state, the dashed red line accompanied by a P indicates the date upon which we record the state as beginning to privatize their drug benefit. The dotted blue line accompanied by a E indicates the date upon which the state expanded Medicaid. The y-axis shows the share of drug spending by MCOs.
Figure A.15: Lags and leads of relationship between privatization and $\ln(\text{drug utilization per enrollee})$

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, are presented with the dotted lines.

Figure A.16: Lags and leads of relationship between privatization and generic efficiency

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.
Figure A.17: Lags and leads of relationship between privatization and generic accessibility

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

Figure A.18: Lags and leads of relationship between privatization and Simulated generic accessibility

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Observations are weighted by state Medicaid drug spending in the second quarter of 2010. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.
Figure A.19: Effect of privatization for high offset drugs

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.

Figure A.20: Effect of privatization for non-high offset drugs

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.
Figure A.21: Effect of privatization
(Robustness to restricting to treatment states to 6 quarters pre- and post-privatization)

Notes: Includes state fixed effects, quarter fixed effects, and a post-Medicaid expansion indicator. Excludes Iowa and Mississippi, which have under 6 quarters of pre- and post-privatization data. Point estimates for the effect of quarter pre-post privatization are presented in the dark line and a 95 percent confidence interval for those coefficients, based upon standard errors that are clustered by state, is presented with the dotted lines.